Social Science and National Security Policy

Deterrence, Coercion, and Modernization Theories

Janeen M. Klinger

palgrave
macmillan

Social Science and National Security Policy

Janeen M. Klinger

Social Science and National Security Policy

Deterrence, Coercion, and Modernization Theories

palgrave
macmillan

Janeen M. Klinger
US Army War College
Carlisle, PA, USA

ISBN 978-3-030-11250-9 ISBN 978-3-030-11251-6 (eBook)
https://doi.org/10.1007/978-3-030-11251-6

Library of Congress Control Number: 2018968358

This Palgrave Macmillan imprint is published by the registered company Springer Nature Switzerland AG
The registered company address is: Gewerbestrasse 11, 6330 Cham, Switzerland

For the generation that came of age during the Vietnam War

Acknowledgments

I owe an intellectual debt to so many scholars and colleagues that it is impossible to list them all. However, I would be remiss if I did not recognize the role of some key individuals. I thank the US Army War College for the sabbatical year that allowed me to initiate this project. I also thank my good colleague Frank Jones for his substantive insights and his editorial suggestions. Thanks also goes to the chair of the Department of National Security and Strategy, Mark Duckenfield, for his support. For some sound editorial advice, I thank Walter McDougall and Karen Rood. Finally, I thank my mentor, the late Paul Seabury, for inspiring my interest in American National Security Policy. It goes without saying that any shortcomings in this book are all my own.

Contents

CHAPTER 1

Evolution of a Partnership: Social Science and National Security Policy

Numerous contemporary commentators have observed that there is a gap that might accurately be described as an estrangement between academic social scientists and policy-makers—especially those charged with national security policy. Indeed, some scholars have asserted that the gap between the two worlds is growing wider.[1] Consequently, there are frequent calls for social scientists to become more involved with policy communities by conducting research of greater practical application. Robert Putnam's 2002 presidential address to the American Political Science Association that urged political scientists to have a greater public presence is but one example of such calls.[2] This issue concerning the academic/policy divide is, of course, a subset of the larger questions concerning the nature of the relationship between knowledge and power, theory and practice, and ideas and action. While there is compelling logic for encouraging efforts to bridge this gap, we need to recognize the problematic nature of doing so. We might best accomplish this by examining the emergence of the symbiotic relationship that grew up between policy-makers and social scientists in the early days of the Cold War and tracing the impact of this relationship. Indeed, even as that symbiotic relationship was forged, it generated controversy leading to congressional hearings like those convened in the aftermath of diplomatic fallout associated with "Project Camelot."[3] Yet, for a variety of reasons, including the changes in the strategic environment after World War II, the influence of social scientists on national security policy grew, and the early Cold War has been characterized as the heyday for such an influence.

J. M. Klinger, *Social Science and National Security Policy*,
https://doi.org/10.1007/978-3-030-11251-6_1

The problematic nature of the close relationship between social science and policy-makers is well demonstrated through an examination of two social science frameworks that came to be reflected in national security policy in that era. The two conceptual frameworks that will be discussed in detail in subsequent chapters are deterrence/coercion theory and modernization theory.[4] The incorporation of these two frameworks in policy offers a cautionary tale concerning potential hazards of drawing directly on social science for national security policy. Indeed, David Easton once likened political science to medieval medical practices and raised the question of whether its use in policy might do more harm than good.[5] Both frameworks, as we shall see, can be implicated in the strategy that the United States used in its prosecution of the war in Vietnam. At the same time that the story of the incorporation of deterrence and modernization theory into policy provides a warning to policy-makers, it should stand as a sobering reminder to scholars of the limits to their theory and the potential hazards to the discipline of abandoning older approaches and terminology in the interest of scholarly innovation.

But before we can describe the aspect of each framework and their respective association to policy, we need to provide some background concerning the evolution of ties between social scientists and policy-makers to show how both groups would, for their own reasons, become attracted to a scientific approach for a US grand strategy. While this book includes a variety of disciplines under the general category of "social science," we do recognize a difference among the disciplines in terms of their receptivity to the generalizing potential of "science." We can therefore distinguish between anthropology and history with their greater tendency to contextualize their analysis in specific circumstances and the more universalizing discipline of economics. Perhaps political science and sociology might be viewed appropriately as splitting the difference and containing research strands reflecting each tendency, and therefore were more prone to reflect what I have labeled a conflicted identity.[6]

To begin with, the development of any ties between social scientists and policy-makers needed to overcome the fact that each group inhabited different professional environments requiring different organizational cultures to perform their respective tasks. Academic social scientists tend to pursue knowledge for its own sake to enhance their disciplines. Such analytical work means abstraction is a virtue that may require long time horizons to complete. Moreover, because collaboration is not required, research can easily be conducted within a horizontal organizational structure. In addition, the task

of furthering disciplinary knowledge demands specialization that often results in jargon-laden analysis that can, at times, remain unintelligible to outsiders. Indeed, so acute is the communication problem that one scholar writing in the 1960s, those halcyon days of collaboration between social scientists and policy-makers, observed that the social sciences "have come to make almost a fetish of non-communication."[7]

For their part, the tasks of policy-makers provide a stark contrast to that of the social scientists. Concrete practical problems are frequently time-sensitive requiring action that must reconcile competing interests, thereby sacrificing the "best" solution for one that is feasible. In such cases, as Carol Weiss expressed the point, "Political rationality may eclipse scientific rationality."[8] What is more, the work of policy-makers must take place within a hierarchical organization with clear lines of authority and responsibility. Given this cultural disparity between social scientists and policy-makers, it is not surprising that when Franklin Roosevelt sought to incorporate social scientists in his "brain trust," they were caricatured in cartoons of the day as cross-eyed professors with their academic robes askew.[9]

That the specialized nature of academic social science would necessarily provide knowledge that was remote from the needs of policy-makers was even recognized by men who straddled the policy and academic divide in the 1960s when social science enjoyed its greatest influence on policy. For example, McGeorge Bundy expressed criticism of scholarship that was not useful for statesmanship or diplomacy and suggested that there was "perhaps too much, analysis aimed at scholarly rigor and scientific validity."[10] Paul Nitze echoed similar criticism when he noted that most of what had been written under the heading of political science since World War II "has been contrary to experience and common sense. It has also been of limited value, if not counterproductive as a guide to the actual conduct of policy."[11] Indeed, even such an established discipline as economics, which had proven its value in addressing the problems of the Great Depression so that it became institutionalized in the Council of Economic Advisors, suffers from a concern that its specialized knowledge may be of decreasing relevance to policy. In one survey conducted by the American Economic Association, nearly two-thirds of graduate-level economic professors considered their profession too unrelated to the real world.[12] A stronger note of caution concerning social science as a basis for policy was expressed by Paul Johnson, who asserted that both Hitler and Stalin had relied on the "inexact sciences" of economics, sociology and psychology to construct

"the juggernaut of social engineering which had crushed beneath it so much wealth and so many lives."[13]

Yet, despite such reservations concerning the value of social sciences for policy, they did come to play an important conceptual, shaping role during the early stages of the Cold War. Before describing the processes and factors that enabled the social sciences to play such a role, we must delve into some earlier history of the disciplines that helped set the conditions for their rise to prominence in the 1950s and 1960s. The starting point of this history recognizes that, relative to the natural sciences, the social sciences are fairly young disciplines, for their emergence required a detachment that became possible only when the ecclesiastical authority and the traditional belief that supported it had weakened.[14] One convenient way to mark their emergence is through the founding of their professional associations. Economics and psychology formed their professional associations at the end of the nineteenth century (American Economic Association, 1885; American Psychological Association, 1892), while anthropology, political science and sociology founded theirs in the early twentieth century (American Anthropological Association, 1902; American Political Science Association, 1903; and the American Sociological Society, 1905).[15] Moreover, in the nineteenth century, the various disciplines did not even conceive of themselves as related. For instance, psychology and anthropology felt a closer connection to biology, and political science saw itself as more closely allied to history and law.[16]

Given this relatively recent origin, it is not surprising that systematic efforts by policy-makers to use knowledge derived from the social sciences would only emerge in the twentieth century. For example, the establishment of the Bureau of the Census in 1902 created a place for social scientists to serve in the government. As such, the solidification of the social sciences corresponds roughly with the Progressive Era (1900–1918) that was characterized by various reform efforts that aimed to eliminate government corruption, regulate business practices and improve the health and working conditions for the common man. As we shall see, this Progressive Era heritage helped to lay the foundation for what may be termed a conflicted identity in the social sciences and created some tension between the normative concerns for reform and a grounding in objective and normatively neutral science.

Under the influence of Progressive Era notions, the US government took its first tentative steps toward incorporating specialized knowledge into government policy. These initial efforts focused on using the natural

sciences with the creation of the National Research Council in 1915 by President Wilson to coordinate the scientific work of academia, industry and government. Although the coordination focused more on the natural sciences, under the exigencies of World War I, the Council also provided assistance to the military in order for them to apply psychological principles to soldiers. Similar, if more systematic, work was later undertaken during World War II by the Office of Scientific Research and Development (OSRD). At the same time, social scientists recognized the need for a more rational system of economic planning and social control, as the responsibilities of the state were expanding. Beginning in the 1880s, a number of graduate schools in social science were founded for the purpose of training students for careers in the civil service. Columbia, Johns Hopkins, Stanford and the University of Chicago all inaugurated such programs.[17] In both the cases, namely, the support for the world wars and its contribution to economic management by the state, the social sciences found opportunities for influence because of dramatic social change and crisis.

While the Progressives recognized the role for social science in domestic policy a little earlier than in foreign policy, the crisis entailed by the Great War began to alter that view. Woodrow Wilson realized the value of social science for the war effort and that the diplomatic service lacked the kind of ethnic and geographic knowledge that would be needed for the peace conference at the war's end. Consequently, he had his advisor, Colonel Edward House, recruit the appropriate experts from the universities. The group, known as "The Inquiry," was supervised by Isaiah Bowman, the director of the American Geographic Society. After World War I, some efforts were made to retain the kind of planning used during the war. The director of the War Industries Board, Bernard Baruch, in his final report suggested the retention of an emergency planning body, although no action was taken on his proposal.[18] Then, in 1929, President Hoover established the Research Committee on Social Trends. Economist Wesley Mitchell chaired the committee, and Charles S. Merriam, a University of Chicago political scientist, acted as the committee's vice chair. Their two-volume report was presented to President Hoover in the fall of 1932 on the eve of Franklin D. Roosevelt's (FDR's) first administration and when the United States was in the grips of the Great Depression.[19] While the committee report addressed the issue of using social science for government policy, the economic collapse of the 1930s, which facilitated the expansion of the role for government, reinforced the conclusions

contained in the report. However, the fact that the social sciences were mobilized by FDR during the Great Depression had one unintended impact on them. The association of the social sciences with the New Deal left them politically vulnerable to conservative criticism—criticism social scientists would attempt to overcome by distancing themselves from normative reforms and moving toward scientific objectivity.

During the interwar years, the evolution of the social sciences in the direction of scientific objectivity was furthered by institutional developments. This ferment during the interwar years began laying the groundwork for a more scientific approach to social science that would be strengthened during World War II. Charles S. Merriam, with the support of funding from the Rockefeller Foundation, induced other disciplines to support the creation of the Social Science Research Council (SSRC), which brought together six fields: anthropology, sociology, political science, psychology, statistics and history. The Council was formally incorporated in 1923 and had, by the 1930s, established itself as the central national body for American social science and a major source of grants and funding for research.[20]

Merriam was an important figure shaping the scientific direction of the social sciences in his role as chairman of the SSRC from 1923 to 1927—he continued to be on the Council's board of directors until his retirement in 1948. Merriam himself was critical of the legalistic direction within his own discipline, which he saw as irrelevant to the practical concerns of politics. Thus, for example, in his day, international relations scholarship tended to be dominated by international lawyers, who, as late as the 1930s, excluded the study of military strategy because such study ran counter to the spirit of the Kellogg-Briand Pact.[21] Merriam also developed his own political science department at Chicago that came to dominate the discipline for 30 years. Yet, at the same time that Merriam championed a scientific approach to the study of politics, he never quite severed his roots from the reformist spirit of the Progressive Era. For example, as a member of Franklin Roosevelt's Resource Planning Board, he called for cradle-to-grave welfare programs.[22] In fact, he wedded the notion of a more scientific social science to the idea that it would have greater practical use for political actors.[23] Of course, not all social scientists saw a more scientific social science as useful for policy-makers. Lucian Pye, for example, observed that social science "worshipping a strangely distorted and graven image of science" had limited utility for policy and, in fact, contributed to a certain estrangement between academics and policy-makers.[24]

In some sense, Merriam provided a manifestation that illustrates the conflicted identity pervading the social sciences, although that conflicted identity was perhaps most pronounced in political science. Political science proved most effective of all the social sciences in compartmentalizing normative and empirical approaches so that the normative work of political philosophy, over time, became a virtual occupational ghetto of the discipline. Be that as it may, Merriam's legacy is long-lasting because one of his students, Gabriel Almond, became influential in the SSRC as chair of its Committee on Comparative Politics and his work—as we shall see in Chap. 4—championed the more scientific approach to politics.

The mass mobilization engendered by World War II created the crisis that brought social scientists into the war effort and really marked the beginning of their ascendance to influence on national security policy. The presence of social scientists in the government grew dramatically during the war. One civil service commission expert estimated that the number of social science positions within the US government doubled to roughly 16,000 in the first six months of the war.[25] Moreover, the World War II experience was important for shaping the conceptual direction and issues of concern for the social sciences in the postwar period. European émigré scholars brought to the United States the European tradition of the social sciences embodied in the work of people like Max Weber and Emile Durkheim, both of whom inspired the work of Talcott Parsons—another figure who was important for pushing the social sciences, in his case sociology, in a scientific direction.

Social scientists served in a number of capacities during the war. Some, like Talcott Parsons, participated through membership in university organizations like Harvard's Committee on National Morale and American Defense. Psychologists, in a carryover from some of their work during World War I, helped develop tools that would improve the selection and training of soldiers. New research tools like public opinion surveys were developed and new fields like social psychology emerged. These research innovations were captured in Samuel Stouffer's classic study, *The American Soldier* (1949)—he has noted that in World War I emphasis had been to study aptitudes, while in World War II the study was on soldier's attitudes.[26] The assessment of the impact of bombing on citizen morale as well as evaluation of propaganda campaigns drew on the expertise of psychologists and sociologists in the Foreign Morale Analysis Division of the Office of War Information (OWI). These psychological operations used in Europe were documented in Daniel Lerner's 1949 book, *Sykewar:*

Psychological Warfare Against Germany, D-Day to VE Day.[27] Social psychology was also used to analyze the factors that contributed to cohesion within the Wehrmacht to account for German resistance even in the final phases of the European war.[28]

Economists also aided the war effort and helped develop plans for rationing and other aspects of organizing the wartime economy. Economic analysis also entered into, what was for them, a new field of military decisions because with the application of air power, analysis of how to use it to cripple the enemy economy was needed. Therefore, the American Embassy in London created an Economics Objectives Unit in its Economic Warfare Division. Walt Rostow, an economic historian whose role in influencing policy will be discussed later, served as a member of that unit, and he described their work as seeking "target systems where the destruction of the minimum number of targets would have the greatest most prompt, and most long lasting direct military effect."[29] Economists, perhaps more than other social scientists, emerged from their wartime experience with great confidence that their discipline was uniquely suited to an analysis of conflict.[30]

The largest and most well-known wartime use of social scientists belonged to the Office of Strategic Services (OSS). A list of social scientists that served in the OSS reads as a veritable Who's Who of preeminent academics of the 1950s and 1960s. The list includes Paul Baran, Barrington Moore, Alex Inkeles and Herbert Marcuse.[31] McGeorge Bundy, commenting on the OSS experience, described the organization as "half cops-and-robbers and half faculty meeting." Bundy also noted that, in large measure, the area studies programs built in American universities after the war were staffed and directed in large measure by graduates of the OSS so that the OSS experience cast a shadow over postwar work in comparative politics.[32] One way that the area studies programs created after the war took their cue from OSS alumni was the insistence that research might best be organized on an interdisciplinary basis.

One other wartime institution is worthy of mention and that is the Office of War Information (OWI) which housed the Foreign Morale Analysis Division and utilized the talents of anthropologists like Margaret Mead and Ruth Benedict. Writing after the war, Alexander Leighton who commanded the division dealing with Asia, saw social scientists as contributing to the war against Japan. He believed that the division's work acted as a "break" on some policy-makers in the OWI who tended to assume they could, using leaflets and radio broadcasts, change the Japanese

way of thinking and, through logical argument, convince the Japanese that they were wrong to cling to their belief in Shinto or their form of government.[33] Perhaps the most important finding to come out of OWI research—one that had direct impact on the war—concerned the Emperor of Japan. While most policy-makers were divided over the question of treatment of the Emperor, scholars in OWI asserted that an attack against him would likely bolster Japanese morale and consolidate their resistance. On the other hand, if the Emperor were used to sponsor peace terms, the Japanese people would be more likely to accept them.[34] After the end of the Pacific War, social scientists were valuable in assessing Japan's motive for surrender. The Strategic Bombing Survey concluded that the atomic bomb had less impact on the morale of the Japanese people who were already tired of the war. Moreover, the atom bomb may not even have had a decisive impact on Japanese leaders who had made a decision that surrender was necessary as early as May 1945.[35]

Given this kind of positive experience derived from World War II service, it is not surprising that social scientists came away with an optimistic view (if not hubris) concerning the prospects for applying social science to policy after the war. Reflecting this optimism was a 1950 report of the Russell Sage Foundation that, in noting the accomplishments of social science in the prosecution of the war, enthused that "social scientists were converted into social practitioners."[36] However, some social scientists were more skeptical than others on the ultimate utility of the social sciences for policy. Thus, one former member of the OWI concluded that "Many social scientists employed by the government or in its armed services during the war found their research and scientific wisdom not eagerly accepted, wisely interpreted or sensibly followed by policy-makers." In fact, he argued that the purpose of social science during the war seemed to be justifying the decisions already made.[37]

Even though the World War II experience left social scientists ready to contribute to national security after the war, they remained at a relative disadvantage when compared with their natural science colleagues. For one thing, scientists had a long-standing interest in contributing to American war efforts extending back to the nineteenth century, with the creation of the National Academy of Sciences in 1863 for the purpose of aiding the union war effort.[38] Of all the social sciences, economics was favored slightly more than the other disciplines because some of its analysis in the Economic Objectives Unit impressed the Air Force and the Joint Chiefs of Staff for the "concreteness and immediacy" of their work.[39]

However, for the other disciplines, the disadvantage grew from the more ambiguous and diffuse nature of the contributions that social science made to the war. In contrast, the natural sciences made very visible and dramatic contributions, not the least of which was the atom bomb. From this positive contribution to innovations in weaponry emerged "an almost religious faith in the power of science to transform international relations."[40] The clear contribution of science was reinforced by an organizational difference. The efforts of the natural scientists relied upon a central organization in the form of the Office of Scientific Research and Development (OSRD), the successor to President Wilson's National Research Council, that enhanced their ability to pursue their professional objectives after the war and whose advantages will be described in the next chapter. Nowhere was the disparity between the natural and social sciences more apparent than in the debates concerning the creation of a national science organization that could carry on the work of the OSRD after the war. What is more, these debates over the science foundation shed light on why social scientists, already tending in the direction of science, made a more concerted effort to place their disciplines on a more secure scientific foundation.

President Roosevelt wanted the continuation of an OSRD-type organization after the war, and the impressive achievements of OSRD had even "dazzled congress."[41] Therefore, FDR asked OSRD Director Vannevar Bush to prepare a report on how science might be harnessed to the task of postwar reconstruction. That report, titled "Science and the Endless Frontier," was presented to President Truman in July 1945. Not all scientists supported the creation of a science institution linked directly to the state because, in the words of Frank Jewett, president of the National Academy of Sciences, scientists did not want to be made the "intellectual slaves of the state."[42] Furthermore, another issue involved in the debate over a science foundation was whether or not the social sciences should be included as members. The initial position of the SSRC was that there should be a separate agency for supporting the social sciences but the Council abandoned this position and sought instead to become part of the same foundation as the natural sciences. An integral aspect of SSRC's effort for inclusion in the foundation was its emphasis, following the inspiration of Charles Merriam, on the objective scientific quality of the social sciences. Indeed, under the leadership of Pendleton Herring, who served as the president of the SSRC from 1948 to 1968, the institution sought to develop a social science that would be more reliant on quantitative methods.

For their part, the natural scientists feared that the inclusion of the social sciences might jeopardize the legislation to establish a science foundation. The most extreme expression of the view of natural scientists was made by George A. Lundberg in the *Scientific Monthly* in May 1947. He wrote that there was a consensus that "the social and physical sciences have nothing in common and that at best the social sciences are a propagandist, reformist, evangelical sort of cult."[43]

Besides his belief in science, Herring also believed that the nature of the technological advance in weaponry demanded a greater emphasis on diplomacy and negotiation that required a synthesis of political, psychological, cultural and economic analysis, thereby reinforcing the advantages of interdisciplinary approaches.[44] The interdisciplinary synthesis, already in place in the SSRC from its founding, had been amplified by the OSS model of World War II, and the drive for science provided another rationale for increased integration of the disciplines.[45] That interdisciplinary synthesis was pursued by the SSRC's Committee on Comparative Politics and came to be engendered in modernization theory. Similarly, the US Navy supported a major interdisciplinary effort with its Project Michleson that sought insights from psychology, sociology and political science that would undergird analysis of the deterrent function of nuclear weapons.[46]

Two other disputes concerning the proposed foundation emerged and were embodied in two alternative proposals in the US Senate. The first version of the bill submitted by Senator Warren Magnuson and the one closest to the vision of the Bush report, called for a foundation governed by a nine-member board that would be composed principally of eminent scientists who were not directly tied to the government. In this version, any patents coming out of federally funded research would be owned by private interests. An alternate bill was submitted by Senator Harvey Kilgore, and in this version, the foundation was to be governed by an administrator appointed by the president with an advisory board whose members would be a combination of government and outside scientists. In addition, the Kilgore version prohibited the patenting of research funded by the US government. At the heart of the dispute illustrated by these two bills was the need to reconcile public accountability with private expert control and whether or not the foundation would be used to advance public welfare or to advance scientific progress.[47] Indeed, the growth of federal patronage of science, evidenced from the 1880s by the employment of scientists in organizations like the weather service and the geological survey,

raised a fundamental question of public policy. That is, how was government science best managed? Should management come through a democratic political mechanism or through a politically elitist one?[48] Thus, the key difference between the Kilgore and Magnuson bills was that the former sought to mobilize scientific research in support of national needs, while the latter aimed to create an elitist mechanism to ensure the autonomy and advancement of the best science.[49]

Isaiah Bowman (of "The Inquiry" fame) led a committee to support the Magnuson bill, and the committee's hostility to inclusion of the social sciences hardened as that issue became entangled in the debate over governance and patents.[50] Talcott Parsons entered the fray of the controversy and wrote in *The Washington Post* that in the aftermath of Hiroshima, a high-level study needed to be conducted "to explore the needs which the social sciences must fill in a world equipped for suicide." He expanded on the theme in an article for *The American Sociological Review* where he noted that given the disruptive effects of technology, it did not seem wise to support technological developments—those concrete manifestations of the natural sciences—without also supporting the social sciences that offered the best hope for coping with the social consequences of technological change.[51]

All the controversies concerning the fundamentals of the science foundation delayed its creation so that President Truman did not sign it into law until May 1950. During the interim, the Congress created the Office of Naval Research (ONR) as one way, for the military at least, to establish permanent ties to civilian scientists, including social scientists. The ONR filled in until the science foundation was created, and its intent was to judge research on its scientific merits or promise rather than its direct utility for the Navy. In the end, the National Science Foundation (NSF) had a director appointed by the president along with a twenty-four-member National Science Board. Even though the SSRC provided groups to testify on behalf of the social sciences and commissioned Talcott Parsons to write a defense of its membership,[52] the social sciences were not included in the final legislation. In part, the failure of social scientists to lobby successfully on their own behalf was a result of the fact that there really was no consensus among them about the value of membership in the foundation because of the conflicted identity we have already noted, with some of the disciplines having very distinct "scientific" and "humanistic" wings.[53] The loss of inclusion in the NSF did not prove too detrimental for the social sciences because the financing of science through the NSF proved to be

modest and was less than Vannevar Bush originally hoped for. In 1952, the foundation asked for $14 million and received $3.5 million, and that pattern of appropriations persisted until the Soviet launch of the Sputnik prompted greater funding.[54]

The launch of the Sputnik, besides prompting the Defense Department to create a division devoted to scientific research (Advanced Research Project Agency-ARPA) in 1958,[55] also generated renewed support for funding the social sciences. In 1957, both Senator Hubert Humphrey and Vice President Richard Nixon spoke up for government funding of the social sciences.[56] By 1957, the National Science Foundation established a unified social science research program. Finally, in 1969, Lyndon Johnson signed a bill amending the NSF founding legislation and granting the social sciences a formal status. This success of the social sciences must partly be credited to their ability to shed the stigma of being social reformers through their embrace of the model of the natural sciences, thereby helping to weaken conservative criticism of their work. Nevertheless, NSF funding for the social sciences remained substantially less than that for the physical sciences, and, from 1950 to 1982, the social sciences were granted only 30 percent of the funding that the natural sciences were granted.[57]

Whatever federal financing might be lacking in the post-war, the social sciences were able to draw on financial support from private philanthropic institutions. The Rockefeller Foundation, the Carnegie Corporation and the Ford Foundation provided more than $85 million for academic social science from 1948 to 1958.[58] In addition, all three foundations supported various area studies programs and institutes after the war.[59] Recall also that it was the Rockefeller Foundation that provided the initial funding for the SSRC that did so much to promote the scientific status of the social sciences. In fact, in 1934, Frederick P. Keppel, the head of the Carnegie Corporation, observed that foundations had forced the techniques of the sciences on the social sciences.[60] The push by foundations to enhance the scientific reputation of the social sciences was not without irony. The Ford Foundation created its Behavioral Science Division and deliberately eschewed the label "social" science in order to avoid whatever stigma might be attached to that label for its association with socialism or social reform.[61] Yet that division came under sharp criticism from conservatives in the form of two congressional committees: the Cox Committee in 1953 and the Reece Committee in 1954, both of which impugned the empirical direction of research in the Behavioral Sciences Division for the neglect of American values.[62]

The scientific work carried out under the OSRD during World War II that led to a number of technical innovations crucial for the allied victory provided an incentive for the US policy-makers to continue those practices. Indeed, this effort was the reason for the creation of the National Science Foundation in 1950. But the OSRD experience had another effect because it served as a model for decentralized research conducted by scientists at major universities. Some of these arrangements provided an institutional basis for the expansion of social science working on national security issues. For example, the Massachusetts Institute of Technology (MIT) had been responsible for developing radar, and the director of OSRD, Vannevar Bush, had been a one-time vice president there. During the war, MIT's contracts with the US government totaled $117 million.[63] MIT would therefore be a natural home for social science research in support of national security policy so that the end result was the location of the Center for International Studies (CENIS) there.

CENIS grew out of Project TROY, funded by the US Department of State under a contract with MIT. Convened for three months in 1950, the project included scientists, social scientists and historians to study the problem of supplying information to people living in Iron Curtain countries. Part of the group's work was technical, involving the need to overcome Soviet abilities to jam radio broadcasts from the Voice of America. But in addition to the technical work, social scientists were needed to formulate the content of the broadcasts by determining what messages would resonate with people in the Soviet bloc countries. Project TROY's final report was delivered to the State Department in February 1951. One of the report's annexes recommended the creation of research institutes on university campuses that would facilitate collaboration with policy-makers. Max Millikan, an economist who had served in the War Shipping Administration during the war, was selected to be the center's first director, and it was Millikan's notion that a central theme for CENIS should be economic growth and industrial development. In turn, Millikan invited Walt W. Rostow to become one of the institute's founding members. As Donald Blackmer, the center's chronicler, notes, from the beginning its task "has always been conceived as creating new knowledge for the public good." This sense of its mission was undoubtedly reinforced by the experiences of World War II when major research and development contributions were made to the war effort.[64] Indeed, as Nick Cullather points out, the optimism concerning the ability of social science to aid policy grew from a sense that continued progress in those disciplines appeared so certain in 1947.[65]

The story of CENIS highlights a general pattern of a shift in funding for the social sciences, with the federal government gradually increasing its support so that, over time, the social sciences became less dependent on foundation financing. The passage of the National Defense Education Act of 1958 led to large-scale funding by the US government to area studies programs, language training and science. While CENIS did receive support from the Ford Foundation in the amount of $2 million from 1952 to 1961,[66] the bulk of its funding came from the US government. Although CENIS began as part of a State Department initiative, the Department did not have the resources to support or monitor the center's work. Consequently, the Central Intelligence Agency took on funding CENIS research, and the amount of support from them was fairly consistent at $225,000 a year until the center severed it links from the agency in 1966. From 1963 to 1968, the center, on average, also received $600,000 yearly from agencies of the Defense Department.[67] Chapter 4 will highlight the role that CENIS, keeping with its theme of economic development, played in promoting modernization theory especially evident in the work of Walt Rostow.

Government contracts to universities and its support of institutions like CENIS provided one avenue for social science ideas to infiltrate national security policy. But another way was for the US government to create its own system of think tanks. Perhaps the most well known of these was the Air Force's research organization, the Research and Development Corporation (RAND).[68] Greater detail concerning RAND's evolution will be reserved for Chap. 3 because of its role in the development of deterrence/coercion theory. For now, suffice it to say that it was initially established under a contract to Douglas Aircraft Company in 1945, and reported directly to the Air Staff for Research and Development. RAND's ties to that company were severed in 1948, and it was converted into an independent non-profit organization although the bulk of its research was conducted under contract with the US government. RAND's research in its early days centered on weapons' development and strategic planning for which RAND relied on the talents of physicists, mathematicians and economists. Most of the prestige within RAND went to scholars involved in this work, and RAND produced pioneering work on deterrence theory from 1952 to 1966 that provided much of the conceptual framework that is the subject of Chap. 3.

Over time, RAND did broaden its research focus to include the social sciences by creating its own division. Hans Speier, a sociologist who fled Germany in the 1930s and who served with the Office of War Information, was the first director of this social science division. Speier noted that social

scientists tended to be dismissed at RAND and that there was "occasionally a tendency of snootiness, of distain for people who were not, let's say mathematicians."[69] Interestingly, economists had their own division at RAND distinct from Speier's social science division, perhaps because of their more concrete role during World War II, as mentioned earlier. Moreover, the economists may have retained from their war experience a certain reluctance to work with political scientists or historians.[70] The marginalization of the social sciences at RAND is also evidenced by the fact that the division only moved to RAND headquarters in Santa Monica in 1956. Typical of the dismissal by RAND's physicists and economists was the view that social scientists were merely "essayists" that produced the kind of journalistic assessments that one could find in *The New York Times*.[71]

Both CENIS and RAND became home to social scientists conducting research that informed US grand strategy in the early Cold War, which did raise questions concerning the ethics of social scientists working in the service of the state. On this issue, we again see a sense of the conflicted identity at work within the social sciences. Some, like Talcott Parsons, believed that such links to the state as were provided by federal funding did not jeopardize the independence of academics as long as the research remained centered in universities.[72] Others feared such ties threatened the autonomy of social scientists that would prove detrimental to the discipline. Military patronage in particular seemed especially vulnerable to the charge that it "represents an extreme form of the general trend of the patron calling the piper's tune."[73] Anthropologists were especially sensitive to the potential negative effects on the profession growing from explicit ties to the state. The discipline had been shaken earlier by charges made by Franz Boas (whose famous protégés included Ruth Benedict and Margaret Mead) in 1919 that four anthropologists had "prostituted" science by using it as a cover for spying in World War I.[74]

Beyond the ethical concern of the potential for government funding to restrict the autonomy of social scientists lies a broader issue of the role of normative stances and value judgments within a scientific enterprise. After all, the very label "social science" implicitly contains both a notion of scientific method as a means for accomplishing a task and a notion of social welfare. The combination suggests a certain ambivalence about which notion should take precedence. Moreover, given their roots in the Progressive Movement, the social sciences, from their inception, combined their belief in the desire for social reform with a belief in science as the optimal means for that reform. In fact, the first official American social

science organization, The American Social Science Association, was founded by reformer Samuel Gridly Howe in 1865 because of a concern that increasing economic inequality posed a threat to social stability. This early generation of social scientists saw science as synonymous with reform, and one early college textbook asserted that the very purpose of sociology was "to formulate a scientific program of social betterment."[75] As we have already suggested, Charles Merriam, shaped by the Progressive Era, was a personification of welding a scientific approach to social reform.

Although the reformist impulse in the social sciences becomes muted in the early twentieth century, with some historians interpreting the 1930s as a triumph for scientific objectivity,[76] there remained a debate between proponents of a scientifically objective social science whose function was to gather facts to describe social reality and those who saw its role as one of social advocacy. The debate between the two views crystalized in the 1930s and has persisted in a number of ways into the present day. Perhaps more than other social science disciplines, political science has always been shaped by the conflicting desires to make a normative stand on behalf of American democracy while solidifying its identity as a true science.[77] In fact, some scholars have drawn links between American liberalism and a belief in the universalizing propensity of science.[78]

The divergent views concerning the fundamental characteristics of the social sciences became magnified, of course, by the postwar debate concerning the membership of the social sciences in the National Science Foundation. The promise of federal funding of research provided a clear incentive for the social sciences to develop their disciplines in a scientific manner that would provide an apolitical view thought to be more acceptable to the government. Yet, we have already noted that during the controversy over founding of the NSF, social scientists were divided over the issue concerning membership in such an organization. Some social scientists found the moralism and sentiment that imbued their reformist colleagues as an embarrassment and after World War II were drawn to the model of science as indicative of professional maturity.[79] In addition, a scientific approach had the perceived advantage of promising prediction in an uncertain world, and this latter characteristic would be especially attractive to policy-makers grappling with dramatic changes in the postwar strategic environment.

This division in viewpoints that encapsulates the conflicted identity within social science is well illustrated by a debate that took place between William Foot Whyte and John H. Hallowell, appearing in the pages of the

American Political Science Review in 1943 and 1944. William Whyte was clearly committed to the consolidation of science which he believed required the discovery of "certain uniformities or laws." He asserted that American political scientists had been handicapped by their normative commitment and unquestioning acceptance of a democratic ideology. Whyte then went on to recommend that political scientists needed to focus on description and analysis of political behavior and leave questions about ethics to philosophers.[80]

John Hallowell's rejoinder to Whyte began with the observation that political science had already become sufficiently positivistic in outlook. He went on to say that the experience of the war and the emergence of fascism necessarily demanded renewed questioning of whether the purpose of the social sciences could merely be to provide a description and analysis of political behavior. Such a positivistic perspective, he believed, "leads inevitably to that kind of intellectual paralysis that is most conducive to the emergence of the fascist mentality."[81] Hallowell observed "objectivity parading too often under the thin disguise of tolerance is more often than not a confession of despair or a cloak for indifference."[82]

Hallowell's views were reinforced by some of his contemporaries. Louis Wirth, a sociologist at the University of Chicago, too believed that the social sciences had an obligation to be concerned with the nature of the good life and the institutions that served it, and he was critical of any effort to establish close connections or similarities with the natural sciences rather than with the humanities.[83] Wirth's observations were also echoed by Harvard's Benjamin Wright who saw an excessive reliance on quantitative methods to the exclusion of important questions that could not be readily addressed in such a fashion.[84] Charles Beard, who succeeded in the presidency of the American Political Science Association after Charles Merriam, denounced the latter's leadership of the discipline. Beard charged that under Merriam's leadership, political science adopted an unthinking reliance on the scientific model that "led researchers to concentrate on minutia [rather than] great causes and ideas."[85]

Perhaps the most comprehensive statement on behalf of an explicitly normative reformist social science can be found in Robert S. Lynd's (who served as secretary for the SSRC from 1927 to 1931) book of 1939, *Knowledge for What? The Place of Social Science in American Culture*. Written against the backdrop of crisis—economic collapse and impending war—Lynd was critical of the social sciences of his day. He saw social scientists as falling into one of two groups: scholars and technicians, neither

of which he saw as suitable for addressing the pressing problems of the day. Lynd believed the "scholars" to be too remote from the day-to-day concerns and disregarded the need for relevance. In contrast, the "technician," with a desire to address real problems of the day, too often accepted the definition of problems in terms provided by the institutions of the moment, which meant they were inclined to support the status quo. Such technicians, moreover, merely described reality, "with the aloofness of a reporter covering a fire in a warehouse" rather than attempt to answer important questions like whether or not democracy is workable in a world of unequal men or whether it can survive in a culture dominated by the power of concentrated private wealth.[86]

Yet, despite concerns of abandoning reformist impulses, efforts to strengthen the scientific identity of the social sciences intensified in the wake of the NSF debates. Talcott Parsons provided the leading voice in this effort in an article for the *American Sociological Review* in 1946. In that article, Parsons said that there should be no rigidly drawn boundaries to the scope of science and that:

> It should and must be extended wherever its methods are intrinsically applicable. That this includes man's social life and behavior there can be no shadow of a doubt despite the many difficulties and differences among the varied fields of scientific endeavor. In the last analysis science is inherently a unified whole.[87]

Parsons' belief in the unity of the method of social and natural sciences relates to the notion that both express a rational and technical culture, and so strong was his conviction of the rationalization brought by science and technology that he thought the most effective way for preventing the outbreak of the kind of pathological politics evidenced by National Socialism was the diffusion of science and technology.[88] He also thought that such diffusion would inevitably lead Soviet society away from "ideological fantasies."[89] Such ideas led logically to the expectation of a certain cultural convergence among all industrial countries. What is more, Parsons was especially critical of political science for its inability to provide a major channel for scientific advance, and he attributed its weakness, wrongly perhaps, to its failure to segment empirical conceptual theory from normative theory.[90] At the same time, and contrary to the view of Charles Merriam, Parsons also conceived of social science as best divorced from solving practical problems. In his essay commissioned by the SSRC to

convince politicians and scientists that the NSF needed to include the social sciences, Parsons expressed skepticism of a "policy relevant" focus for the social sciences. He saw a hazard in the pursuit of practical activities that might raise expectations about the importance of some finding that could easily lead to disillusionment that would undermine the public's confidence in social science should that expectation not be met.[91]

The issue at the center of the difference between those social scientists seeking ethical neutrality and objectivity and their normatively oriented counterparts is in a real sense the difference between assessment of means and ends. In John Hallowell's rebuttal to William Whyte, he cites a representative example of the positivistic viewpoint that is found in G.E.G. Catlin's 1927 book, *Science and Method of Politics*. In that book, the author suggested that the study of politics can only become scientific when the study of means is sharply divorced from any consideration of ends.[92] Catlin's view was reiterated by Wesley Mitchell (who Robert Lynd described as the "arch empiricist without purpose") when he remarked that science could not direct action to an end but it could determine the consequences of particular actions so that a more intelligent choice of means might be selected.[93] A similar conclusion was reached at a roundtable of the American Political Science Association held in 1946. Participants noted that "no scientific method has yet been devised to determine the superiority of any ends or purposes over any other…We can only state the relative superiority of means…"[94]

Yet, as Hallowell suggested, the wartime experience with fascism necessarily called into question a social science that remained concerned exclusively with assessing means.[95] And, of course, in practice, policy-makers can ill afford to ignore the question of ends, and in this differ from an academic social scientist's ability to ignore such questions. As McGeorge Bundy observed, any policy must have an objective that must be justified by normative standards and that "Behind all technical counsel there will be intent."[96] Further, in some cases like that of an economist, should they actually remain neutral technicians, they would be, in the end, irrelevant to policy.[97] Even that exemplary proponent of science, Talcott Parsons, in his early writings, exhibited such concerns about the problem of social order that he tried to combine his positivism with a level of idealism and thereby made implicit judgments concerning the good ends. Parsons believed the most effective way of controlling the pathological outbreak of both National Socialism and Japanese militarism was through the spread of science and technology which would enable the continuing process of rationalization.[98]

Neatly severing means from ends is difficult, and reconciling scientific neutrality and ethics problematic. Even though an organization like the SSRC might insist in its annual reports that it was not an "action body" and that its primary purpose was merely to serve as a clearing house for research techniques and findings, its president acknowledged that those findings needed to be ordered by some conceptual framework that would necessarily make ethical judgments about ends.[99] Gabriel Almond, as heir to the legacy of Charles Merriam, attempted, as we shall see in his work on comparative politics and as a proponent of the behavioral sciences, a synthesis of objective scientific neutrality and normative theory. In 1946, Almond admitted that while science could not create values, it could demonstrate how alternative public policies could contribute to the realization of values. In this way, he observed that political science could discover the appropriate pathway to "good ends," thereby making, however indirectly, ethical judgments.[100]

The scientific rigor embodied in the methodology that came to dominate the social sciences in the 1950s and 1960s under the general heading of "behavioral sciences" allowed for the identification of recurrent patterns necessary for drawing generalizations. Although the terms social science and behavioral science were often used interchangeably, the latter signifies a concern with individual and group behavior found in psychology, sociology, anthropology as well as some parts of economics and political science. The behavioral approach emphasized a certain rigor in research methods that demonstrated that social phenomenon could be studied scientifically by drawing on quantitative analysis and testable theories.[101] All of the characteristics of the behavioral approach were outlined by David Easton in his book, *A Framework for Political Analysis*, where, notably, he emphasized the need to keep ethical evaluation and empirical explanation analytically separate.[102] Although, to be sure, other scholars saw the approach less as a specific framework and characterized it more as a "mood."[103] Robert Dahl commented that the growth of the behavioral movement in the United States grew from certain predispositions in US culture, namely, pragmatism and confidence in science. Dahl measured the growth and success of the behavioralist approach by the extent to which a behavioralist occupied the presidency of the American Political Science Association.[104] John Gunnel made a broader argument and suggested that much of the tradition of American social science has grown out of an attempt to "replace religion as a cohesive social force with a science of social control."[105] Perhaps the quintessential example of the scientific

thrust of the behavioral approach was Project Michelson, which was a government research program that ran from 1959 to 1966, that was designed, in the words of the project director, to generate "scientific" knowledge about strategic deterrence and international stability.[106]

However the approach is characterized, and whatever its sources, behavioralism laid the foundation for an assumption of universality that contained problematic implications for the application of theory. In the case of deterrence/coercion theory, the problematic implications involved the universality of cost/benefit decision-making predicated on a unified psychology that was assumed to apply to all actors, whether individuals or nations. Assumptions of universality were perhaps even more damaging in the case of modernization theory where it led to the conclusion that knowledge of a country's history or culture was less important for understanding social change. Thus, for example, Clyde Kluckhohn, originally a specialist on the Navaho, came to believe through his work with Ruth Benedict in the Office of War Information, that one did not even require linguistic ability in order to understand the Japanese.[107] This assumption of universality was made explicit by Alex Inkeles in his remarks to a conference held under the auspices of the SSRC's Committee on Comparative Politics in 1958. For at that conference, Inkeles asserted that "industrial" man was such a universal proto-type that it transcended nationality, ethnicity and religion.[108] For those scholars working in the field of comparative politics in the 1950s and 1960s, one way to avoid messy cultural explanations for behavior was to eschew old-fashioned concepts and, as we will see in Chap. 4, to work at creating a new vocabulary for analyzing politics. The shortcomings of an assertion of universality—assumptions made more plausible by the extent they seemed grounded in science—may now be more readily apparent than when they were taking root in the post-war years. The experience of World War II and the allied victories gave confidence to social scientists to apply their theories to emerging national security issues. The fact that the social sciences were excluded from the original design of the NSF provided a reinforcing incentive to pursue approaches modeled on the natural sciences.

That social science framework came to be incorporated into national security in the early Cold War was therefore the result of both supply and demand factors. From the standpoint of supply, scholars' experiences during World War II had them poised to continue making contributions to policy. Yet more than this readiness was needed for social science theories to enter the realm of policy. Three interrelated pathways or processes were

necessary. The first pathway occurs somewhat passively and involves diffusion of social science conceptualizations to provide the intellectual framework for analyzing a problem. Once a theoretical framework gains currency, it becomes institutionalized and forms "patterns or regularities of belief to which successive generations of scholars and consumers of scholarship become socialized."[109] The second pathway requires activism and occurs when what we might call policy entrepreneurs or what Theodore White labeled as "action intellectuals"[110] actively push particular ideas from a social science framework. The role these individuals play is especially important if a major new policy initiative or change is to occur. The conjunction of an established intellectual framework and activist policy entrepreneurs may then call forth public constituencies that provide political leaders with an incentive for acting in a manner consistent with the framework.[111] The passive process by which social science provides a supply of ideas for policy-makers was effectively summarized by Leslie A. Pal and is worth quoting at length:

> Social Science influences public policy in the same way that water seeps through limestone. Tiny rivulets of ideas flow unpredictably through institutions and may swirl briefly around decisions; moreover these rivulets may combine in unanticipated ways. This all becomes a bit clearer when one thinks of the various ways a social scientist distributes ideas and research: specialized articles and books, routine undergraduate and graduate teaching, conferences, colloquia, public speeches…Who knows by what strange alchemy one's ideas might affect public policy?[112]

Perhaps the greatest hazard to the diffuse way that social science knowledge enters public policy derives from the fact that should a "policy paradigm" become sufficiently entrenched, it becomes, in Peter Hall's words, a kind of gestalt that is then largely taken for granted and rarely scrutinized.[113] We shall see these supply processes at work in the discussion of deterrence/coercion theory contained in Chap. 3 and in the analysis of modernization theory in Chap. 4.

What then was the demand side that led policy-makers to seek the expert advice from social science during the early Cold War? While the formation of public constituencies may more likely drive demand in domestic policy,[114] the creation of such a constituency plays a smaller role in national security policy because the general public tends to be less attentive to foreign policy. From the standpoint of both deterrence/coercion theory and modernization theory, the impetus for policy-makers demand

for the expert advice of social scientists came from the profound changes in the strategic environment after World War II. The two most dramatic and salient changes involved the new technology of atomic weapons and the creation of new states as the European colonial system disintegrated. The unprecedented nature of both changes presented policy-makers with a great degree of uncertainty—an uncertainty that led them to seek reassurance from social science. What is more, the need for certainty was enhanced by the extent that social scientists could demonstrate their objective, scientific grounding. One can especially see the faith of policy-makers in science in the case of economics which, as it became increasingly geared toward verification through mathematical modeling from the 1940s on, increased the respect that policy-makers had for their advice.[115] This faith in science may have been, at times, stronger in military leaders to the extent they were imbued with the Jominian tradition of scientific warfare, and hence were more susceptible to the appeal of "scientific" social science. Indeed, Gabriel Almond, in his congressional testimony in 1966, was critical of the training of the State Department's Foreign Service because it relied too heavily on law and history at the cost of neglecting the behavioral approaches of the social sciences.[116] In that testimony, he said critically of such policy-makers that "they believe in making policy through some kind of intuitive and antenna-like process, which enables them to estimate what the prospects of this and that are in this or the other country."[117]

Before we examine the substantive issues of the scientific theories of deterrence/coercion and modernization that supported the US grand strategy in the early days of the Cold War, we need to pause and reflect on the nature of "science" itself and some of the central debates in the philosophy of science. Doing so will allow us to ascertain the extent to which social scientists were worshipping that "strangely distorted and graven image of science." We turn to this task in the next chapter.

Notes

1. Bruce W. Jettleson, "The Need for Praxis: Bringing Policy Relevance Back In," *International Security* 26 (Spring 2002): 169; Janice Gross Stein, "Autobiographical Reflections on Bridging the Policy-Academy Divide," *Cambridge Review of International Affairs* 22 (March 2009): 120.
2. Robert D. Putnam, "APSA Address: The Role of Political Science," June 2003, downloaded from http://www.apsanet.org on July 31, 2015.

3. Congress, House, Committee on Foreign Affairs, Subcommittee on International Organizations and Movements, *Behavioral Sciences and the National Security*, report no. 4, together with part ix of the hearings on winning the Cold War, Washington, D.C.: US Government Printing Office, 1965. For more details on Project Camelot and the controversy surrounding military patronage of the social sciences, see Janeen Klinger, "Caveat Emptor: Social Science and U.S. National Security Strategy," *Comparative Strategy* 33 (2014): 167–176.
4. Other historians have identified four major networks of theoretical practice that they see dominating policy in the early Cold War. These are systems sciences, rational choice theory, behavioral sciences and modernization theory. See Joel Issac, "Tangled Loops: Theory, History and Human Sciences in Modern America," *Modern Intellectual History* 6 (2009): 410. I have chosen to reduce these to two because rational choice theory can readily be subsumed under deterrence theory and behavioral sciences are linked directly to modernization theory. Moreover, both deterrence and modernization theory drew on systems analysis.
5. David Easton, "The New Revolution in Political Science," *The American Political Science Review* 63 (December 1969): 1056.
6. Lucian Pye described this conflicted identity differently. He distinguished two approaches in social science: the scientific, which searches for regularities on the assumption that men and society behave in law-like ways; and humanistic, which sees knowledge as a search for meaning requiring both interpretation and normative judgment. See: Lucian Pye, "Political Science and the Crisis of Authoritarianism," *American Political Science Review* 84 (March 1990), 4.
7. William R. Polk, "Problems of Government Utilization of Scholarly Research in International Affairs," in *The Rise and Fall of Project Camelot,* ed. Irving Louis Horowitz (Cambridge, MA: MIT Press, 1967), 261.
8. Carol S. Weiss, *Social Science Research and Decision-Making* (New York: Columbia University Press, 1980), 20.
9. Allen S. Whiting, "The Scholar and the Policy-Maker," *World Politics* 24 (Spring 1971): 229.
10. McGeorge Bundy, "The Battlefields of Power and the Searchlights of the Academy," in *Dimensions of Diplomacy,* ed. E.A.J. Johnson (Baltimore: The Johns Hopkins Press, 1964), 9.
11. Quoted in Joseph S. Nye, "Bridging the Gap Between Theory and Policy," *Political Psychology* 29 (2008): *594.*
12. Thomas Bender, "Politics, Intellect, and the American University," in *American Academic Culture in Transformation: Fifty Years, Four Disciplines,* eds. Thomas Bender and Carl E. Schorske (Princeton: Princeton University Press, 1997), 32.

13. Cited in Robert H. Nelson, "The Economics Profession and the Making of Public Policy," *The Journal of Economic Literature* 25 (March 1987): 57.
14. Edward Shils, "Tradition, Ecology and Institutions in the History of Sociology," *Daedelus* 99 (1970): 761.
15. Gene M. Lyons, *The Uneasy Partnership: Social Science and the Federal Government in the Twentieth Century* (New York: Russell Sage Foundation, 1969), 22–23.
16. Dorothy Ross, "The Development of the Social Sciences," in *Discipline and History: Political Science in the United States,* eds. James Farr and Raymond Seidelman (Ann Arbor: University of Michigan, 1993), 81.
17. Lisa Anderson, *Pursuing Truth, Exercising Power: Social Science and Public Policy in the Twenty-First Century* (New York: Columbia University Press, 2003), 15.
18. Lyons, 29.
19. Lyons, 47.
20. Mark Solovey, "Riding Natural Sciences Coattails onto the Endless Frontier: The SSRC and the Quest for Scientific Legitimacy," *Journal of the History of the Behavioral Sciences* 40 (Fall 2004): 397.
21. Jeffrey A. Frieden and David A. Lake, "International Relations as a Social Science: Rigor and Relevance," *The Annals of the American Academy* 600 (July 2005): 137.
22. Solovey, 397.
23. For a discussion of Merriam's efforts to enhance the practical application of the social sciences, see Michael T. Heaney, "The Chicago School that Never Was," *PS: Political Science and Politics* 40 (October 2007): 753–758.
24. Lucian W. Pye, "Description, Analysis and Sensitivity to Change," in *Political Science and Public Policy* ed. Austin Ranney (Chicago: Markham Publishing Company, 1968), 239–261.
25. Mark C. Smith, *Social Science in the Crucible: The American Debate Over Objectivity and Purpose, 1918–1941* (Durham, NC: Duke University Press, 1999), 255.
26. Raymond Bowers, "The Military Establishment," in *The Uses of Sociology,* eds. Paul F. Lazarsfeld, William H. Sewell and Harold L. Wilensky (New York: Basic Books, 1967), 250.
27. Bowers, 243.
28. Edward A. Shils and Morris Janowitz, "Cohesion and Disintegration in the Wehrmacht in World War II," *Public Opinion Quarterly* 12 (Summer 1948): 280–315.
29. Stephen Rosen, "Systems Analysis and the Quest for Rational Defense," *The Public Interest* 76 (Summer 1984): 5–6. For additional details concerning the role of economists in the war, see Robert J. Leonard "War as a 'Simple Economic Problem': The Rise of an Economics of Defense" in *Economics and National Security,* ed. Crauford D. Goodwin (Durham, NC: Duke University Press, 1991), 261–283.

30. Leonard, 267.
31. David C. Engerman, "Social Science in the Cold War," *Isis* 101 (June 2010): 396.
32. Bundy, 2.
33. Alexander H. Leighton, *Human Relations in a Changing World: Observations on the Use of the Social Sciences* (New York: E.P. Dutton and Company, Inc., 1949), 118.
34. Talcott Parsons, "Social Science: A Basic National Resource," in *The Nationalization of the Social Sciences,* eds. Samuel Z. Klausner and Victor M. Lidz (Philadelphia: University of Pennsylvania Press, 1986), 93. Gene Lyons disputes Parsons' claim concerning the impact of OWI on this decision and suggests instead that OWI scholars were not successful in communicating their views of the Japanese to military planners, most of whom clung to the image of Japanese fanaticism that influenced their assessment of Japanese surrender. Lyons, 119.
35. Leighton, 74.
36. Quoted in Terrence Ball, "The Politics of Social Science in Post War America," in *Recasting America: Culture and Politics in the Age of Cold War*, ed. Lary May (Chicago: University of Chicago Press, 1989), 81.
37. Leonard W. Doob, "The Utilization of Social Scientists in the Overseas Branch of the Office of War Information," *American Political Science Review* 41 (August 1947): 649, 658.
38. Seymour J. Deitchman, *The Best-Laid Schemes: A Tale of Social Research and Bureaucracy* (Cambridge, MA: MIT Press, 1976), 95.
39. Leonard, 266–267.
40. Walter A. McDougall, "Commentary: The Cold War Excursion of Science," *Diplomatic History* 24 (Winter 2000): 119.
41. T.H. White, "The Action Intellectuals: Part II," *Life* 62 (June 16, 1967): 68.
42. Daniel J. Kevles, "The National Science Foundation and the Debate over Postwar Research Policy, 1941–1945: Political Interpretation of Science—the Endless Frontier," *Isis* 68 (March 1977): 15.
43. Harry Alpert, "Congressmen, Social Scientists and Attitudes Toward Federal Support of Social Science Research," *American Sociological Review* 23 (December 1958): 682.
44. Pendleton Herring, "Expand the School," *The Saturday Review* (February 1, 1958): 40.
45. Donald Fisher suggests that the SSRC's pursuit of science did not have its intended effect and divided the social sciences rather than integrating them. See Donald Fisher, *Fundamental Development of the Social Sciences: Rockefeller Philanthropy and the United States' Social Science Research Council* (Ann Arbor, MI: University of Michigan Press, 1993), 111.

46. For a list of the studies conducted for Project Michelson and that illustrates its interdisciplinary design, see Thomas W. Milburn, "Intellectual History of a Research Program," in *Theory and Research on the Causes of War*, eds. Dean G. Pruitt and Richard C. Snyder (Englewood Cliffs, NJ: Prentice Hall Inc., 1969), 280–283.
47. Roger Geiger, *Research and Relevant Knowledge* (New Brunswick: Transaction Publishers, 2004), 4; Kevles, 16.
48. Daniel J. Kevles, *The Physicists: The History of the Scientific Communities in America* (New York: Alfred A. Knopf 1978), 49, 54.
49. Kevles, 348.
50. Dael Wolfle, "Making the Case for the Social Sciences," in *The Nationalization of the Social Sciences*, 191.
51. Bender, 31. Talcott Parsons, "The Science Legislation and the Role of the Social Sciences," *The American Sociological Review* 11 (December 1946): 662.
52. Parsons' defense of the social sciences was not completed until 1948, and the manuscript was so problematic and reviewers believed it to be so poor, that it would not convince its audience to include the social sciences in the science foundation. For a sample of reviewer comments, see Samuel Z. Klausner, "The Bid to Nationalize American Social Science," in *The Nationalization of the Social Sciences*, 23.
53. Klausner, 11.
54. Geiger, 160, 174.
55. Mai Elliot, *RAND in Southeast Asia: A History of the Vietnam War Era* (Santa Monica, CA: The RAND Corporation, 2010), 12.
56. Alpert, 683.
57. Wolfle, 192. Details concerning the portion of federal funding going to the social sciences compared with that going to the natural sciences can be found in NSF documents, "Federal Funds for Science" published by the US Government Printing Office for various fiscal years. Attacks against the NSF funding social science continue to this day with the latest attack coming from Senator Tom Coburn in October 2009. See: Patricia Cohen, "Field Study: Just How Relevant is Political Science," *The New York Times* section C (October 20, 2009), 7.
58. Anderson, 29.
59. The extent and nature of the support to the social sciences provided by foundations for area studies can be found in a number of sources. For examples, see Bruce Cumings, "Boundary Displacement: Area Studies and International Studies During and After the Cold War," *Bulletin of Concerned Asian Scholars* 29 (1997): 6–27; Geiger, *Research and Relevant Knowledge*, 93; Blackmer, *The MIT Center for International Studies: The Founding Years, 1951–1969* (Cambridge, MA: MIT Press, 2002).

60. Daniel J. Kevles, *The Physicists: A History of a Scientific Community in America* (New York: Alfred A. Knopf, 1978), 246.
61. Roger Geiger, "American Foundations and Academic Social Science, 1949–1969," *Minerva: A Review of Science and Learning Policy* 26 (1988), 326–327.
62. William Buxton, *Talcott Parsons and the Capitalist Nation-State: Political Sociology as a Strategic Vocation* (Toronto: University of Toronto Press, 1985), 173–174.
63. Blackmer, 1.
64. Blackmer, xvi.
65. Nick Cullather "Bombing at the Speed of Thought: Intelligence in the Coming Age of Cyber War," *Intelligence and National Security* 18 (Winter 2003), 144.
66. Blackmer, 6.
67. Blackmer, 169, 203. Blackmer asserts that the State Department declined to support CENIS for another reason, that is McCarthyism made the Department leery of ties to what might be perceived as left-wing academic research.
68. Other branches of the armed services set up research institutions after the war, and some of these were linked to universities. For example, the US Army established the Operations Research Office under contract to Johns Hopkins University and the Special Operations Research Office at American University. The latter generated controversy with a project funding social scientists to study ways to avert internal wars in developing countries. Once the project was discovered to be linked to the US military, the project known as "Camelot" was abandoned. As a response to the controversy, American University severed its ties with the US Army.
69. Daniel Bessner, "Organizing Complexity: The Hopeful Dreams and Harsh Realities of Interdisciplinary Collaboration at the RAND Corporation in the Early Cold War," *Journal of the History of the Behavioral Sciences* 51 (Winter 2015):31.
70. Leonard, 265. Leonard also quotes Charles Kindleberger comments on the tension between historians and economists during the war that belies interdisciplinary cooperation that was sought after by institutions like the SSRC.
71. Ron Robin, *The Making of the Cold War Enemy: Culture and Politics in the Military-Intellectual Complex* (Princeton: Princeton University Press, 2001), 48.
72. Buxton, 126.
73. Hunter Crowther-Heyck, "Patrons of the Revolution: Ideals and Institutions in Postwar Behavioral Science," *The History of Science Society* 97 (September 2006): 424.

74. Ellen Herman, "Project Camelot and the Career of Cold War Psychology," in *Universities and Empire: Money and Politics in the Social Sciences During the Cold War* ed. Christopher Simpson (New York: the Free Press, 1998), 110–111. For a recent expression of anthropologist's concern regarding ties to the government see: David H. Price, *Cold War Anthropology: the CIA, the Pentagon and the Growth of Dual Use Anthropology* (Durham: Duke University Press, 2016).
75. Mark Smith, 20.
76. Isaac, 409.
77. Roger M. Smith, "Still Blowing in the Wind: The American Quest for a Democratic, Scientific Political Science," in *American Academic Culture in Transformation: Fifty Years, Four Disciplines*, 271.
78. James S. Coleman and C.R.D. Halisi, "American Political Science and Tropical Africa: Universalism VS Relativism," *African Studies Review* 26 (September–December 1983): 33.
79. Bender, 22.
80. William Foote Whyte, "Instruction and Research: A Challenge to Political Scientists," *American Political Science Review* 37 (August 1943): 692, 695, 697.
81. John H. Hallowell, "Politics and Ethics," *American Political Science Review* 38 (August 1944): 640, 650.
82. Quoted in Arnold Brecht, "Political Theory: Beyond Relativism in Political Theory," *American Political Science Review* 41 (June 1947): 485.
83. Klausner, 14.
84. Brecht, 48.
85. Mark Smith, 183.
86. Robert S. Lynd, *Knowledge for What? The Place of Social Science in American Culture* (Princeton: Princeton University Press, 1939), 1, 140.
87. Parsons, "The Science Legislation and the Role of the Social Sciences," 663.
88. Buxton, 25, 111.
89. David C. Engerman, *Know Your Enemy: The Rise and Fall of America's Soviet Experts* (Oxford: Oxford University Press, 2009), 182.
90. Parsons, "Social Science: A Basic National Resource," 56.
91. Parsons, "Social Science: A Basic National Resource," 104.
92. Hallowell, 640.
93. Lyons, 77. Solovey, 400.
94. Brecht, 471.
95. Natural scientists too face an ethical dilemma when considering means divorced from ends. The scientific community was divided over whether or not to build a hydrogen bomb, because doing so meant creating a weapon with no tactical utility, merely because it was feasible. For some scientists, like Robert Oppenheimer, doing so would set a dangerous

precedent. He was opposed in his view by Edward Teller. See Fred Kaplan, *The Wizards of Armageddon* (New York: Simon and Schuster, 1983), 83.

96. Bundy, 5.
97. Nelson, 50.
98. Buxton, 25, 111.
99. Mark Smith, 99.
100. Gabriel Almond, "Politics, Science and Ethics," *American Political Science Review* 40 (April 1946): 285.
101. Lyons, 279.
102. David Easton, *A Framework for Political Analysis* (Englewood Cliffs, NJ: Prentice Hall, 1965), 7. In 1969, Easton took note of an intellectual backlash against behavioralism. See: David Easton, "The New Revolution in Political Science," *The American Political Science Review* 63 (December 1969): 1051–1061.
103. Coleman and Halisi, 39.
104. Robert A. Dahl, "The Behavioral Approach to Political Science: Epitaph for a Monument to Successful Protest," *The American Political Science Review* 55 (December 1961): 763, 766.
105. John G. Gunnell, "American Political Science, Liberalism, and the Invention of Political Theory," *The American Political Science Review*, 82 (March 1988): 78.
106. Thomas W. Milburn, "Intellectual History of a Research Program," in *Theory and Research on the Causes of War*, eds. Dean G. Pruitt and Richard C. Snyder (Englewood Cliffs, NJ: Prentice Hall Inc., 1969) 263.
107. Engerman, *Know Your Enemy: The Rise and Fall of America's Soviet Experts*, 44.
108. Engerman, *Know Your Enemy: The Rise and Fall of America's Soviet Experts,* 187.
109. Ann Ruth Willner, "The Underdeveloped Study of Political Development," *World Politics* 16 (April 1964): 471.
110. T.H. White, "The Action Intellectuals: Part I," *Life* (June 9, 1967): 43–76.
111. I am indebted to Robert Nelson for his framework concerning the pathways by which social science influences policy. For a case that illustrates the three steps at work, see Robert H. Nelson's discussion of the deregulation of the airline industry in Nelson, 49–91.
112. Leslie A. Pal, "Knowledge, Power and Policy: Reflections on Foucault," in *Social Scientists, Policy and the State*, eds. Stephen Brooks and Alain G. Gagon (New York: Praeger, 1990), 143.
113. Peter Hall, "Policy Paradigms, Experts and the State: the Case of Macroeconomic Policy-Making in Britain" in *Social Scientists, Policy and the State,* 59.

114. For example, Carol Weiss cites the example of the deregulations of the airline industry as a case where policy-makers' demand for expert economic advice mattered less than the tax payer's revolt and general public's rebellion against "big government". See Carol Weiss, "The Interaction of the Sociological Agenda and Public Policy," in *Sociology and the Public Agenda,* ed. William Julius Wilson (Newbury Park: Sage Publications, 1993), 46.
115. Nils Gilman, *Mandarins of the Future: Modernization Theory in Cold War America* (Baltimore: The Johns Hopkins University Press, 2003), 35.
116. Lyons, 185–186.
117. Quoted in Christopher Simpson, ed. *Universities and Empire: Money and Politics in the Social Sciences During the Cold War* (New York: the New Press, 1998), 107.

CHAPTER 2

The Natural Sciences and Public Policy: Insights from the History and the Philosophy of Science

To the extent that Lucian Pye is correct to suggest that social science worships a "strangely distorted and graven image of science," that distortion revolves around two aspects. The first aspect concerns the very nature of conceptual, theoretical features of science. Any discussion of this feature of science necessarily touches on some of the issues at the heart of the philosophy of science.[1] These issues became pronounced and entered into public debate with the publication of Thomas Kuhn's *The Structure of Scientific Revolutions* in 1962. The second feature of science is the one it shares with the social sciences and that is the growth of science's contribution to public policy. In this regard, the practical accomplishments of science in application have faced obstacles and its accomplishments have often been exaggerated. This chapter will elaborate on both aspects of the image of science. Doing so will allow us to show the impact of science on social science conceptions of itself that affected the development of the theories of deterrence/coercion and modernization.

The conceptual confusion concerning the fundamental nature of science revolves around three interrelated issues. These are the extent to which the development of scientific ideas is cumulative, the extent to which the ideas are objective which, in turn, relates to the extent that scientific ideas are or can be divorced from norms and the broader social and institutional context.

To begin with, the very term "scientist" is of nineteenth-century origin and was coined by William Whewell in 1840.[2] Gradually, the term came to replace the earlier one of "natural philosopher" that had contained a

J. M. Klinger, *Social Science and National Security Policy*,
https://doi.org/10.1007/978-3-030-11251-6_2

connotation of a close affinity with the humanities. As such, the spread of the term marks a symbolic shift toward a view that scientific disciplines have characteristics that are distinct from other fields of learning. These characteristics included the notions that scientific knowledge is cumulative, objective and readily divorced from any broader social and institutional context. In part, scientists themselves have done much to project the image of science as cumulative and objective. As Thomas Kuhn points out, scientists tend to remember the great pioneers of science in a way that suggests the history of science is one that follows a linear, cumulative trajectory. This way of remembering scientific change derives, in part, from the way that scientists are trained. They do not read classics of science by the great pioneers like Isaac Newton or Albert Einstein; rather, they read textbooks whose narratives suggest that science develops along a smooth, cumulative path.[3] Sociologist Robert Merton similarly argued that the very presentation of scientific work in papers and monographs represents final products of research that do not reveal the "intuitive leaps, false starts, mistakes and happy accidents" that exist in the growth of science. Merton goes on to quote physicist A.A. Moles as observing that scientists are trained to "exaggerate unconsciously the rational aspect of work done in the past."[4]

While definitions of science vary, most recognize that the term science includes a number of interrelated items. Thus, Merton views science as referring to "a set of characteristic methods by which knowledge is certified" and "a stock of accumulated knowledge from the application of these methods." Stephen Toulmin's definition provides some overlap in his list of elements. For him, science means the current repertory of concepts and explanatory procedures and the accumulated experience of scientists working in their respective fields. Merton and Toulmin's definitions do differ in one respect. Merton considers cultural values and mores governing scientific activities as elements that imply some connection to the broader social context. Toulmin includes the current explanatory goals of science as part of his definition and succinctly summarizes his definition as:

> comprising a repertory of currently established explanatory procedures, together with a pool of more tentative conceptual variants; and its development is governed by a general consensus about selection criteria for judging variants from that pool…[5]

Neither Merton nor Toulmin provides a definition of science that necessarily leads to the conclusion that science progresses in a cumulative manner. J.D. Bernal, pointing to the difficulty of defining science given that it

entails so many of the elements identified by Merton and Toulmin, adds that science has been alternatively defined "as a cumulative tradition of knowledge."[6] Defined in this way, science, by definition, is assumed to be cumulative, and any intellectual endeavor that is not cumulative cannot be considered scientific.

Any precise definition of science requires some criterion for marking scientific endeavors off from other non-scientific ones. One must be able to distinguish astronomy from astrology. Karl Popper points out that since they are both based on observation and experiment, these criteria do not help us distinguish between the two. As an alternative, Popper offers his famous falsifiability criterion and goes so far as to say that every genuine test of a theory is an attempt to falsify it. This criterion means that no amount of confirming evidence to support a theory is sufficient for it to claim status as scientific because such confirmation is always easy to find if one is looking for it. Popper points to the example of Sigmund Freud's theories regarding psychoanalysis that cannot genuinely be scientific because any human behavior can be construed as confirmation of his theories. For Popper, confirmation should only count if it is the result of a risky prediction—one that is expected to be incompatible with the theory. In this way, any theory that is not refutable by any conceivable event is non-scientific. Although such theories may still be important, they cannot claim to be backed by empirical evidence in the scientific sense even though they may be the result of direct observation. In short, "statements or systems of statements, in order to be ranked as scientific, must be capable of conflicting with possible or conceivable observations."[7]

Imre Lakatos takes exception to Popper's falsifiability criterion for demarcation of science from other fields because he believes it does not account for the "remarkable tenacity of scientific theories" and the tendency of scientists to invent some hypothesis to rescue a theory. Indeed, Popper asserted that such ad hoc adaptations of theory were confined to non-scientific ones such as Marxism. Lakatos also notes that, in actual practice, scientists do not talk about refutation but anomalies or recalcitrant instances, frequently ignoring these and moving on to other problems. Lakatos begins his critique of Popper with the notion that the appropriate descriptive unit of great scientific achievements is not an isolated hypothesis, but rather a research program. He illustrates the value of using research programs as the unit with reference to Isaac Newton. He observes that Newton's theory was, in fact, refuted because it did not explain the motion of the moon. Yet the overall research program was not undermined, and

Edmond Halley was able to use it successfully to calculate the return of the comet that bears his name. For Lakatos, the best criterion for differentiating science from other fields is that its research program predicts novel facts: "facts which had been either undreamt of, or have indeed been contradicted by previous rival programs."[8] With this stance, Lakatos seems to identify science with a certain degree of objectivity—a characteristic of science elaborated on later.

Whether or not science develops in a cumulative, linear fashion is a question that lies at the center of Thomas Kuhn's analysis in *The Structure of Scientific Revolutions.* Consequently, Kuhn provides a good starting point for a discussion of this conceptual, theoretical aspect of science. He states at the start that his aim is to refute the more conventional view of science as cumulative or, as he expresses it, "development-by-accumulation."[9] Kuhn's refutation of the conventional view begins by distinguishing between two processes for scientific development: normal science and scientific revolutions. He argues that normal science is typical of the bulk of scientific work—the day-to-day activities that engage most scientists most of the time. Normal science involves widespread consensus over theoretical frameworks for understanding and explaining nature. During periods of normal science, individual scientists take theoretical frameworks for granted and do not have to build a "field anew, starting from first principles and justifying the use of each concept introduced." Hence, progress during periods of normal science can indeed be described as cumulative, with advances occurring in increments. Normal science is successful in extending "the scope and precision of scientific knowledge" because consensus concerning the framework allows scientists to "select problems that can be solved with conceptual and instrumental techniques close to those already in existence."

Kuhn's category of scientific revolutions contrasts sharply with normal science. Scientific revolutions are, for him, "tradition shattering" events that necessitate a scientific community's "rejection of time honored scientific theory in favor of another incompatible with it." Scientific revolutions serve as important turning points in the development of science, and these disjunctions or discontinuities show that under some circumstances science does not develop in a linear, cumulative way. Kuhn cites as examples of scientific revolutions the ideas of Copernicus, Isaac Newton and Albert Einstein. Perhaps Kuhn's view of the discontinuities in scientific advance is best captured by a metaphor used by Arthur Koestler that scientific advance resembles the work of a sleepwalker rather than that of an electronic brain.

That science progresses in an uneven fashion "by occasional leaps and bounds alternating with delusional pursuits, cul-de-sacs, regressions, periods of blindness and amnesia."[10]

If Kuhn's answer to the question of whether or not science is cumulative is both yes and no, how does he explain shifts between periods of normal science and scientific revolutions? Kuhn's discussion of the contrasting way that science advances is organized around the concept of paradigm. Paradigms are those theoretical frameworks that scientific communities agree upon and that provide it with criteria for choosing problems to investigate. Consensus concerning a paradigm emerges because it has shown itself to be more successful than alternatives in solving problems. Moreover, because of consensus over paradigms, normal science involves little innovation but rather "further articulation and specification of the paradigm under new and more stringent conditions." In this way, Kuhn argues that most normal science consists merely of a "mopping up operation" that extends "the knowledge of those facts that the paradigm display as particularly revealing, by increasing the extent of the match between those facts and the paradigm's predictions and by further articulation of the paradigm itself."

For Kuhn, consensus regarding a particular paradigm breaks down with the emergence of increasingly glaring anomalies that the paradigm cannot adequately account for. The increasing number of anomalies becomes so acute that they generate an intellectual crisis that can only be resolved by articulation and acceptance of a new paradigm that makes a sharp break with the earlier one. The transition between paradigms constitutes the dramatic shift of a scientific revolution. Thus, for example, in the sixteenth century, astronomers came to recognize that Ptolemy's Earth-centric solar system was so cumbersome and inaccurate that it could not possibly exist in nature. Europe's best astronomers came to see the Ptolemaic paradigm as failing in application to its own problems, opening the way for the acceptance of Copernicus' heliocentric system. Kuhn does admit that a new paradigm will incorporate aspects and vocabulary of the old one but that those aspects come to be seen in a new light. In the case of Copernicus' ideas, critics who called him mad for his assertion that the Earth moved were not wrong in one sense. Part of what his critics meant by the term "Earth" was something that, by definition, was fixed and immobile. Copernicus' innovation still used the term Earth but gave the word new meaning—as something that moved. In this way, Kuhn suggests that opposition or resistance to a new paradigm is often legitimate and based

on the confidence that the older paradigm would eventually be able to solve anomalies. Because resistance to a new paradigm is to be expected, the acceptance of a new paradigm will take time. In the end, however, scientists come to accept a new paradigm because it has a greater ability to solve problems than its predecessor.

Although the bulk of Kuhn's analysis and the examples he uses suggest that scientific revolutions are clear-cut and involve dramatic changes in a paradigm, he is inconsistent regarding this point. For in the second enlarged edition of his book where he responds to his critics, Kuhn describes scientific revolutions in a way that blurs the line between normal science and scientific revolutions. Rather inconsistently, he asserts that scientists accept a new paradigm only if it promises to preserve a large portion of the problem-solving ability of the earlier paradigm. Kuhn then says, and he is worth quoting at length here:

> A revolution is for me a special sort of change involving a certain sort of reconstruction of group commitments. But it need not be a large change, nor need it seem revolutionary to those outside a single community, consisting perhaps of fewer than twenty-five people. It is just because this change, little recognized or discussed in the literature of the philosophy of science, occurs so regularly on this smaller scale that revolutionary, as against cumulative, change so badly needs to be understood.[11]

If, as Kuhn suggests in this passage, smaller revolutions occur regularly, how can one distinguish between periods of normal science and scientific revolutions? As Stephen Toulmin notes, by shifting to a discussion of lesser scientific revolutions that occur regularly, Kuhn "transformed the historical development of scientific theory into a 'revolution in perpetuity' even in cases hitherto labeled as normal."[12]

Ernan McMullin salvages the blurring of the lines that Kuhn seems to make in *The Structure of Scientific Revolutions* by distinguishing between two types of scientific revolutions: shallow revolutions and deep revolutions. The former involves some change that leaves most theory intact. McMullin uses the invention of x-rays as an example of a shallow revolution because it left much electromagnetic theory in place. In contrast, the Copernican Revolution was a deep revolution because, according to McMullin, it led to the "transformation in the very idea of what constitutes valid evidence for a claim about the natural order, as well as people's beliefs about how the world is ordered at the most fundamental level."[13]

Kuhn is not the only scholar to see progress in science proceeding in both a cumulative and a discontinuous way. Thus, Robert Merton suggests that science advances along both paths, at times moving in a cumulative way and at others when there are "quantum jumps" in the formulation of ideas and the discovery of empirical uniformities. He believes that such jumps occur when new increments of previous knowledge lead to discoveries that were not anticipated.[14] Other scholars believe that the cumulative character of science only emerged in the nineteenth century and that earlier eras of science experienced greater discontinuity. A.R. Hall argues along these lines and asserts that prior to the nineteenth century, the discontinuous nature of progress was because discoveries tended to be accidental or based on faulty reasoning. Hall concludes that Copernicus "like many other original thinkers…uttered the truth for the wrong reasons."[15]

Although some scholars see both continuity and discontinuity in the development of science, they question whether or not scientific leaps derive from a sense of crisis due to growing anomalies that a paradigm cannot explain. Some like J.D. Bernal see the major advances in science in the seventeenth century as driven by concerns over solving pressing technical problems like those associated with hydraulics, gunnery and navigation.[16] Still others suggest that the shift from one paradigm to another is not so abrupt because alternative paradigms often coexist side by side and therefore any change cannot accurately be described as a scientific revolution. John C. Greene argues this case by noting that in the eighteenth century, the work of Swedish botanist Carl Linnaeus and French naturalist Count Buffon represented two quite distinct paradigms concerning natural history that were diametrically opposed in spirit, presuppositions and concept of scientific method. One saw nature as static and unchanging according to God's plan, while the second saw the natural world as matter that was constantly in motion. What is more, these incompatible views were not a response to anomalies and did not lead to an intellectual crisis where one paradigm replaced another. Rather a compromise between the two views emerged, and George Cuvier, a French scientist, explained the change embodied in species extinction to be the result of catastrophic geological upheavals, but in the periods between the upheavals, permanence and the wise design of the creator prevailed.[17]

Whatever objections scholars have to the dynamic of scientific change described by Thomas Kuhn, most share in common a belief that the seventeenth century does mark a significant watershed in the development of science that might accurately be called a scientific revolution. The major

point of departure for Kuhn's critics is to link dramatic changes in scientific thought to the broader social context, a point we will return to later in the chapter. For now, suffice it to say, as one example, that Greene argues that the greatest change in the development of natural history was not due to crisis-generating anomalies, but due to developments in the British political economy from 1775 to 1835.[18] Indeed, some scholars go so far as to support a narrower definition of scientific revolution—one that is closely linked to conditions in the twentieth century. Thus, C.P. Snow believes that the industrial society composed of electronics, atomic energy and automation is so distinct from other time periods that only it deserves the title of scientific revolution.[19]

Stephen Toulmin provides the most pronounced contrast to Thomas Kuhn's depiction of scientific change. Toulmin doubts whether or not the change in scientific ideas can ever accurately be described as a scientific revolution and he doubts whether or not change within a scientific discipline has ever produced a radical discontinuity. Toulmin points out that even in the case of Copernicus, the changing view took a century and a half to complete, and was debated every step of the way. He concludes from this example that:

> We must face the fact that paradigm-switches are never as complete as the fully-fledged definition implies, that rival paradigms never really amount to entire alternative world views; and that intellectual discontinuities on the theoretical level of science conceal continuities at a deeper methodological level. This done, we must ask ourselves whether the use of the term 'revolution' for such conceptual changes is not itself a rhetorical exaggeration.[20]

In rejecting Kuhn's notion of scientific revolution, Toulmin presents an alternative view of change in scientific ideas—one that is more suggestive of continuity in scientific thought. He begins with the assertion that both the stability in scientific thought and the changes in it need to be explained in the same terms. Toulmin describes his approach to theory development in science as an evolutionary one. He applies Darwin's scheme for species change, to change in scientific ideas. From the standpoint of species evolution in biology, change occurs when genetic novelties or variations prove more advantageous for species survival, and therefore the genetic variation spreads throughout the species. The same process is at work for changing scientific ideas and that those that prove more successful in solving outstanding problems will be accepted by more and more scientists. Seen in this

light, scientific disciplines are like organic species in that they are "evolving 'historical entities', rather than 'eternal beings.'" Hence, Toulmin says:

> In both the zoological and intellectual case, accordingly, historical continuity and change can be seen as alternative results of variation and selective perpetuation, reflecting the comparative success with which different variants meet the current demands to which they are exposed.[21]

Toulmin returns to the Copernican example to illustrate his evolutionary view of the change in scientific ideas. He notes that Copernicus provided a scheme that was more coherent and consistent than the one provided by Ptolemy. But Copernicus achieved this success at a cost—that is with a system that was less simple and less exact at some points than the Ptolemaic construction. (To say nothing of the fact that the idea of the Earth moving was, on the surface, counterintuitive.) What is more, the full computational merits of the Copernican system could only be realized later when Johannes Kepler replaced the traditional circular representation of planetary orbits with elliptical constructions.[22] Such change to the original Copernican system lends credence to a process of change that is indeed cumulative.

Because his notion of scientific revolution implies that the acceptance of a new paradigm by scientists might be subjective, if not irrational, critics charge that Kuhn undermines scientific claims of objectivity. Objectivity is a second feature of science that is thought to distinguish it from other disciplines. Science is thought to be objective in two senses. The first is that scientific knowledge is true—that is, it provides an accurate depiction of the facts of the natural world. The second relates to the mode of inquiry which draws on non-arbitrary and non-subjective criteria for accepting or rejecting theories.[23] Yet, as Ernan McMullin points out with reference to Kuhn's earlier book on Copernicus, Kuhn clearly asserts there that the criteria that contemporary astronomers had for converting to the Copernican view were less than rational. Kuhn says in his earlier work: "The real appeal of sun centered astronomy was aesthetic rather than pragmatic. To astronomers the initial choice between Copernicus' system and Ptolemy's could only be a matter of taste, and matters of taste are the most difficult of all to define or debate."[24]

Subsequently, Kuhn distanced himself from his earlier assertion. In the postscript in the second edition of *Structure of Scientific Revolutions*, he denies the charge that his analysis made science a subjective or irrational enterprise.[25] Kuhn further elaborated his defense in an essay published in 1977. In that essay he notes that he had asserted the best criterion that scientists use to choose between paradigms was the collective judgment of

trained scientists that emerges from the peer review process. Denying that such criterion made theory—in the words of his critics—"a matter of mob psychology," he proceeds to provide a list of characteristics of good scientific theory. His list asserts:

1. Good theory should be accurate within its domain, and the consequences deducible from it should be congruent with the results of existing experiments and observations.
2. Theory should also be consistent internally and with other currently accepted theories applicable to related aspects of nature.
3. Good theories should be broad in scope, and its consequences should extend beyond the particular observations or laws it was initially designed to explain.
4. Theories need to be simple and bring order to phenomena that without it would be individually isolated and as a set confused.
5. Good theory should also be fruitful for further research.

Kuhn argues that this list provides the shared basis for theory choice, and hence that group choice is objective, rational and not subjective.[26] Stephen Toulmin reinforces Kuhn's insistence on objectivity in science and says that when evaluating theories, scientists do not ask if it is true or false, but rather if it improves explanatory powers better than its rivals.[27]

While Kuhn's list does imply that there is a certain objectivity to science in terms of choosing paradigms, he does admit that a good theory may not meet all five of his characteristics. For example, he says that Copernicus' theory was not more accurate than Ptolemy's until it was revised by Kepler after Copernicus' death. Ptolemy's was not only consistent internally, but it was also consistent within related fields in a way that Copernicus' was not. However, Copernicus' system was simpler, and this was important to the choice made by later scientists like Kepler and Galileo. In shaping subsequent research, thinking of theory as a research program in the manner of Imre Lakatos lends credence to the objectivity of science even when all of Kuhn's criteria are not met. Progressive research programs help discover unknown and novel facts and are therefore superior to degenerating programs where theory is created only to accommodate known facts.[28] Meanwhile, Kuhn concludes from his example of Copernicus and Ptolemy that "When scientists must choose between competing theories, two men fully committed to the same list of criteria for choice may nevertheless reach different conclusions."[29]

Nevertheless, Kuhn does admit that different choices made according to the shared criteria may ultimately hinge on individual, idiosyncratic factors related to an individual's personality and biography.[30] In this way, subjective elements may still influence theory choice, but we must assume that the impact of such factors is negligible if we wish to adhere strictly to the notion that science is objective.

Helen Longino provides analysis that can minimize the impact of subjective factors on science. She emphasizes that it is the social character of science that minimizes the influence of subjectivity—a social character that is especially pronounced in the twentieth century. She argues that the very process of peer review and ongoing critical scrutiny from alternative viewpoints ensures that any individual scientist's interpretation of data remains free from their subjective preferences. The objectivity of science in this formulation is therefore a characteristic of a community's practice of science rather than an individual's.[31] Indeed, Longino's argument reinforces Kuhn's point that the criterion of collective judgment was not a matter of mob psychology but rather one that ensured objectivity. While it is true that the objectivity of science is sometimes wrongly construed as attributing a certain disinterestedness of individual scientists,[32] this attribution should not undermine the objectivity of science as practiced by a community.

For Longino, the objectivity of a scientific community still depends on the degree that it satisfies four conditions she outlines:

1. The existence of recognized avenues for critical examination of evidence, methods, assumptions and reasoning.
2. The existence of shared standards like those enumerated by Thomas Kuhn that critics can reference.
3. A degree of responsiveness to such criticism that can be measured by things like content in textbooks and access to financial support.
4. Equal sharing of intellectual authority among qualified practitioners so that alternative views are not suppressed.[33]

Given these conditions and the social character of the scientific process as posited by Longino, the objectivity of science and its progress necessarily depend on the broader social and institutional context. We now turn to the question then, of the extent to which scientific ideas occur autonomously or are embedded in the larger social and institutional setting.

Thomas Kuhn recognized the impact of the larger social context on the development of science because he believed external conditions could transform a mere anomaly into a source of acute crisis.[34] Indeed,

Toulmin credited Kuhn for emphasizing the connections between the socio-historical development of scientific schools and institutions with conceptual development.[35] That external influence on science has two components: normative and institutional. From a normative perspective, the growth and progress of science require a cultural milieu that is receptive to it. Arthur Koestler calls this precondition for scientific development the "ripeness" of the era or the climate of the age.[36] For Toulmin, one crucial element in that cultural milieu is a certain openness to new ideas. The cultural setting cannot view innovation as a threat. And Toulmin points out that such a condition is rare in many civilizations. More typical is a pattern of heresy hunting or intellectual conformism because political and ecclesiastical authorities are rarely happy to see critical examination of the foundation of their conceptual inheritance for fear that such an examination might put their authority at risk. Toulmin suggests this feature was absent in China and accounts for the fact that they did not develop astronomical ideas.[37]

The influence of the broad cultural patterns is apparent in the transition in Europe from the medieval worldview to the more modern scientific one. That medieval view conceived of God as the First Cause of things, and A.R. Hall suggests that the notion of "laws of nature" arose from the interaction between religious and philosophic ideas of the medieval European world.[38] Medieval ideas regarding nature rested on the natural history provided by Aristotle combined with Christian belief. Those beliefs were propagated by monasteries that were then the primary educational institutions in the medieval world.

The transition to what we might recognize as a more scientific cultural setting was not abrupt and emerged gradually. In part, the emergence of a normative framework more conducive to scientific processes grew from practical needs. Thus, the growth and development of astronomy was partly driven by the social pressure for calendar reform. Astronomers developed increasingly accurate procedures for predicting celestial motions to support the need for a calendar controlled by authorities. Since planets were regarded as divine, piety made keeping planetary records and improving forecasting techniques matters of national concern.[39] Along with this, the voyages of discovery and the demand for improved navigational techniques reinforced the importance of astronomy so that the Copernican breakthrough can be similarly linked to social needs.[40]

Economic changes in late medieval Europe were also conducive to growth in a scientific approach, for in contrast to feudal society ruled by tradition and custom, nascent capitalism depended on a certain rationality supported by

measurement and calculation. In this way, "the rise of economic rationality furthered the development of rational scientific methods."[41] At the same time, emerging capitalism under the consolidation of monarchs and the end of decentralized political arrangements provided patronage and financial support to scientists as an alternative to ties with the church.[42]

Nowhere were the religious normative roots of science more in evidence than in the sixteenth- and seventeenth-century Europe. The Protestant ideal to glorify God facilitated the emergence of science. By the seventeenth century, major natural philosophers recognized a clear connection between scientific concerns and religious and social ones. Robert Merton emphasized the religious motivation for science. Drawing on insights of Max Weber, Merton saw in the Puritan ethic a driving force of seventeenth-century science. This ethos contained "the exaltation of the faculty of reason" based partly on the view that rationality provided a means for curbing passions which led, in turn, "to a sympathetic attitude toward those activities which demand the constant application of reason." Merton notes that the leading figures in one of the first scientific organizations, London's Royal Society, were eminently religious, and, in 1663, 62 percent of its members were Puritans—this at a time when Puritans constituted a small minority in the English population.[43] The study of nature as a means to glorify God became such a commonplace notion that Francis Bacon made it a dominant theme for his program to reform leaning. He believed scientific enterprise to "constitute the truest form of religious worship."[44]

The crucial role of religious belief was not easily severed. Up to the mid-seventeenth century, a high proportion of scientists were men of deep religious conviction. Even Isaac Newton saw God as the Final Cause of things. Arthur Koestler quotes Newton on this point as believing that the placement of the planets around the Sun provided proof that creation was the work of an "intelligent agent…not blind or fortuitous." Without divine power to support it, the pressure from gravity would collapse the universe and "the small irregularities in the planetary motions would accumulate and throw the whole system out of gear if God did not from time to time set it right."[45] Stephen Toulmin argues that up until 1800, scientists retained an ahistorical worldview that combined philosophical doctrines inherited from the Greeks and the biblical time scale that saw the age of the Earth to be only a few thousand years. Consequently, seventeenth-century scientists had no grasp of the antiquity of the world which concealed from them the mutability of all natural things.[46] This

religious heritage carried over into the eighteenth century, providing a conception of nature:

> as a system of matter in motion with a dominant view of stability and wise design of the fundamental structures of nature by supposing that God had so contrived the properties of matter and the laws of motion that would perpetually produce the stable world required for his moral purpose.[47]

Given the religious heritage and vision that carried over in the nineteenth century, how startling must Darwin's theory of natural history have been. Yet his ideas concerning natural selection and elimination of species in a struggle for survival that undermined notions of immutability found fertile soil to take root in Britain because Englishmen were already "steeped in the tradition [of political economy] of Adam Smith, Malthus and Ricardo."[48]

Whatever the influence of the cultural setting for the development of science, that influence did not run in one direction. That is, changing scientific ideas and concepts had a reciprocating impact on the larger social setting. Indeed, Robert Merton formulated his idea of the sociology of science on the recognition of the dynamic interdependence between science and its social environment.[49] Here, the examples of Copernicus, Kepler and Galileo are instructive. These pioneers set out to understand the universe, but the unintended consequence of developing their understanding left an indelible impact on the social order. They helped undermine "The medieval vision of an immutable social order in a walled in universe together with its fixed hierarchy of moral values, and transformed the European landscape, society, culture, habits and general outlook."[50] Hence, the interaction between science and society may be described as one where, in the short-run, science is shaped by existing ideas, and social norms help define the value and purpose of scientific work. In the long run, scientists may alter the general conceptions that shaped their inquiries so that "the lines between science, ideology and world views are seldom tightly drawn."[51] However, there is nothing inevitable about the reciprocating influence of science on the larger society. Such mutual interaction is only possible where there are no high barriers between science and the broader society. Toulmin points to the example of Babylonian astronomy whose ideas and methods were state secrets that could not be revealed to those outside a narrow community of scholars. Hence, in this case, "…the 'resonance' between the specialized discipline and the larger public mind is so weak that innovations within the professional guild—however striking in local effect—woke no echoes in the broader ideas of the culture."[52]

While the cultural milieu and social context may be necessary conditions for progress in science, norms are not sufficient in themselves to prompt scientific advance. They need something else. Institutions to support science and forums for discussing scientific ideas are also required. The formation of the Royal Society in 1660 in the "invisible college" of natural philosophers facilitated the growth and spread of scientific ideas. Because, as we noted earlier, the objectivity found in science is an attribute of collective judgment, such judgment is not possible without a forum or institutional framework for scientists. Among other things, early scientific societies made it possible to exclude charlatans and prevent fraud, making it easier for the general public to evaluate scientific claims.[53] The eighteenth century saw the construction of long-distance networks that allowed for communication, replication of practice and circulation of instruments and texts. This expansion of scientific commerce consolidated knowledge while at the same time reinforcing the social character of science that became global, leading to greater confidence in the universal validity of scientific knowledge.[54] It goes without saying that institutions of science proliferated in the nineteenth century so that it is hard to conceive of twentieth-century science without recognition of its institutions.

To be sure, institutions always have the potential to back the status quo in a way that curtails or inhibits new ideas. Thus, for example, Arthur Koestler notes that the institutional obstacles that confronted Galileo did not come from Jesuit astronomers who were already won over by the Copernican system. Rather, his opposition came from Aristotelians at universities who had a vested interest in tradition and in retaining their monopoly of learning. Innovation threatened their authority, and they had a "deeper fear that their whole, laboriously constructed edifice might collapse."[55] Despite the hazards that entrenched institutions might stifle new ideas, the absence of institutional connections can be equally stultifying. This was certainly the fate of Gregor Mendel's ideas concerning genetic inheritance. Isolated as he was from fellow scientists, it was hard for them to recognize the significance of his work and the wider theoretical implication of his ideas.[56]

Just as early scientific institutions like the Royal Society facilitated the growth and development of scientific ideas, so did institutions, particularly in the nineteenth and twentieth centuries, enhance the possibility of using science in public policy. In its contributions to public policy, the role science played, the path it followed and the issues encountered along the way were similar to those experienced by the social sciences outlined in Chap. 1.

To begin with, scientific communities make a clear distinction between "abstract" and "practical" science, that is, between "pure" research and "applied" research. The purpose of abstract or pure research is, of course, knowledge for its own sake and the development of scientific ideas. Practical or applied science is directed at material improvements and hence has the potential to be harnessed directly for public policy.[57] This distinction emerged as early as the Royal Society whose research focus tended to concentrate on topics with practical application like improving navigation. Such a research emphasis proved a useful way for scientists to justify their work to political authorities and the general public.[58] Indeed, Edgar Zilsel attributes the very advancement of science in the early days to practical men like mariners, shipbuilders and foundrymen who, he argues, were "the real pioneers of empirical observation, experimentation and causal research."[59] Moreover, although the Industrial Revolution itself owed little directly to science, "the men who directed its progress were thoroughly imbued with scientific spirit."[60] Over the course of the nineteenth and twentieth centuries, the distinction between pure and applied research seemed to sharpen, leading C.P. Snow to observe that engineers as practitioners and pure scientists often misunderstood each other to such an extent that pure scientists tended to dismiss applied research as an occupation for second-rate minds.[61] While Snow's assertion may be an exaggeration, the relationship between pure and applied research was not always clear to outside observers and the extent to which pure research contributed solutions to practical problems was murky. Because of this, public colleges in the United States taught a science dominated by the practical considerations of engineering and agricultural studies, while in private colleges, such teaching was considered vocational training and inappropriate for a liberal education. Herbert Hoover provided one explicit recognition of the link between pure and applied research when he stated that the unemployment caused by technological advance could be relieved by new industries creating new products and services. These new industries, in turn, would be dependent on "pure science research feeding its raw materials into the hoppers of applied laboratories."[62]

Despite Hoover's claim, the emphasis of American science in the nineteenth and early twentieth centuries remained focused on the practical as opposed to abstract work. Nowhere was this emphasis more obvious than in government organizations to foster science. Realizing the potential for science to aid in economic development of the country, Congress founded the US Geological Survey in 1879 following on a recommendation of the National Academy of Sciences. The Survey was part of the US Department

of the Interior and was charged with classifying public lands and examining geological structures and mineral resources. Because the Survey had access to the US Government Printing Office, geologists had, in essence, a forum for peer review providing the equivalent of their own major research journal. Throughout the nineteenth century then, with the Geological Survey, the Weather Service and the Naval Observatory, the bulk of federal science remained limited to fields pertinent to land usage.[63]

Reflected in the government's growing interest in science was the changing nature of research institutions and the basis of their financial support. J.D. Bernal breaks the changing institutions of science into three stages. From its origins to the 1890s was the era of private science where well-to-do aristocrats could support small laboratories and individual scientists. Between the 1920s and the 1930s came the era of industrial science with expanded university departments and subsidized research institutes. This second era included work done in corporations like AT & T, DuPont, Westinghouse and General Electric. The third phase identified by Bernal includes World War II and the postwar. In this era, government became an important (if not primary) source of support for science.[64] Reliance on public sector finance provided some challenge for scientists as scientific research became vulnerable to the capriciousness of politics. For whenever political leaders became concerned about excessive government spending, what might look like "frivolous" science seemed a logical place to cut spending.[65]

Nevertheless, there was a steady growth in the number of people in the scientific professions. By 1914, membership in the American Association for the Advancement of Science saw a fourfold increase since the turn of the century, reaching past 8000. The growth of the community of physicists increased, and membership in the Institute of Physics went from 908 in 1938 to 6863 by 1962.[66]

Both the gradual reconciliation of the pure/applied science divide and the growth of the role of the government in scientific research were driven by the same variable: the exigencies of war. To be sure, national security concerns, broadly defined, lay at the center of practical scientific research since the sixteenth and seventeenth centuries. The improvement in navigation and metallurgy were central interests of governments engaged in imperial pursuits. However, by the nineteenth century, the link between science and national security grew even stronger. Thus, as noted in Chap. 1, in the United States, the National Academy of Sciences was founded in 1863 as a private organization with a federal charter, specifically to provide expert scientific advice to the government in the Civil War.

This pattern of government support and the use of science continued in World War I. While land use continued to be an area for science to aid public policy, its application in war became more pronounced. The National Research Council (NRC) was founded in 1915, with the objective of encouraging scientific research that would be useful for war, and Woodrow Wilson viewed the Council as part of the US preparedness in the event of war. One of the Council's first tasks was technical, to find some way to detect U-boats then threatening the Atlantic Ocean. However, the NRC lacked the resources and authority to conduct research independently of the military. Consequently, an inherent tension emerged between civilian scientists and their methods and the military chain of command.[67] At the same time, there emerged in some circles a growing concern that scientific research was becoming dominated by the needs of the military. The estrangement between the scientists and the government was exacerbated after World War I because the NRC sought to exclude and isolate scientists from the Central Powers—the former enemy states and the NRC fell into disuse after the war.[68] During the interwar period with the crisis of the Great Depression, critics of the military orientation of scientific work found support for calls to redirecting research away from the military and toward solving pressing economic and social problems.

As the country began to mobilize for World War II, the need for scientific research to support the war effort was recognized in policy circles. What is more, the problems highlighted by the NRC experience provided lessons for the organization of science in this war. Vannevar Bush, who participated in technical mobilization in World War I, was skeptical of the military's ability to foster innovation of new weapons, and so planned a new federal agency for this purpose. President Roosevelt approved this organization—the National Defense Research Committee (NDRC)—in June 1940. As its head, Bush agreed to conduct defense research by contracting with universities and industrial corporations. While freeing scientific research from direct control by the military, the NDRC still did not have the authority to approve development of its preliminary models into prototypes for production. Greater autonomy of scientific work was still needed. After a report by the Bureau of the Budget on the organization of defense science was issued in 1941, President Roosevelt issued an executive order that made the Bush organization eligible for direct congressional appropriations as well as authority to take ideas for new weapons to the production stage. The NDRC then became a subsidiary organ of a new agency: the Office of Scientific Research and Development (OSRD) under the direction of Vannevar Bush.[69]

The success of OSRD for developing new weapons was derived from the fact that Bush had direct access to the president and was able, therefore, to interact with general officers on an equal footing. OSRD had the ability to develop weapons that generals might not necessarily want. In addition, as Don Price points out, Bush was able to shield younger scientists from the draft, which was one factor that contributed to the successful development and use of radar.[70] The contrast between the organizations harnessing science in the two world wars was stark. By 1917, the less effective NRC had contributed little to national defense, but by December 1941, the NDRC and then the stronger OSRD provided a good 18 months of military research and development.[71]

OSRD was not only successful in creating the hardware and technology to support the war effort, it ventured into the realm of strategy as well. Vannevar Bush believed that scientists should play a role in shaping strategy and asserted, "In a scientific war, scientists should aid in making plans."[72] The successful push for scientific input in strategy can be illustrated by the change in anti-submarine strategy. The Navy had relied on the defensive approach using convoys to protect shipping in the Atlantic. Admiral King opposed the integration of scientists into his strategic council with their recommendation of shifting to an offensive anti-submarine strategy. OSRD scientists believed that using their devices like airborne rockets and magnetic detection would improve the effectiveness of aircraft hunting down and attacking German submarines. Admiral King was sufficiently impressed with the results of the offensive strategy such that, by 1943, operational scientists assumed more general duties beyond anti-submarine warfare in the Navy's strategic planning. By 1944, Admiral King established a Scientific Council in the Navy staff as the right hand of the commander of the fleet.

A similar story of the successful use of scientists for strategy can be found in the use of radar and strategic bombing on the European continent. In fact, the discipline of operations research defined as "the application of the scientific method to study operations of large complex organizations" began its use in the military with scientists who developed radar and who were asked how it ought to be used.[73] By 1945, worldwide, the US Army Air Force commands had 17 groups of scientists and operational analysts employing 32 mathematicians, 21 radio and radar engineers and 11 physicists. Daniel Kevles concludes of the role of scientists in strategy: "By V-J Day from Africa to Southeast Asia and on to the Aleutians, civilian scientists were in vogue as strategic and operational advisors to a degree without precedent in the annals of American military history."[74]

As might be expected from this wartime success, Bush and others sought to carry the relationship between science and the military over to the postwar. The struggle to create the National Science Foundation described in Chap. 1 was part of this effort. Yet with this push for institutional government ties to science came the fear that scientific research was becoming dominated by the military. In particular, scientists were concerned that atomic research, because of its use for weapons, would be controlled by the military. *The Bulletin of Atomic Scientists* was founded, in part, to assuage this fear. Indeed, the Atomic Energy Act of 1946 and the creation of the Atomic Energy Commission (AEC) headed by David E. Lilienthal were viewed as points of victory for civilian control over research in the field because they created a state monopoly over atomic research whose members were appointed by the president. Moreover, most policy relating to atomic weapons, from the Baruch Plan, the hydrogen bomb and ballistic missiles to the test ban, originated from scientists. Only the doctrine of massive retaliation originated elsewhere.[75]

The contributions of science in World War II related to the increasingly technical nature of weapons and conduct of warfare. The same challenge that atomic power presented for social scientists grappling with the new strategic environment in the postwar, led policy-makers to seek permanent technical advice from scientists. Thus, in 1954, President Eisenhower requested that the Science Advisory Committee undertake a comprehensive review of military technology. The Soviet success in launching the Sputnik in 1957 increased concerns of the danger should the United States fall behind the Soviets in science. The National Defense Education Act of 1958 was intended to foster scientific education. At the same time, President Eisenhower created a new position of special assistant to the president of Science and Technology, and named James Killian, president of Massachusetts Institute of Technology (MIT), to the post.

President Kennedy continued with the pattern of drawing on scientific expertise and appointed a Nobel Prize chemist, Glenn T. Seaborg, as chairmen of the Atomic Energy Commission in 1961.[76] President Kennedy explicitly affirmed a commitment to science in a speech before the National Academy of Sciences in 1963. In that speech, the president recognized the contribution that abstract scientific research made to progress in technology. He then went on to declare: “I think that never in the history of science has the time been brighter, the need greater for the cooperation between those of us who work in government and those of you who may work in laboratories.”[77]

The very prominent and public role for science in policy during the Cold War sharpened concern over protecting the autonomy of scientists from state—especially the military—control at the same time scientific research was becoming even more dependent on government financial support. Both factors raised a conundrum regarding the relationship between values and science similar to the one raised by Gabriel Almond in 1946, cited in Chap. 1, concerning social science and public policy. Almond reconciled values with social science by noting that social science could not create values but could offer alternatives for how given values might be realized. So too Don Price offers a reminder:

> Scientific methods are most useful in determining how a specific thing is to be done…they are less often and less immediately useful in determining whether or when such things are to be done and how much effort or money is to be spent on them.[78]

The story of the decision to build a hydrogen bomb offers a morality tale that illustrates the above limits to scientific knowledge for public policy, and the extent to which objective science can easily become politicized. Although the General Advisory Committee of the Atomic Energy Commission recommended the United States not push for a crash program to develop a hydrogen bomb, events outside of science led President Truman, supported by Secretary of State Dean Acheson, to approve such a program. These events included the successful Soviet test of an atomic bomb in 1949 and the fall of China to communist forces in the same year. Both events created political pressure to offset the apparent advantages of the Communist Bloc. The physics community was divided over the issue. Edward Teller was the major proponent of the hydrogen bomb project. Robert Oppenheimer opposed the program on the grounds that the weapon's great destructiveness amounted to a weapon for mass genocide. Once approved, the hydrogen bomb then was first tested in 1952.

Because of his opposition to the hydrogen bomb, Oppenheimer had his security clearance revoked and was charged with having communist and left-wing associations. In this process, Edward Teller served as a witness that testified against Oppenheimer. Eventually, Oppenheimer's reputation was restored when President Kennedy decided to give the Fermi Award of the AEC to Oppenheimer. In this case, although scientists remained on advisory panels, a minority view among leading nuclear scientists was the view that was most closely heeded.[79]

Our narrative outlining debates over the philosophy of science suggests that social science has tended to hold a distorted image of science—an image that, to some degree, has been fostered by scientists themselves. As there is no consensus among scholars of the philosophy of science of the extent to which science develops in a cumulative fashion or what the source and nature of scientific objectivity in science are, the pursuit of these characteristics by social science is at best misguided. We saw in Chap. 1, regarding the debates over membership in the National Science Foundation, that scientists themselves have repeatedly denigrated the scientific pretensions of social scientists. Thomas Kuhn, for example, questions whether the social sciences have acquired paradigms, that essential ingredient for normal science to proceed. Kuhn goes on to observe that, at best, the road to a firm research consensus is "extraordinarily arduous." In its absence, researchers must forever rebuild their field anew, justifying the use of each concept introduced. Kuhn says: "If we doubt, as many do, that non-scientific fields make progress, that cannot be because individual schools make none. Rather it must be because there are always competing schools, each of which constantly questions the very foundations of the others."[80] The validity of Kuhn's observation will be illustrated in Chaps. 3 and 4, in the discussion deterrence/coercion theory and modernization theory, respectively. Yet, at the same time that our discussion of particular social science theories shows the flaw in adopting science as a model, we would do well to remember Karl Poppers observation—even though a theory may not warrant the label science, it does not mean that it is not important.

For the last feature of science concerning whether or not it can be divorced from the broader social context or whether scientific ideas spring autonomously from pure logic, there is greater agreement among philosophers of science. Scientific progress depends on the broader cultural and institutional setting. In this feature, social sciences do resemble the natural sciences with one very important difference. Applied science in public policy does not necessarily require normative judgment, although, as the controversy over the hydrogen bomb illustrates, scientists may actually attempt to do so. In application, a scientist does not need to ask if, say, new navigational techniques are "good," only that they are effective in achieving their goal. In contrast, any applied social science must necessarily make a judgment about whether a policy based on its findings is good or bad. Because of the manner that social science applied to public policy is embedded in normative judgment, we see in the social sciences that conflicted identity noted in Chap. 1 and the tendency for social science

disciplines to divide into humanist and scientific wings. John Greene sees the origin of these two identities as emerging in the work of Auguste Comte. Greene says of this legacy:

> Given the conviction that human history was a gradual ascent from brutelike beginnings and the concomitant conviction that all natural and historical events are subject to law, it was inevitable that there should have been attempts to formulate the laws of historical development. It was inevitable too, that these attempts should have involved heavy borrowing of concepts and principles from older fields of inquiry…it was natural that social evolutionists of the nineteenth century, should have attempted to validate their programs of political and social action by claiming the sanction of science for their philosophy of history. As supernatural sanctions were discredited and the prestige of science grew, social prophets assumed the role of scientists.[81]

One feature that science and social science shared as their roles in public policy grew, was a concern that government financial support for their work might circumscribe the autonomy of their research. Most worrisome, given the fact that scholars made contributions to the war effort during World War II, was that research might come to be dominated by the military. Such concerns will be highlighted in the following chapters.

Notes

1. This discussion of issues related to the philosophy of science is necessarily limited to those germane to the image that social scientists tend to hold concerning science. For a comprehensive analysis of issues, see Martin Curd, J.A. Cover and Christopher Pincock, eds., *The Philosophy of Science: The Central Issues,* 2nd ed., (New York: W.W. Norton and Company, 2013).
2. Daniel J. Kevles, *The Physicists: The History of a Scientific Community in America* (New York: Alfred A. Knopf, 1978), 4.
3. Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd enlarged ed. (Chicago: University of Chicago Press, 1970), 139, 165.
4. Robert K. Merton, *Social Theory and Social Structure*, enlarged ed., (New York: The Free Press, 1968), 4, 5.
5. Merton, 605. Stephen Toulmin, *Human Understanding: The Collective Use and Evolution of Concepts* (Princeton: Princeton University Press, 1972), 175, 282.
6. J. D. Bernal, *Science in History* (New York: Hawthorne Books, Inc. 1965), 5, 6.
7. Karl Popper, "Science: Conjectures and Refutations," in *Philosophy of Science: The Central Issues,* 2nd ed., eds. Martin Curd, J.A. Cover and Christopher Pincock (New York: W.W. Norton and Company, 2013), 3–10.

8. Imre Lakatos, "Science and Pseudoscience," in *Philosophy of Science: The Central Issues,* 2nd ed., eds. Martin Curd, J.A. Cover and Christopher Pincock (New York: W.W. Norton and Company, 2013), 20–26.
9. Kuhn, 1. Unless noted otherwise, the source for this discussion is *The Structure of Scientific Revolutions.*
10. Arthur Koestler, *The Sleepwalkers: A History of Man's Changing Vision of the Universe* (New York: Grosset and Dunlap, 1959), 15, 513.
11. Kuhn, 180–181.
12. Toulmin, 112.
13. Ernan McMullin, "Rationality and Paradigm Change in Science," in *The Philosophy of Science: The Central Issues*, 2nd ed., eds. Martin Curd, J.A. Cover and Christopher Pincock (New York: W.W. Norton and Company, 2013), 111–130.
14. Merton, 9.
15. A.R. Hall, *The Scientific Revolution 1500–1800: The Formation of the Modern Scientific Attitude*, 2nd ed., (Boston: Beacon Press, 1954), xiv, 68.
16. Bernal, 310.
17. John C. Greene, *Science, Ideology and World View* (Berkeley: University of Berkeley Press, 1981), 5, 36, 38–39.
18. Greene, 47.
19. C.P. Snow, *The Two Cultures and the Scientific Revolution* (New York: Cambridge University Press, 1959), 31.
20. Toulmin, 105–106.
21. Toulmin, 140–141.
22. Toulmin, 228.
23. Helen E. Longino, "Values, and Objectivity," in *Philosophy of Science: The Central Issues* 2nd ed., eds. Martin Curd, J.A. Cover and Christopher Pincock (New York: W.W. Norton and Company, 2013), 144–145.
24. Quoted in McMullin, 124.
25. Kuhn, 175.
26. Thomas S. Kuhn, "Objectivity, Value Judgement, and Theory Choice," in *Philosophy of Science: The Central Issues* 2nd ed., eds. Martin Curd, J.A. Cover and Christopher Pincock (New York: W. W. Norton and Company, 2013), 95.
27. Toulmin, 225.
28. Lakatos, 25.
29. Kuhn, "Objectivity, Value Judgment, and Theory Choice," 95–96.
30. Kuhn, "Objectivity, Value Judgment, and Theory Choice," 98.
31. Longino, 148, 154, 153.
32. See, for example, Koestler, 115, and Toulmin, 267, Toulmin goes on to say that scientists themselves often cultivate the image of disinterestedness to foster an image of science as a rational enterprise exempt from general principles of political and social action.

33. Longino, 154–155.
34. Kuhn, *Structure of Scientific Revolutions*, x.
35. Toulmin, 116.
36. Koestler, 519–520.
37. Toulmin, 219, 220.
38. Hall, xvii, 172.
39. Toulmin, 213.
40. Bernal, 276.
41. Edgar Zilsel, "The Sociological Roots of Science," *Social Studies of Science* 30 (December 2000): 937.
42. Bernal, 258.
43. Merton, 633, 239.
44. P.M. Rattansi, "The Social Interpretation of Science in the Seventeenth Century," in *Science and Society, 1600–1900* ed., Peter Mathias (Cambridge: Cambridge University Press, 1972), 13.
45. Koestler, 525–526.
46. Toulmin, 13–16.
47. Greene, 14.
48. Greene, 49.
49. Merton, 585.
50. Koestler, 13.
51. Greene, 2.
52. Toulmin, 296–298.
53. Bernal, 318.
54. Jan Golinski, "Science in the Enlightenment, Revisited," *History of Science* 49 (June 2011), 221–222.
55. Koestler, 427.
56. Toulmin, 210.
57. Kevles, 6.
58. Merton, 681.
59. Zilsel, 940.
60. Bernal, 370–371.
61. Snow, 33.
62. Kevles, 9, 263.
63. Kevles, 37, 59.
64. Bernal, 508.
65. Kevles, 53, 63–64.
66. Kevles, 94; Bernal, 519.
67. Kevles, 112, 115, 117–138.
68. Kevles, 194–195, 291.
69. Kevles, 296, 299, 300.
70. Price, 44–48.
71. Kevles, 301.

72. Kevles, 309.
73. Price, 126.
74. Kevles, 312–316, 320.
75. Kevles, 351–352. James R. Killian, "Science and Foreign Policy," in *The Dimensions of Diplomacy* ed., E.A.J. Johnson, (Baltimore: The Johns Hopkins Press, 1968), 74.
76. Kevles, 390. The AEC was abolished in 1974 and replaced with the Nuclear Regulatory Agency and became a component of the US Department of Energy.
77. Kevles, 390. Killian, 59–60.
78. Price, 166.
79. Kevles, 377–382.
80. Kuhn, *The Structure of Scientific Revolutions*, 15, 18, 20, 162–163.
81. Greene, 87.

CHAPTER 3

The Science of Strategy: Deterrence and Coercion Theory

Novel conditions in the strategic environment after World War II called forth an imperative for novel analytical frameworks for understanding them. The most dramatic change, of course, one we might call a revolution in weaponry, came with the introduction of a new technology of warfare, the atom bomb. The destructive capability that could now be delivered with unprecedented speed, accompanied by the potential for even a weak country to inflict national destruction on a stronger one, raised the question of whether it would be possible to follow the maxim of Karl von Clausewitz that the military means used must be commensurate with the political ends sought. What is more, the technological innovation was embedded in a geopolitical setting where two continental-sized superpowers embracing antagonistic ideologies were seeking to incorporate other countries into their respective spheres of influence. For the United States, the key question was how to wield this new weapon in a manner that would influence the behavior of its Soviet adversary. Finding the answer to the question of how to use this new weapon was difficult, as evidenced by the fact that international control of atomic power remained the only official policy enunciated by the United States through the summer of 1948. As noted in the previous chapter, ideas about controlling or limiting these weapons came, for the most part, from nuclear scientists. Such a slow response can be understood by the fact that throughout the 1940s, the United States possessed a limited atomic stockpile—9 in 1946, 13 in 1947 and 50 in 1948—with none of the weapons assembled for use.[1]

J. M. Klinger, *Social Science and National Security Policy*,
https://doi.org/10.1007/978-3-030-11251-6_3

The difficulty in formulating an answer was also compounded by interservice rivalries concerning control over the new weapons because control over the new weapon would naturally translate into a bigger share of the defense budget for the service with the control. Indeed, President Eisenhower reorganized the Defense Department twice in his administration in an attempt to end such rivalries.[2] In the end, the answer to the question was outlined in deterrence and coercion theory whose point of departure was to conceive of the military instrument less as a tool for victory on the battlefield, and more as a tool for diplomatic bargaining. Such thinking was made possible in the atomic age because unlike earlier eras when it was necessary to defeat an opponent's military forces before inflicting punishment, now it was possible to punish an opponent even though his military forces remained intact.

This chapter provides an overview of deterrence and coercion theory as it emerged in the early days of the Cold War. In order to do so, we must examine the evolution of the RAND Corporation and some of its key individuals who played the role of policy entrepreneur and whose ideas seeped into the policy circles from the late 1940s to the mid-1960s. From the outset, deterrence and coercion theory sought to broaden the very concept of strategy away from a focus on military victory to include pursuit of diplomatic advantage. By so doing, as Stephen Maxell suggests, the theory "turns the strategist into a political analyst, and the politician into a general."[3] Further, as William Kaufmann observed in 1964, the atomic bomb "inevitably brought into question both the relevance of wartime experience to the conduct of future military operations and the utility of maximum force as an instrument of foreign policy."[4] Within such a context, the tension between military and civilian strategists would inevitably sharpen. Such tension can be illustrated by the contrasting views that emerged concerning the role of civilian strategists. Bernard Brodie, writing in *World Politics* in 1949, took great pains to explain why conventional military thinking was inadequate for the nuclear age because it remained wedded to "enduring principles" that induced a certain rigidity of thought. Moreover, he believed the novelty of nuclear weapons meant that past experience no longer provided a reliable framework for making military judgments.[5] Brodie's view was reinforced by Joseph Kraft writing for *Esquire* in 1962, who noted that academic theorists moving into the field of military policy brought "a degree of subtlety, sophistication and intellectual rigor that was long overdue."[6]

A contrasting view, however, was presented by General Thomas White writing in *The Saturday Evening Post* in 1963, who made a scathing indictment of the defense intellectuals he characterized as "pipe smoking, tree-full-of-owls types" who lacked sufficient worldliness to stand up to the Soviets. What is more, he asserted that such defense intellectuals lacked the military training that "teaches the philosophy of victory" rather than political compromise.[7] In particular, General White found ideas promoted by these intellectuals to be pernicious and said that the notion of a stable nuclear deterrent was "the most misguided military theme yet conceived." He believed that true security lay in unlimited nuclear supremacy.[8]

Despite their different views, the civilian and the military strategists of the early Cold War did share one thing in common: both remained under the shadow of their World War II experience. The civilian thinkers who defined deterrence and coercion theory remained haunted by the prospect of another Pearl Harbor and surprise attack, a concern sufficiently pronounced that George Kennan characterized it as "simply obsessive."[9] The military strategists and planners for their part were modeling campaigns against the Soviets on the assumption of a total war, much like the previous one, but with the addition of nuclear weapons. Our narrative depicting the evolution of deterrence and coercion theory will expand on these themes concerning the civilian military divide and the lessons drawn from World War II. In particular, we shall see how core concepts and maxims contained in deterrence and coercion theory ran against the grain of established military thinking. However, one other lesson from World War II remains important for our discussion, and that is the widespread consensus that science and social scientific links with policy-makers that were forged during that war needed to continue. Furthermore, those links needed to include a broader disciplinary focus that integrated insights from psychology, sociology, political science and economics. The continuation of such links also required creating the appropriate institutional setting for collaboration, and part of that setting was the establishment of the RAND Corporation.

Despite some early efforts at integrating scientific research into military policy and strategy, some members of the Air Force were concerned that the military might not retain sufficient control over scientific research. Thus, for example, when General Henry "Hap" Arnold testified at the Senate hearings on the National Science Foundation, he both applauded the effort to foster a national research capability and expressed concerns that the interests of the military would not be represented there.[10] As such, General Arnold represented a partially parochial view that sought to protect and expand the

role of the Air Force (which had become an independent service in 1947) through its possession of the most important technology of the postwar—atomic weapons. Martin Collins describes this view as creating a certain "ideology of preparedness" that would overcome any lasting trauma of Pearl Harbor.[11] Technological innovation meant that the United States could no longer afford to continue the pattern of both world wars and begin mobilization after hostilities began. The nuclear age had abolished this luxury of time and distance so that advanced preparation was essential. This ideology of preparedness, in turn, required scientists working directly for the military and not mediated through contractual arrangements with universities. And this ideology of preparedness helped shape the content of deterrence and coercion theory, and all found an institutional home in that offspring of the Air Force—the RAND Corporation.

The idea for a new kind of scientific community devoted to the study of air warfare, and one especially suited to the nuclear age, grew from a report prepared by General Arnold's science advisor, Theodore von Kármán. This organization called Research and Development, or RAND as it came to be known, emerged in the late 1940s as "an effective halfway-house between academia and officialdom."[12] Such a hybrid organization was bound to attract criticism about its purpose as it sought to serve two masters: the needs of problem-oriented officials, and the standards of scholarship that maintained policy neutrality. Doing so, Colin Gray asserted in 1971, meant that RAND products offered the worst of both worlds: "irrelevant policy advice and poor scholarship."[13]

RAND's founding members included General Arnold and, because of his longtime friendship with David Douglas of Douglas Aircraft Corporation, Franklin R. Collbohm from that company. Founders also included lawyer H. Rowan Gaither (who would serve as chairman of the Board of the Ford Foundation, that incidentally provided a $400,000 grant for RAND, and chairman of the Board for RAND)[14] and Edward Bowles, a consultant from Massachusetts Institute of Technology (MIT). Bowles and Collbohm had worked together previously (along with Robert McNamara) on the B-29 Special Bombardment Project in 1944. General Arnold pledged $10 million in seed money from unspent wartime research funds for the group, and RAND was launched in March 1946.[15] Nominally autonomous from Douglas Aircraft Company, it was initially part of that company and headquartered in Santa Monica, California. However, the direct link with Douglas did not last, in part because it was difficult to provide objective assessments of hardware while working

under the auspices of one competitive contractor. Indeed, other aircraft companies thought that Douglas' ties to RAND gave them an unfair competitive advantage.[16] In 1948, with the assistance of H. Rowan Gaither, RAND became on independent non-profit organization.

RAND's founding charter contained the following statement that explicitly established its links to the Air Force:

> Project RAND is a continuing program of scientific study and research on the broad subject of air warfare with the object of recommending to the Air Force preferred methods, techniques and instrumentalities for this purpose.[17]

The Air Force remained RAND's primary client throughout the 1950s, although RAND did some work for the Atomic Energy Commission. Given its origins and ties to the Air Force, the relatively small RAND staff—initially fewer than 100, although it would expand to several hundred by 1950[18]—was composed primarily of engineers from the aircraft industry and some mathematicians, and its work was bound to be of direct applicability to the Air Force mission.[19] RAND's very first report issued in May of 1946 was an engineering feasibility study for a proposed satellite. Then, during its first two years, RAND published nearly 100 reports whose content was heavily weighted toward mathematics and engineering. RAND covered such research topics as long-range bomber design and aerial refueling developments.[20] From the standpoint of the Air Force, the research from RAND that contributed to better armaments meant an Air Force that would be better at war and stronger as an institution—the better to recover from its subordinate status as adjunct to the Army.

These ties to the Air Force were reinforced by the fact that RAND research was initially supervised by General Curtis LeMay in his role as Chief of the Air Staff for Research and Development, a position first created in 1945. In 1949, when General LeMay was appointed the head of the Strategic Air Command (SAC), he retained a strong interest in the research from RAND that would support SAC priorities. LeMay was also important for removing institutional obstacles to RAND's research by overriding requirements that RAND focus only on very specific equipment development.[21] But despite the close ties to the Air Force, RAND research, at times, reached conclusions that were contrary to Air Force institutional interests. Thus, for example, RAND's 1950 study comparing airplanes suitable for strategic bombing missions suggested that old-fashion propeller planes staged out of Newfoundland would be adequate for the task.

Such a conclusion was not consistent with Air Force interest in convincing the Congress of the need to fund the latest jet aircraft development. More egregious from General LeMay's viewpoint was that RAND would champion the Navy's Polaris submarine program while calling for elimination of LeMay's pet projects, the B-58 and B-70.[22] The Navy Polaris program threatened a major challenge to the Air Force monopoly control over the nuclear arsenal.

The fact that RAND was willing to challenge Air Force interests was related to the fact that it began to broaden its research mandate beyond the narrow hardware/weapons questions that had direct utility to the Air Force. Original inspiration to broaden the RAND research agenda came from John Williams, an astronomer turned mathematician, who had spent the period of World War II in OSRD and joined RAND as director of its mathematics division. Williams took the concept "military worth" from a 1946 paper on air warfare written by part-time RAND consultant Warren Weaver. From this central concept came the ambition to create a comprehensive theory of war—along the lines of Einstein's unifying theory of physics—whereby all of its variables might be modeled by a series of mathematical functions.[23] From an organizational standpoint, Williams' project suggested that mathematical modeling would provide the means for manipulating the inputs that would be provided by other academic disciplines. Certainly, the conditions of warfare ushered in by the nuclear age meant that any questions of choice or application of technology could not be separated from questions of broader strategy or politics. In fact, the RAND mantra in its early years was that technology had erased the boundaries between civilians and the military and between peace and war.[24] With this recognition, the door was opened for recruiting to RAND a greater variety of disciplines to include the social sciences. This RAND quest for greater interdisciplinary work was yet another part of the general trend in social science we noted in Chap. 1, that was derived from the World War II experience of the OSS and was fostered by, among others, the Social Science Research Council (SSRC).

It goes without saying, that one obstacle to recruiting social scientists for RAND was its direct ties to the Air Force that, some thought, jeopardized scholarly objectivity. Thus, for example, Kenneth Boulding remarked that because of its ties to the Air Force, RAND studies "must be accepted with the same kind of reserve that, shall we say, we might greet a study of the Reformation by Jesuits based on unpublished and secret documents of the Vatican."[25] Even those scholars like Margret Mead, who worked with the military during the war, were skeptical of the RAND project.[26] RAND

addressed the problem of recruiting other disciplines by convening a conference of social scientists in New York in 1947 and was able to attract a variety of leading scholars to attend. Sociologist William Ogburn, political scientist Harold Lasswell, economist Jacob Viner, anthropologist Ruth Benedict and historian Bernard Brodie were all in attendance. In addition, Donald Young, the executive director of the SSRC, addressed the gathering to allay ethical concerns about working for the government by emphasizing the fact that there was nothing "nefarious" about working for the military. Overall, conference attendees tended to represent a view that was sympathetic to the application of scientific methodology to the social sciences, and many of the participants had attempted such an application in their own disciplines.[27] As a result of the conference, RAND recruited economist Charles Hitch to lead its economics division and sociologist Hans Speier to lead the social sciences division. RAND's success in recruiting social scientists earned it a reputation of being one of the leading centers for integrating social science into military policy. RAND came to symbolize a new elite social community likened to "a kind of secular monastery."[28]

Although William's original plan was to integrate the "softer" social sciences with the more quantitative disciplines of the hard sciences, many social scientist were reluctant to relocate from the East Coast to Santa Monica. Consequently, RAND established another office in Washington, D.C. to house them. This geographical separation from Santa Monica—they did not move west until the mid-1950s—made interdisciplinary integration more difficult, so that, in the end, the potential of the social sciences to add a humanistic perspective did not offset the predominance of the quantitative approach.[29] Beyond the physical separation, as already noted in Chap. 1, there lay a certain antipathy between the hard science divisions at Santa Monica and the social science division, with the latter accorded less prestige.[30] In particular, the physics division at RAND questioned the value of the work done by the social science division. In fact, once when Herman Kahn—then a member of the physics division—was asked about his evaluation of a book by his social science colleague, Nathan Leites, he said: "I read the *New York Times*, what the hell should I read Nathan Leites for?"[31] Similarly, mathematician Albert Wohlstetter denigrated work done in the social science division as being in the essay tradition.[32] Such slights from the hard scientists provided another incentive for social scientists to emphasize their "scientific" qualities. Albeit some social scientists in RAND's social science division tended to reject the behavioral science approach for a more traditional historical approach.[33]

Besides broadening the research focus of the organization, one other development loosened RAND's ties with the Air Force while creating another source of tension among the RAND staff, and that was the transition to a new administration with the election of John F. Kennedy in 1960. Even though President Eisenhower had organized his national security strategy around nuclear weapons and hence enhanced the role of the Air Force—this did not necessarily enhance RAND's fortunes. President Eisenhower saw the nuclear option as enabling budget cuts to the Defense Department and these cuts were applied to RAND contracts. To be sure, these cuts were reversed when the Soviets successfully launched their Sputnik satellite that demonstrated to even a budget-conscious president like Eisenhower that technical innovation required greater financial support. Therefore, after Sputnik, $2 million that had been subtracted from defense contracts with RAND were restored, and another $4 million added for fiscal years 1959 through 1961.[34] In addition, the Kennedy administration, through the office of the Secretary of Defense Robert McNamara gave RAND a larger base of support in the Defense Department by granting RAND lucrative contracts with his office while allowing it greater access to the Pentagon. Moreover, important RAND analysts moved to the Defense Department during the Kennedy years. For example, Alain Enthoven, who worked at RAND from 1956, joined the Defense Department in 1960. In addition, Charles Hitch, who headed RAND's economics division, served as assistant secretary of Defense from 1961 to 1965. Albert Wohlstetter served as a consultant to the department, and Robert McNamara was sufficiently impressed with his work that he awarded Wohlstetter a medal for distinguished public service in 1965.[35] Their presence ensured that RAND had entry into the highest levels of national security policy-making so that their intellectual dominance was "nearly absolute."[36]

RAND's contribution to national security began with the development of research methodologies intended to explain strategic choices and trace strategic interaction: systems analysis and game theory, respectively. Any pretense of authentic scientific objectivity necessitates a consensus over methodology. One prerequisite for RAND to develop the two research methodologies was to expand its expertise beyond an emphasis on technical aspects of military hardware into strategy, where it became necessary to "try to impose the order of a rational life on the most unimaginable vast hideous maelstrom of nuclear war."[37] To the extent that both approaches appeared to be expressions of science, they offered

a promise of that rational approach for the nuclear age. While this rational approach to military strategy neglected cultural and historical forces shaping choice, the very nature of nuclear weapons technology appeared to make this oversight reasonable.[38] At the same time, the scientific basis of the methodology offered some comfort to policy-makers navigating a sea of uncertainty generated by rapid technological change. Moreover, this search for scientific certainty had its parallels with the movement in the social sciences to distinguish the new "behavioral sciences" with its emphasis on quantitative methods from the earlier legal and historical approaches. As such, both RAND methodologies contributed to what some observers characterized as the "mechanistic view of international action" that became RAND's legacy.[39] In the end, both methodologies, because they elaborated upon what were, in fact, very simple and commonplace notions, perhaps proved to be less than meets the eye.

Whatever the scientific merits of systems analysis, it remains widely recognized as RAND's "signature product" and one of its most notable contributions to policy-making.[40] For RAND analysts, the approach provided an escape from relying on intuitive judgments and the possibility for objectivity that distinguished the scientific from the political. As Herman Kahn, one of its proponents, emphasized, objectivity required replacing descriptive terms like "intolerable" and "catastrophic" with quantitative measures.[41] Therefore, systems analysis symbolized for RAND's practitioners, a certain faith that scientific method would make important contributions for organizing modern warfare.[42] The term systems analysis originated in 1947 with a RAND engineer, Ed Paxon, who had been a scientific advisor to the US Army Air Corps and a consultant to the US Strategic Bombing Survey, when John Williams assigned him to the evaluation of military worth.[43] The term then became the successor to Williams' notion of military worth. As conceived, systems analysis differed from the operational analysis conducted during World War II. Charles Hitch made a clear distinction between operations research and systems analysis. The former dealt with ways of using equipment—the realm of physical scientists. The latter dealt with broader issues concerning what kind of military forces to have and which weapon systems to develop. Because the latter issues are tied closely to objectives, they lay more within the realm of social science.[44]

Beyond the aforementioned generalizations, RAND analysts described systems analysis in various ways, and one RAND document admitted that there was no precise, commonly accepted definition of the term.[45] For example, Herman Kahn, in a lecture and workshop given by RAND,

described classical systems analysis as an attempt to determine what level of performance might be achieved with a given amount of spending within the context of certain assumptions.[46] Defined in this way, systems analysis was about applying economic reasoning to engineering choices.[47] Bernard Brodie saw the value in systems analysis to lie with the fact that it brought what was "modern" into strategic analysis. His interpretation of systems analysis ran as follows and captures some sense of the confusion embodied in the use of the term:

> The central idea is that no weapon can be considered independently of the other weapons and commodities that are used with it, that all endure through some period of time and require men to service them and to be trained in their use, that all these items involve costs, and therefore relative costs of different systems, as considered against some common standard of function, are basic to the problem of choice between systems.[48]

A more simple definition of systems analysis found in one RAND document states it means "a systematic examination of a problem of choice in which each step of the analysis is made explicit wherever possible."[49] The underlying simplicity behind the term can be illustrated further by Alain Enthoven who described systems analysis as nothing more than taking "a complex problem and sort[ing] out the tangle of significant factors so that each can be studied by the method most appropriate to it."[50] Given the varied definitions, it is no wonder then that the term generated sufficient confusion that one outside observer saw it as vaguely referring to "something to do with looking at a problem as a whole, looking at it over time, being quantitative where possible, being realistic about potential conflict, and drawing on a wide range of technical expertise."[51] What is more, while the approach might be appropriate for some narrow studies like comparing weapon system performance, it was less appropriate for broader questions of strategy. To be sure, systems analysis was put to good use and manifested in a concrete way in the Planning Programming Budgeting System still used by the Defense Department, which was instituted by Charles Hitch head of RAND's economics division when he became comptroller in the Defense Department under Robert McNamara. But given the lack of consensus (if not confusion) concerning its definition, it is difficult to claim that it offered a methodological consensus.

Like systems analysis, game theory, the second methodology developed by RAND, aimed at building a basis for a scientific strategy. The foundation for game theory can be found in a book by John Von Neumann and Oskar

Morgenstern, *Theory of Games and Economic Behavior*. John Williams' interest in game theory led him to hire John Von Neumann, who then assisted Williams' mathematics division in applying game theory to military problems. The close association of game theory to economics has led to some criticism that it too readily substituted "economic man" for the equally fictitious "rational strategic man."[52] Of course, the one simplifying assumption in game theory, perhaps more germane in economic interactions than in military ones, is that each side in a contest is a rational, unitary, purposive player. For game theorists, rationality includes the requirement that both opponents calculate their values and expected payoffs of choices and each is able to guess the payoff function of the other.[53] Nevertheless, RAND's 1950 annual report noted that much of the research in its mathematics division found its "guiding philosophy" in the Neumann-Morgenstern theory of games.[54]

Von Neumann's inspiration for game theory came from a poker game where he observed that any player's win or loss depended on what other players did.[55] Oscar Morgenstern took the poker analogy further and applied it to the Cold War interaction between the United States and the USSR. In a 1961 article in *The New York Times Magazine*, Morgenstern noted that a key lesson from the poker analogy was that even though one might not win a particular contest, the best strategy in foreign policy was to use threats and bluffs in a manner that would minimize the worst that your opponent could do. He then made the claim that the Soviet threat to use missiles during the Suez Crisis in 1956 illustrated the most successful use of bluffing up to that time. Morgenstern concluded his description of the application of the poker analogy by noting that the most fundamental question of the day was how to make a threat effective and how to distinguish a genuine threat from a bluff by your opponent.[56] As we shall see subsequently, making a threat credible so that it is not perceived as a bluff became a major concern of deterrence theorists.[57]

Prisoner's Dilemma, which is perhaps the most famous illustration of the dynamics in the game, provided a model for tracing the implications of the interdependence in decision-making. First outlined by RAND analysts Merrill Flood (who was a student of John Von Neumann) and Melvin Dresher in 1951, the game drew on the following scenario from which it derives its name. The police arrest two people for armed robbery who they know committed the crime but lack the evidence to convict them. The police do have sufficient evidence to convict them for stealing the getaway car and that conviction carries a penalty of two years. The police then separate the

offenders so they cannot communicate and make them each a deal. Implicate your partner and you go free while the partner will receive a ten-year prison sentence. If you both stay silent, you each get two years for the stolen car. However, if you both confess to implicate the other you each will receive a five-year sentence. Within the scenario, the best outcome for the robbers is to cooperate and remain silent and serve the least amount of time, but because each must fear the consequences of remaining silent while his partner confesses, both robbers will each opt to hedge his bet by confessing so they both end up with the five-year sentence. Prisoner's Dilemma shows that although cooperation would really lead to the best outcome for two parties, the payoff structure is such that the temptation to defect is too strong. Prisoner's Dilemma then was thought to be especially useful for modeling problems of nuclear strategy, and it suggested that in the event of a crisis between the United States and USSR, the temptation to defect could easily lead to a preemptive strike, reconfirming a fear of surprise attack.

Although game theory was useful for reminding policy-makers that outcomes were dependent on choices made by others, in this sense it merely reiterated a lesson as old as Clausewitz who emphasized that because war always involved interaction, in formulating a strategy, one had to take into account the opponent's strategy. In fact, when President Eisenhower met John Von Neumann, he remarked that game theory was similar to commander's guidelines that he had been taught 30 years earlier by General Fox Conner who stressed the need to assess a course of action in terms of what the enemy would do.[58]

Besides elaborating on old truths, game theory provides a very static representation of decision-making—a point that Von Neumann and Morgenstern readily admitted. The real world of national security strategy is not characterized by unrelated single-shot games, but rather by a sequence of events. Indeed, George Kennan was critical of the tendency of military planners "to view Soviet intentions as something existing quite independently of our own behavior. It was difficult to persuade these men that what people in Moscow decided to do might be a reaction to things we had done."[59] Therefore, game theory as a description of a purely instrumental choice, unhindered by ignorance, or emotional or ideological factors, illustrates a dynamic that only exists in the abstract.[60]

What then were some of the core ideas to grow from the aforementioned scientific methodologies? To best illustrate the core ideas that came from the methodologies, we turn to a discussion of the contributions of key RAND personnel that shaped the intellectual milieu at RAND and spread into policy circles. The discussion will culminate with

a comprehensive look at the work of Thomas Schelling whose ideas loom large because he made a systematic and comprehensive attempt to create a theory of deterrence and coercion. As we shall see, while the definition of deterrence is straightforward—that it is the ability to dissuade an opponent from an action by fear—operationalizing deterrence in practice proved to be less so.[61]

The starting point for RAND analysts was the need to overcome the view that the atom bomb was just another weapon like any other. While from today's perspective, the notion that the atom bomb represented an important watershed for warfare is self-evident, it was less so in 1945, particularly in military circles. Indeed, when General Arnold requested three generals study the impact that the atom bomb had on strategy and organization, their October 1945 report concluded that the atom bomb did not require change in either organizing ideas or operations.[62] To his credit, President Truman did not share the views of the military. Thus, he saw the bomb as a weapon of terror that should only be used as a last resort. To ensure this was the case, he established the arrangement that made atomic weapons a separate part of the US arsenal, with the president as the sole authority for their use codified in the Atomic Energy Act of 1946.[63]

Although not a comprehensive list of all the individuals at RAND during this time, the following discussion highlights those individuals who were most important to the theory's development. What is more, these individuals most clearly sought to have an impact on policy, albeit their efforts were not always successful. Finally, these individuals all shared a common perspective on the nature of the Soviet challenge which, in turn, pervaded the US policy during the early Cold War. However, among RAND analysts, there was a difference of opinion concerning the likelihood of nuclear war, and the best way to use this weapon and this difference is reflected in their approaches to deterrence. To some extent, many of these thinkers can aptly be described in the words that John Kennedy used at his inaugural address to denote the watershed signified by his election, as the new generation "born in this century, tempered by war and disciplined by a hard and bitter peace."

Perhaps the best place to begin our discussion of this group of elites is with the head of RAND's social science division from 1948 to 1960, sociologist Hans Speier. Speier who immigrated to the United States from Germany in 1933 worked during World War II for the Foreign Broadcast Information Service, analyzing German propaganda and evaluating German morale. Eventually, Speier became the propaganda policy advisor to the chief of the overseas branch of the Office of War Information.[64] Speier's

work at RAND included a study of German rearmament and, as we saw in Chap. 1, was critical of the status given to his division at RAND. His work on nuclear weapons reflected his concern about their impact on the Atlantic Alliance, and he was especially concerned that the Soviets would be able to leverage nuclear weapons in a way that would divide the alliance. He explained this concern in an article in *World Politics* in 1957, where he noted the different views of nuclear weapons among members, of The North Atlantic Treaty Organization (NATO) with Europeans less concerned about responsibility for aggression and more concerned with what they saw as an American determination to make any European war an atomic one. He characterized Soviet nuclear threats during the Suez Crisis as a major diplomatic defeat of the West,[65] which, he believed, exemplified a model that the Soviets might use to divide the Western alliance. Speier believed that the Soviet ability to exert such leverage with their nuclear weapons would be less likely if other NATO members acquired their own nuclear capability. Speier was on outlier on the issue of using quantitative methods in the social sciences.[66]

Bernard Brodie who worked at RAND from 1951 to 1966 is the second important figure in the evolution of nuclear strategy. During World War II, he served in the Office of Naval Intelligence writing propaganda whose purpose was to convince German U-boat crews to surrender. After the war, he spent a brief period at Yale's Institute of International Studies and as a consultant for SAC before joining RAND. Brodie provided the first effort at conceptualizing the role for atomic weapons in his essays contained in his edited book, *The Absolute Weapon: Atomic Power and World Order*, published in 1946. There he made the famous assertion concerning the atomic age that encapsulated the modern notion of deterrence: "Thus far the chief purpose of our military establishment has been to win wars. From now on its chief purpose must be to avert them."[67] Consequently, Brodie became a consultant to the State Department on methods for controlling atomic weapons and chaired the Atomic Energy Committee of the SSRC. Although Brodie was horrified by the implications of the hydrogen bomb (a RAND team had calculated that the bomb was so powerful it could miss a target by two or more miles and still destroy the target), his study and briefings solidified support for it. Indeed, RAND briefings to President Truman and Dean Acheson helped to overcome the opposition to the H-bomb by the scientists at Los Alamos, led by Robert Oppenheimer.[68] Given Brodie's early contribution at the opening of the atomic age, it is not surprising he is attributed as a central figure in RAND's "oral tradition" and that "He more than anyone else, helped us to learn to think about how to survive in a world with nuclear weapons."[69]

Bernard Brodie was the first theorist to challenge the military thinking reflected in a 1945 report, and he called on the military "to bestir themselves to a wholly unprecedented degree in revising military concepts inherited from the past."[70] Subsequently, in *Strategy and the Missile Age*, Brodie rejected the analogy comparing the advent of atomic weapons with the invention of gunpowder, because the latter required centuries to accomplish and its gradual introduction permitted time for doctrinal adjustment in military thinking. Further, the gun always remained a tactical weapon that did not require a major recalculation concerning the use of force. Brodie noted that the atom bomb had several implications for the very conduct of operations that ran counter to the experience acquired during World War II. Because the weapons essentially shrank time and distance, the geographic separation of the United States and the USSR offered them no immunity from damage, thereby bringing an end to the era of "mutual limited liability."[71] Related to this observation was the fact that the United States would not have the time to mobilize resources so that any new war would have to rely on arsenals already in place. Further, campaigns to seize advanced bases would no longer be necessary, and air superiority offered no protection against the kind of damage done to Germany in World War II due to their lack of air superiority. In short, the possession of superiority in the number of atom bombs would not endow their owner with the kind of military security that superior arms had done in the past.[72]

Brodie then must be credited with enunciating what became the centerpiece of nuclear deterrence theory when he recognized the dual impact of atom bombs: that if they could be used *without* fear of retaliation, they encouraged aggression, but if a potential aggressor knew the weapon would be used against him in return, then the weapon provided an inhibition against aggression. A further observation followed which was that the more horrendous the damage from an atomic attack, the more an aggressor would be deterred by "even a marginal chance of retaliation." As a corollary to this observation, the destruction now, possible by even an inferior retaliatory force, meant that deterrence did not depend on superior numbers. Therefore, Brodie asserted the vital first step for the US security was to guarantee the ability to retaliate, and doing so required a dispersal of the retaliatory force in violation of the well-established military principle of the concentration of force.[73] What Brodie termed the "reserve force" came to be known as second-strike capability and provided the cornerstone of the US nuclear deterrence doctrine. But maintaining a secure retaliatory force was not just essential for the United States, the

Soviets too had to believe that their retaliatory capability was secure. For Brodie and for other nuclear strategists, stability in the nuclear era required that each side retain confidence in their ability to retaliate, thus eliminating any advantage of striking first in a surprise attack.[74]

While Brodie was convinced that given the weapons, deterrence must not fail, he was not confident of finding concrete ways for policy-makers to ensure that deterrence did not fail. He observed, "Yet there is little in the experience of our own or any other nation to tell us what kind of behavior, military as well as diplomatic is truly consistent with a purposeful strategy of deterrence."[75] Moreover, he worried that World War I came close to illustrating the real danger should deterrence fail and provided an example of future war. According to Brodie, unlike World War II, the first war illustrated the suppression of all rational concerns with the political aims of the war. Rather, the war's protagonists pursued victory so blindly and "therefore at wholly incommensurate costs which destroy its meaning."[76] Such a dynamic, Brodie thought, might well engulf the participants in an atomic war.

Brodie retained a certain ambivalence concerning RAND's pursuit of a science of strategy. Thus, while Brodie proclaimed the need to recognize strategy as a science and saw systems analysis as a way to achieve it, he did so in an effort to wean the military from its reliance on traditional "principles of war." Drawing on the most recent experience in World War II was erroneous because Brodie believed it offered few lessons for the nuclear age.[77] Furthermore, despite Brodie's aforementioned characterization of systems analysis, as adding a modern approach to strategy, he recognized the limits to the method and observed that its results would be no better than the planning factors or assumptions which are, after all, "estimates untested in war." Brodie went on to say: "The truth, unfortunately, is that the profound issues in strategy, those likely to affect most deeply the fates of nations and even mankind are precisely those which do not lend themselves to scientific analysis, usually because they are so laden with value judgments." Brodie noted one other limit to scientific analysis, which was that even if science could determine the optimal weapon system, if that system required increased spending, it would encounter political obstacles to being accepted.[78]

While cautious in his celebration of the "science of strategy," Brodie the historian was also wary of the use of history as a guide to strategy because the "lessons" of history can so easily be misused. Thus, he noted in 1946 that, at best, history is an imperfect guide, "but when imperfectly

understood and interpreted it is a menace to sound judgment."[79] Much of Brodie's own historical analysis, then, revolved around showing that the past provides few reliable guideposts for navigating in the nuclear age. Brodie's analysis, showing as it did his ambivalence toward both science and history for understanding atomic age strategic issues, left policymakers without a reliable source of ideas for bolstering their policy choices. Within this context, it is not surprising that Brodie would become marginalized at RAND and that those who were wholly committed to the more "scientific" approach would gain preeminence and move into the policy world of the Kennedy administration.[80]

It was Brodie's protégé, William Kaufmann, who specified the three requirements for deterrence. Kaufmann believed that for deterrence to be effective, you first needed the capacity to retaliate and with that retaliation convince your opponent that the costs of his action were far greater than any advantage he might gain. Finally, deterrence required convincing your opponent of your intentions.[81] All three interlocking elements constitute the issue of credibility which various RAND analysts sought to address in different ways. The problem of credibility dominated attempts to formulate nuclear strategy taking the subject into the realm of psychology. For as Oskar Morgenstern noted, convincing your opponent of your intentions in a nuclear standoff was akin to convincing another poker player that you were not in fact bluffing.

In the context of American defense policies in the 1950s, this last aspect of credibility found its most serious challenge in the doctrine of "massive retaliation." The massive retaliation doctrine was enunciated in a speech by the Secretary of State John Foster Dulles to the Council on Foreign Relations in 1954. To some extent, the doctrine was a reflection of the frustration of the limited war experience in Korea and as such suggested that the United States needed to contemplate using atomic weapons in any and all conflicts. Thus, massive retaliation has been characterized as the first systematic theory of deterrence in the Cold War.[82] Yet, Bernard Brodie and William Kaufmann both saw massive retaliation as a flawed doctrine. For Brodie, while massive retaliation provided a credible threat for what he termed as "basic deterrence" that is for a direct attack on the United States, the threat would never be believed by an opponent for lesser aggression. Furthermore, Brodie saw massive retaliation as an attempt to apply traditional principles of war under circumstances where they were no longer appropriate. For example, he saw massive retaliation as appealing to the principle of concentration by virtue of the fact

that it rejected dispersion of the American forces around the globe.[83] For Kaufmann, massive retaliation served to erode the US credibility because the Soviets would never believe it to be an accurate representation of the US intentions. After all, as Kaufmann noted, intentions are measured by, among other things, past performance, public opinion and allied support.[84] Once a bluff had been called, as perhaps it already had been in Korea when the United States did not use atomic weapons, then the threat would not be believed. In addition, various peace movements and efforts of citizen groups to "ban the bomb" indicated a public opinion that, at a minimum, was divided over the issue of using atomic weapons. If that was not enough to underscore American reluctance to use the weapon, the fact that the doctrine weakened the Atlantic Alliance offered yet another measure that the threat to use atomic weapons was merely a bluff.

Despite the weaknesses and criticisms of massive retaliation and its detrimental effect on the credibility of the US threat, military planners were devising a way to manifest the doctrine of massive retaliation. During the last year of the Eisenhower Administration, Secretary of Defense Thomas S. Gates, Jr., saw the need to create an integrated plan for the use of nuclear weapons. The result was the Single Integrated Operational Plan (SIOP), best described as "a highly inflexible plan for massive retaliation or massive pre-emption against all categories of targets within the Sino-Soviet bloc."[85] A total of some 2164 megatons was to be launched to hit targets in the USSR, Eastern Europe and the People's Republic of China (PRC) in the event of an actual or impending Soviet invasion of Western Europe. As such, SIOP-62 has been criticized for its failure to provide analysis of which targets might achieve American objectives because it remained a capabilities plan aimed at using all available means to achieve maximum destruction.[86] The plan was briefed and endorsed by the Joint Chiefs of Staff and the Secretary of Defense. At the conclusion of the briefing, the then Commandant of the Marine Corps, General David Shoup, offered an objection. He said, "Any plan that kills millions of Chinese when it isn't even their war is not a good plan. This is not the American way." Despite General Shoup's objection, SIOP highlighted the enormous gap in the thinking of RAND's civilian strategists and the military. Daniel Ellsberg, for one, was appalled at the prospect of a plan that would automatically escalate a conflict to an all-out nuclear war.[87]

While Brodie may have been appalled at the destructiveness of the H-bomb, another RAND luminary Herman Kahn generated controversy with his effort to outline a strategy for surviving a nuclear war should

deterrence fail. Kahn had served with the Army in Burma during World War II and joined RAND's physics department in 1947 where he remained until 1961. However, his security clearance was revoked in 1953 because of allegations that his wife's sister and her husband were members of the communist party. Because of this, Kahn moved from weapons science in physics to becoming a nuclear strategist.[88] His book, *On Thermonuclear War*, was a treatise aimed to refute the view that deterrence grew automatically from the nature of the weapons. His book also provided a warning that the belief that nuclear war meant automatic annihilation was dangerous because it provided an open invitation for the Soviets to engage in Munich-type blackmail.[89]

Much of Herman Kahn's analysis in *On Thermonuclear War* revolved around his estimates concerning the postwar environment and making the preparations that might mitigate the damage from a nuclear war. Kahn outlined the findings from his RAND study that asserted that a nuclear war was not likely to lead to world annihilation. RAND estimates rather optimistically asserted that the time needed for recovery from such a war was likely to be between one to ten years for a well-prepared attacker and "somewhat longer for the defender."[90] Because Kahn believed that the USSR would likely be the aggressor, it was imperative for the United States to prepare civil defense. Such preparation, Kahn thought, could make the difference between casualties in the 2–20 million range rather than the 50–100 million range. He also believed that the United States could well afford to allocate much more of its Gross National Product (GNP) for military purposes. He estimated the United States could increase spending by 10 or 20 percent because it was already allocating "resources lavishly, even frivolously, on unprecedentedly high living standards."[91] For Kahn, the preparations for enhancing society's recuperative powers were linked directly to a more general deterrence of Soviet actions, and he says: "there is an enormous difference in the ability of a nation to conduct international negotiations and stand up to threats if it can put its people in a place of safety in 24 or 48 hours than if it cannot do this." Kahn differed from others at RAND in that he believed the biggest problem facing the West was sheer survival and not that the West might be "nibbled to death" with conflicts in the periphery.[92]

In addition, Kahn explicitly sought to link his understanding of nuclear issues with scientific analysis and proclaimed his work on nuclear issues was distinct from others in the field because of its use of systems analysis.[93] Kahn's complete faith in the scientific basis for analyzing war carried over to his vision concerning the nature and conduct of a future nuclear war.

Kahn argued that while both world wars were characterized by the "democratization" of participation so that war aims had to be formulated in a way that would enlist the enthusiasm of the common man, the next world war would be quite different. His view of the brave new world of scientific rationality at work in the conduct of war—a celebration of science itself—is captured by his statement:

> There would probably not be any drafting, training, war mobilization, bond drives, or voting between the first and last shots. Such a war most likely would be relatively technical, run by government authorities and technicians, with little or no attention paid to immediate problems of support from, or the morale of, the civilian population. It would probably be fought relatively coolly, and be guided by considerations of national interest little affected by propaganda or popular emotion.[94]

Although Kahn emphasized the importance of civil defense and protection of the population in *On Thermonuclear War*, because of the casual way he dealt with the aftermath of a nuclear war, the book was not well received in some quarters. In a typical passage on the aftermath of a nuclear war, Kahn says:

> *Despite a widespread belief to the contrary, objective studies indicate that even though the amount of human tragedy would be greatly increased in the postwar world, the increase would not preclude normal and happy lives for the majority of the survivors.* (Emphasis in the original)[95]

It is not surprising then, that one reviewer for the *Scientific American*, James Newman, described the book as "a moral tract on mass murder: how to plan it, how to get away with it, how to justify it."[96] Despite the book's reception, it was sufficiently successful that Kahn, with a grant of $1 million from the Rockefeller Foundation, left RAND and moved to New York to establish his own think tank, the Hudson Institute.

Albert Wohlstetter, like his colleague Herman Kahn, came to strategic analysis from a more "hard science" perspective—in this case mathematics. He had spent World War II on the War Production Board of Atlas Aircraft Products. He joined the mathematics division at RAND in 1951 where, by 1963, he had become a senior policy analyst. In addition, beginning in 1961 and continuing through 1967, Wohlstetter served as a consultant to the secretary of Defense. Wohlstetter established his reputation by his study of the basing practices of the Strategic Air Command where he

noted that SAC plans were derived from World War II when strategic bombing was only one piece of a larger campaign. Within such a context, there was little danger of destruction of the aircraft and hence little need for dispersing their bases. In the nuclear age, however, he argued that strategic bombardment might be the primary and perhaps only force used so that concentrated basing that offered a tempting target for a surprise preemptive attack was no longer sound. The vulnerability of the US nuclear forces to a surprise attack became a major theme in Wohlstetter's analysis and contributed to the most widely held view at RAND of the prospect of a Soviet surprise attack.[97] Wohlstetter even viewed the Soviet missiles in Cuba through the lens of vulnerability and saw their danger as their ability to attack without warning because they could outflank the early warning system.[98] In fact, Fred Kaplan suggests that Wohlstetter imposed vulnerability on everything he analyzed so that, from RAND, the concern with vulnerability seeped into the policy community where it "grew from infatuation, to obsession and finally a fetish of sorts."[99]

In the conclusion of his basing study, Wohlstetter asserted that a preemptive strike by the Soviets would be attractive to them because, according to his estimates, with only 120 tactical nuclear weapons, the Soviets could destroy 85 percent of SAC's European-based bomber fleet. Although the Air Force admitted that its basing practice always assumed sufficient warning time for planes to get airborne and not be vulnerable to destruction, SAC remained opposed to Wohlstetter's findings for two reasons. First, Curtis LeMay opposed the idea of spending money to protect bombers and preferred to purchase more planes to assure a higher survival rate. Second, bureaucratic considerations came into play because although SAC was an Air Force command, it received its orders from the Joint Chiefs of Staff. To accept a RAND proposal meant accepting direction from the Air Staff, which might portend greater Air Staff incursions into SAC prerogatives.[100] Nevertheless, the Wohlstetter study offered yet another indication, already noted earlier, that RAND analysis did not always cater to Air Force preferences. Given Albert Wohlstetter's focus on SAC vulnerability in his basing study described earlier, it is not surprising that for him the problem of credibility revolved around the capacity to retaliate after an attack. SAC vulnerability, he reasoned, left the United States with limited capacity to retaliate, which he believed provided the Soviets with an incentive for a first strike. The successful Soviet launch of the Sputnik satellite in October 1957 renewed concern—never really dissipated from the Pearl Harbor experience—of a surprise Soviet attack. To be sure, by the 1960s, with the development of a more

secure second-strike strategic force, the fear of the next world war starting with a surprise attack receded and was gradually replaced with a concern that the next world war might start with the escalation of a lesser conflict.[101]

Wohlstetter, perhaps inadvertently, became a policy entrepreneur during the Eisenhower Administration. Prior to Sputnik, Eisenhower had created the Security Resources Panel headed by H. Rowen Gaither to study a proposal by the Federal Defense Administration for a $40 billion civil defense program, something that would have pleased Herman Kahn. In the wake of the Soviet satellite launch in 1957, the committee broadened its study to a more open-ended assessment of American military policy. The panel's final report drew heavily on Albert Wohlstetter's RAND work and was written by Paul Nitze (Gaither had become too ill to complete the report), and true to the hawkish proclivities of the author of NSC-68, Nitze concluded that SAC was indeed vulnerable to a surprise attack. Thus, the Gaither Report, as it came to be known, included many of the recommendations contained in Wohlstetter's RAND study. These included recommendations to improve the radar system, construct widely dispersed shelters for SAC, expand aerial reconnaissance, massively increase the military budget, accelerate the construction of missiles and undertake a series of civil defense measures. The total cost for the recommendations was expected to run to $44 billion.[102]

The cost-conscious Eisenhower had no intention of implementing these expensive recommendations and ignored the report's findings. He had, after all, reduced President Truman's defense budget from $41.3 billion to $36 billion in fiscal year 1954.[103] Eisenhower is reported as saying in response to the Gaither Report, "You can't have this kind of war. There aren't enough bulldozers to scrape the bodies off the street."[104] However, the report was leaked to the press, which generated the famous "missile gap" controversy that the Democrats used very effectively to castigate the Republican administration for its weak defense posture during the 1960 election.[105] Albert Wohlstetter, because he saw Eisenhower as unresponsive to the suggestions raised in the Gaither Report, went public with his ideas in *Foreign Affairs* in 1959. Reiterating points he outlined in a report for RAND the previous year, he noted the well-established point that retaliation was the cornerstone for deterrence, but he contended that the public had *overestimated* the difficulties of the Soviets launching a surprise attack, while *underestimating* the difficulty of Western retaliation.

Wohlstetter presented a more sophisticated conceptualization of the missile gap (RAND analysts preferred to call it a deterrence gap) which saw the issue not in terms of greater Russian numbers, but in the fact that

SAC was so vulnerable that with the missiles the Soviets already possessed they could eliminate American power to strike back, thereby shattering America's ability to deter Soviet aggression. Nevertheless, the long-term impact of Wohlstetter's analysis was that it acted as "a powerful engine driving at least the American side of a nuclear arms race for over the next quarter of a century."[106]

In the same article, Wohlstetter also threw into doubt another of Kaufmann's elements for enhancing the credibility of deterrence, namely, making the costs for aggression outweigh any gains. Wohlstetter reminded his readers that Russia had recovered extremely well from the loss of 20 million people in World War II, and that given the US vulnerabilities, the Soviets might well be confident that they could limit their damage to less than they experienced in that war.[107] Wohlstetter's assertion was challenged on a number of fronts. Bernard Brodie, for one, took exception to Wohlstetter's views concerning the fact that the cost to the Soviets would be such as to weaken the credibility of the American nuclear deterrent. He said in what can be read as a rejoinder to Wohlstetter: "the fact that a nation has in the past undergone and successfully recovered from great injury does not mean that it will be blasé about a possible repetition of such a catastrophe. The Soviet leaders are not eager to see 1941–1942 repeated, let alone run the risk of having the damage and casualties of those years greatly exceeded."[108]

A more comprehensive challenge to Wohlstetter's point was provided by Thomas Milburn, the director of Project Michelson. Drawing on insights from psychology, he was skeptical that a deterrent policy that focused almost exclusively on punishment would necessarily shape behavior in the desired direction. While threats of punishment might suppress behavior, they would do little to change the underlying motives, which were, after all, the real objective of policy. Further, Milburn noted that outside threats had one other consequence, namely, the tendency to increase group cohesion. Finally, Milburn pointed out that at an emotional level, people who have experienced a particular disaster are more likely to try to avert a repetition of it than those who have no such experience. Thus, the Soviets could be expected to be more likely to try to avoid a replay of the damage they suffered in World War II than the United States who had no experience of a similar level of destruction. Milburn concluded that "Because this fact is not generally realized, the United States could very easily underestimate the influence of the *possibility* that its weapons might be used" (emphasis in the original).[109]

We must note the role of two more RAND alumni because of their importance as policy entrepreneurs during the Kennedy Administration: Alain Enthoven and Daniel Ellsberg, both economists by training and both too young to have served in World War II. Alain Enthoven had a short stint at RAND, joining in 1956 and departing for the Defense Department in 1960. In 1965, President Johnson appointed him assistant secretary of Defense for Systems Analysis where he was responsible for implementing cost-effectiveness methods. While at RAND, Enthoven worked with Albert Wohlstetter on the SAC vulnerability study. On leaving RAND, he confided his frustrations with the organization in a letter to Bernard Brodie. In it, he observed: "I have lost patience with the whole climate that fosters the treatment of subjects of the utmost gravity and complexity in a slick 45 minute briefing."[110]

Daniel Ellsberg joined RAND in 1959 and departed for the Defense Department in 1964 as special assistant to Assistant Secretary of Defense John McNaughton. In the interim, he had worked as a consultant to Department of Defense (DOD) and served as a member in two of the three working groups that reported to the decision-making body Excom during the Cuban Missile Crisis. In a 1959 lecture delivered under the auspices of the Lowell Institute in Boston, he admitted his analysis of nuclear strategy was derived from his interest in economic bargaining. Moreover, the logic of his analysis presented in that lecture foreshadowed subsequent work of Thomas Schelling.[111] Ellsberg was important as the connection that facilitated RAND analysts moving to work in the Kennedy campaign in 1960 and eventually into policy positions. While Ellsberg was on leave to finish his PhD, he met Deirdre Henderson who was the coordinator of an academic advisory group for John Kennedy's presidential campaign.[112]

Given how appalled Ellsberg was with SIOP-62, it is not surprising that one of RAND's first concrete influences on the Kennedy administration was to convince them to drop the plan. The lingering bitterness left in the minds of some military men can be detected in General White's denunciation of the civilian defense intellectuals in 1963 that was quoted earlier. White stubbornly clung to the notion that victory had to remain the objective of an atomic war and indicated his inability to see the function of a secure second strike as removing the incentive for a first strike. Rather, White, steeped in the logic of conventional armaments, saw Soviet achievement of a secure second-strike capability as merely a major increase in their military strength.[113]

The final RAND analyst who made important contributions to deterrence/coercion theory is Thomas Schelling. However, given the fact that his ideas were so important to notions of limited war, it is necessary to describe how concepts of limited war emerged at RAND. Rather than reject the Korean War experience as anomalous and one that should never be repeated, RAND analysts tried to create a framework for the use of military force in the atomic era, seeing in limited war a new species of war. Bernard Brodie had outlined one aspect of limited war, which meant placing self-restraint on the means used. For Brodie, limited war connoted a war in which there was no strategic bombing between the United States and the USSR, with nuclear weapons held in abeyance. Therefore, keeping the costs limited for the United States meant curtailing its "taste for unequivocal victory." Again, drawing on the World War I example, Brodie characterized that war as the antithesis of limited war, for while the objectives were limited the means used to pursue them were not.[114] For the superpowers to engage in limited war necessitated "a deliberate hobbling of a tremendous power that is already mobilized and must *in any case be maintained at a very high pitch of effectiveness* for the sake only of inducing the enemy to hobble himself to like degree" (emphasis in the original). Brodie explained:

> The use of any kind of nuclear weapon greatly increases the difficulties in the way of maintaining limitations. For one thing, it is much easier for an observer to distinguish between use and non-use of, say, a 10-kiloton atomic weapon and a weapon two or three times as large.[115]

But limited war also implied limits on objectives, and, in contrast to Brodie, Alain Enthoven stressed more the need for limits here as the indispensable characteristic of limited war—a view he articulated as deputy assistant secretary of Defense for Systems Analysis.[116] However limited war was defined, the conception of it as it emerged from RAND, was contrary to the traditional military view that it was necessary to smash your opponent's capabilities in order to destroy his will to fight—a view that seemed confirmed by the experience of World War II. William Kaufmann provided the incisive statement of limited war bound to be anathema to conventionally minded military strategists:

> All the emotions traditionally associated with war must be inhibited. We are flung into a straightjacket of rationality, which prevents us from lashing out at the enemy. We are asked to make sacrifices and then cheer lustily for a tie game that we did not even ask to play.[117]

And that straightjacket of rationality that could limit war was thought to apply equally to nuclear contests between the superpowers and to local conflicts involving their proxies. In either setting, nuclear weapons played, in William Kaufmann's phrase, the role of "Constant Monitor," an incentive for both sides to keep the conflict limited.[118] Let us look at the logic then of what limits meant in both nuclear and non-nuclear war.

Kahn's analysis also contributed to notions of limiting the means of war with what came to be known as the "counterforce" doctrine. The idea behind counterforce was a simple one beginning with Brodie's recognition that the level of destruction possible with the H-bomb meant that indiscriminate attacks against cities would be ineffective militarily.[119] In addition, Brodie's expectation was that unlike weapon systems in the past, defense against nuclear weapons was unlikely.[120] Therefore, rather than targeting an opponent's cities, the optimal way to limit the damage in a nuclear war was to focus on destruction of enemy nuclear arsenals. While in the mid-1950s RAND analysts thought that a counterforce targeting strategy was not feasible because of the difficulty in locating Soviet targets, improvements in aerial reconnaissance gradually made counterforce viable. However, it proved difficult to create a consensus in the military that counterforce was a sound strategy. In particular, SAC clung to the World War II notion of strategic bombing and was reluctant to accept the restraint implied in counterforce.[121] Indeed, when Bernard Brodie was asked by the then Air Force Chief of Staff Vandenberg in 1950 to comment on SAC's target list, Brodie noted critically that the target list showed "no calculated strategy for destroying Soviet capability to make war." Rather, the planners simply assumed the Soviet Union would collapse as a result of the bombing campaign.[122]

Nevertheless, William Kaufmann briefed Secretary of Defense Robert McNamara on the counterforce strategy, who, given the grim prospects of a nuclear war provided in SIOP, was receptive to counterforce as a way to place limits even on a nuclear war. Of course, implicit in the counterforce strategy was the assumption of the efficacy of nuclear coercion and that the prospect of greater damage would encourage an opponent to capitulate. The counterforce strategy had the further attraction that it would strengthen NATO because by explicitly avoiding the targeting of cities, it eliminated the question of whether the United States would ever sacrifice its cities to save European ones.[123] For all these reasons, Robert McNamara announced the US commitment to a "no-cities" counterforce doctrine in June of 1962.[124] Finally, there was Albert Wohlstetter's contribution for

limiting nuclear war, in this case avoiding accidental war by putting in place the "fail safe" system of a series of checkpoints at which bombers would receive either confirmation of their targets or a call to return home.[125]

As a complement to counterforce, limited war demanded a force structure that included strong conventional capabilities which offered greater flexibility to the president in responding to contingencies in the periphery. Gone then was the excessive reliance on the massive retaliation and nuclear threats that were inherently not credible and its replacement with flexible response as a more appropriate deterrent in local wars. In this way, limited wars provided a mechanism for the two superpowers to continually gauge each other's relative power. According to William Kaufmann, limited wars in this context "perform a function midway between the abstractness of a show of force and the terrible concreteness of annihilative conflict. They become partial or token tests of strength, limited in scope, destructiveness, and time, and limited accordingly to political significance. They cannot represent decisive showdowns of power, but they can constitute more accurate indices of relative power."[126]

As contests to test the relative power of the United States and USSR, limited war relied on nuclear threats as that "constant monitor" for keeping war limited, and early conceptions of limited war suggested that it could not be divorced from the latent threat to use nuclear weapons. This role for nuclear weapons was identified by Daniel Ellsberg in his 1959 speech at the Lowell Institute where he suggested that nuclear weapons had one preeminent use in politics, which was to support threats. His choice of the word blackmail to describe the effect suggested, at the same time, an opponent's likely, ready compliance. Earlier, in 1948, Bernard Brodie had observed in a speech that there was "more strategic leverage to be gained in holding cities hostage than in making corpses" and that the Japanese surrender was less the result of damage suffered in the atomic bomb attacks and more the result of the implicit threat that more were on the way.[127] If such was in fact the case, the coercive potential of nuclear weapons might be great.

The notion that the value of military capability, whether nuclear or conventional, lay more in its coercive potential than in actual destruction led RAND analysts to the conclusion that force could be managed as part of a bargaining process.[128] If so, the economist's cost/benefit reasoning seemed especially applicable for describing the dynamic behind wielding military threats. The close kinship of strategy to economic theory had been pointed out by Brodie in 1949, and reiterated again in 1959.[129] As William Kaufmann noted, the very assumption of limited war was that it

involved "a calculating individual with a multiplicity of values, aware of cost and risk as well as advantage and *capable of drawing significant inferences from symbolic acts*"[130] (emphasis added). What is more and even more pernicious from the standpoint of traditional military thinking was that within a bargaining context the proper aim on the battlefield might well be "sustained stalemate."[131] To elaborate on these and other themes that percolate from RAND culture into the policy arena, we must explore the ideas of Thomas Schelling.

Thomas Schelling is arguably the one who exerted the greatest influence on the development of deterrence and coercion theory. Schelling is an economist who had worked with John McNaughton from 1948 to 1953 for the Economic Cooperation Administration, negotiating the European Payments Union as part of the Marshall Plan. Schelling served as adjunct fellow at RAND in 1956 and a senior staff member from 1958 to 1959, and his time at RAND prompted his decade-long interest in nuclear weapons and arms control.[132] Like others from RAND, Schelling was offered (but declined) a job in the Defense Department by Paul Nitze who had been chosen by Robert McNamara to serve as assistant secretary of International Security Affairs. Schelling did, however, recommend his friend John McNaughton for the post, and McNaughton continued to receive tutorials from Schelling as he served as McNamara's general counsel.[133] In 1961, Schelling ran several war simulation games sponsored by the Pentagon and held at Camp David. Known as the "Berlin Games," the participants included John McNaughton, Alain Enthoven and McGeorge Bundy.[134]

Although some of Thomas Schelling's analysis was foreshadowed in the work of his RAND colleagues, one way to conceive of the relationship among them is to think of Schelling's work as lying in the center of a wheel, with spokes radiating outward. The influence between Schelling and other RAND analysts was mutually reinforcing. Schelling himself acknowledged the impact of his RAND colleagues in the preface of his 1960 book, *The Strategy of Conflict*. There he noted his appreciation of Charles Hitch, Bernard Brodie, Daniel Ellsberg, Herman Kahn, William Kaufmann and Albert Wohlstetter.[135] Yet, more than his colleagues, Schelling's work has often been described as having an enduring impact on strategic discourse because his ideas were so fundamental in shaping thought on nuclear strategic problems.[136] Indeed, Schelling's influence continues to be felt on policy-makers, and his name continues to be invoked in contemporary discussions of various international crises.[137]

In part, Schelling's stature derives from the fact that he played two roles: that of a "speculative theorist" and pragmatic policy advisor and that his accomplishment lay in his ability to embed theoretical speculation in policy discussions.[138] In fact, Schelling exemplifies the two pathways, mentioned in Chap. 1, by which ideas affect policy: both passively as diffusion of ideas providing an intellectual framework for analyzing strategic choice and as policy entrepreneur as mentor to John McNaughton. That said, Schelling himself was more inclined to emphasize his role as theorist—he did, after all, decline a position in the Kennedy administration—and he acknowledged the rather slow refinement and lack of theoretical development of the concept of deterrence. Consequently, he sought to correct this shortcoming with what he described as the "strategy of conflict." For this endeavor, Schelling stressed that his definition of strategy was not a conventional military definition of the efficient application of force. Rather, strategy in his sense meant the exploitation of potential force. At the same time, Schelling recognized that while such an approach permitted the identification of "our own analytical thought processes with those of the hypothetical participants in a conflict," whether that theory provided "good or poor insight into actual behavior is…a matter of subsequent judgment."[139] Moreover, the preface of his 1966 book, *Arms and Influence,* which offers, perhaps, the highest expression of Schelling's theorizing, explicitly states that his aim is to derive principles that provide a foundation for the diplomacy of violence, but these do not lead directly to policy. And he states that the book makes no effort to make specific policy recommendations.[140] Despite Schelling's disclaimer, the logic of his analysis does suggest certain lines for policy so that it is difficult to always discern where the theoretician ends and a policy advisor begins.

For convenience, and at the risk of simplification, we can group Schelling's contribution to deterrence and coercion theory under two general topics: those that deal mainly with the nuclear relationship between the United States and the USSR and those that deal with local contests and limited war. To be sure, and as we shall see, there is a close linkage between the two, and much of Schelling's analysis on coercion and limited war is scarcely intelligible without reference to the nuclear relationship between the two superpowers. Schelling established the linkage between nuclear weapons and limited war by casting limited war as a bargaining process. He says: "To characterize the maneuvers and actions of limited war as a bargaining process is to emphasize that, in addition to the divergence of interest over the variable in dispute, there is a powerful common interest in

reaching an outcome that is not enormously destructive of values to both sides."[141] And, of course, avoiding that "enormously destructive" outcome that provides the common interest means avoiding a nuclear exchange. Thus, the linkage between the nuclear relationship and limited war becomes especially problematic for drawing policy conclusions from Schelling's analysis because it is easy to conclude that the dynamic of the superpower nuclear relationship is at play in other quite different circumstances.

In some of Schelling's earliest examinations of nuclear weapons, we can glimpse the influence of Albert Wohlstetter and his concern with surprise attack. The theme of surprise attack appears in some of Schelling's writings in 1959. Closely associated to his concern about surprise attack is his definition of stability in the context of the nuclear balance between the United States and USSR. Schelling characterized President Eisenhower's 1955 "Open Skies" proposal[142] as one arrangement designed to reduce fear of surprise attack. (Although Schelling realized that by 1959, the proposal was less useful because of the increase use of mobile weapons platforms.[143]) Not only would it reassure the United States that the Soviets were not preparing to attack but it also provided the Soviets with an assurance that the United States was not preparing for one. Such assurance would have a stabilizing impact because, for Schelling, the most potent incentive for either side initiating a total war with a surprise attack was "the fear of being a poor second for not going first." Schelling provides a clear statement of his concept of stability, and here we can see his intellectual debt to Bernard Brodie by differentiating stability from balance when he says:

> There is a difference between a balance of terror in which either side can obliterate the other, and one in which both sides can do it no matter who strikes first. It is not the "balance"—the sheer equality or symmetry in the situation—that constitutes mutual deterrence; it is the stability of the balance. The balance is stable only when neither, in striking first, can destroy the other's ability to strike back.[144]

Schelling's concern with surprise attack led him to conclusions quite inconsistent with more traditional military views, and he observed "that there are not only secrets we prefer not to keep, but even military capabilities we might prefer not to have."[145]

Like other RAND colleagues, Schelling identified one key for removing the incentive for a surprise attack—that is for both sides to have an invulnerable retaliatory force and for each to recognize that the other's ability to retaliate was invulnerable. Drawing on an economist's terminology,

Schelling expressed the difference between the first-strike and second-strike weapons: "A large vulnerable force has a comparative advantage in striking first; a smaller less vulnerable force has a comparative advantage in striking back."[146] For Schelling as for Brodie, that retaliatory force should necessarily be those weapons designed to punish and hurt the enemy's population. Such weapons should therefore be the most horrific, and while there might be a certain moral repugnance associated with them, the weapons were viewed by Schelling as defensive in character because their sole purpose was to strike back after an attack. Echoing Bernard Brodie's observation, Schelling described the basic logic behind retaliatory forces as simply "a massive and modern version of the exchange of hostages."[147]

For Schelling then, any disarmament schemes should not be aimed indiscriminately at all kinds of weapons or even selectively at the most horrifying ones used to target populations because doing so would produce instability into the nuclear equation. Any restrictions on armaments needed to be directed at the vulnerable first-strike weapons because they provide "a tacit declaration to the enemy that one expects to strike first." Moreover, the very vulnerability of such weapons makes them doubly dangerous to the extent they create a "use or lose" mentality, thus providing their possessor an incentive "to jump the gun in the event of an ambiguous warning." What is more, Schelling justified efforts for mitigating chances for surprise attack because he believed it to be an area where success was most likely which would increase the chances for establishing some tradition of cooperation on nuclear issues.[148]

Schelling goes on to extend the logic concerning stability to a hypothetically disarmed world. In this case, he stressed the importance of decreasing any advantage of a head start to rearmament. One way to reinforce the stability of deterrence in a disarmed world would be for both parties to disperse and duplicate facilities for rearmament. In this way, the destruction of the facilities would require such substantial military capabilities that neither side would be able to acquire or exploit a small advantage in rearmament. Hence, neither side needs to fear falling behind in any rearmament race. Schelling also identified the central paradox of disarmament and that if it reduced fears of a general war, there would then be fewer inhibitions on limited war so that disarmament represented a choice between minimizing war's destructiveness and minimizing its likelihood.[149] Schelling's extension of deterrence and stability to cover disarmament and arms control led Bernard Brodie to assert that Schelling's was the most incisive contribution to the literature on disarmament.[150]

While ensuring the survival of retaliatory capabilities seemed to promise a relatively easy and straightforward way to deter a deliberate nuclear attack on the US homeland—what Bernard Brodie called "basic" deterrence and Alexander George calls "strategic" deterrence—Schelling's analysis focused on a possibility of even greater concern. This was the realization that a superpower war might be more likely to occur as a result of an escalatory process. Because of this possibility, Schelling examined more closely psychological aspects of the process—a process he saw as growing from the bargaining power to hurt.

Schelling began by specifying the difference between brute force which aims to overcome an opponent's strength and the power to threaten pain that would work at the psychological level by structuring the opponent's motives. Schelling saw the distinction between brute force and the power to hurt as especially important to modern war, and he observed that brute force succeeds through its use, while the power to hurt is most successful when held in reserve. Nuclear weapons provided the possibility for threatening pain without needing to overcome the opponent's strength. Schelling noted that no one believed that the Soviets could overcome the United States and take over New York City but that no one doubted that the Soviets would be able to destroy the city. The situation was quite different from strategic bombing against Germany in World War II which may have been intended to have a psychological impact, but that effect was not sufficient in the absence of the defeat of the German army. The use of the atomic bomb against Japan, on the other hand, produced its effect not by military destruction but because of the pain and shock and the implicit threat that more might come. Schelling captured what was unique about nuclear technology: it "enhances the importance of war and threats of war as techniques of influence, not of destruction; of coercion and deterrence, not of conquest and defense; of bargaining and intimidation."[151] This shift in the very function of the military was bound to enhance the role of academic social scientists at the expense of military professionals in the formulation of strategy. In this formulation, coercion is explicitly viewed as the other side of the deterrence coin, and it suggests that traditional military planning with its emphasis on capabilities was inadequate. Rather, enemy intentions needed to be assessed and, more importantly, those intentions and motives needed to be shaped.

The starting point for shaping an opponent's intentions and motives, of course, was the need to communicate one's own intentions. The vehicle for communicating intentions is to make commitments, and because verbal

statements are often insufficient, actions like military deployments, defense budgets and weapons procurement decisions become more than the preparation for war and instead serve as means for communicating intentions to an opponent. Schelling, crossing into policy-making, suggested therefore: "Perhaps it is not altogether unwise deliberately to plan and to communicate a somewhat excessive military build-up ratio relative to the Soviet force in order to enhance their inducement to moderate their own program. (This sort of thing is not unknown in tariff bargaining.)"[152]

Schelling also outlined other techniques that can be useful for strengthening the credibility of commitment which can be stated simply but prove hard to operationalize in practice. First, one can place oneself in a position where you cannot retreat. The presence of American troops in Germany offers an example that provides the strategic equivalent of "burning the bridges" behind you. A country might also pledge its honor, prestige or diplomatic reputation to convey a position from which it cannot retreat. For the United States, a congressional resolution can be used for this purpose. Certainly, practitioners have recognized the value of prestige for deterrence. For example, Dean Acheson remarked about the Korean intervention that: "To back away from this challenge, in view of our capacity for meeting it, could be highly destructive of the power and prestige of the United States. By prestige, I mean the shadow cast by power, which is of great deterrent importance."[153]

American troops in Germany also served to create a degree of automaticity—a second technique for reinforcing a commitment to make a threat credible. Their presence foreclosed the option of not responding to a Soviet attack, and Schelling believed that maximum credibility obtains for a threat when there is little room for judgment or discretion.[154] Similarly, President Kennedy's speech on October 22, 1962, provides another example of automaticity because he indicated that should any missile be launched from Cuba, the United States would interpret it as an attack by the USSR and retaliate against them.[155] Related to automaticity is the tactic of relinquishing initiative and leaving the "last clear chance" for avoiding an unfavorable outcome to the opposing party after "having rigged the incentives so that the other party must choose in one's favor."[156]

A final technique for enhancing the credibility of a commitment lies with what Schelling viewed as one paradox of deterrence: namely, that it is not always helpful to be fully rational and in control. In other words, cultivating a reputation for impetuosity can strengthen a commitment. This behavior is sometimes described as the rationality of irrationality, and Schelling has

been criticized for seeming to suggest this as a valuable strategy. However, Schelling does add a caveat and noted the limitation of the approach because leaders will have to appear responsible to allies and the public. Schelling also stated explicitly in the preface of *Arms and Influence* that he does not approve of using irrationality as a tool for making threats credible.[157]

Schelling adds another layer of complexity to the art of commitment by differentiating between deterrence and compellence. The former involves some degree of passivity to the extent it requires incurring an obligation and setting a trip wire. The latter case requires the initiation of some punishing action that only ceases when an opponent responds by complying. Schelling draws his illustration of the difference between the two from the Cuban Missile Crisis. While President Kennedy's public statements that the United States would not tolerate offensive missiles in Cuba provided a deterrent threat, establishing a naval blockade around Cuba to intercept Soviet shipments was a compellent one.[158] As we can see from this illustration, compellent threats have a characteristic feature different from deterrent threats in that compliance is more conspicuous and more recognizable as submission under duress, and therefore more potentially humiliating. Given this characteristic, one must expect that such threats make compliance more difficult. While Schelling admits that designing compellent threats takes skill, what he does not say is the compellent threats may, by their very nature, place an opponent in a position where he cannot back down—in effect the party making the threat "burns the bridges" for his rival. In the nuclear context then, Schelling describes the technique of compellence as one of shared risk "that best deserves the name brinkmanship" so that relations between the United States and the USSR become contests in risk-taking under conditions of uncertainty that Schelling likens to a game of Russian Roulette.[159]

All the dynamics that Schelling saw as intrinsic to brinkmanship were on display during the Cuban Missile Crisis. Here was a case where the superpowers engaged in manipulating the shared risk of war and exploited the danger that "somebody may inadvertently go over the brink, dragging the other with him."[160] However, the fact that an escalatory process might get out of hand does not mean that either party would be able to manipulate risk to their advantage, although Schelling suggests that a proclivity or aversion to risk is a "strategic variable subject to deliberate manipulation."[161] Schelling's analysis concerning this point has been criticized. For example, Stephen Maxwell points out that one "cannot load the risk, as a die can be loaded…The important factor in determining the outcome is not any

appropriate manipulative skill, but the relative value to the contestants of the disputed objective."[162] In any case, Schelling characterized any ensuing nuclear war as one of pure coercion where neither side gains military advantage from the pain inflicted, but would continue to inflict it to show the adversary that more could come.[163]

Schelling saw in the contemporary use of military force a continuum running from a general thermonuclear war to a limited local war. In between the two extremes lay what he described as "a strategy of risky behavior, of deliberately creating a risk that we share with the enemy, a risk that is not entirely within our own and the Soviet's control." What is more, Schelling saw the Cuban Missile Crisis as providing a paradigm for a new species of limited war, one conducted under the shadow of the threat from nuclear weapons.[164] In fact, for Schelling, the very function of limited war was to pose a deliberate risk of all-out war. Using limited war as a tactic required more than verbal warnings. Actions to communicate the danger were necessary, for example, relying on support from a headstrong ally or enlarging the conflict geographically or introducing new weapons.[165] Since, for Schelling, the very meaning of limited war lay with limits on *means*, he believed that nuclear weapons represented an important threshold of violence that was not to be crossed. The Korean War set a certain precedent for the non-use of nuclear weapons and the test ban treaty reinforced an important psychological distinction between conventional and nuclear weapons. Schelling counseled careful consideration of the use of even "tactical" nuclear weapons because the value of their use was not for any mere battlefield advantage but to signal to the Soviets a heightened sense of the risk of a general war.[166]

This very non-military signaling role for nuclear weapons was explained in a document that Schelling—lapsing into a role as policy entrepreneur—prepared for the Kennedy administration in July of 1961 as a response to the Berlin Crisis of that year. In it, Schelling posited the thesis that the role for nuclear weapons in Europe should not be to win a "grand nuclear campaign" but to pose a greater level of risk for the Russians. Given this, Schelling recommended that the US plan for a "war of nerve, of demonstration and of bargaining" and that destruction of targets would be incidental to the message the weapons would convey to the Soviets. He then says: "The difference between one weapon, a dozen, a hundred, or a thousand, is not in the number of targets destroyed but in the Soviet (and American) perception of risks, intent, precedent, and implied 'proposal' for the conduct or termination of the war."[167]

So, if for Schelling limited war meant restraint on means and never crossing the threshold of using nuclear weapons, how might we reasonably expect such restraint to emerge in the antagonistic atmosphere of the East-West rivalry? Schelling begins his answer by acknowledging the fact that the two participants in the conflict, despite differences in interests, also share a common one, namely, avoiding a mutually damaging war. Indeed, Schelling believed that the very concepts of deterrence and limited war were concerned with mutual dependence and common interests. Hence, for the superpowers, limiting war does not require reconciliation of incompatible interests, but only the ability to coordinate their actions for mutual benefit. The key for doing so lies with "tacit bargaining" which is the ability to concert intentions or expectations when the other party is trying to do the same. Moreover, such tacit bargains can be struck even in the absence of communication when "the conflict of interest in the choice of action may be overwhelmed by the sheer need for concerting *some* action" (emphasis in the original).[168]

Schelling's conception of tacit bargaining, in effect, introduces a new element to the Prisoner's Dilemma game outlined earlier because it suggests that the prisoner's might arrive at a coordinated choice even in the absence of communication. In fact, in an interview, Schelling indicated his surprise at winning the Nobel Prize in economics in 2005 for his "game theoretic" analysis because he thought his work was not recognizable as game theory. In the interview, he went on to elaborate:

> Most game theory is concerned with identifying rational choice when the optimal choice depends on the choice or choices that another is or others are anticipated to make…I have been almost entirely concerned with how individuals rationally attempt to influence, not to anticipate the choices of others.[169]

In addition, some of his earlier writings pointed out that the game theory was less useful as a source for insights in non-zero-sum games where mutual dependence that is part of the logical structure demands some kind of collaboration or mutual accommodation.[170]

Within Schelling's tacit bargaining framework is the crucial element of the existence of some natural, obvious focal point to which both sides of a conflict gravitate. In some sense, the process is conceptually similar to the emergence of an equilibrium point in market economics. These focal points will be characterized by prominence or uniqueness and are so qualitatively different from other alternatives that they eliminate ambiguity in the choice. Schelling based his analysis concerning tacit bargaining and focal points on

a number of experiments. In one, subjects were instructed to write some positive number and told that if they all write the same number they would win. In the experiment, 40 percent of the subjects were able to pick the same number. In all of the experiments, the problem provided some focal point for concerted choice—some clue for coordination and a rationale for the convergence of the participant's expectations.[171]

More pertinent to our discussion is another illustration that Schelling draws from World War II, where the participants refrained from using poison gas. The bargain of "no gas" was simple and unambiguous and hence an obvious focal point, whereas other alternatives would raise questions of degree—like, use gas only against particular targets. Similarly, Schelling pointed out in reference to a recommendation that appeared in a newspaper, that the West unilaterally declare that in the event of a nuclear war, cities would not be targeted and nuclear weapons would only be used against military targets. This recommendation might yield a tacit bargain with the help of a prior suggestion. However, Schelling pointed out that this tacit bargain might still lack precision because it required judgments of degree, like the difference between a large and small city and the difference between military and non-military targets. Therefore, for Schelling, the obvious focal point for reaching a tacit bargain in a Soviet-American war is no use of nuclear weapons. This choice has prominence and uniqueness and is therefore conducive to concerted action. For, as Schelling observed, the distinction between nuclear weapons and conventional strategic bombing "is less physical and more psychic, perceptual, legalistic, or symbolic."[172]

Schelling summarized the process of tacit bargaining in war:

> In sum, the problem of limiting warfare involves not a continuous range of possibilities from most favorable to least favorable for either side; it is a lumpy, discrete world that is better able to recognize qualitative than quantitative differences, that is embarrassed by the multiplicity of choices, and that forces both sides to accept some dictation from the elements themselves.[173]

Once a tacit bargain is reached, according to Schelling, it gains a level of authority and sets a precedent whose violation would collapse the bargain. Thus, any breach of the "no nuclear" practice with even the use of tactical nuclear weapons would shatter the precedent and "the principle inhibition against the use of atomic weapons in limited war may disappear with their first use."[174] Schelling did not make a judgment concerning the probability of reaching a tacit agreement, but he did believe that his analysis of focal point characteristics provided a reasonable guide for where to find one.[175]

It does seem that policy-makers validated Schelling's notion of focal points and that using nuclear weapons would cross a "firebreak" or threshold that represented a qualitative distinction that both the United States and USSR could recognize. Alain Enthoven said as much in a speech he gave as deputy assistant secretary of Defense for Systems Analysis. Similarly, John McNaughton noted a recognition of the process of tacit bargaining in arms control and that such agreements need not necessarily be formally negotiated.[176] Perhaps even more dramatically, Schelling's ideas may well have given force to the US restraint in the 1961 Berlin Crisis. For then, as Fred Kaplan points out, the United States had such preponderant nuclear superiority that a disarming counterforce strike appeared to be technically feasible but that even a hardliner like Paul Nitze balked so that "approaching the height of the gravest crisis that had faced the West since the onset of the Cold War, everyone said, 'No'."[177]

We have already noted some difficulty with operationalizing Schelling's concepts. Numerous scholars have identified other problems with his framework. Perhaps most notable is Hans Speier who thought such threats with the potential to demoralize the whole population might just as easily lead the threatened government to react aggressively.[178] Later commentators have also recognized the problematic nature of nuclear threats. A country using such threats can easily exaggerate their effectiveness, and, in a worst case, the threats may backfire on their user. For example, President Eisenhower overestimated the impact of his threat to use atomic weapons on mainland China as the factor responsible for the first breakthrough in the Korean War truce negotiations when the death of Stalin was likely a critical variable. Nikita Khrushchev demonstrated how threats can backfire when he conveyed the impression that the Soviets were racing to deploy Intercontinental Ballistic Missles (ICBMs) and spoke as if the USSR was catching up with the United States. This talk prompted the United States to respond with its own rapid build-up, a move that was contrary to Soviet intentions and interests.[179]

In addition, critics have observed the extent to which Schelling's strategic analysis rested on extrapolating from economic reasoning. Schelling had, after all, learned about bargaining as a trade negotiator at international conferences dealing with foreign aid.[180] As Richard Ned Lebow points out that while the tactic of burning one's bridges so one cannot retreat may give bargaining leverage in international trade negotiations, such a tactic may prove completely inappropriate for policy-makers managing a nuclear crisis where the tactic could be catastrophic.[181] Economic issues are, after all, dominated by conflicts that Albert Hirschman labeled "more or less conflicts," while international politics and war in particular

are dominated by Hirschman's "either or" category which makes them less amenable to compromise.[182] In a similar vein, Kenneth Boulding warned in another context, that coercion could backfire because the "economic ethic" of making decisions on the basis of weighing loss and gains is often supplanted by a "heroic ethic" with attitudes involving "death wishes and the whole panoply of political paranoia."[183]

Associated with economic reasoning is a tendency toward abstraction that is another attribute of Schelling's framework (and deterrence theory in general) that has been criticized. For example, Alexander George and Richard Smoke portray deterrence theory as "abstract" and "deductivist" in a way that makes it inappropriate for policy application. While admitting that "strategic" deterrence might be amenable to mathematical methodologies, those methods are not useful for understanding deterrence of limited war. Concerning deterrence at this lower level, they say:

> It is dependent not upon comparatively few technical variables, known with high confidence on both sides, but upon a multitude of variables, many of them partially 'subjective' that fluctuate over time and are highly dependent upon the context of the situation.[184]

Yet, in fairness to Schelling, he does provide answers to these criticisms and was cognizant of limitations to his analysis. In *The Strategy of Conflict*, he identified the issue of using economic problems as proto-types for all bargaining situations because they were like Hirschman's category of "more or less" conflicts and "they tend disproportionately to involve divisible objects and compensable activities." Later in the same book, Schelling also warned against too much abstractness because, "we change the character of the game when we drastically alter the amount of contextual detail that it contains…It is often contextual detail that can guide the players to the discovery of a stable or, at least, mutually non-destructive outcome."[185] In other writings, Schelling noted an excessive reliance on mathematics to the detriment of a theory of strategy and a "willingness of social scientists to treat the subject as though it were or should be, just a branch of mathematics."[186] Given the fact that Schelling provided caveats to his analysis, it is surely unreasonable to expect the bulk of it to focus on points contrary to the lines of his logic.

Perhaps a more serious oversight in Schelling and the RAND work on deterrence lies with the unintended consequences of abstract economic reasoning, which is the tendency to treat events in isolation as singular occurrences. We have already quoted George Kennan's observation con-

cerning the proclivity of military analysts to see Soviet actions as disconnected from American moves. Richard N. Goodwin (who served as assistant special counsel and member of a Latin American Task Force in the Kennedy Administration) also identified this shortcoming. In a review of *Arms and Influence* that appeared in *The New Yorker* in 1968, he argued that one could not reduce the use of violence to any simple calculus of gains and losses, treating each clash as a self-contained incident "ignoring the radiating impact, across the world and over the years." What is more, Goodwin indirectly illustrates a flaw in Schelling's (and RAND's) view of the Soviets. Goodwin notes that Schelling interpreted Soviet intervention in Hungary in 1956 as a victory for them. Goodwin's interpretation of the event is quite different. He believed the action was a disaster for the Soviets because it came at a time when they were gaining a reputation as champion of the poor and oppressed. The intervention in Hungary showed them to be opponents of nationalism which Goodwin says, "helped to initiate that slow erosion of Russia's revolutionary appeal."[187]

Schelling's interpretation of Soviet actions in Hungary and Hans Speier's and Oskar Morgenstern's assessment of Soviet actions during the Suez Crisis, that we noted earlier, are part of a deeper flaw and blind spot in RAND analysis of deterrence and coercion theory. At its heart lay key assumptions about Soviet intentions and behavior that some critics viewed as unrealistic and skewed in one direction. In addition, there was no real internal debate in RAND concerning the nature of the USSR, and Alex Abella suggests that RAND helped propagate the belief that the Soviets were out to "devour" the world. Abella links this consensus RAND view to Nathan Leites' 1951 book *The Operational Code of the Politburo* which drew on writings from Lenin and Stalin, and which Abella asserts became RAND's house doctrine.[188]

How much of the view of the Soviets was a RAND invention and how much was merely a reflection of an already pervasive view of the Soviets in policy circles will never be known. However, at a minimum, RAND analysis reinforced a hawkish perspective concerning Soviet intentions. Thus, for example, William Kauffmann noted in the introduction to his edited book that even though Stalin had died, the USSR (and the PRC for that matter) remained aggressive powers that placed a high priority on expansion as a goal.[189] Herman Kahn too emphasized the malign intentions of the USSR and went so far as to describe their very effort to act as a great power and presumably pursue their national interest as "menacing."[190] Albert Wohlstetter, with his focus on the dangers of surprise attack, predicated his analysis on the

notion of aggressive Soviet intentions. Fred Kaplan points out a certain irony here because Wohlstetter's work "legitimized a basic fear of the enemy" through mathematical calculation and scientific analysis.[191] Furthermore, in the wake of the Cuban Missile Crisis, Albert Wohlstetter dismissed the view that the Soviet Union was a status quo power and noted that if so, Chairman Khrushchev did not seem to know it.[192]

Perhaps the strongest statement concerning Soviet intentions was supplied by another RAND analyst, Herbert S. Dinerstein. Writing in *Foreign Affairs* in 1958, Dinerstein asserted that the Soviets saw nuclear weapons as valuable for both deterrence and war-making and that because the ballistic missile was "the most perfect weapon of surprise," they would make a preemptive strike if the United States appeared about to attack. Dinerstein believed the Soviets to be striving for nuclear superiority and concluded:

> If they should acquire such preponderant military strength, they would have policy alternatives even more attractive than the initiation of nuclear war. By flaunting presumably invincible strength, the Soviet Union could compel piecemeal capitulation of the democracies. This prospect must indeed seem glittering to the Soviet leaders.[193]

One problem that deterrence theory had when estimating Soviet intentions (or payoffs) is the fact that the analysis omitted one important consideration for ascertaining an opponent's intentions, and that is knowing how they see risk. As difficult as it is to know what an opponent's payoff function is, understanding what they calculate as risk is even more difficult. From the standpoint of actual US policy, this is one factor that policymakers frequently got wrong. For example, George and Smoke observe that the United States saw the Soviet approval of the North Korean attack in 1950 as high risk, while the Soviets likely viewed it as low risk because a massive invasion would present the United States with a fait accompli and hence would not be likely to respond. Similarly, while American policy-makers saw the Soviet missile deployment in Cuba as a very high-risk venture, because the Russians viewed President Kennedy as weak and irresolute, they likely estimated a much lower risk. This very inability to assess an opponent's view of risk means that it remains difficult to accurately assess his intentions.[194]

The one outlier in the RAND consensus concerning the Soviets was Bernard Brodie who at least showed some sensitivity to the Soviet point of view. He commented in *Strategy in the Missile Age*:

> In general terms, we can hardly be too strong for our security, but we can easily be too forward and menacing in our manipulation of that strength. For example, it may be true that an ICBM deep in our own country menaces the Soviet Union as much as a shorter-range missile pointed at her from outside her frontiers, but the chances are that the Soviet leaders will be more disturbed by the latter. Unlike the ICBM, the nearby missile seems to denote arrogance as well as strength, and perhaps also a wider dispersal of the authority to fire it. If it is left unprotected, it trumpets the fact that it is intended for a first strike attack, not retaliation.[195]

Brodie also drew a different lesson than Wohlstetter from the Cuban Missile Crisis and saw it as demonstrating the Soviet's aversion to direct hostilities with the United States. At the same time, Brodie cautioned that Russian thinking regarding nuclear weapons was likely to be dissimilar to American views because the kind of civilian analysts outlining nuclear strategy in the United States did not exist in the USSR. Brodie cites as evidence a Soviet manual on strategy written under the direction of former Chief of Staff Marshall Sokolovsky, which expressed contempt for the "modern school of American economists who consider that it is possible to juggle with nuclear weapons."[196]

What then, can we say about the Soviet Union's views of nuclear weapons and deterrence during the early days of the Cold War? After all, one key lesson of game theory is that outcome in a conflict depends on your opponent's actions as well as your own. Were Soviet views consistent with those promulgated by RAND? One of the more interesting findings to come out of the Soviet archives is that they took for granted the aggressive intentions of the West and they assumed any war would start with a surprise attack by the Western forces. So the Soviets too were haunted by their World War II experience on this score. Soviet generals were "mesmerized" by the German surprise attack in 1941 and could not imagine any other strategy than preparing to strike before they were attacked. The reciprocal fear of surprise attack that was a concern of RAND analysts was a real one. According to the Parallel History Project on Cooperative Security, the 1964 Russian war plan for the invasion of Western Europe included the following assumptions:

- NATO defensive preparations were a sham.
- Only a swift offensive operation could guarantee success for the Warsaw Pact.
- The operation was feasible regardless of Europe's nuclear devastation.

– Technically superior Soviet air defenses could destroy incoming NATO missiles before they could do unacceptable damage.
– The USSR would prevail because of the West's greater vulnerability to nuclear devastation.

What is most striking in the plan was the fact that Soviet military planners did not believe they would be paralyzed by any US nuclear strikes. Their naiveté on this point is much like assumptions in the American SIOP plan that sought to conduct a nuclear war like a World War II strategic bombing campaign. Vojtech Mastny concludes from the document that while Soviet military planners did not see themselves as deterred by the West's nuclear arsenals and believed they could plan and win a nuclear war, their political leaders had no intention of starting one.[197] Indeed, Nikita Khrushchev is reported to have remarked: "We should not explode too powerful a bomb because we can break windows in our own house."[198] It is true that there was a difference in Warsaw Pact exercises and plans and NATO's and that the former did not abandon the war aim of victory—defined as destruction of enemy forces and occupation of enemy territory—until the late 1980s. While this plan is not necessarily indicative of aggressive intentions, it does illustrate a different value system.[199] We can also say, based on Soviet behavior, and the fact that they repeatedly backed down in confrontations (Iran 1946, Berlin 1949, 1959, 1961 and Cuba in 1962), that the Soviets were less concerned with reputation and the problem of credibility. As Patrick Morgan observes, Soviet literature on deterrence placed little emphasis on the importance of credibility.[200] The fact that the Soviets could retreat lends credence to a criticism of deterrence theory offered by George and Smoke, that theorists oversimplified commitment and portrayed it as necessarily strong and unequivocal.[201]

Where does this leave us when evaluating the scientific status of RAND's work on deterrence and coercion theory? Any assessment must begin with a reminder of Karl Popper's observation that even if a theory is not scientific, it may still be important. In the broadest sense, deterrence/coercion theory did represent a paradigm change concerning the use of force, but it did not contain a consensus concerning the application of nuclear weapons to strategy. Ideas ranged from Brodie's belief that their function served only to deter a nuclear attack, to Kaufmann and Schelling's conception of them as a "constant monitor" for keeping great power conflicts limited, to Kahn's view that atomic weapons were likely to be used so policy needed to plan for surviving a nuclear war. Despite then the claims

that their analysis drawing on systems theory was scientific, critics saw the flaw in "attempt[ing]t to fit essentially political questions into the straightjacket of so-called scientific analysis" and "to separate the 'analytical components' of a policy problem from the political and moral ones." For other critics, the very characteristics of the "RANDites" were the issue because they were people dedicated "to abstract theory and a sense of absolute self-righteousness married to an amoral approach to politics and policy."[202] The abstraction most detrimental to policy lay in the extent that it characterized the cost/benefit of decision-making as universal, along with the tendency to conceive of military force solely in terms of symbolic acts and signals. The closest thing to consensus among nuclear strategists at RAND was the questionable assumption that Soviet intentions were malign and aimed at expansion and conquest. Given this consensus, perhaps the most appropriate assessment might be to apply one that Albert Wohlstetter made concerning natural scientists. He said: "The basic failure of the physical scientists and engineers in their turbulent history during the Cold War is not their lack of prescience, but their acting frequently as if they had it."[203] The same charge might be made against those who sought to turn nuclear strategy into a science.

Notes

1. David Alan Rosenberg, "The Origins of Overkill: Nuclear Weapons and American Strategy, 1945–1960," *International Security* 7 (Spring 1983): 12, 14.
2. William W. Kaufmann, *The McNamara Strategy* (New York: Harper and Row Publishers, 1964), 23.
3. Stephen Maxwell, "Rationality in Deterrence," *Adelphi Papers* 50 (August 1968): 2.
4. Kaufmann, *The McNamara Strategy*, 4.
5. Bernard Brodie, "Strategy as a Science," *World Politics* 1 (July 1949): 467–488.
6. Philip Green, *Deadly Logic: The Theory of Nuclear Deterrence* (Columbus: Ohio State University Press, 1966), xiv.
7. Thomas D. White, "The Defense Intellectuals," *The Saturday Evening Post* 236 (May 4, 1963): 10–13.
8. Arthur Schlesinger, *A Thousand Days: John F. Kennedy in the White House*, (Boston: Houghton Mifflin Company, 1965), 901.
9. Albert Wohlstetter, *The Delicate Balance of Terror*, (Santa Monica: The RAND Corporation, 1958), 2.

10. Martin J. Collins, *Cold War Laboratory: RAND, the Air Force and the American State, 1945–1950* (Columbus: Ohio State University Press, 2002), 33.
11. Collins, 14.
12. Robert Ayson, *Thomas Schelling and the Nuclear Age: Strategy as Social Science*, (London: Frank Cass, 2004), 3.
13. Colin S. Gray, "What RAND Hath Wrought," *Foreign Policy* 4 (Fall 1971): 112.
14. Alex Abella, *Soldiers of Reason: The RAND Corporation and the Rise of the American Empire*, (New York: Harcourt, Inc. 2008), 32.
15. Abella, 13.
16. Collins, 94.
17. Abella, 12–14.
18. Collins, xii.
19. In this, RAND differed from the Navy's Office of Naval Research that evaluated research proposals based on general scientific merit rather than any utility to the Navy. See Lyle H. Lanier, "The Psychological and Social Sciences in the National Military Establishment," *American Psychologist* 4 (May 1949): 137.
20. Collins, 146.
21. Robert J. Leonard, "War as a 'Simple Economic Problem': The Rise of an Economics of Defense," in *Economics and National Security,* ed. Craufurd D. Goodwin (Durham, NC: Duke University Press, 1991), 270.
22. Abella, 63, 139–140. By 1961, General LeMay was Chief of Staff of the Air Force, and he was furious at what he viewed as RAND treason against the interests of the Air Force which had done so much to nurture and protect RAND.
23. Collins, 117–118, and Abella, 21.
24. Collins, 213.
25. Kenneth E. Boulding, *Conflict and Defense: A General Theory* (New York: Harper and Row Publishers, 1962), 332.
26. Margaret Mead would agree to work as a consultant for RAND. Collins, 126.
27. Collins, 135, 133.
28. Collins, 149–150. For a more negative appraisal of life in that monastery, see Anthony Russo, "Inside the RAND Corporation and Out: My Story," *Ramparts* (April 1972): 49.
29. Abella, 28–29.
30. Ron Robin, *The Making of the Cold War Enemy: Culture and Politics in the Military-Intellectual Complex* (Princeton: Princeton University Press, 2001), 48. John Williams by the 1950s, becomes disappointed in the social sciences division because he thought Speier hired too many people

who were using RAND as a base to research their doctoral dissertations. See Fred Kaplan, *The Wizards of Armageddon* (New York: Simon and Shuster, 1983), 76.

31. Kaplan, 76. One added element that may have aggravated tensions between the physics division and others was the fact that for security reasons, they were separated from the others by an electronic locking door.
32. Kaplan, 121.
33. Daniel Bessner, "Organizing Complexity: the Hopeful Dreams and Harsh Realities of Interdisciplinary Collaboration at the RAND Corporation in the Early Cold War," *Journal of the History of the Behavioral Sciences* 51 (Winter 2015): 33.
34. Abella, 108–109.
35. Bernard Brodie, "The McNamara Phenomenon," *World Politics* 17 (July 1965), 679.
36. Gray, 119.
37. Kaplan, 73.
38. Critics of this rational approach that seemed to ignore culture and other factors informing strategic decisions may not appreciate the extent to which the novel characteristics of the weapons made such oversight reasonable. See Bessner, passim. For an analysis of nuclear deterrence that denies its universal applicability and rather links it to US culture, see Adam Garfinkle, "The Anglo-Protestant Basis of US Foreign Policy," *Orbis* (Winter 2018): 116.
39. William Polk, "Problems of Government Utilization of Scholarly Research in International Affairs," in *The Rise and Fall of Project Camelot*, ed. Irving Horowitz (Cambridge, MA: The MIT Press, 1967), 263.
40. Collins, 119 and Abella, 23.
41. Herman Kahn, *On Thermonuclear War* (Princeton: Princeton University Press, 1961) 55.
42. Collins, 173.
43. Abella, 57.
44. Charles Hitch, "Economics and Military Operations Research," *The Review of Economics and Statistics* 40 (August 1958), 200.
45. Malcolm W. Hoag, "An Introduction to Systems Analysis," Memorandum RM1678, April 18, 1956. Downloaded from http://www.rand.org/content/dam/rand/pubs/researchmeoranda/2006/R1678, March 2014.
46. Herman Kahn and Irwin Mann, "Techniques of Systems Analysis," Memorandum, RM 1829–1-PT. June 1957, downloaded on March 31, 2014 from http://www.rand.org/content/dam/rand/pubs/researchmemoranda/2006/R
47. Leonard, 272.
48. Bernard Brodie, *Strategy in the Missile Age* (Princeton: Princeton University Press, 1959), 384.

49. Hoag, "An Introduction to Systems Analysis," Memorandum RM1678, April 18, 1956. Downloaded from http://www.rand.org/content/dam/rand/pubs/research_memoranda/2006/RM1678
50. Quoted in the Official Biography of Robert McNamara, at http://history.defense.gov/McNamara.shtml
51. Green, 21, 91. Green is especially critical of Herman Kahn's use of systems analysis in his book, *On Thermonuclear War*, and says that Kahn's "calculations" are merely assigned values.
52. Gray, 111.
53. Green, 98.
54. Kaplan, 91.
55. Kaplan, 64.
56. Oskar Morgenstern, "The Cold War is Cold Poker," *The New York Times Magazine,* February 5, 1961. Downloaded from www.nytimes.com/mem/archive on March 6, 2014. Although Morgenstern admits that American pressure on her allies was the reason for the British and French withdrawal from Suez, the fact that world opinion attributed the withdrawal to the Soviet threat meant that this event constituted a "win" for the Soviets.
57. Some outside observers believed the concern with credibility to be excessive and attributed it to the peculiarly American "engineering approach" to foreign policy. See Patrick M. Morgan, "Saving Face for the Sake of Deterrence," in *Psychology and Deterrence,* ed. Robert Jervis, et al. (Baltimore: The Johns Hopkins University Press, 1995), 144.
58. Kaplan, 177–178.
59. Quoted in Alexander L. George and Richard Smoke, *Deterrence in American Foreign Policy: Theory and Practice* (New York: Columbia University Press, 1974), 169.
60. Green, 109, 98.
61. This straightforward and broad definition of deterrence applies to shaping a variety of choices in international politics. A more narrow definition of deterrence that sees its function as merely the prevention of war can be found in Raoul Naroll, "Deterrence in History," in *Theory and Research on the Causes of War* eds. Dean G. Pruitt and Richard C. Snyder (Englewood Cliffs, NJ: Prentice Hall, Inc. 1969), 150–164.
62. Collins, 30.
63. Rosenberg, 11, 28.
64. Janet Farrell Brodie, "Learning Secrecy in the Early Cold War: The RAND Corporation," *Diplomatic History* 35 (September 2011): 657.
65. Hans Speier, "Soviet Atomic Blackmail and the North Atlantic Alliance," *World Politics* 9 (April 1957): 315, 322. Speier's discussion of the Suez Crisis, which is couched entirely in East-West terms, is indicative of a kind of tone deafness toward other aspects of international relations—like Arab nationalism—that pervaded RAND analysis of nuclear issues.

66. Bessner, 35.
67. Bernard Brodie, "Implication for Military Policy," in *The Absolute Weapon: Atomic Power and World Order*, ed. Bernard Brodie (New York: Harcourt, Brace and Company, 1946), 76.
68. Kaplan, 79, 82, 84. Reinforcing Oppenheimer's opposition was a manifesto issued by Bertrand Russell in 1955 and signed by such notables as Albert Einstein and Linus Pauling, that called for an assembly of scientists to discuss the threat of nuclear weapons. For one RAND member's view that questioned the role of scientists in formulating military policy and weapons choice, see Albert Wohlstetter, "Scientists, Seers and Strategy," *Foreign Affairs* 41 (April 1963): 466–478.
69. Thomas Schelling, "A tribute to Bernard Brodie and (incidentally) to RAND," downloaded on March 11, 2014 from https://www.rand.org/content/dam/rand/pubs/papers/2006/P6563.pdf
70. Brodie, "Implications for Military Policy," 81.
71. Brodie, *Strategy in the Missile Age*, 147, 225.
72. Brodie, "War in the Atomic Age" and "Implications for Military Policy," passim.
73. Brodie, "Implications for Military Policy," 73–74, 107, 76, 95.
74. Brodie, *Strategy in the Missile Age,* 302.
75. Brodie, *Strategy in the Missile Age,* 393.
76. Brodie, *Strategy in the Missile Age,* 55.
77. Brodie, "Strategy as a Science," 467–488.
78. Brodie, *Strategy in the Missile Age*, 386,388, 389.
79. Bernard Brodie, "War in the Atomic Age," in *The Absolute Weapon: Atomic Power and World Order,* ed. Bernard Brodie (New York: Harcourt, Brace and Company, 1946), 28.
80. Abella, 92.
81. William Kaufmann, "The Requirements of Deterrence," in *Military Policy and National Security,* ed. William Kaufmann (Princeton: Princeton University Press, 1956), 19.
82. George and Smoke, 27.
83. Brodie, *Strategy in the Missile Age*, 250.
84. Kaufmann, "The Requirements of Deterrence," 19.
85. Scott Sagan, "The Nuclear War Planning Briefing to President Kennedy," *International Security* 12 (Summer 1987): 23.
86. Rosenberg, 4–5.
87. Kaplan, 263–285.
88. Farell Brodie, 651.
89. Kahn, *On Thermonuclear War*, 562. Kahn subsequently revised his view of people who saw deterrence as automatic. See Herman Kahn, *On Escalation: Metaphors and Scenarios* (New York: Frederick A. Praeger Publisher, 1965), 246.

90. Kahn, *On Thermonuclear War,* 22.
91. Kahn, *On Thermonuclear War*, 98, 564. For all of his assertions concerning the scientific basis of his analysis, the book is heavily weighted with outlandish scenarios and analogies. For example, when discussing the risks that city dwellers are likely to accept, he makes a comparison with lions running loose in the city, 376. What is more, even though Kahn casts the book as representative of systems analysis, it is difficult to gain an understanding of the methodology from the book.
92. Kahn, *On Thermonuclear War,* 553, 554, 523.
93. Kahn, *On Thermonuclear War*, viii. Philip Green's critical account of Kahn's work suggests that most of his analysis was so hypothetical and the information mostly supposition, that it hardly qualified as science. See Green, 38–39.
94. Kahn, *On Escalation*, 201.
95. Kahn, *On Thermonuclear War*, 21.
96. Abella, 103.
97. Kaplan, 124. Wohlstetter's concern with surprise attack may have been influenced by his wife Roberta's famous study of Pearl Harbor. See Abella, 82. And, of course, the Pearl Harbor analogy begs the questions of whether the Japanese would have bombed even a tempting target if they could expect retaliation with ten or more Nagasaki-sized atom bombs.
98. Albert and Roberta Wohlstetter, "Controlling the Risks in Cuba," *Adelphi Papers* 17 (February 1965): 11.
99. Kaplan, 104.
100. Kaplan, 104.
101. George and Smoke, 32.
102. Kaplan, 144. Abella, 111, 113. For a critical appraisal of the Gaither Committee, see Greg Herken, "Commentary: In the Service of the State: Science and the Cold War," *Diplomatic History* 24 (Winter 2000): 107–115.
103. Kaplan, 145.
104. Herken, 112.
105. One reason that Eisenhower did not succumb to the panic over the missile gap is because of the intelligence provided to him by the U-2 flights which were kept secret from both the public and the Congress. Consequently, the evidence from the U-2 flights was not made available to the Kennedy campaign so that his assertion of the missile gap was not a deliberate fabrication. George and Smoke, 455. Furthermore, one scholar has suggested that the motive for Eisenhower's famous speech warning of the military-industrial complex was the leaking of the Gaither Report and its impact on the 1960 elections. See Abella, 132.

106. Kaplan, 169, 110, 289. The need for the United States to engage in such an arms race was questionable because the Discoverer reconnaissance satellite in August 1960 revealed a nearly ten to one missile gap in favor of the United States. Even after JFK learned that the missile gap was in the US favor, he continued to accelerate the missile programs and tripled the rate of construction of Polaris submarines and doubled production capacity of minuteman rockets. See Jerome H. Kahan and Anne K. Long, "The Cuban Missile Crisis: A Study of Its Strategic Context," *Political Science Quarterly* 87 December 1972): 565.
107. Albert Wohlstetter, "The Delicate Balance of Terror," *Foreign Affairs* 37 (January 1959): 222.
108. Brodie, *Strategy in the Missile Age*, 278. Brodie differed from Wohlstetter in another important way. While Wohlstetter seemed to accept the inevitability of nuclear war, Brodie sought to avoid it by all means. See, Abella, 88.
109. Thomas w. Milburn, "What Constitutes Effective Deterrence?" *The Journal of Conflict Resolution* 3 (June 1959): 139, 142–143.
110. Kaplan, 253.
111. Daniel Ellsberg, "The Theory and Practice of Blackmail," July 1968, downloaded on February 22, 2014 from http://www.rand.org/pubs/papers/P3883. Although Ellsberg outlined his ideas in a lecture in 1959, he put them in a document form only in 1968 in response to requests for his analysis.
112. Kaplan, 249.
113. White, 10. Some of the preferences of the military reflected in SIOP reemerged in another guise when President Reagan's Secretary of Defense Caspar Weinberger enunciated the Weinberger Doctrine in 1984. One of its principles was to use overwhelming force, with the clear intention to win.
114. Brodie, *Strategy in the Missile Age,* 309, 310, 314, 312.
115. Bernard Brodie, "More About Limited War," *World Politics* 10 (October 1957): 114–115, 117.
116. Alain Enthoven, "American Deterrent Policy," in *Problems of National Strategy: A Book of Readings,* ed. Henry Kissinger (New York: Frederick A. Praeger Publishers, 1965), 121.
117. William Kaufmann, "Limited Warfare," in *Military Policy and National Security,* ed. William Kaufmann (Princeton: Princeton University Press, 1956), 129.
118. Kaplan, 219.
119. Kaplan, 78.
120. Brodie, *Strategy in the Missile Age*, 121.
121. Kaplan, 245.
122. Rosenberg, 18.
123. Kaplan, 270, 283.

124. McNamara's announcement had one unintended effect. Because it suggested that the United States could locate the Soviet Union's strategic arsenal, it added to their concerns regarding their nuclear inferiority and added another incentive for them to deploy missiles to Cuba. See Kahan and Long, 566.
125. Abella, 73.
126. Kaufmann, "Limited Warfare," 118.
127. Kaplan, 47.
128. For a critical analysis of the shift toward the view of military force as a coercive tool, see Stephen Peter Rosen, "Vietnam and the American Theory of Limited War," *International Security* 7 (Fall 1982): 83–113.
129. Brodie, "Strategy as a Science," 478–479, *Strategy in the Missile Age*, 361.
130. Kaufmann, "Limited Warfare," 117.
131. Kaplan, 198.
132. Thomas C. Schelling, Interview downloaded from www.gametheorists.com/interviews/schelling on March 28, 2011.
133. Bruce Kuklick, *Blind Oracles: Intellectuals and War From Kennan to Kissinger* (Princeton: Princeton University Press, 2006), 137.
134. Esther-Mirjam Sent, "Some Like It Cold: Thomas Schelling as a Cold Warrior," *Journal of Economic Methodology* 14 (December 2007): 461–462. The same source also notes that Schelling served briefly as advisor on the film, *Dr. Strangelove*.
135. Thomas C. Schelling, *The Strategy of Conflict* (Cambridge, MA: Harvard University Press, 1960), vi.
136. Colin Gray for one, conceives of Schelling's influence as such. See his preface in Robert Ayson's book, *Thomas Schelling and the Nuclear Age: Strategy as Social Science*, vii–viii.
137. Richard Ned Lebow, "Reason Divorced from Reality: Thomas Schelling and Strategic Bargaining," *International Politics* 43 (2006). For an example of a recent use of Schelling's framework, see Tyler Cowen, "Crimea, Through a Game-Theory Lens," *The New York Times* (March 16, 2014): 6. Because our interest lies in the early Cold War period, our discussion does not include Schelling's later works.
138. Green, 131, 134.
139. Thomas C. Schelling, "The Retarded Science of International Strategy," *Midwest Journal of Political Science* 4 (May 1960): 109, 108. For an example of Schelling's work that is less theoretical and more policy oriented, see Thomas C. Schelling, "Managing the Arms Race," in *Problems of National Strategy: A Book of Readings*, 361–375.
140. Thomas C. Schelling, *Arms and Influence* (New Haven: Yale University Press, 1966), vi, vii.

141. Schelling, "The Retarded Science of International Strategy," *Midwest Journal of Political Science*, 110.
142. This was a proposal made during a Geneva Summit meeting attended by Britain, France and the USSR. The proposal called for the United States and USSR to exchange maps showing the exact location of all military installations so that the maps would enable surveillance and ensure against surprise attack. The Soviets rejected the proposal, and two months later, President Eisenhower approved the use of U-2 flights over the Soviet Union.
143. Thomas C. Schelling, "Surprise Attack and Disarmament," in *NATO and American Security,* ed. Klauss Knorr (Princeton: Princeton University Press, 1959), 191.
144. Thomas C. Schelling, "Surprise Attack and Disarmament," *Bulletin of the Atomic Scientists* 15 (December 1959): 414. This article is a shorter version of the chapter in the Knorr volume cited earlier. Robert Ayson stresses in his book on Schelling, that his concern with stability was a consistent aspect of his thinking. Ayson, 1.
145. Schelling, "Surprise Attack and Disarmament," *Bulletin of the Atomic Scientists*, 413.
146. Thomas C. Schelling, "The Strategy of Inflicting Costs," in *Issues in Defense Economics* (New York: Columbia University Press, 1967), 114.
147. Schelling, "Surprise Attack and Disarmament," *Bulletin of the Atomic Scientists*, 416.
148. Schelling, "Surprise Attack and Disarmament," in *NATO and American Security,* 188–189, 176.
149. Thomas C. Schelling, "The Role of Deterrence in Total Disarmament," *Foreign Affairs* 40 (April 1962): 399, 397.
150. Brodie, *Strategy in the Missile Age*, 300–301.
151. Schelling, *Arms and Influence*, 3, 6, 33.
152. Schelling, "Managing the Arms Race," 362, 374.
153. Quoted in Patrick M. Morgan, "Saving Face for the Sake of Deterrence," in *Psychology and Deterrence* eds. Robert Jervis, et al. (Baltimore: The Johns Hopkins University Press, 1985), 138.
154. Schelling, *The Strategy of Conflict*, 40.
155. Schelling, *Arms and Influence,* 38–40.
156. Schelling, *The Strategy of Conflict,* 37.
157. Schelling, *Arms and Influence*, 40. Stephen Maxwell is one who suggests that Schelling seems to regret that it is not possible for a state to deliberately choose to be insane and seems to recommend recklessness or irrationality. Maxwell, 10.
158. Schelling, *Arms and Influence*, 70–71, 81.
159. Schelling, *Arms and Influence,* 84, 91, 94–95.

160. Schelling, *Arms and Influence*, 99.
161. Schelling, "The Retarded Science of International Strategy," 129.
162. Maxwell, 13.
163. Schelling, *Arms and Influence*, 201. Although Schelling does not explicitly make a distinction between coercion and coercive diplomacy, Alexander George does and says that the latter requires some genuine concessions and compromises with the opponent, which incidentally must also be credible. See Alexander George, "The Development of Doctrine and Strategy," in *The Limits of Coercive Diplomacy: Laos, Cuba, Vietnam* eds. Alexander L. George et al. (Boston: Little Brown and Company, 1971), 25–26.
164. Schelling, *Arms and Influence,* 109, 166.
165. Schelling, *Strategy of Conflict*, 193.
166. Schelling, *Arms and Influence,* 114–115, 264.
167. Thomas C. Schelling, "Nuclear Strategy in the Berlin Crisis," in *Foreign Relations of the United States, 1961–1963* Volume XIV, Berlin Crisis, 1961–1962, Document 56. Downloaded on June 18, 2013 from http://history.state.gov/historicaldocuments/frus1961-63v14/d56. Schelling composed a more academic version of the paper, "Nuclear Strategy in Europe," *World Politics* 14 (April 1962): 421–432.
168. Thomas C. Schelling, "The Strategy of Conflict Prospectus for a Reorientation of Game Theory," *Journal of Conflict Resolution* 2 (September 1958): 205.
169. Schelling, Interview.
170. Schelling, "Strategy of Conflict Prospectus for a Reorientation of Game Theory," 203.
171. Schelling, "Strategy of Conflict Prospectus for a Reorientation of Game Theory," 207.
172. Thomas Schelling, "Communication and Limited War," *Journal of Conflict Resolution* 1 (March 1957), 32, 35.
173. Schelling, *Strategy of Conflict,* 77. Schelling also expected that the dynamics at work in a tacit bargain would apply to cases of overt negotiation, 67.
174. Thomas C. Schelling, "Nuclear Weapons and Limited War," *US Air Force Project RAND Research Memorandum* RM-2510, (Santa Monica, 1959), 10.
175. Schelling, "Communication and Limited War," 32.
176. Enthoven, "American Deterrent Policy," 127. John T. McNaughton, "Arms Restraint in Military Decisions," *The Journal of Conflict Resolution* 7 (September 1963): 228.
177. Kaplan, 301.
178. Speier, 308. Speier also notes that while the target of such atomic threats in peacetime sees them as blackmail, the country making the threats prefers to see them as deterrence.
179. George and Smoke, 238, 450, 451.

180. Sent, 459.
181. Lebow, 442. Lebow also sees Schelling's work as representing the broader intellectual development of the colonization of the social sciences by micro-economics, 430.
182. Albert O. Hirschman, "Social Conflicts as Pillars of Democratic Market Society," *Political Theory* 22 (May 1994): 213.
183. Kenneth E. Boulding, *The Impact of the Social Sciences* (New Brunswick, NJ: Rutgers University Press, 1966), 68.
184. George and Smoke, 503, 39, 54.
185. Schelling, *Strategy of Conflict*, 31, 162.
186. Schelling, "The Retarded Science of International Strategy," *Bulletin of the Atomic Scientists,* 104.
187. Richard N. Goodwin, "The Unthinkable and Unanalyzable," *The New Yorker* (February 17, 1968): 127.
188. Abella, 35, 36.
189. Kaufmann, "Introduction," in *Military Policy and National Security,* 3.
190. Kahn, *On Thermonuclear War*, 313.
191. Kaplan, 125.
192. Albert and Roberta Wohlstetter, 9.
193. Herbert S. Dinerstein, "The Revolution in Soviet Strategic Thinking," *Foreign Affairs* 36 (January 1958), 249, 252.
194. George and Smoke, 160, 465.
195. Brodie, *Strategy in the Missile Age*, 398.
196. Bernard Brodie, "What Price Conventional Capabilities," in *Problems of National Strategy: A Book of Readings,* ed. Henry Kissinger, 324, 321.
197. Vojtech Mastny, "Planning for the Unplannable," *The Parallel History Project on Cooperative Security* downloaded on April 7, 2014. From http://www.php.isn.ethz.ch/collections/colltopic.cfm?ing=en&ID=15365
198. Quoted in Neil MacFarquahar, "Amid a Revived East-West Chill, Cold War Relics Draw New Interest," *The New York Times* (April 30, 2014): A8.
199. Beatrice Heuser, "Victory in a Nuclear War? A Comparison of NATO and WTO Aims and Strategies," *Contemporary European History* 7 (1998): 326.
200. Patrick M. Morgan, "Saving Face for the Sake of Deterrence," in *Psychology and Deterrence*, 142–143.
201. George and Smoke, 80.
202. Green, 259. Abella, 21.
203. Wohlstetter, "Scientists, Seers and Strategy," 478.

CHAPTER 4

A Scientific Approach to Development: Modernization Theory

Just as the new technology of warfare provided an impetus to a scientific military strategy in the form of deterrence theory, so too did a change in the strategic environment in the aftermath of World War II produce an incentive for the creation of a scientific approach to development.[1] After all, the gradual disintegration of colonial empires necessitated policies to foster development and provided social scientists with near-laboratory conditions for studying social change in the newly independent colonies. Studying the former colonies was attractive to scholars because of the possibility of combining scientific pursuits with practical utility as one way to evade the value problem that we saw raised by Robert Lynd in his 1939 book, *Knowledge for What*. The expanded interest in the former colonies can be measured in part by the renewed interest and geographical scope in comparative politics reflected in the fact that in 1948, less than 10 percent of political science doctoral dissertations were in the subfield of comparative politics; by 1958, a total of 25 percent, and by 1968 over 35 percent of doctoral dissertations were on this topic.[2]

Such an interest is understandable because as Edward Shils observed in 1963, these "new states" (defined as those countries that gained independence after 1945) provided social scientists with an opportunity to observe firsthand the social processes that presented fundamental puzzles for them, such as the emergence of social order and the legitimation of authority.[3] At the same time, the trauma of World War II raised doubts about the utility of what had been the dominant approach for studying politics that focused

J. M. Klinger, *Social Science and National Security Policy*,
https://doi.org/10.1007/978-3-030-11251-6_4

on legal forms to the exclusion of broader social conditions. For what utility did studying the Weimar Constitution have for understanding the rise of National Socialism? What is more, the legacy of the experience in Office of Strategic Services (OSS)—McGeorge Bundy had characterized it as the "first great center of area studies in the US"[4]—and the War Information Office demonstrated the advantages of a more interdisciplinary approach for studying the "new states" that would take into account these broader social conditions.

That interdisciplinary approach emerging after the war was embraced by scholars of comparative politics and became embodied in modernization theory. There is no small irony in the fact that proponents of modernization theory saw factors like secularization, industrialization and urbanization as benevolent forces shaping human society because, during the 1930s, those same factors were viewed with alarm to the extent they were thought to contribute to the erosion of community leading to alienation and anomie.[5] This chapter reviews key aspects of the modernization literature through the work of some of the leading scholars of comparative politics. Doing so, the chapter will draw links among the various approaches and highlight both the strengths and weaknesses of interpreting social change through the lens of modernization theory. In addition, the chapter highlights the extent to which scholarship reflected that conflicted identity within the social sciences that we noted in Chap. 1. As Lucian Pye pointed out regarding his discipline of political science, the problem of political and economic development emerged just as political science was shedding its earlier normative tradition, adopting an empirical approach to real-world conditions rather than focusing on some hoped for imagined future.[6] The chapter will then trace the influence via the two pathways of how ideas associated with modernization theory impacted policy in the New Frontier era of John F. Kennedy.

To begin with, to some degree, modernization served as an extension of an earlier tradition of literature on social change and development. As Robert Bellah observed, especially in the United States, modernization theory "was a kind of late child of the enlightenment faith in progress." Indeed, the earliest use of the verb "to modernize" in the sense of some universal progression linked to technical change was William Thackeray's suggestion in 1860 that "printing and gunpowder tend to modernize the world."[7] What was new with modernization theory was the deliberate effort to anchor its analysis in science. At the same time, modernization theorists often took on an almost proselytizing tone that Nils Gilman attributed to the fact that some of its most important theorists were either

children of missionaries (Lucian Pye, David Apter) or other clerics (Talcott Parsons, Gabriel Almond).[8] With its emphasis on science and the related notion of progress, modernization theory represented an important point of departure from the more pessimistic theories of social change of the 1920s and 1930s that were often couched in terms of a cyclical process of improvement and decay.[9] With its emphasis on science, modernization theory follows the pattern we described in the discussion of deterrence/coercion theory in Chap. **3**.

Following the model of deterrence theory, modernization theory adopted the concept of "system" in its efforts to avoid the flaws of earlier scholarship that were viewed as rigidly formal. Thus, for example, the concept of the state was abandoned as too narrow and replaced with the broader, more comprehensive notion of political and social system because the broader term encompassed the activity of informal groups. Indeed, the modernization theorists proved so successful at banishing the notion of the state from the lexicon of political science, that by the 1980s, a new body of scholarship found it necessary to "bring the state back in."[10] In addition, political system was thought to be superior to the term political process because while the latter implied merely a relationship and interaction, the former encompassed a fuller notion of multidirectional interaction involving equilibrium and disequilibrium.[11] As such, the political system came to be viewed as a component of the social system, with the state reduced to epiphenomenon of the political system. And in its use of "system"—as we shall see—modernization theorists tended to rely on biological or mechanical analogies as the most appropriate way for understanding the dynamics behind social change. The success that modernization theorists had in distinguishing their scientific approach to social change from the earlier development literature is reflected in the fact that the term made its first appearance in *The Encyclopedia of the Social Sciences* in 1968.[12]

Whether intended or not, formulating a scientific approach to development served two purposes for modernization theorists. The first purpose was to provide a basis for seeking financial support. As we saw in the first chapter, part of this search for funding related to recognition and inclusion of the social sciences in the National Science Foundation. The second purpose for offering a scientific framework for development was to shape policy toward the "new states" by enhancing the prospects for successful social engineering in these countries. The very idea of a systematic public policy of "nation-building" had been ignored in Western political theory under the assumption that "states" and "nations" were somehow natural

phenomena that emerged more or less spontaneously.[13] Enhancing the prospects for social engineering had broad appeal to policy-makers because, as Walt Rostow observed, social scientists could aid in the formulation of an effective American policy in the former colonies much like the role physical sciences played in the arms race.[14] Moreover, confidence concerning the possibility of social engineering in the new states was undoubtedly strengthened by the experience with rebuilding Europe after the war. For during the life of the European Recovery Program (1947–1951), the aggregate GNP of Europe increased by 32 percent, so it seemed logical to think that a similar miracle might be created in the new states.[15]

To be sure, not all social scientists working with modernization theory were sanguine about the ability to apply the lessons of the Marshall Plan to the new states, but there was sufficient enthusiasm for social engineering to give "nation-building" in these countries a try. Thus, for example, when Paul Hoffman, who had headed the administration of the Marshall Plan, became the first president of the Ford Foundation, he turned the focus of that foundation to nation-building. Policy-makers like Hoffman as well as the scholars of modernization theory assumed, what Francis Sutton termed the classic postwar mentality, concerning development, "as a process willed and guided by governments that were committed to the advancement of their peoples and were to be assisted by the more advanced and affluent nations in un-intrusive and culturally neutral ways."[16]

The kind of projects envisioned in this classic mentality is exemplified by the comprehensive dam project constructed in Lashkar Gah, Afghanistan, and administered by the Helmand Valley Authority. This project, modeled as it was on the US Depression Era, experiment of the Tennessee Valley Authority (TVA), so that the city came to be known locally as the New York of Afghanistan. The city was described in 1960 by visiting historian Arnold J. Toynbee: "The domain of the Helmand Valley Authority has become a piece of America inserted into the Afghan landscape…the new world they are conjuring up out of the desert at the Helmand River's expense is to be an America-in-Asia."[17] At the time, some observers questioned the value of the project. For example, Peggy and Pierre Streit, in their description of the project in 1956, noted the extent to which the project was detrimental to the Afghan economy.[18] Certainly, in retrospect, such grand schemes for transforming less-developed countries seem naïve. Yet, at the time, modernization theory held great promise, for it seemed to perform three different tasks simultaneously. It provided a descriptive explanation for the emergence and

evolution of the West while it allowed for conclusions drawn from that experience to offer policy guidelines for nation-building. Finally, modernization theory might contribute to creation of a cumulative social science of change by integrating insights from classical sociological literature and thereby creating abstract theory that could serve as the basis for empirical research programs in comparative politics.[19]

In these tasks, modernization theory advanced under the auspices of three primary institutions. The first of these was Harvard's Department of Social Relations founded by Talcott Parsons in 1945, following the interdisciplinary model utilized by the OSS during the war. The department, which brought together the fields of sociology, social psychology and social anthropology into a single large department, came to embody Parsons' view of science as representing a progressive, rationalizing force shaping the society. As we have already noted, Parsons' conviction concerning the rationalizing influence of science led him to become a force for modeling the social sciences on the natural sciences. Many leading sociologists of modernization theory had some connection to Parsons' department, and these included people like Edward Shils and Alexander Inkeles. Under the auspices of the Department of Social Relations, Shils and Parsons produced their edited book, *Toward a General Theory of Action*, in 1951, which offered one early effort to lay the foundation for a grand sociological theory. Further, the department educated a new generation of scholars that included people like Marion Levy and Glifford Geertz. One of these students, Francis Sutton, who we quoted earlier in the chapter, joined the Ford Foundation and eventually became a vice president for economic development.[20]

The second institution that played a role in adding a scientific edge to modernization theory was, of course, the Social Science Research Council (SSRC). Under the leadership of Pendleton Herring, the SSRC embodied the belief that objectivity in the form of a scientific approach and public advocacy were mutually reinforcing tasks. Therefore, Herring asked Gabriel Almond to bring the "scientific" behavioral approach to comparative politics. Almond organized the council's Committee on Comparative Politics and convened a series of conferences, beginning with the first research planning conference in 1956 at Dobbs Ferry, New York, to outline methodological approaches for studying the new states. The committee, through the SSRC, sponsored a series of books from 1963 to 1966 under the general title, *Studies in Political Development*.[21]

While the SSRC attempted to combine grand abstraction with policy, the third institution associated with modernization theory, Massachusetts Institute of Technology's (MIT's) Center for International Studies, was the most explicitly concerned with shaping policy. Indeed, the Center's annual report in 1955 asserted that its research was planned from the standpoint of scholarly value and public policy.[22] We have already described the Center's origins elsewhere; here, we need to emphasize the extent to which Third World development became the Center's priority. As the Center's chronicler, Donald Blackmer, noted, that work began in 1954 concerned with shaping the US foreign aid policy. Some of this early work involved close engagement assisting with India's economic planning programs. By 1959, the Center's conceptual approach moved beyond a narrower focus of economic development to embrace the broader concept of modernization.[23] Walt Rostow, perhaps the most well known of the Center's alumni, subsequently said of this early development work that it represented "a kind of critical mass of somewhat overactive students and crusaders on the subject" who were responsible for elaborating "a reasonably coherent and defensible definition of the national interest which embraced development aid."[24] One of the Center's early opportunities to exercise its influence came in 1958 when the Senate Committee on Foreign Relations sought to undertake a review of trends and conditions in the world in the wake of Soviet scientific advances dramatized by the launch of the Sputnik. The MIT Center's contribution to the Senate review focused on transitions occurring in the less-developed countries. The report emphasized the need for a general framework—the kind of generalization that science could provide—to understand the core problems of the transition. The report also asserted that whatever the weaknesses of the social sciences, the government "will act better rather than worse" with their analysis.[25]

Before delving into our detailed discussion of the scholarly work associated with modernization theory, a few preliminary observations are in order. To begin with, both political and economic developments were subsumed under modernization theory. Moreover, theorists tended to use the terms political development and modernization interchangeably. Although both political and economic development were included in the theory, economists retained a clearer meaning and criteria for measuring economic development like the growth in GNP or the ratio of agricultural to industrial production. In contrast, the meaning and measure for political development remained more ambiguous. In fact, Lucian Pye identified

ten different meanings associated with the term "political development."[26] In the context of the early Cold War, however, policy-makers and theorists tended to define political development in terms of democracy, stability, anti-communism and pro-Americanism.[27] As might be expected, since modernization theory involved both a political and economic dimension, theorists had ongoing debates about the relationship between the two. Perhaps the most commonplace assertion regarding the relationship was that economic development would act as some sort of solvent to facilitate political development. Indeed, Seymour Martin Lipset noted the widespread generalization that the emergence of democracy was linked to the level of economic development. Even in those circumstances where state-directed economic activity seemed to weaken the foundation for political democracy, that state direction might be necessary for the economic progress required for the eventual emergence of democracy.[28] A contrary view and example was offered by Talcott Parsons who believed that because the Common Law of England privileged procedural regularity and equality before the law, it laid the foundation for the first Industrial Revolution and the accompanying economic development.[29]

Given the fact that both political and economic developments were included within modernization theory, the process of modernization was defined in various ways. These ran the gamut from Marion Levy's more narrow one that focused on changes in the inanimate sources of power and the nature of tools used to a broader notion contained in Cyril Black's definition as: " the transformation of political ideas and institutions that accompanies the economic and social changes flowing from the scientific and technological revolution." Reinhard Bendix's definition anchored modernization to the Western experience and the kinds of social changes that originated with the Industrial Revolution in England and the political revolution in France. Focusing on modernization as a non-economic process, David Apter's definition asserted that the process originates "when a culture embodies an attitude of inquiry and questioning about how men make choices—moral (or normative), social (or structural) and personal (or behavioral)…to be modern means to see life as alternatives, preferences, and choices." A retrospective definition of modernization—and one that illustrates the multidimensional nature of the process—was supplied by Manning Nash as "the growth in capacity to apply tested knowledge to all branches of production; modernity is the social, cultural and psychological framework that facilitates the application of science to the processes of production."[30]

What the varied definitions of modernization imply is the interdependence of many variables in social change that suggested a need for interdisciplinary analysis. Moreover, the extent to which social transformation is thought to derive from the Western experience suggested that changes might tend to follow a single trajectory. If so, modernization theory contained within it an implicit teleology. Although not all modernization theorists worked on such an assumption, it was pronounced in the work of some. For example, Edward Shils assumption was expressed in a speech in 1959 when he observed: "In the new states 'modern' means democratic and egalitarian, scientific, economically advanced…'Modern' means being Western without the onus of following the West. It is the model of the West detached in some way from its geographical origins and locus." In the audience listening to Shils were scholars who built their reputations as part of the modernization school—Gabriel Almond, Lucian Pye, David Apter, Cyril Black, Karl Deutsch and Daniel Lerner.[31]

Beyond the variation in definitions and the differences concerning the degree of teleology within the theory, modernization theorist also varied in terms of the extent that they saw authoritarian means as necessary for forging modern societies. Some theorists were convinced that a revolutionary change in remaking men's identities was necessary, while others were more reformist to the extent they saw modern societies as gradually emerging out of the traditional ones.[32] At the same time, modernization theory harbored within it a tension between area studies specialists and social scientists of the behavioral school. The former group retained a preference for detailed, historical studies characterized by Clifford Geertz as "thick description,"[33] while the latter strove for abstract conceptualizations and methods for measurement that might facilitate cross-country comparison. James Coleman and C.R.D. Halisi neatly summarize the differences in the two groups:

> The images held then, and still dominant among a surprising number of non-area American political scientists, was that they [area studies specialists]were parochial descriptivists and relativists unengaged in the pursuit of scientific universality, whereas the image of behavioralists harbored in the area studies camp was that they ignored the critical significance of unique cultural and historical contexts, proclaimed as universal their own culture-bound generalizations, and naively sought a chimerical universalism.[34]

Given the differences among modernization theorists, their work more closely resembled what Imre Lakatos called a research program rather than a Kuhnian paradigm.

Yet despite differences in approaches and emphasis, the modernization theorists did share some things in common. In a very general way, scholars in this school saw the importance of social structures and values as well as social integration regardless of whether they were more conservative or liberal minded.[35] More specifically, the modernization literature tended to include the view that political and social changes are interdependent which demanded a "systems" framework. Thus, there was a compelling logic to their analysis that assumed a unilinear direction to change, and such compelling logic provided by the assumption easily seeped into the intellectual frameworks of policy-makers. Therefore, the virtue of modernization theory was that it had the potential to offer a genuinely "grand theory" of a comprehensive model of social change that integrated social, political and economic elements. As Michael Latham has pointed out, because it related specific structures with particular functions, it offered a method by which social scientists might compare different societies across time and space so that: "as a universal process it also made the complex variations of particular cultures appear far less important than the common factors believed to unite them."[36] At the core of this model of social change, which stood as the very essence of social theory in the 1950s and 1960s, was the notion of social systems with differentiated and functioning parts and that the transformation in the new states could be understood as a move from undifferentiated traditional social systems to complex, differentiated modern ones.[37]

The best place to begin a more thorough analysis of modernization theory is with the concept that lies at its heart: the distinction between traditional and modern societies. This dichotomy of traditional and modern societies has a distinguished pedigree extending back to the nineteenth century. Aspects of the dichotomy can be found in Henry Maine's 1861 distinction between status and contract and in Ferdinand Tönnies' 1887 distinction between Gemeinschaft and Gesellschaft. The dichotomy also permeates Max Weber's discussion of traditional and rational sources of authority. These early sociologists created such ahistorical categories for social relationships as a way to overcome a more deterministic philosophy of history that limited the ability of sociologists to make cross-country comparisons.[38] In addition, within this classical tradition of sociology lay a sense of evolutionary change moving in the direction of more complex social and organizational structures driven by improvements in technology.[39]

The characteristics of each kind of society are almost self-evident given the definitions of modernization quoted earlier. Traditional societies were those with hereditary, hierarchical rule based on custom where the value of an individual rested on his personal kinship ties. Such a social structure was inherently unequal because roles and status were inherited characteristics. Traditional societies were also backward-looking in the sense that behavior was guided by past practices and contained no small amount of fatalism. That fatalism embraced a static world that allowed for minimal economic growth. Modern societies, in contrast, were characterized by the opposite kinds of traits. They were governed by the rule of law that created the basis for a more equitable social structure. Movement within that social structure was possible because individuals were judged more by their ability to perform certain functions in roles that were segregated from their kinship ties. Perhaps most importantly, modern societies were forward-looking with the confidence and belief in innovation that welcomed change. Consequently, modern societies produced a steady stream of technical inventions that contributed to sustained economic growth. In short, modern societies were contrasted with traditional ones by the fact that they celebrated rationality, empiricism, efficiency and progressive change. Furthermore, not only did modern societies value these elements, but embedded them in institutions in a way to insulate them from attack by alternative values.[40]

Of course, the difference between traditional and modern societies was not really as clear-cut and self-evident. This fact was recognized from the outset by the SSRC's Committee on Comparative Politics that noted in its report from the 1960 Dobbs Ferry conference that "determining the *respects* (italics in the original) in which any society or its political system is modern or traditional is a central problem in analysis."[41] Marion Levy, too, noted the fact that it was difficult to determine the cut-off point for distinguishing between the two types of societies, and goes on to suggest that the difference between traditional and modern societies was merely a difference in degree. Gabriel Almond and G. Bingham Powell echoed this point and noted that most political systems contained a mix of modern and traditional elements.[42] Furthermore, the simplified dichotomy was problematic for analysts because it provided no guidelines concerning which aspects of tradition facilitated or retarded the modernization process, a problem that—as we shall see—David Apter attempted to correct.[43] Despite the difficulties with the tradition/modern distinction and

recognizing Max Weber's warning of the danger that conceptual ideal types may become confused with reality, scholars concerned with social change cannot easily dispense with the distinction because, as Reinhard Bendix points out, scholars need to refer to some "before and after" model of the social structures being examined.[44] To be sure, modernization theorists did develop different terminology for the classic distinction of traditional and modern, so, as one example, Francis Sutton preferred to distinguish societies as agricultural or industrial.[45]

Talcott Parsons' ideas provide an important starting point for a scientific study of society contributing a vocabulary that was foundational for modernization theorists. Talcott Parsons reinvigorated the ideas of classical sociology and, in the introduction to his 1951 book, *The Social System,* acknowledged his debt to "the great founders of social science," Vilfredo Pareto, Emile Durkheim and Max Weber.[46] From this classical base, Parsons provided inspiration for much of the subsequent scholarship in comparative politics associated with modernization theory. Parsons' ambition was to create a general theory of action that contained three theoretical subsystems: a theory of social system, a theory of personality and a theory of culture. Of these three subsystems Parsons noted, "This fundamental relationship between need-dispositions of the personality, role-expectations of the social system and the internalized-institutionalized value pattern of culture, is the fundamental nodal point of the organization of systems of action."[47] So, from the outset, Parsons sought an interdisciplinary approach for studying society, and he continually stressed the inextricable link between personality and motivation and the social structure, that is, the link between sociology, psychology and anthropology. And as we saw earlier, Parsons founded the Department of Social Relations at Harvard in order to house interdisciplinary scholarship.

This merging of social science disciplines might be considered as laying a basis for a consensus on a paradigm. Certainly, Seymour Martin Lipset hinted as much when he observed that the merger of political science and sociology may "establish a common endeavor that accepts the basic premise of a general social science, asserting the primary concerns of both disciplines to understand and account for human behavior in terms of theory relevant to every society."[48]

Parsons' analysis, interweaving these disciplines as it did, was renowned for a dense prose and a high level of abstraction. Both traits are evident in Parsons' definition of social system:

> [A] social system consists in a plurality of individual actors interacting with each other in a situation which has at least a physical or environmental aspect, actors who are motivated in terms of a tendency to the "optimization of gratification" and whose relations to their situation, including each other, is defined and mediated in terms of a system of culturally structured and shared symbols.[49]

Adding to the difficulty of Parsons' analysis is the fact that, at times, he provided definitions of terms that were not the conventional ones. Thus, for example, his definition of "institutions" is not a reference to concrete organizations; rather, he defined institutions as "generalized patterns of norms which define *categories* (italics in the original) of prescribed, permitted and prohibited behavior in social relationships." For Parsons, then, individuals might be a member of a collectivity, but they could not be a member of an institution in his sense.[50]

Despite its high level of abstraction, Parsonian theory was intended to serve two practical purposes. The first purpose was a narrow disciplinary one that would establish sociology's reputation as an authentic form of social scientific inquiry. His concern here was prompted by the fact that within the inner circle of Harvard, sociology was not taken very seriously as a valid way to study society. After all, the sociology department there was a relative newcomer, having only been formed in 1930, and could, therefore, hardly compete for status against the more prestigious Department of Economics.[51] Parsons' focus on psychology and motivation provided a challenge to economic theory because it demonstrated the inadequacy of a theory that gave prominence to a "rational instrumental goal orientation" that Parsons believed was applicable only within a narrow range of specialized circumstances.[52] Parsons' concern with the reputation of sociology may well account for his giving precedence to social conditions in terms of England's Common Law as the ultimate source for the economic changes embodied in the Industrial Revolution. In fact, Parsons pointed out the uniqueness of this law in the West and noted that neither Islamic, nor Chinese nor Hindu legal systems had anything comparable to it. English Common Law, Parsons argued, had institutionalized a pattern of rights and obligations that applied to all, thereby cutting across the lines drawn by "traditional" bases of social solidarity.[53]

Of course, one path for enhancing the prestige of the social sciences was to stress its reputation as a science. Parsons organized his analysis around "systems" in part, because he saw the concept as important for any

scientific theory. Moreover, Parsons noted that all scientific theory is concerned with analysis of uniformity in empirical processes, and he believed social systems to be amenable to identifying uniformity. It goes without saying that Parsons believed that scientific advance required both abstraction and generalization, and both characteristics are evident in his work.[54] Given Parsons' commitment to science, he was the obvious choice in 1946 for writing on behalf of the SSRC for inclusion of the social sciences in the National Science Foundation.[55] Although Stephen Toulmin, writing in retrospect, characterized Parson's science as flawed because it was ahistorical and assumes the social structure is a coherent whole much like a counterpart in the philosophy of science that assumes incorrectly, that science is a coherent, logical system.[56]

Parsons' second purpose and one equally pragmatic related to his concern with the Hobbesian problem of social order and finding a solution that did not require creation of a Leviathan. For given the world as posited in the utilitarian tradition where individuals were assumed to be relentlessly pursuing their own self-interest, the emergence of social order appeared especially problematic. Parsons' concern with social order was sharpened by his study of the classical sociologists, particularly the work of Emile Durkheim. Durkheim's concept of "anomie" that suggested normless conduct driven by a release of appetites and interests portended the inevitability of social breakdown.[57] Parsons was adamant that the solution to "anomie" and social breakdown could never, over the long term, rely on force, and he observed:

> A relatively established "politically organized community" is clearly a "moral community" to some degree, its members sharing common norms, values and culture—which is to say that I start with a view that repudiates the idea that any political system that rests entirely on self-interest, force or a combination of them, can be stable over any considerable period of time.[58]

Against the prospect of breakdown, science with social science included represented the central rationalizing force to avert it. Parsons observed that "it is impossible to draw any rigid line between science as the pursuit of knowledge as such and its practical applications to the rational management of human interests and affairs."[59] What is more, the rise of National Socialism in Germany during the 1930s offered proof that social order was indeed fragile and society always vulnerable to backsliding into more primitive social relations. Parsons' pointed out that under National Socialism,

Germany replaced "rational knowledge and technical competence" with membership in the "mystical body" of the German people that emphasized racial particularism and loyalty to the Fuhrer, thereby substituting a traditional order for a rational-legal one.[60] Indeed, Nazi Propaganda Minister Joseph Goebbels recorded in his diary that he listened to his mother because "she knows the sentiments of the people better than most experts who judge from the ivory tower of scientific inquiry, as in her case the voice of the people itself speaks."[61]

Parsons' concern with social order led him to develop frameworks for understanding processes related to stability and change. One way to analyze social stability was to conceive of all societies as functioning systems where individual motivation (or value orientation) could be correlated with different social structures. Here, psychology and sociology were brought to bear, and individual actions "were mediated by a set of regulating values transmitted through the institutions that ensured social order."[62] For Parsons, a disconnect between value orientation and social structures would create a strain that would likely prompt a reequilibrating process. Because of his concern with social stability and equilibrium, Parsonian analysis is often criticized as having a conservative political bias. But as Gabriel Almond pointed out, Parsons' assertion that social systems tend toward equilibrium was meant to convey that social systems, whatever their particular character, tend to preserve that character and only change slowly.[63] Furthermore, while true that Parsons was concerned with a stable equilibrium of the interactive process between motivation of the individual and the structure of the social system, his intent was to provide a theoretical point of reference only. He pointed out, therefore, that in reality, "no social system is perfectly equilibrated and integrated," a point he underscored in an essay on McCarthyism titled, "Social Strains in America." In that essay, he suggested that changes in the structure of American society growing from the expansion of political responsibility both internally and externally generated strains culminating in McCarthyism.[64]

If a conservative tone is to be found in his discussion of what he saw as a moving equilibrium, it lies in his assertion that if a social breakdown occurs and leads to the emergence of an alienated, revolutionary movement, their ascendance will necessarily lead to the reestablishment of some equilibrium involving some change, but not as much as their ideology might suggest.[65] For Parsons, the Soviet Union's abandonment of utopian aspects of their ideology under the pressure of running the country illustrated this reality. Indeed, Alex Inkeles offered validation of Parsons'

view in a study on social stratification in the USSR. In that study, Inkeles detailed the inherent social differentiation in modern industry that required different reward and status structures contrary to the ideological claim of creating a classless society.[66] Parsons concluded that the very process of industrialization implied "a kind of 'individualism' which it will be exceedingly difficult to reconcile with the present character [Stalinism] of the regime."[67]

Because social structures were linked to specific functions, the Parsonian framework came to be known as "structural-functionalism." Parsons asserted that the crucial characteristic of structural-functional theory lay in its use of the concept of system. For the social scientist, more important than identifying any original impetus to change was tracing its repercussions, and for Parsons, the value of the concept of system lay in its ability to assist with this task. Parsons believed that applying the concept of system corrected the flaw of earlier theories of change that "almost uniformly committed the error of postulating the continuance of a trend without taking account of the interdependence of the factors involved in the trend with the other variables in the social system." Although the concept of social system might draw on a biological analogy, there were limits to the comparison because while biological systems all experience a typical life cycle, there was, for Parsons, no overall series or phases experienced by all social systems. Hence, one could not expect a single, linear pattern of change. The closest that Parsons came to assuming a linear direction to change was in the case of the belief system related to science that drove the "rationalization" process as formulated by Max Weber. What is more, Parsons recognized, in answer to critics concerned with the static nature of his analysis, limits on his ability to outline a general theory of change because knowledge concerning the laws of process with the social system was lacking. At best then, the theory of change in the structure of the social system "must therefore, be a theory of particular sub-processes of change within such systems, not of the overall processes of change of the system as system."[68]

Although Parsons did not view change as necessarily occurring in any particular sequence, there remained one element of teleological reasoning in his framework related to the universality of functions in social systems. For Parsons, all societies have four essential functional needs. These are pattern maintenance or socialization, adaptation to the environment, goal attainment and integration or social control. While the teleology inherent in positing necessary functions might not be consciously purposive, by

suggesting a "logic of functionalism," Parsons introduced an end or a purpose within a social system. This teleology is readily admitted by Parsons, and he noted as much, saying: "A process or set of conditions either 'contributes' to the maintenance (or development) of the system or it is 'dysfunctional' in that it detracts from the integration and effectiveness of the system."[69] Despite the teleological reasoning, Parsons' four universal functional needs can be viewed as providing a useful "filing system" for ordering empirical data. In this way, cross-society comparisons can be made according to the resources and attention devoted to any one of the functions. From this, one can then characterize modern industrial countries as oriented more toward the adaptive function, while traditional societies tend to emphasize pattern maintenance.[70]

Parsons elaborated on the categories traditional and modern as part of his reinvigoration of classical sociology because he saw the dichotomy as too simplistic. Therefore, he took the two categories as a point of departure and elaborated on them with his scheme known as the "pattern variables" or "dilemmas of orientation." Originated as a way to formulate a theoretical interpretation of the role definition within the professions,[71] these pattern variables suggested polar alternatives of possible orientation that correspond to the traditional/modern dichotomy. Parsons listed the pattern variable framework as composed of universalism/particularism, achievement/ascription, self-orientation/collective orientation, specificity/diffuseness and affectivity/affective neutrality. These pattern variables provided a starting point for classifying types of societies, with societies tending to cluster around one set of variables or the other. Applying these to the traditional/modern distinction then means that traditional societies will be dominated by occupational structures with ascriptive roles and particularistic elements engaged in diffused functions. Modern societies with their industrial occupational structure will have roles based on achievement and universalistic elements engaged in specifically defined functions. For Parsons, the important aspect of occupational roles in a modern society is that these be segregated from the kinship system, which then allows for the possibility of changing status and upward mobility. Parsons concluded from this framework that a society can have a predominant kinship (particularistic) system or a highly industrialized economy, but it cannot have both in the same society.[72] Further, Parsons attributed the Western cultural tradition of universalism as acting as a bulwark for science that lay at the heart of the Weberian process of rationalization.

Parsons made the evolutionary notion lurking behind his pattern variables explicit in an article for the *American Sociological Review* in 1964. There he outlined an idea of "evolutionary universal," which he saw as an organizational development that was sufficiently important to further social evolution that it would recur even under different conditions. Here, Parsons drew explicitly on a biological analogy to note that such an evolutionary universal reflected not just a passive adjustment to the environment, but rather the capacity of a living system to cope with its environment. He went on to recognize that some evolutionary universals provide their societies with major advantages, and he identified two that are essential for moving societies away from primitive to modern arrangements. These are the development of a well-marked system of social stratification and a system of explicit cultural legitimation of differentiated functions that are independent of kinship.[73]

Parsonian theory has been praised as "one of the great intellectual feats of that generation," and Robert Bellah's tribute to Parsons characterized his work as an exemplar fitting Wallace Stevens' aphorism: "The world is the world through its theorists. Their function is to conceive of the whole and, from the center of their immense Perspectives, to tell us about it." Not all appraisals were so complimentary, and others suggest that in Parsons' aim for a grand theory, his reach may have exceeded his grasp.[74] Nevertheless, three important implications derive from Parsonian analysis. First, it established the importance of viewing societies as integrated systems where change in any one element, political, social or economic, would bring changes to the others. Second, by developing functions that appeared universal, they allowed for comparative analysis that could be divorced from normative or ideological considerations. Finally, with the use of his pattern variables, one had a tool to measure and trace social change, particularly the transition from traditional to modern society.[75] Perhaps more importantly for our purposes, Parsonian analysis provided a basis for "uniting the particularistic studies being made in Area Studies programs into a single, coordinated research and policy agenda."[76] Parsons' work, then, casts a shadow over scholarship concerning the new states, so it is appropriate that we now turn our discussion to those who took up his work as a point of departure and who clarified Parsonian analysis to make his work more intelligible for a wider audience and apply it to the policy problems of transitional societies or the new states.

The sheer volume of literature relating to modernization theory—covering as it does works of general theory and concrete case studies—seems to demand some kind of taxonomy of approaches as a way to organize any discussion of

scholars contributing to the theory. For our purposes, therefore, it is useful to classify theorists under one of four possible categories that suggest alternative, competing paradigms. These four approaches can be labeled as follows: a structural-functional approach, a political culture approach, a process or administrative approach and an economic approach.[77] Admittedly, the distinctions among these approaches are inexact, and as we shall see, various scholars might legitimately be placed under one or more headings. In particular, the line between structural-functional and political culture approaches is blurred, as will become apparent in the discussion of Gabriel Almond's work. Furthermore, given the extent of the literature, the following discussion of various scholars does not claim to be exhaustive but rather intends to provide a good representative sample of the variety in the work associated with modernization theory.

Our first category, a structural-functional approach, is perhaps the one most closely derived from Parsonian analysis. It was an approach, moreover, that emphasized a research focus divorced from any particular culture. Thus, as an SSRC report observed, the approach "requires searching for ways in which particular functions—such as the articulation and aggregation of interests—are performed without any predetermined conclusions concerning the structures or institutions that are involved in performance of the functions."[78] The work of three important modernization scholars can be placed neatly under this category of approach. The three are Marion Levy, David Apter and Gabriel Almond, who all made conceptual contributions to modernization theory.

We begin with the work of Marion Levy because he was a student of Talcott Parsons. Indeed, he dedicated his 1966 book, *Modernization and the Structure of Societies*, to Parsons. During World War II, Levy served as a Japanese language specialist, and he drew on his knowledge of Japan and China to support his theoretical notions. Levy shared with Parsons the desire to make sociology more scientific, and as we observed earlier, with his definition of modernization that centered on inanimate sources of power and tools used, it was logical that his science would assert that industrialism necessarily imposed a certain engineering criteria of efficiency on society. Levy justified choosing his narrow definition of modernization because it was parsimonious, and in scientific contexts, definitions "are not intended to approximate detailed descriptions."[79] Following Parsons, Levy conceived of government, like any organization, as a "system of action." He also made a distinction between modern and non-modern societies, although he saw the distinction as less clear-cut,

involving a matter of degree. Levy did believe, however, that societies in each category had more in common with other countries in the same category than they did with countries in the other one. What is more, Levy believed that the social change engendered in the modernization process was qualitatively different from any earlier kinds of change. He said:

> I believe that there is something peculiar, something "new under the sun," about the structures of relatively modernized societies. This new factor hinges on the fact that the structures of modernization, once they have reached certain levels of development, constitute a sort of universal social solvent.[80]

The view that modernized societies were distinctive from everything that had gone before was bound to be appealing to policy-makers bent on creating a new world out of the ashes of World War II.

Given Levy's connection to Talcott Parsons, it is not surprising that he drew on Parsons' pattern variables as a way for analyzing what Levy termed "relatively modern societies." Thus, Levy examined a list of six pairs of polar opposites—reminiscent of the pattern variables—that could be applied to analyzing relationships in any society. Of these pairs, two were especially important to modernization theory because they provide a consistent theme within it. These two pairs are rational-traditional and universalistic-particularistic.[81] From Levy's point of view, an increased rationality in the form of scientific thinking provides the necessary precondition for adoption of increased use of inanimate sources of power and the use of increasingly efficient tools. Once these are developed, the necessity for organizations to select people on the basis of universal criteria—their ability to perform a task—rather than the particularistic criteria of kinship ties becomes apparent. The imperative for performance criteria, in turn, provides opportunity for individuals to move within a social structure based on achieved characteristics rather than ascribed ones. Establishing, as he does, the link between technical change and social structures and functions carries with it a certain level of technological determinism. Levy says, as modernization increases, it establishes a trend that leads "overwhelmingly and irreversibly" toward more centralized social structures. In short, for Levy, the structural characteristics of relatively modern societies reflect an increasing emphasis on rationality, universalism, functional specificity and emotional neutrality.[82]

The second modernization scholar that fits under the structural-functional approach is David Apter. Apter's disciplinary expertise straddled the cusp between sociology and political science, although he asserted his preference for political science because he believed it had a greater concern with balancing moral and technological considerations.[83] Indeed, Apter's inclusion of moral considerations takes on a proselytizing tone when he describes modernization as "a special kind of hope. Embodied within it are all the past revolutions of history and all supreme human desires."[84] Furthermore, Apter has been characterized as a "tireless field worker" because of his extensive interviews of cross-sections of people in the new states. Such work with the developing countries led him to serve from 1961–1962 as director of the Peace Corps' first training program for volunteers going to Ghana.[85] David Apter's affiliation with the modernization school was demonstrated by his participation in the Dobbs Ferry Conference on Comparative Politics. Moreover, in the paper presented there, he too acknowledged his intellectual debt to Talcott Parsons.[86]

Consistent with the views of other scholars, Apter recognized one legacy of the OSS model from World War II, and he described the social science literature as "problem clustered" so that reading through the literature demanded interdisciplinary awareness or "cutting through a variety of disciplines." He characterized science as possessing norms of empiricism, predictability and rationality as guides for conduct. Furthermore, he saw the social sciences as increasingly accepted as scientific so that scientific norms could be used as guides for social conduct. Thus, science played a key role for modernization in the West, and Apter noted: "Indeed, we have come to consider science as the antidote for faith, with Galileo as a kind of folk hero of modernization. His triumph is the triumph of reason, and reason as applied to human affairs is the foundation of modernity."[87]

Like other scholars using the structural-functional methodology, Apter was committed to the belief that such an approach was useful to the extent that it facilitated comparison. Moreover, the approach opened up the possibility of moving beyond mere description to stimulate new ideas and test propositions. Despite his commitment to the approach, he was well aware of its limits, and it is worth quoting him at length on this point:

> [I]t remains true that systems analysis of the structural variety presents many problems. It is, on the whole, a tiresome method of working. It remains excessively pompous. There is a sort of neo-Hegelianism about it, stemming

> as much from the jargon with which it is associated as any analytical analogy. Moreover, formidable problems of operationalization seriously curtail its immediate usefulness.[88]

As we shall see next, Apter's own work proved problematic in application.

Despite the aforementioned reservation concerning the approach, Apter's work did attempt to make his own unique contributions for understanding the process of modernization. Perhaps his first notable contribution was contained in his 1959 paper presented at the Dobbs Ferry Conference subsequently published in his compilation of essays titled: *Some Conceptual Approaches to the Study of Modernization*. In that paper, he recognized the fact that the simple tradition/modern dichotomy provided no way to identify aspects of tradition that facilitate modernization and those aspects that inhibit it. Therefore, Apter offered a refinement of the concept of traditionalism, classifying it as either instrumental or consummatory, with the former more conducive to modernization and the latter less so. Apter defined instrumental traditionalism as a system having a large group of intermediate or pragmatic ends that were quite independent of transcendental ones. Consummatory traditionalism, in contrast, intertwined intermediate ends with transcendental ones. In making this distinction concerning types of traditionalism, Apter challenged the notion commonly assumed by scholars that traditional societies are more alike than modern ones.[89]

Each kind of traditionalism is associated with its own structural tendencies and with its own implications for the process of modernization. In particular, each kind of traditionalism shaped the problems that leaders faced as they sought to transform their societies into modern ones. Thus, instrumental traditionalism, with its hierarchical structure, could expect the early stage of modernization to be relatively easy but would encounter difficulties when, at a later stage, the hierarchy itself might be challenged. Consummatory traditionalism, on the other hand, with what Apter described as a pyramidal pattern of authority that granted autonomy to lower levels, faced obstacles in imposing even the initial aspects of modernization. Apter saw a military-type system as the structural expression of instrumental traditionalism that places heavy reliance on performance criteria, with religion serving as a secondary value. From the standpoint of modernization, instrumental traditionalism makes innovation acceptable by cloaking change in tradition. Consequently, alterations in the social institutions do not appear to be deviations from the past. For Apter, the

traditionalism of the consummatory system is such that it is hostile to innovation because it embodies an elaborate cognitive style where religion with its ultimate, transcendental ends is pervasive.[90]

A second contribution Apter attempted was to integrate the more general, abstract theorizing into the more concrete empirical world of area studies. In part, his inspiration for doing so derives from an insight of Richard von Mises, the Austrian-American mathematician who advanced probability theory. Apter quotes his insight: "It is always the search for and the exposition of typical and recurring elements *within the unique course of the world* (italics in the original) that is the subject of science."[91] Indicative of Apter's efforts are the essays he compiled in 1968 that drew on empirical work and dealt with concrete systems that range from entire political systems to subgroups within them such as political parties and bureaucracies. At the more abstract level of theorizing, the essays delineate three distinct analytical levels: structural, behavioral and normative. The interdisciplinary flavor of Apter's three analytical levels is apparent in his description of them. The structural level includes the sociological and political and institutional constraints that place limits on the choices that individuals make. The behavioral level is fundamentally psychological because it is concerned with which choices are made and why. Hence, the behavioral level is necessarily concerned with motivation and moral aspects that shape choice. Finally, the normative level that constitutes part of moral considerations is the level that distinguishes the social sciences from the natural sciences. Thus, although Apter saw the social sciences as scientific in method and technique—their uniqueness lay with the fact they must consider the meaning of social acts, and in this manner Apter was able to reconcile the conflicted identity—the scientific and humanistic strands forming two sides of the same coin.[92]

The third aspect of Apter's contribution to modernization theory involves his effort to develop new terminology. Like other modernization theorists, part of this terminology aims to displace concepts from the earlier legalistic and formal political science. Thus, for example, the term "state" is downplayed in favor of political system or government. In Apter's view, the term government is very generic and can apply to society at large or to other groupings like trade unions or churches. For Apter, the salient characteristics of government are two: it must have defined responsibilities for the maintenance of the system, and it must also have a monopoly over the coercive powers for the system it governs.

More importantly, Apter outlined the traits of three different types of development systems, each with different implications for the process of modernization. His three types are mobilization systems, reconciliation systems and modernizing autocracies. It is apparent from Apter's elaboration of mobilization system and reconciliation system that these terms are substitutes for what might alternatively be labeled totalitarian systems or pluralist systems, respectively. The linkage to this alternative terminology is most apparent by the fact that Apter cites the People's Republic of China as an example of mobilization system, and India as an example of a reconciliation system.[93] Apter's third developmental system of "modernizing autocracy" can be viewed as a stand-in for authoritarian system which, if placed along a continuum with the other types, positions it midway between totalitarian and pluralist. For Apter, each type of system holds different implications for modernization, both in terms of the strategy they adopt to achieve it and in terms of outcomes. Reconciliation systems are likely to rely on localized initiatives and individual entrepreneurship as the means for achieving modernization and are therefore least likely to result in the establishment of a brand new modernizing polity. In contrast, mobilization systems use centralized planning and government enterprises to achieve their aims and are likely to succeed in establishing a new polity or moving toward more advanced modernization.[94] In addition, there is an uncertain relationship between a mobilization system and reconciliation system, and Apter raises the question of whether the former can ever be transformed into the latter. One factor that he saw as working in favor of such transformation was the extent to which a mobilization system is successful in promoting economic development, thereby generating conditions conducive to transformation into a reconciliation system. In this, Apter's view is consistent with the conventional wisdom that economic development is the necessary precondition for pluralist political systems.

On one level, Apter's terminological innovation contributed to a vocabulary that is perhaps more neutral and less emotionally laden than labels like totalitarian or pluralism. Yet, such verbal innovation might not advance knowledge, for, as Sidney Verba recognized, the "unfortunate tendency in the social sciences to oversee new concepts and to assume that the mere labeling of an old phenomenon with a new term represents a breakthrough in our understanding."[95] Stephen Toulmin also points out that a change in scientific ideas only occurs when innovations take root and do not die out with their creator.[96] In the end, Apter's terminology and elaborate typologies are never widely adopted and remained confined

to the work of their originator. At the same time, Apter's elaboration of his typologies and categories prove cumbersome to apply, in part because he creates numerous variations with his main categories. Thus, he distinguishes modernizing autocracy, military oligarchy and neo-mercantilist societies as all lying on the continuum between mobilization and reconciliation systems. Of these variations, Apter admits that "These three types are confusing because of similarity in their basic components."[97]

Like many of the social scientists who were predominant theorists in the 1950s and 1960s, Gabriel Almond was a graduate of the World War II Office of War Information where he analyzed enemy propaganda. After the war, he was a participant in the US Strategic Bombing Survey. Almond came away from this experience—like so many of his colleagues—with a keen appreciation of the role for social science in public international policy and a recognition of the growing importance of the new states. This appreciation is captured in his comment:

> Our foreign policy must be based on a clearly understood conception of the interdependence of economic, social, political and cultural factors in the processes of social change. Our diplomatic, military, propaganda, and foreign aid programs must operate the interdependent levers of change with virtuosity. Without this kind of social-science thinking we will be unable to affect the course of change in the non-Western world in the direction favorable to the preservation and spread of our own culture.[98]

Like other scholars coming out of their experience in World War II, Almond's work reflected an optimistic faith in the scientific method favoring an interdisciplinary approach that could lay the foundation for social engineering in the new states.

For our purposes, Almond's work is important for at least two reasons. First, his contributions can be included in two of our categories of approaches we listed earlier. Thus, Almond's work stands as a sort of bridge between the structural-functional methodology and the political culture approach. For Almond asserted in 1966, that learning about a system's structure and culture allowed scholars to explain its performance and to make predictions.[99] As such, Almond's work can be thought of, as we suggested Thomas Schelling's work on deterrence could be thought of, as lying in the center of a wheel with spokes radiating outward to influence much of the comparative politics literature in the 1950s and 1960s. The book he edited with James Coleman, *The Politics of the Developing*

Areas, more closely illustrates a structural-functional framework, and it is worth noting that his ideas seeped into policy because the book was required reading at the Foreign Service Institute in the mid-1960s.[100] Almond's 1963 study with Sidney Verba, *The Civic Culture*, is more illustrative of the political culture approach. Second, as chair of the SSRC's Committee on Comparative Politics from 1954 to 1963, Almond was central in framing the methodological tools for studying the new states. At that committee's first research planning seminar in 1956, he stressed the importance of using function as an analytical category valuable for making cross-country comparisons. By stressing functions, Almond believed researchers would be able to answer important questions concerning political systems, like identifying the manner that articulation of interest takes place and tracing their transmission to the political system in a way that translated into public policy.[101]

As might be expected of one who had been a student of Charles Merriam, and consistent with other modernization scholars, Almond was committed to the notion and development of political science as a science. Indeed, he justified shifting the vocabulary of political science away from formal/legalistic expression on this basis. Thus, like Apter and others, he replaced "state" with "political system." He also replaced the term institutions with "structure," and "offices" with "roles." Almond hoped that using this new vocabulary might ultimately aid scholars in formulating a statistical or mathematical model of politics.[102] Part of a more statistical approach to political systems involved measuring aspects of political performance. For example, Almond noted that one could measure a political system's ability to perform regulatory tasks by the number and kinds of actions regulated, the severity of the rules and the procedural limits on the systems' regulative actions. Similarly, one could measure the extractive performance of a political system by the extent to which it was able to extract resources from society. Success in these and other measurements might then enable scholars to compute scores for abstractions, like "justice" or "welfare" laying the basis for what Almond called a new discipline of "polimetrics." In this way, tensions growing from the conflicted identity in social science might be eliminated. Hence, such measurement might offer a way to bridge the gap between classical normative political theory and empirical theory.[103]

In addition, the so-called behavioral approach to political science was part of the effort at greater conceptual precision because it involved "the study of actual behavior of incumbents in political roles, rather than the

content of legal rules or ideological patterns."[104] What is more, this more scientific framework would not only benefit the discipline, but it would also contribute to policy, particularly policy related to engineering change in the new states. Here is how Almond expressed the policy purpose:

> We are confronted here with the ultimate question of the Enlightenment. Can man employ reason to understand, shape, and develop his own institutions, particularly those concerned with power and coercion, to plan political development with the least human cost and with bearable risks? Can we find solutions to the state-building and nation-building problems of the developing areas which will not indefinitely prejudice or postpone the effective confrontation of their problems of participation and welfare? The modern political scientist can no longer afford to be the disillusioned child of the Enlightenment, but must now become its sober trustee.[105]

Almond outlined his framework by separating out the political system that performed the Parsonian functions of adaptation and integration from the broader social system, and defined the political system as "the legitimate, order maintaining or transforming system in the society." Like other scholars who relied on the concept "system," Almond saw the two important attributes of system as the interdependence of parts and the existence of boundaries providing the point where the political system ended and the environment began.[106] Almond recognized that the concept of system derived as it was from biology and mechanics had some limitation in the context of social systems because the interdependence of parts was not as great and boundaries between the system and the environment were not as clearly marked.[107] From the concept of political system, Almond then noted that *all* political systems have in common both structures and functions, so that the most fruitful way for comparing transitional societies with the West was to identify the structures that performed key functions. For Almond, one virtue of organizing analysis of political systems around functions is that doing so provided a way to avoid norms and ideological definitions, thus allowing for that "affective neutrality" so important to the rationalization of society and central for a scientific understanding of change.[108]

Almond began his discussion of functions performed in the political system by dividing them into two broad categories of input and output functions. From there, he described functions that were much more specific than the four general functions outlined by Parsons. Almond's four

well-known input functions are interest articulation, interest aggregation, communication and recruitment (or socialization).[109] His output functions were three, and these included the authoritative government functions of rule-making, rule application and rule adjudication. In the case of the output governmental functions, Almond deliberately avoided using terms like legislation or administration because he thought transitional societies were likely to perform these functions with different structures than in the West. Furthermore, Almond noted that the unique aspect of modern political systems lay in the relatively high degree of structural differentiation performing both the input and output functions. In contrast, for "primitive" or traditional societies, the structures performing various functions lacked specificity and hence might not be clearly visible to an outsider. In these societies, the rule to follow, according to Almond, was to see a function and know that some structure was performing it. This analytical focus on functions therefore captured what is universal in political systems and was thought to allow for a more accurate depiction of political systems in transitional (or traditional) societies. In Almond's formulation, a functional approach allows analysts to "break through the barriers of culture and language and show that what may seem strange at first sight is strange by virtue of its costume or name, but not by virtue of its function."

Because Almond and his collaborators are interested in expanding the field of comparative politics into the developing countries, they cannot dispense with the classic distinction between traditional and modern societies. Like Marion Levy, Almond clearly recognized that one could not draw a sharp distinction between the two types of societies. Indeed, Almond saw the pattern variable scheme of Talcott Parsons as partly responsible for laying the basis for an "unfortunate theoretical polarization" in these two categories. However, Almond went beyond Levy's simple recognition to elaborate systematically on what he saw as the dualism in all societies—a dualism that included both informal and formal structures. Almond concluded concerning modern societies that:

> in modern political systems, the specialized structures of interest articulation (interest groups), aggregation (political parties), and communication (the mass media), exist in relation to persisting non-specialized structures which are certainly modified by the existence of the specialized ones, but are by no means assimilated to them.[110]

In comparing modern societies with the non-modern, the important task was to assess the way that Parsonian categories of universalism, specificity, achievement and affective neutrality were combined with particularism, diffuseness, ascription and affectivity in each of the functions Almond described. As an illustrative example, all societies contain a stage of political socialization (recruitment) via informal structures like the family that are particularistic and ascriptive. Socialization in primitive societies stops at this stage, while in modern ones, political socialization continues and is carried out via specialized formal structures like political parties.

It is with the function of political socialization that Almond established a link between the structural-functional approach contained in *The Politics of Developing Areas* and the political culture approach that dominates his book, *The Civic Culture*. Political socialization, to the extent it produces basic attitudes toward the political system, provides the starting point for political culture. Political culture, in turn, serves as a building block for understanding political functions because it produces basic attitudes toward the political system. Almond defined political culture as "the specifically political orientations—attitudes toward the political system and its various parts, and attitudes toward the role of self in the system."[111] Almond used the term "secularization" to capture the developmental aspect of political culture, which he defined as the process "whereby men become increasingly analytical and empirical in political action."[112] One hazard with such a formulation is that, if taken too literally, it may lead to conclusions that the more emotive, identity politics are fated to disappear.

According to Almond, this political culture approach provides yet another way to integrate anthropology, sociology and psychology. Almond's political culture framework outlines three types of political cultures that, to some degree, correspond to David Apter's categories of reconciliation and mobilization systems. Almond's three types of political culture are parochial, subject and participant. Parochial political cultures are most closely associated with traditional societies that have few, if any, specialized political roles. Subject political cultures are those with a differentiated system for outputs, but no such orientation toward the inputs of active participation. In other words, the relationship between the individual and political system is a passive one where individuals make no demands on the political system. David Apter might well recognize this political culture as typical in his mobilization system. Almond's third type of political culture is participant where the individual is oriented toward the entire political system providing inputs

to it as well as experiencing the system's outputs. David Apter would likely recognize this as a political culture typical in his reconciliation system. For Almond, each type of political culture he describes is congruent with its own type of political system so that a parochial political culture is congruent with traditional structures, subject political culture is congruent with an authoritarian structure and a participant political culture is congruent with a democratic political structure.[113]

While Gabriel Almond's work can be included under the aforementioned two approaches—structural functional and political culture—the bulk of the work of Lucian Pye, Almond's contemporary, is more closely identified with a political culture approach. Pye, who spent World War II working as an intelligence officer with the Marines, was, at times, more guardedly pessimistic than other modernization theorists concerning the prospects for political development in the new states. In fact, Pye suggested that scholars who focused on the cultural dimensions of development tended to see greater obstacles to the process than colleagues who could be categorized as stage theorists.[114] Indeed, Pye's ideas reflected greater sensitivity to historical conditions rather than any universalizing functions. Pye saw the social sciences in general as developing a certain skepticism about the inevitability or even the desirability of "progress" as a result of the experience with the dictators of World War II and the holocaust. Moreover, Pye saw the work of classical sociologists like Main and Tönnies as accepting progress, "but not without a note of nostalgia for the comfortable relationship of traditional societies and some anxiety over the prospects of a chillingly impersonal and ruthlessly calculating modern society."[115] However, Pye was consistent with other modernization theorists on his preference for an interdisciplinary approach, and he was especially attracted to establishing links between psychology and sociology. His interest in psychology may have been influenced by the work of Harold Laswell who contributed political psychology to the intellectual foundation of MIT's Center for International Studies.[116]

Lucian Pye helped shape research on the new states through his chairmanship of the SSRC's Committee on Comparative Politics, which he assumed in 1963 when Gabriel Almond stepped down. Under Pye's leadership, the committee produced the book series on political development, and because Pye recognized the many meanings of the term, he intended that the series avoid a rigid definition of "political development" and rather attempted to incorporate most dimensions.[117] Nonetheless, Pye reduced the many meanings of political development to three characteristics that he saw as most fundamental.

First was some general commitment toward equality that included several manifestations. These involved some sort of mass participation, the existence of universal laws and explicit legal procedures and recruitment to roles that reflected achievement criteria. Second was the capacity of the political system to affect the rest of society. This characteristic required the efficient implementation of public policy that, in turn, rested on a professionalization of government and administration. Third was the political development that required differentiation and specialization that included a division of labor in government. While Pye saw the latter two characteristics as amenable to structural-functional analysis, he saw the first characteristic as related to the realm of political culture and its association with sentiments concerning legitimacy and commitment to the political system.[118] Gilman suggests that the series on political development was sufficiently successful, and that while few social scientists at the time would admit to a belief in "progress," most would come to believe in "modernization."[119]

Besides sharing with Gabriel Almond service to the SSRC's Committee on Comparative Politics, Lucian Pye acknowledged his intellectual debt to Almond's work (and indirectly to Talcott Parsons). In the introduction to *Political Culture and Political Development* edited with Sidney Verba, Pye drew the link between his use of the concept of political culture and Almond's observation that "every political system is embedded in a particular pattern of orientation to political actions."[120] At the same time, Pye's focus on political culture distanced itself from structural-functional analysis because, as noted earlier, Pye saw structural-functionalism as most applicable to just two of the three fundamental characteristics of political development. Even in the chapter that Pye contributed to Almond and Coleman's structural functionalist volume, *The Politics of the Developing Areas*, Pye seems to be straining to apply the framework to the region he knew best, Southeast Asia. In that chapter, Pye suggested that the West's impact on the region led to the establishment of well-developed governmental structures that became the primary active element in politics but were unable to perform the input functions of "interest aggregation." This inability to perform one of Almond's universal input functions was attributed to the fact that associational interest groups were non-existent for articulating specific limited interests. Without interest articulation, there could hardly be interest aggregation.[121] Pye shows that while he can identify particular structures in transitional states, these structures do not perform functions attributed to them, nor are these functions performed elsewhere, making Almond's rule of seeing a function and knowing some

structure is performing it impossible to apply. Whether intended or not, Pye reveals that Almond's assertion of the existence of universal functions might merely be an assumption.

Pye believed the existence of structures and the absence of the input functions devised by Almond as very consequential for the politics of the developing areas. Because limited, specific interests are not articulated and nationalist leaders have no inputs to aggregate, they have no way to measure the relative support for one interest over any other. Therefore, nationalist leaders are forced to speak only in the most general terms and avoid specific issues that might be divisive. In the end, for Pye, the Westernized political structures were also unable to perform the output functions of rule-making, rule enforcement and rule adjudication so that these functions continue to be performed within informal traditional structures providing one more aspect of the widening gap between leaders and masses.[122]

If Pye only tentatively touched on problems with the structural-functional framework in his chapter for the Almond and Coleman book, he elaborated on them in subsequent work. For example, Pye explicitly challenged the input-output notion and suggested that while the concept might be useful for understanding the American system, it had little value for understanding the new states.[123] In his 1966 book, *Aspects of Political Development*, Pye reemphasized the limited applicability of Almond's structural-functional framework and at the same time provided a voice of caution for applying a Western model to the new states. For in the new states, the process of industrial growth and modernization had not yet reached the stage where the social structure was sufficiently differentiated and the division of labor not yet specialized to produce that "wide range of specific interests with quite definite but still limited objectives." Thus, interest articulation cannot take the form that it does in the modern countries of the West. Rather, Pye suggested that what emerged as an input to the political system is either the highly personal demands of individuals or "the uncompromising and unnegotiable assertions of distinctive ethnic, religious, or other communal groups." The lack of associational interest groups articulating limited interests results, in Pye's view, in the particular character of politics that rejects the very notion of independent, politically neutral institutions, so that there could be no impartial press, independent judiciary or neutral civil service. Pye then concluded that because of the absence of specialized interest groups articulating demands, leaders in the new states have nothing to aggregate and are forced to rely on broad general statements and "ideological abstractions."[124]

While Pye did not think conditions in the new states allowed for structures associated with interest groups and their functions, he did admit that modern armies were somewhat easier to create in transitional societies. In part, this was because militaries were able to look abroad for examples in a way that civil bureaucracies could not. Given this fact, it is not surprising that many social scientists would come to view the military as an effective modernizing force. Indeed, Pye himself made the point in 1963, that in eight Afro-Asian countries where the military assumed power, they focused government efforts on economic development.[125] Yet such a focus merely supported the fact that while the military might contribute to strengthening administrative functions, they would not be able to assist with creating the skilled politicians that Pye believed essential to the process of political development.[126]

Because of the shortcomings and limits of structural-functional analysis for the new states, Pye offered an alternative with a political culture framework that he outlined in a 1965 book that he edited with Sidney Verba, *Political Culture and Political Development*. Here, Pye added greater nuance and substantive content to Almond's typology of political culture. Pye saw the central problem of political development as growing from the gradual diffusion of world culture by the nation-state system. The key characteristics of this world culture—repeated throughout the modernization literature—included a scientific and rational outlook where human relations were premised on secular considerations.[127] While Pye did not see political development in terms of any particular sequence or stages, the fact that diffusion of the world culture provided the impetus for social change introduces a certain level of teleology into the process because, as Pye observed, "powerful international currents are pushing various societies roughly in the same direction."[128]

More importantly, the world culture necessarily comes into conflict with local, parochial and particularistic cultural traditions. The clash between the two cultures creates a certain tension within the new states and opens up fissures between various segments of society. These gaps are between leaders and masses, as well as between those who are more acculturated to modern ways and those who remain wedded to traditional forms. Pye believed that all the cases described in *Political Culture and Political Development* demonstrated the emergence of these gaps.[129] For Pye then, the heart of the problem of political development in the new states was to somehow relate the parochial and the universal and to manage the relationship between national unity and local diversity. In fact, in

his list of the crises that all new states face, Pye lists forging an identity as the first and most fundamental.[130] Pye had noted in an earlier work on communism in Malaya, based on interviews with "surrendered enemy personnel," that the very appeal of communism could be attributed to the fact that it served the psychological need of the search for identity.[131] Pye then equated the crisis of identity to the essence of nation-building in the new states and the search for a new sense of identity "which will be built around a command of all the potentialities inherent in the universal and cosmopolitan culture of the modern world, and a full expression of self-respect for all that is distinctive in one's own heritage"—a task likely to prove difficult in practice.[132] Viewed in this way, political culture, rather than structures or functions, becomes the most relevant factor in political development in the new states.

Pye believed political culture to be "the manifestation in aggregate form of the psychological and subjective dimensions of politics," and because it is a product of both the collective history of a political system and the life history of individuals, it provided an intellectual tool for linking psychology, sociology and political science. Pye suggested that the emergence of the concept of political culture was a response of the need to "bridge a growing gap in the behavioral approach in political science between the level of micro-analysis based on psychological interpretations of the individual's political behavior and the level of macro-analysis based on the variables common to political sociology." What is more, political culture, because it addresses non-rational aspects of human behavior, overcomes the inadequacy of that instrumental rational choice associated with economics, with its more narrow range of applicability.[133] As one illustration of this more emotive, non-rational behavior, Pye noted that elites in the new states experience such widespread anxiety over the tension between the parochial culture and the world culture that they come to distrust all manifestations of assertive traditionalism, thereby exacerbating the gap between elites and masses.[134]

Pye identified four sets of dichotomous values that he believed were crucial for shaping political culture and that played a role in modernization. The first of the pairs of values is the extent of trust and distrust and involves the identification of friends and opponents. This pair of values also has implications for political development because it affects expectations concerning whether public institutions or private individuals are more trustworthy. The second pair is hierarchy and equality, and all political cultures must address attitudes toward power and the relationship

between superiors and subordinates. Pye believed that while a hierarchy may be necessary for effective leadership, political development required the hierarchy to be tempered with some level of equality—at least to the extent that arbitrary distinctions in status be eliminated. The third set of values is liberty and coercion, and Pye concluded from case studies that coercion was ineffective for laying the basis for political development. In fact, Pye thought that the authoritarian government had a limited capacity for imposing the world culture and repressing the parochial one.[135] Finally, the last pair of values relates to whether people identify with parochial groups or the national level. Pye stressed the fact that there is no single way these values are combined; rather, the way they are combined "provides much of the distinctive character of the processes of development in each country."[136]

Although Pye did not explicitly incorporate his analysis of these values into Almond's political culture typology, the relative weight of each of the values can be viewed as a determinant of the type of political culture. Thus, for example, one might expect to find in a participant political culture, greater weight in the direction of trust, equality and liberty, with primary loyalty directed toward the nation. In contrast, a subject political culture would likely consist of values weighted most heavily in the direction of hierarchy and coercion with perhaps greater distrust of public institutions and some ambivalence regarding commitment or loyalty to the nation as a whole. In a parochial political culture, one might expect primary loyalty to be directed to particular groups and a greater trust in individuals who were members of the same group. At the same time, the parochial political culture would be weighted toward hierarchy while perhaps giving equal weight to liberty and coercion.

Given the central role of political culture in the process of political development, Pye was able to boil down the problem of modernization to two essential tasks that are, on a fundamental level, psychological. These are to change attitudes of the population and reduce the gap in the political culture between the elites and the masses.[137] In particular, change in attitudes included three distinct aspects that, in turn, help define the political culture. The starting point is to educate and train people in the skills necessary for a modern society. Along with developing these skills, the motivations and goals of individuals must be altered in such a way that they derive satisfaction from manipulating the material world. Finally, Pye believed that changing attitudes also included creation of "psychic mobility," that is, the ability to place oneself in other roles. This last change was

indispensable for determining the capacity of people to form effective associational relationships. While the nature of the change in attitudes central to political development is fairly easy to describe, it is more difficult to produce in practice and measuring change is likely to be problematic. These difficulties become apparent in Pye's conclusion concerning the impact that these changes have on the definition of political culture:

> by the distribution of particular "skills" and "techniques" among the population as a whole and among the political leadership. At another level political culture is defined by the motivations that inspire both leaders and followers. This motivational aspect governs the spirit in which interests are articulated and aggregated and the intensity with which loyalties and commitments are evoked. The third level of political culture—that which relates to associational sentiments—encompass the tone and general spirit of interpersonal relations throughout the political system.[138]

Pye's focus on political culture perhaps made him the most sensitive of the modernization scholars to the tension between the world culture and parochial ones, as the former was diffused across the globe in a way that foreshadowed the work of the subsequent school of dependency theorists. This tension generated in the new states a search for identity that would include "all the potentialities inherent in the universal and cosmopolitan culture of the modern world, and a full expression of self-respect for all that is distinctive in one's own heritage." The importance of forging a national identity did not lead Pye to a rejection of traditional ones, and he emphasized that:

> [i]n acknowledging the place of pride, loyalty and devotion in the process of nation-building we must also recognize the positive role of traditions, of parochial concerns and of primordial sentiments that are all an ineradicable part of human life.[139]

For Pye, the central importance of the quest for identity necessarily suggested the need for a representative or democratic political system because the imperative of integrating universal and parochial identities demanded a close relationship between the government and the masses. Indeed, Pye suggested that using authoritarian methods to introduce the world culture could be counterproductive. Such methods might well increase fragmentation of society along parochial lines by strengthening the people's view that because the world culture was foreign, it presented a threat

to local identity. Consequently, Pye asserted that it was the process of blending the universal and parochial "which justifies our faith that there is a close association between democratization and modernization." Pye's faith in democracy was further reinforced by his belief that pluralist politics—rather than detracting from economic development—would act instead as a stimulus to it.[140]

Yet, while Pye believed in the close affinity between democracy and modernization, his analysis of conditions in the new states reflected an ambivalence, if not inconsistency, concerning the prospects for democracy. Pye noted that most transitional societies lacked the two essential prerequisites for a stable system of representative government. First, they lacked a social mechanism that makes it possible to determine and clarify values and interests in society and relate them to a pattern of power through an aggregating process. Second, they lacked an efficient bureaucracy for carrying out public policy. In addition, as already noted, the fact that politicians in the new states speak only in abstract terms works to the detriment of stable representative institutions, and Pye observed that "Democratic politics must be built upon a bargaining process in which the particular interests of all are represented and in which the politician seeks to perform a brokerage role in aggregating interests into various policy mixes."[141] In short, Pye seems to be arguing that representative politics is the only way to synthesize the world culture with the local *and* that the conditions in the new states preclude the establishment of representative politics. Moreover, viewed from the perspective of the twenty-first century, Pye may well have been right to suggest authoritarian regimes would not be able to impose world culture on the recalcitrant masses, but he may have missed the fact that such regimes might provide the most effective means for limiting or resisting the world culture and its potentially pernicious effect on local culture.

The third methodology used by modernization scholars, we have labeled the process or administrative approach. Scholars falling within this category favored using quantitative methods for measuring the process of change in the new states. The starting point for such measurement involves the concept of "social mobilization," a term originated by Karl W. Deutsch. Deutsch, who emigrated from Czechoslovakia in 1938, was yet another alumnus of the OSS. He first used the term social mobilization in a paper presented at the 1959 Dobbs Ferry session convened by the Committee on Comparative Politics. The paper, subsequently published in *The American Political Science Review*, defined social mobilization as

"the process in which major clusters of old social, economic and psychological commitments are eroded or broken and people become available for new patterns of socialization."[142] As the term was defined, it focused on changes in the living situation of individuals. For Deutsch, the term was not synonymous with modernization, but was linked to it because it described the process of change involved in the move from traditional to modern society. Deutsch identified seven indicators for social mobilization. These were the percentage of the population exposed to aspects of modern life, like machinery and technology; the percentage of the population exposed to mass media; the percentage of the population experiencing a change in residence; the percentage of the population residing in urban areas; the percentage of those in non-agricultural occupations; the literacy rate and, finally, GNP per capita. As may be evident from this list, the advantage of using such indicators according to Deutsch is that they were amenable to quantitative study and hence the very process of the transition from traditional to modern society could be traced and measured in a scientific manner.[143]

For Deutsch, the importance of measuring these processes to determine the pace and trajectory of their growth demonstrated the extent and strength of the change in human needs in any given society. From this, one could then expect the emergence of pressure for transformation of political practices because the expansion of needs was not likely to be satisfied by traditional (pre-commercial, pre-industrial) government structures. With increased demands—or inputs, to use Almond's term—the very quality of politics was thought likely to change. As a result of the need for greater scope of government services and functions, the government would necessarily have to increase its capabilities which, in turn, would lead to administrative reform to improve the competence of the bureaucracy. None of this is to suggest a single direction to change toward improved integration, and Deutsch emphasized that social mobilization could just as easily lead to disintegration. He observed:

> *Other things assumed equal, the stage of rapid social mobilization may be expected therefore, to promote the consolidation of states whose peoples already share the same language, culture and major social institutions; while the same process may tend to strain or destroy the unity of states whose population is already divided into several groups with different languages or cultures or basic ways of life.*[144] (emphasis added)

What is more, demands in the new states were likely to be sufficiently acute that such states were not likely to adopt a Western laissez-faire style of government structure, but rather could be expected to establish government structures more characteristic of a "modern welfare state."[145] In this regard, Deutsch missed the extent to which the structure of the modern welfare state might be susceptible to corruption in societies, where the very process of social mobilization has fragmented society along ethnic or linguistic lines.

While Deutsch provided the groundwork for tracing and measuring the processes of change linked to modernization, two other scholars using interview techniques sought to validate the extent to which those processes altered traditional attitudes. Daniel Lerner and Alex Inkeles both conducted research using interviews for this purpose. In this, they were following a methodology suggested by Lucian Pye as the optimal way for studying mass political culture.[146] At the same time, they placed greater emphasis on individual psychology than Pye, who was more interested in the link between the psychological and the social.

For Daniel Lerner, a sociologist specializing in communication, his work on the role of the media as a source for the diffusion of modernization was a logical extension of his work in World War II for the Supreme Headquarters Allied Expeditionary Force (SHAEF) and the US Office of Military Government. His war work documented the impact of psychological operations which were reported in his 1949 book, *Sykewar: Psychological Warfare Against Germany, D-Day to VE Day*. While Lerner noted that colonialism had already contributed to the spread of Western (modern) ideas to elites, the postwar diffusion of modernization through the mass media (radio, film and newspapers) influenced the masses.[147] Therefore, Lerner's research design involved interviews with these masses, and he interviewed 2000 men from six Middle-Eastern countries (Turkey, Lebanon, Egypt, Syria, Jordan and Iran).[148]

Lerner admitted some limits to his interview data because he purposely controlled the sample in each country to overrepresent the population of movie-goers, radio listeners and newspaper readers, but he does not seem to appreciate the extent to which the sample skewed his results and his conclusions. Lerner concluded that the passing of traditional society was clearly visible throughout the Middle East. In particular, he singled out Turkey and Lebanon as the two most modern and stable countries in his sample, because he saw both as having passed—in what we shall see is a Rostowian conception—their "take-off" stage. Consequently, for both countries, their

only problems seemed to be economic so that "programs designed to solve these problems do not include violent transformations of the social system as a method, the political life has reduced the cataclysmic issues of ideology to the manageable dimension of planning." Economic growth then, because it reduces the salience of class cleavages, eliminates the major source for instability.[149] On this point, Lerner seems to downplay the sectarian divide as a source for instability in Lebanon, although he does admit that sectarianism "authorized by public law and administrative fiat perpetuates ancient loyalties at the expense of the larger national identification."

Not surprisingly, in the case of Turkey, Lerner sees the positive role played by the military in facilitating modernization.[150] Moreover, Lerner suggested that the Turkish case illustrated a certain irreversibility of changes associated with modernization because of Turkish success in separating the "Muslim institution" from the secular state.[151] While Lerner did recognize the major policy problem for leaders in the Middle East to be choosing between "Mecca or mechanization" and whether religion could be made compatible with modernization, in the end, his conclusion is wholly optimistic that the secular state will triumph. He said, "Whether from the East or West, modernization poses the same basic challenge—the infusion of 'a rationalist and positivist spirit' against which, scholars seem agreed, 'Islam is absolutely defenseless.'"[152]

Alex Inkeles, who monitored Soviet radio and newspapers for OSS during the war, and who is known for his early studies of the USSR and his contribution to the literature on national character, also used interviews to study changing attitudes in the new states. His field work was broader in scale and scope than that of Daniel Lerner, and he included countries from three continents. Conducted under the auspices of Harvard's Center for International Affairs as the Project on Social and Cultural Aspects of Economic Development, Inkeles outlined his research design and methodology in journal articles before his findings were published in a book in 1974, *Becoming Modern Individual Change in Six Developing Countries.* His research drew on interviews of 6000 men from six countries: Argentina, Chile, India, Israel, Nigeria and East Pakistan (Bangladesh). The men selected for interviews came from varied backgrounds in order to represent points on a continuum that ranged from those most rooted in traditional rural communities to those of experienced industrial, urban workers. The field work began in 1964, and the interviews contained some 300 items that sometimes required up to four hours to complete. According to Inkeles, the interviews demonstrated that there was such a

thing as a "modern man" that included personal qualities like openness to new experiences and a belief in science that led individuals to reject fatalism in the face of life's hardships as well as a shift in loyalties, away from traditional authority figures to government representatives. What is more, the research suggested that change in attitudes directly translated into behavior that Inkeles believed was characteristically modern. That is, that men who were rated as modern in attitude were also likely to have done things like join voluntary organizations (read interest articulation), relied on newspapers for information or have talked to or written to an official about some public issue.[153] Such measures and the statistical analysis that accompanies them, may well suggest a modernization in form, although it is less clear that they demonstrate the substance of modernization. After all, frequent readers of newspapers might well be reading publications with content reflecting a traditional orientation.

Inkeles' study identified two underlying sources for changing attitudes. These were education and occupational experience, particularly factory work. The latter variable thus is closely linked to economic development which was thought to have a profound effect on individual psychology and to increase "a man's sense of efficacy, make him less fearful of innovation and impress on him the value of education as a general qualification for competence and advancement." Inkeles did admit that he was optimistic concerning the impact of industrial experience and the fact it was more important for the process of modernization than education. Perhaps the most surprising conclusion that Inkeles drew from the research was that there was a certain "psychic unity of mankind," and that the qualities that define modern men do not differ from one culture to another.[154] Such a finding certainly lent credence to scientific generalizations for social engineering that could be divorced from cultural and historical contexts and seduce policy-makers into believing that they could anticipate reactions to their policies from people in other countries. Unfortunately, as David Engerman noted, the sheer ambition of this project meant that its findings were published in 1974 at precisely the time when modernization theory was coming under increasing criticism.[155]

The last category of approaches for analyzing the new states is economic, and we have seen throughout our discussion of modernization theorists, the extent to which they held an assumption that economic development provided the necessary foundation for political development. Walt W. Rostow is the name most closely associated with the economic approach. Rostow was an economic historian by training who

served in the OSS from 1941 to 1945. After the war, he worked with the State Department on German reconstruction. In 1951, he was invited by Max Millikan to become a member and co-founder of MIT's Center for International Studies. Donald Blackmer claimed that Rostow's forceful personality with its "buoyant optimism" contributed to the extent that Rostow's views on modernization came to dominate the Center.[156] As noted earlier, the Center explicitly sought to bridge the divide between academics and policy-makers, and Rostow personified this effort. Indeed, of all the modernization theorists, Rostow most closely fit the role of policy entrepreneur, and Kenneth Boulding characterized him as one of the "Cambridge hawks" who, like McGeorge Bundy, was influential during the Kennedy-Johnson administrations.[157] It is fitting, therefore, that we consider not only what Rostow contributed by way of content to modernization theory but also sketch out how his contributions to theory entered into policy. Greater elaboration of Rostow's role in policy must necessarily be reserved for the next chapter where we will see Rostow become an adamant supporter of Lyndon Johnson's policies in Vietnam.

The most well known of Rostow's ideas concerning modernization was his assertion that there were distinct stages in the process of economic growth that observers could readily identify. This model of stages of growth first appeared in a document prepared by Max Millikan and Walt Rostow for President Eisenhower in 1954. A subsequent draft was circulated to the Congress, and in 1957, the third draft was published as a book under the title, *A Proposal: Key to an Effective Policy*. As the book's title suggests, the purpose of the book was to explain to policy-makers how expanded economic development aid could contribute to US foreign policy. The book sought to demonstrate the feasibility of an expanded foreign assistance program by suggesting that such programs would be less costly than waging limited wars in the new states. Economic stagnation in the new states was thought to account for instability that generated insurgent movements. Perhaps the most controversial aspect of the proposal, given its Cold War context, was its recommendation that aid funds should be allocated according to economic rather than political criteria. The aid distributed according to economic criteria would have an institutional manifestation because it would be administered by a new agency separate from military assistance.[158] The stages of growth provided the way to identify recipient countries that were at the stage with the absorptive capacity to benefit the most from aid. Moreover, one underlying assumption in the proposal was that economic assistance could be a politi-

cally neutral tool for influencing politics in the new states. In fact, the Helmand Valley project in Afghanistan described earlier in this chapter was considered to be just such a neutral policy. David Lilienthal made a similar case of the political neutrality of a TVA style project in Kashmir that he believed offered a solution for the conflict between India and Pakistan in the region.[159] Yet the stated neutrality asserted by Millikan and Rostow was undermined to some extent because, although the authors sought economic aid that would not be linked to political conditions, they added the caveat, "beyond the requirement that national development goals be democratically established."[160]

Rostow expanded on his analysis of the stages of economic growth in his 1960 book by that title. In this book, Rostow exchanged his policy advisor role for an academic one where he asserted that the stages of growth were both a "hypothesis of social science and a statement of faith."[161] Rostow began his analysis with considerations that anticipated some of the objections of his critics. He acknowledged that "the stages-of-growth are an arbitrary and limited way of looking at the sequence of modern history" but that the stages were intended to dramatize *both* the uniformities and the distinctiveness in every nation's modernization experience. Further, Rostow emphasized that his model provided an economic way of examining the process of modernization but in no way implied Marxist assumptions "that the worlds of politics, social organization, and of culture are a mere superstructure built upon the economy."[162]

Rostow then went on to outline the specifics of his well-known framework that included five stages rather than the three stages set out in the 1957 proposal. These stages are as follows: (1) traditional society, (2) precondition phase, (3) take-off, (4) drive to maturity and (5) age of high mass consumption. The most pertinent stages for understanding the processes at work in the new states are the first three. Rostow's conception of traditional society did not deviate from the views of other modernization theorists we have discussed. However, Rostow did put an economic spin on this stage by suggesting that because such societies were pre-Newtonian, they did not view the physical world as amenable to human manipulation. Consequently, technical innovation remained quite limited so that such societies could not escape the Malthusian trap of resource constraints or the Ricardian problem of diminishing returns.

The second stage in Rostow's framework is the precondition phase, and with this stage, Rostow pointed to variation in the pattern of development. He divided countries into one of two categories—those that were

"born free," which included the United States, Canada, Australia and New Zealand. This group experienced an easier transition through the stages because they "never became so deeply caught up in the structures, politics and values of the traditional society." The second category is the most general case that, according to Rostow, fit most of Europe, Asia, the Middle East and Africa.[163] The most general case therefore applied to the new states, and Rostow believed, echoing Pye's observation concerning diffusion of a world culture from the West, that the precondition stage in these states was laid by the external intrusion from more economically advanced nations. In these new states, therefore, one of the non-economic aspects of the precondition phase is a reactive nationalism against foreign intrusion. A second non-economic aspect of the precondition phase is the emergence of a new political and social elite that replaces the role of the old landed elite. In this, Rostow noted he was merely recognizing a fact that extended back to Adam Smith's *The Wealth of Nations,* that surplus from agriculture must be transferred away from those who would spend it on lavish living to those who would invest in the modern sector. In short, as Rostow argued in a subsequent essay, the precondition phase has an initial requirement of the emergence of "a minimum cadre of modern men."[164] Indeed, it was the search to verify the existence of such a cadre of modern men that sent Daniel Lerner and Alex Inkeles into the field with their ambitious interview projects.

All the elements of the precondition phase serve to put in place the factors necessary for what Rostow famously labeled as "take-off." Critics of the stages framework tended to see the line between the precondition phase and the take-off to be so blurred as to make the stages meaningless.[165] For Rostow, however, the difference between the two stages was clear. Although he believed that there were earlier periods of economic growth, these did not have the scale to generate the momentum for self-sustaining growth. He saw three conditions as essential to this stage that made it highly visible: (1) a rise in the rate of productive investment, (2) the development of one or more substantial manufacturing sectors and (3) the existence of a political, social and institutional framework which exploits the impulses to expand the modern sector.[166] Of these three necessary conditions, the first and second were especially visible and could be measured. Albeit Rostow does admit that data on investment for the historic cases were sparse. Rostow stressed the importance of the leading sector for its dynamic impact that created backward and forward effects. The backward effects involved setting up requirements for new inputs. For

example, building railroads stimulated creation of new institutional arrangements for mobilizing capital. Forward effects of a leading sector involved opening up the possibility for new activities. In the case where cotton textiles were the lead sector, their expansion created incentives for developing cheaper modes of transport.[167]

As should be apparent from Rostow's examples of leading sector, there is no single pattern for which sector paves the way for self-sustaining growth, and this adds the distinctive element to each individual case. Rostow repeatedly emphasized the point that there was no single pattern in the evolution of a country's economic development, and that there were only a similar set of choices posed at each stage.[168] Rostow's belief in the variability of the evolution of stages in each case drew him to rely on a biological metaphor, and his use of it was more pronounced than the other modernization theorists. In his 1960 book, he explicitly characterized economic growth as a biological field. Later in his book, *Politics and the Stages of Growth*, he again emphasized the biological metaphor when he noted that the efficient absorption of new technologies carried with it powerful imperatives, "[b]ut those imperatives do not produce political and social uniformity. We are confronted in this mixture of uniformity and uniqueness with the classic problem of biological science."[169] In recognition of all the variables at work in economic development, Rostow then commented:

> How these variables relate to one another—the patterns they may form—can vary greatly. There is no single set of linkages that logic or historical experience decrees as universal. Like biologists we are examining different arrangements of the building blocks of growth.[170]

Although Rostow's detractors often point out that the very notion of stages in the process of economic development suggest a certain inevitability of progression through these stages, Rostow emphasized the fact that there is nothing automatic about the exploitation of the effects of a leading sector. And it is here that the unmeasurable, non-economic conditions necessary for take-off come into play—that is, whether the political, social and institutional framework exists that is conducive for taking advantage of the benefits generated by the leading sector. With his recognition of the importance of non-economic factors for take-off, Rostow's framework avoids the charge of economic determinism, so that his ideas incorporated an understanding of the linkage between economic develop-

ment and cultural attitudes. By so doing, Rostow shared some common ground with Lucian Pye and his assertion that economic development was not independent from cultural attitudes and political practices.[171] But if these non-economic factors are in place, they do provide a certain rationale for viewing the process as irreversible because, as Rostow pointed out, such large psychological, social and institutional changes may not easily be undone.[172]

Rostow's framework was subject of so much criticism that a veritable cottage industry grew up around it. For example, both Samuel Huntington and Hans Morgenthau were critical of the approach. Morgenthau, in particular, dismissed the very notion of economic aid as a modern form of bribery.[173] In a similar vein, Hans Speier, RAND's director of the Social Sciences Division, doubted the efficacy of foreign aid as a tool for preempting political instability and insurgency. Speier remarked in this respect that "deterrence of subversion by economic aid is more precarious than is deterrence of subversion by military means."[174] From the left of course, Rostow's ideas were criticized in the 1970s by dependency theorists, and the most caustic denunciation of modernization theory in general appeared in Immanuel Wallerstein's *The Capitalist World Economy* published in 1979.[175]

Perhaps the vehement rejection of Rostow's ideas had much to do with his staunch anti-communism and the role he played as a policy entrepreneur during the Vietnam War. Rostow's anti-communist view is apparent in the subtitle of his 1960 book: *A Non-Communist Manifesto*. His views were also made clear in a 1964 speech he gave at a seminar on democracy, where noting all the difficulties associated with transitional societies, he asserted that communist policy aimed to heighten the difficulties, thereby becoming "the scavengers of the modernization process."[176] Even given the anti-communist sentiments that Rostow expressed, it is less clear that his ideas necessarily supported a conservative status quo, for he and Millikan had argued that sustainable growth required the understanding of workers and peasants, which demanded, in turn, "an equitable distribution of growing national income, as well as the freedom to organize."[177] For his part, Rostow remained committed to the ideas presented in his 1960 book, and in the preface to its second edition, published in 1971, asserted that the evidence collected over the decade between the two books reinforced the value of the concept of stages.[178]

Despite criticism of Rostow's framework, the very fact that the International Economic Association hosted a conference devoted to his idea of take-off, illustrates the extent to which the idea resonated with

economists. In a review of the book Rostow edited that was generated as a result of that conference, Henry Rosovsky praised Rostow and noted that the economic profession owed a debt of gratitude to him for stimulating members of the profession to reexamine their ideas.[179] Moreover, any fair and non-polemical evaluation of Rostow's concepts must begin with a recognition of the fact that his ideas were consistent with two intellectual traditions. First, from the standpoint of the economics profession in general, which had a definite tendency that placed value on "universalistic theories and powerful models above all else, and was not well-equipped to deal with messy particulars,"[180] Rostow's work was not a deviation. Rostow's analysis was also consistent with the intellectual heritage of Talcott Parsons and his notion that societies be viewed as integrated systems where change in any one component caused automatic changes throughout the entire system. With the foundation of a systems approach to development, it was more than logical to assume that the fundamental changes in an economy experiencing take-off would reverberate with positive effects throughout the entire social system.[181]

Notwithstanding the intellectual milieu within which Rostow's analysis took place, an evaluation of his work must address two serious and related criticisms. The first is the charge that the stages framework suffered from being excessively unilinear. While it is easy to caricature Rostow's stages in this way, we have already noted the extent to which Rostow continually reiterated the point that there was no single pattern to development. What is more, Rostow (as well as other modernization theorists) recognized the fact that evolution of development might be erratic and discontinuous, carrying with it the possibility of moving backward. Related to the criticism of a unilinear evolution is the assertion that the stages of growth provided what Gunnar Myrdal called a "teleological argument," suggesting that evolution was tending in the direction of the proto-typical United States. From the beginning with the proposal, however, Millikan and Rostow denied that their objective was to replicate the United States. In the proposal, they stated explicitly:

> We do not seek societies abroad built in our own image. We do have a profound interest that societies abroad develop and strengthen those elements in their respective cultures that elevate and protect the dignity of the individual as against the claims of the state.[182]

Rostow refuted the charge of a teleological bias in stages in the second edition of his treatise. There he observed that there was a good deal about economic growth that was *not* automatic. However, he did believe that much that was automatic was owed to the "Newtonian Perception" that the physical world could be understood and manipulated to man's advantage. Consequently, those who acted on the Newtonian perception early acquired economic, political and military power that once embedded in the social system was unlikely to disappear.[183]

Whatever the flaws in the stages framework, Rostow's work was influential in policy circles. The MIT report submitted to the Senate cited earlier in this chapter saw the economic approach as more valuable to policy-makers, not "because the economic dimension of modernization will determine its outcome but because American economic aid is a possible and mutually accepted way of affecting the alternatives open to transitional societies."[184] Between 1956 and 1959, Rostow testified three times before congressional committees on foreign relations and economic policy where he undoubtedly caught the eye of Senator John F. Kennedy. Kennedy, who was a member of the foreign relations committee, sought passage of an aid bill for India.[185] As president, John Kennedy sought to alter American foreign economic policy toward the developing world in a manner consistent with the New Deal tradition of the Democratic Party. Kennedy's intention regarding the developing countries was outlined in an article he wrote for *Foreign Affairs* published in 1957, where he specifically cited the Millikan-Rostow framework as providing "useful guideposts" for policy. Kennedy went on to link the issue of the new states to a broader vision of US grand strategy of Cold War:

> We must see that our actions stimulate the healthy development of the new states even if they are neutral; that we do not encourage the prolongation of Western colonialism where it is stagnant, that the position we take against Soviet imperialism in Eastern Europe is not weakened by Western imperialism in Africa and Asia.[186]

Rostow's analysis proved especially well suited to policy because, unlike other modernization theorists who analyzed political development in terms of variables like culture that could not be easily manipulated, Rostow's idea concerning stages of economic development presented a feasible approach because it suggested that aid could be targeted toward those countries most likely to benefit from it. By fostering economic

growth in this way and given the assumptions of a systems analysis framework, political benefits were likely to follow. Evidence that the Kennedy administration applied Rostow's ideas can be found in the changing composition of the US foreign assistance. Whereas the Eisenhower administration, in the wake of the Korean War, preferred military assistance to economic development aid, Kennedy reversed this priority. Thus, in the 1950s, military aid dominated the US economic assistance by a factor of two to one. Furthermore, the average annual economic aid from 1956 to 1960 stood at $2.5 billion, and was increased during the Kennedy years from 1961 to 1963 to $4 billion. In addition, Cold War considerations had dominated President Eisenhower's aid policy in other ways. For example, in 1951, a request by India for two million tons of grain to avert an impending famine was delayed for four months because India chose neutrality in the Cold War.[187]

In the final analysis, the scientific approach to development known as modernization theory left an ambiguous legacy—at times prescient concerning the conditions in the new states—and at others completely missing the mark. Yet, in fairness to this body of scholarship and as Ian Roxborough has pointed out, no sensible theory of historical change can completely dispense with some core propositions in modernization theory.[188] Any review of the major theorists of modernization reveals some consensus concerning the obstacles that the new states faced in their transition from traditional to modern societies. Notable in this regard was Lucian Pye's observation that the crisis of identity was the most significant crisis that the new states faced. His collaborator, Sidney Verba, concurred and emphasized the importance of forging a national identity to which local parochial ones would be subordinated. Verba noted that in the absence of psychological membership in the national unit, an "orderly pattern of change is unlikely."[189] Similarly, Karl Deutsch warned of the hazards that social mobilization—a process inherent in modernization—would likely have on the new states. If societies were deeply divided along ethnic or linguistic lines, social mobilization would be destabilizing.

Although modernization theorists were criticized for seeing change as a unilinear process that would culminate in the same end, there was, in fact, a consensus that modernization in the new states was not likely to replicate the Western pattern. Moreover, there was some recognition that the variation in pattern might well include a stronger role for the state in directing social change. A strong state role might then include a more authoritarian political system. The different role for the state in the modernization

process was especially apparent to those who focused on the economic dimension of modernization. Late modernizers could ill-afford a laissez-faire approach to economic development and would require instead arrangements like state financing of capital formation. Rostow certainly saw the need for an expanded state role in the economy. Indeed, the different structures and institutions necessary for the economies of the late modernizers were central themes of Alexander Gerschenkron's 1962 book of essays titled *Economic Backwardness in Historical Perspective.*

Despite the prescience reflected in the work of some modernization theorists, there remained serious shortcomings in some of the analysis. Perhaps the most egregious was a certain under-appreciation of the strength of local culture, including religion, as an intervening variable that stood as a barrier to the rationalizing process engendered by the modern industrial order. On this point, Daniel Lerner was certainly mistaken to claim that Islam was powerless against the forces of modernization. Lucian Pye, in his presidential address to the American Political Science Association in 1990, captured this point well. In noting the general optimism concerning the behavioral revolution in the social sciences, he noted that it had a:

> vision that by collectively pursuing the scientific method and adhering to the canons of quantification we would be able to generate cumulative knowledge just as chemists and physicists had done. [But] The ambition to discover universal and enduring laws like Boyles' law has been frustrated by the realization that human behavior is too sensitive to the fluctuations of culture and circumstances of history to yield permanently enduring findings.[190]

Albert Hirschman also was critical and remarked of the extent to which the developing countries had "become fair game for the model builders and paradigm molders to an intolerable degree," which subverted understanding.[191]

As science, modernization theory contained less of a consensus over method and paradigms than deterrence/coercion theory. At best, the theory contained multiple methodologies and competing paradigms. In addition, as Stephen Toulmin notes, ideas in social science do not form a cumulative body of concepts but rather consists of pendulum swings. He notes that evidence of the lack of consensus is illustrated by the various specialized journals that do not represent a consensus over paradigms or a well-established science, but instead form a "loose confederation of

proselytizing sects." While Toulmin admits that scientific communities like physics might not achieve unanimity, any disagreements always remain at the margins.[192] We earlier saw the intellectual swings in political science which moved away from a legal, historical approach to embrace an empirical one called behavioral science. The discipline at one time rejected terminology like the state in favor of political system leading to another swing with a recognition of a need "to bring the state back in." From the standpoint of modernization theory, the pendulum moved then again to its successor framework of dependency theory that now faces questions of its own and criticism that it too does not accurately describe the world.

Whatever the flaws of modernization theory from a scientific standpoint, it did help shape American policy toward the new states, albeit the impact was not uniform from all the four categories of approaches outlined earlier. As should be apparent from our discussion of the various approaches, the structural-functional and political culture approach were less conducive to direct application in policy, although some of their assumptions became part of policy-maker's intellectual milieu. In part, the analysis contained in these approaches was too complex, and the variables utilized were not easily manipulated—especially by outsiders. The administrative approach to modernization, whose precept concerning the diffusion of mass media as conducive to forging modern men, could easily be translated into policy instruments like the United States Information Agency. It goes without saying that the economic approach provided policy-makers with an instrument to shape evolution in the new states. Yet, even with this approach, the difficulties with it in practice were recognized even in its earliest days. Peggy and Pierre Streit, in their 1956 appraisal of the Helmand Valley Project, warned that foreign aid "is a Herculean task, long-range in nature, fraught with frustration and criticism, with results that, by American standards, are bound to be agonizingly slow."[193]

In the end, policy-makers in the 1960s were drawn to the economic approach for fostering modernization, perhaps because economics as the most scientific of the social sciences was thought to offer a degree of certainty. Policy-makers were attracted also by the fact that the economic aspect of modernization was something that outsiders might well affect. And if the assumptions provided by thinking of societies as integrated, interdependent systems proved true, then other positive political consequences would follow from manipulating this one variable. The legacy of Walt Rostow extended beyond the Kennedy and Johnson administrations

and continues to cast a long shadow over American foreign policy. For Rostowian assumptions are contained in President George W. Bush's response to the attacks of September 11, 2001. His initiative creating the Millennium Challenge Account to foster economic development was calculated to be the key to preempting religious extremism. This lasting legacy of Rostow's is not without a degree of irony. Of all the modernization theorists, his work has been subjected to some of the harshest criticism while other work has been neglected or forgotten. The reason for this relates to the role he played in the Johnson administration as it tried to apply the science of deterrence/coercion and modernization. How scientific social science was applied to the War in Vietnam is the subject of the next chapter.

Notes

1. Some of this analysis appeared earlier. See Janeen Klinger, "A Sympathetic Appraisal of Cold War Modernization Theory," *International History Review* 39 (2017): 691–712.
2. Lucian W. Pye, "Political Modernization: Gaps Between Theory and Reality," *The Annals of the American Academy of Political and Social Science* 442 (March 1979): 29.
3. Edward Shils, "On the Comparative Study of the New States," in *Old Societies and New States*, ed. Clifford Geertz (New York: Free Press of Glencoe, 1963), 9.
4. Quoted in Mark T. Berger, "Decolonisation, Modernisation and Nation-Building: Political Development Theory and the Appeal of Communism in Southeast Asia," *Journal of Southeast Asian Studies* 34 (October 2003): 430.
5. Samuel P. Huntington, "The Change to Change: Modernization, Development and Politics," *Comparative Politics* 3 (April 1971): 290. The postwar work of Hannah Arendt and Erich Fromm is notable for seeing the negative side to elements celebrated by modernization theorists.
6. Lucian Pye, *Aspects of Political Development* (Boston: Little Brown and Company, 1966), 52.
7. Nils Gilman, *Mandarins of the Future: Modernization Theory in Cold War America* (Baltimore: The Johns Hopkins University Press, 2003), 66.
8. Gilman, 28.
9. Huntington, 290.
10. See, for example, Theda Skocpol, "Bringing the State Back In: Strategies of Analysis in Current Research," in *Bringing the State Back In* eds Peter B. Evans et al. (Cambridge University Press, 1985), 3–37.

11. David Easton is attributed as the first scholar to eliminate the term state in favor of political system, arguing that the Cold War necessitated creation of a lexicon that would transcend specific cultures. Moreover, Easton derived the concept of system from Talcott Parsons. See Berger, 428 and Gabriel Almond, "Political Development: Analytical and Normative Perspectives," *Comparative Political Studies* 1 (January 1969): 448; Gilman, 135.
12. David Ekbladh, *The Great American Mission: Modernization and the Construction of an American World Order* (Princeton: Princeton University Press, 2010), 12.
13. Lucian Pye, "Introduction," in *Communication and Political Development*, ed. Lucian Pye (Princeton: Princeton University Press, 1963), 12.
14. Quoted in Gilman, 160.
15. Michael E. Latham, *Modernization as Ideology: American Social Science and "Nation-Building" in the Kennedy Era* (Chapel Hill: University of North Carolina Press, 2000), 23. A certain US-centric model for development was apparent in the Marshall Plan as well. See John Kenneth Galbraith, "A Positive Approach to Economic Aid," *Foreign Affairs* 39 (April 1961): 444–457.
16. Francis X. Sutton, "Nation-Building in the Heyday of the Classic Development Ideology: Ford Foundation Experience in the 1950s and 1960s," in *Nation-Building Beyond Afghanistan and Iraq*, ed. Francis Fukuyama (Baltimore: The Johns Hopkins University Press, 2006), 42, 59.
17. Nick Cullather, "Damming Afghanistan: Modernization in a Buffer State," *The Journal of American History* 89 (September 2002), 512.
18. Peggy and Pierre Streit, "Lessons in Foreign Aid Policy," *The New York Times Magazine* (March 18, 1956): 15.
19. Hans Joas, "The Modernity of War: Modernization Theory and the Problem of Violence," *International Sociology* 14 (December 1999): 458.
20. Gilman, 73.
21. Interestingly, the final volume in the series was Charles Tilly's, *The Formation of National States in Western Europe*, and marked a turn away from the grand theorizing to a more concrete, historical analysis. See Said Amir Arjomand, "Social Theory and the Changing World," *International Sociology* 19 (September 2004), 337.
22. Gilman, 160.
23. Donald Blackmer, *The MIT Center for International Studies: The Founding Years, 1951–1969* (Cambridge MA: MIT, 2002), 95, 115. India severed its ties with the Center after an Indian newspaper revealed the Center's links with the CIA. See Michael Latham, *The Right Kind of*

Revolution: Modernization, Development and U.S. Foreign Policy From the Cold War to the Present (Ithaca: Cornell University Press, 2011), 72.

24. W.W. Rostow, *Eisenhower, Kennedy and Foreign Aid* (Austin: University of Texas Press, 1985), 43, 55.
25. Francis Bator, et al. *Economic, Social and Political Change in the Underdeveloped Countries and Its Implications for the United States Policy* (Washington, DC: US Senate Committee on Foreign Relations, 1960), 1, 12. The scholars that contributed to this report are well known for their association with the modernization project and included, among others, Daniel Lerner, Max Millikan, Ithiel de Sola Pool, Lucian Pye and Walt Rostow.
26. Lucian Pye, *Aspects of Political Development* (Boston: Little, Brown and Company, 1966), 33–44.
27. Ann Ruth Willner, "The Underdeveloped Study of Political Development," *World Politics* 16 (April 1964), 471; Robert A. Packenham, *Liberal America and the Third World: Political Development Ideas in Foreign Aid and Social Science* (Princeton: Princeton University Press, 1973), 3–4.
28. Seymour Martin Lipset, *Some Social Requisites of Democracy: Economic Development and Political Legitimacy The American Political Science Review* LIII (March 1959) reprint by the Bobbs-Merrill Reprint series in the social sciences, #175, 75 (page reference to the reprint edition.); Edward Shils, "Political Development in the New States," *Comparative Studies in Society and History* 2 (April 1960): 280.
29. Gilman, 93.
30. Marion J. Levy, *Modernization and the Structure of Societies: A Setting for International Affairs* (Princeton: Princeton University Press, 1966), 10; Cyril E. Black, "Theories of Political Development and American Foreign Policy," in *The Role of Ideas in American Foreign Policy,* ed. Gene M. Lyons, (Hanover, N.H. University Press of New England, 1971), 48; Reinhard Bendix, "Tradition and Modernity Reconsidered," *Comparative Studies in Society and History* 9 (April 1967), 329; David E. Apter, *The Politics of Modernization* (Chicago: University of Chicago Press, 1965), 9–10; Manning Nash, *Unfinished Agenda: The Dynamics of Modernization in Developing Nations* (Boulder: Westview Press, 1984), 6. Even more recently, scholarship on social change continues to associate modernity with scientific and technical progress. See James C. Scott, *Seeing Like a State* (New Haven: Yale University Press, 1998), 89.
31. Quoted in Gilman, 1. Gilman also notes another contribution that Shils made to the postwar linkage of science to policy as editor of *The Bulletin of Atomic Scientists*, a journal intended to keep scientists involved in nuclear policy-making, 51.

32. Gilman, 9.
33. Pye, "Political Modernization: Gaps Between Theory and Reality," 34.
34. James S. Coleman and C.R.D. Halisi, "American Political Science and Tropical Africa: Universalism vs Relativism," *African Studies Review* 26 (September–December 1983): 41.
35. Ian Roxborough, "Modernization Theory Revisited: A Review Article," *Society for Comparative Studies in Society and History* 30 (October 1988), 755.
36. Latham, *Modernization as an Ideology*, 45–46.
37. Theda Skocpol, "Commentary: Theory Tackles History," *Social Science History* 24 (Winter 2000), 674.
38. Daniel Bell, *The Social Sciences Since the Second World War* (New Brunswick: Transaction Books, 1982), 5.
39. Pye, "Political Modernization: Gaps Between Theory and Reality," 32.
40. The listed characteristics of traditional and modern societies are described in numerous sources. For representative examples, see S.N. Eisenstadt, "Studies of Modernization and Sociological Theory," *History and Theory* 13 (October 1974): 226; Max Millikan and Donald Blackmer, eds. *The Emerging Nations: Their Growth and United States Policy* (Boston: Little, Brown and Company, 1961), 3–5; Bendix, 293; David C. Engerman, et al. eds. *Staging Growth: Modernization, Development and the Global Cold War* (Amherst: University of Massachusetts Press, 2003), 38; and Nash, 5.
41. Joseph LaPalombara, "The Comparative Roles of Groups in Political Systems," *Items: the Social Science Research Council* 15 (June 1961), 19.
42. Levy, 16, 137; Gabriel Almond and G. Bingham Powell, Jr., *Comparative Politics: A Developmental Approach* (Boston: Little, Brown and Co., 1966), 33. Indeed, Samuel Huntington identified the value in Almond's work to the extent that he insisted that all political systems combined traditional and modern elements, Huntington, 299.
43. S.N. Eisenstadt, *Tradition, Change and Modernity* (New York: John Wiley and Sons, 1973), 101.
44. Bendix, 314, 317.
45. Huntington, 286.
46. Talcott Parsons, *The Social System* (New York: The Free Press, 1951), xi. This book is an extension of ideas contained in a chapter of the book Parsons edited with Edward Shils, *Toward a General Theory of Action*.
47. Parsons, *The Social System*, 540.
48. William Buxton, *"Talcott Parsons and the Capitalist Nation-State: Political Sociology as a Strategic Vocation"* (Toronto: University of Toronto Press, 1985), 237–238.

49. Parsons, *The Social System*, 5–6.
50. Talcott Parsons, *Structure and Process in Modern Societies* (New York: The Free Press, 1960), 177, 171.
51. Buxton, 76.
52. Parsons, *The Social System*, 43.
53. Parsons, *Structure and Process in Modern Societies*, 142.
54. Parsons, *The Social System*, ix, 20, 333.
55. Parsons' commitment to the notion of the scientific stature of the social sciences may have wavered for he raised the question in the introductory chapter of *Structure and Process in Modern Societies*, of whether sociology was in fact becoming a science, "or must it remain a congeries of discrete proto-sciences?" 13. Robert Bellah also claimed that later in life, Parsons turned away from the natural sciences as a model. See Robert Bellah, "The World is the World Through Its Theorists: In Memory of Talcott Parsons," *American Sociologist* 15 (1980), 62.
56. Stephen Toulmin, *Human Understanding: the Collective Use and Evolution of Concepts* (Princeton, Princeton University Press, 1972) 129.
57. Buxton, 20.
58. Quoted in Chalmers Johnson, *Revolutionary Change* (Boston: Little Brown and Company, 1966), 20.
59. Buxton, 120.
60. Buxton, 109.
61. Leonard W. Doob, "Goebbels Principles of Propaganda," *The Public Opinion Quarterly* 14 (Autumn 1950), 422.
62. Latham, *Modernization as Ideology*, 32.
63. Gabriel Almond, "A Developmental Approach to Political Systems," *World Politics* 17 (January 1965), 185.
64. Parsons, *The Social Structure*, 298 and Parsons *Structure and Process in Modern Societies*, 226.
65. For an extension of Parsonian analysis to revolution, see: Johnson, *Revolutionary Change*, especially 145–146.
66. Alex Inkeles, "Social Stratification and Mobility in the Soviet Union: 1940–1950," *American Sociological Review* 15 (August 1950): 478.
67. Parsons, *The Social Structure*, 529–533.
68. Parsons, *The Social System*, 484, 483, 487, 499 and 486.
69. Johnson, 51–52, 46 and 49.
70. Karl W. Deutsch, "Integration and the Social System: Implications of Functional Analysis," in *Integration of Political Communities,* eds. Philip E. Jacob and James V. Toscano, 179–208, (Philadelphia: J.B. Lippincott Company, 1964), 181, 200.
71. Buxton, 85. Buxton also suggests that in Parsons' schema, the professions would play a rationalizing role, 111.
72. Parsons, *The Social System*, 173, 177–178.

73. Talcott Parsons, "Evolutionary Universals in Society," *American Sociological Review* 29 (June 1964): 329–357.
74. Bellah, 60. Daniel Bell was less flattering, 44.
75. Latham, *Modernization as Ideology*, 33–34.
76. Gilman, 73.
77. This taxonomy and placement of scholars within it is a variation of those provided by Robert Packenham, "Approaches to the Study of Political Development," *World Politics* 17 (October 1964): 109–117 and Huntington, 308.
78. LaPalombara, 20–21.
79. Levy, 10.
80. Levy, 4–5, 14.
81. Levy's other four pairs of polar opposites are functional specificity-functionally diffuse, avoidant-intimate, individualistic-responsible and hierarchical-nonhierarchical, 137.
82. Levy, 58, 62–64.
83. David Apter, *Some Conceptual Approaches to the Study of Modernization* (Englewood Cliffs, N.J.: Prentice Hall, 1968), 6.
84. Apter, *The Politics of Modernization*, 1.
85. "David Apter Obituary," *The New York Times*, May 2010. Downloaded from https://www.nytimes.com on February 10, 2015.
86. Apter, *Some Conceptual Approaches to the Study of Modernization Theory*, 115. Unless otherwise noted, the discussion of Apter's contribution to modernization theory draws on this collection of essays.
87. Apter, *The Politics of Modernization*, 43.
88. Apter, *Some Conceptual Approaches to the Study of Modernization*, 5. Because this point appears in the introductory chapter of a compilation of earlier essays, I presume this view offers a reflection on the earlier work.
89. For a study that sees traditional societies as more varied than modern ones, see Raoul Naroll, Vern L. Bullough, and Frada Naroll, *Military Deterrence in History: A Pilot Cross-Historical Survey*, (New York: State University of New York Press, 1974), xlv.
90. Apter elaborates on the distinction between the two kinds of traditionalism, with case material drawn from Ghana and Uganda. See his book *Some Conceptual Approaches to the Study of Modernization*, 113–135.
91. Quoted in Apter, *Some Conceptual Approaches to the Study of Modernization*, 15.
92. Apter, *The Politics of Modernization*, 16, 20, xiv.
93. Apter first outlined his three development systems in 1963, in a chapter of a book edited by Bert F. Hoselitz and Wilbert E. Moore, *Industrialization and Society* (The Hague: UNESCO-Mouton), and he

expanded his analysis of the three systems in his 1965 book, *The Politics of Modernization*.

94. Apter, *The Politics of Modernization*, 39–40.
95. Sidney Verba, "Comparative Political Culture," in *Political Culture and Political Development,* eds. Lucian W. Pye, and Sidney Verba, (Princeton: Princeton University Press, 1965), 515.
96. Toulmin, 209.
97. Apter, *The Politics of Modernization*, 37.
98. From Gabriel Almond's book, *The American People and Foreign Policy*, quoted in Buxton, 204.
99. Gabriel Almond and G. Bingham Powell, Jr., *Comparative Politics: A Developmental Approach* (Boston: Little, Brown and Co., 1966), 23.
100. Packenham, *Liberal America and the Third World: Political Development Ideas in Foreign Aid and Social Science*, 246.
101. Buxton, 206 and Gilman, 129.
102. Gabriel Almond, "Introduction," in *The Politics of the Developing Areas,* eds. Gabriel Almond and James Coleman (Princeton: Princeton University Press, 1960) 4, 59.
103. Gabriel Almond, "Political Development: Analytical and Normative Perspectives," 462–468.
104. Gabriel Almond and G. Bingham Powell, Jr. *Comparative Politics: A Developmental Approach* (Boston: Little Brown and Company, 1966), 7.
105. Almond and Powell, 331–332.
106. Unless otherwise noted, discussion of Almond's structural-functionalism draws on his introduction to *The Politics of Developing Areas*, 3–64.
107. Almond, "A Developmental Approach to Political Systems," 186.
108. Almond, "A Developmental Approach to Political Systems," 186.
109. As an aside, Almond saw the input functions as more important for characterizing non-Western political systems as well as discriminating the stages of political development in those systems. See Almond, "Introduction," 17.
110. Almond, "Introduction," 21.
111. Gabriel Almond and Sidney Verba, *The Civic Culture: Political Attitudes and Democracy in Five Nations* (Boston: Little Brown and Company, 1965), 12. The political culture approach displayed in this book also offered a more nuanced distinction among various types of modern societies.
112. Almond and Powell, 24.
113. Almond and Verba, 16–18, 20.
114. Pye, "Political Modernization: Gaps Between Theory and Reality," 33.
115. Pye, *Aspects of Political Development*, 51, 60.

116. Gilman, 160. Some later-day scholars are critical of Pye's emphasis on individual psychology that they see coming at the expense of ethnicity. See Berger, 435.
117. Lucian Pye, "Introduction: Political Culture and Political Development," *in Political Culture and Political Development,* eds. Lucian Pye and Sydney Verba, (Princeton: Princeton University Press, 1965), 12.
118. Pye, *Aspects of Political Development*, 46–48. Pye admitted that for the new states, there was a tension between a commitment to equality and the capacity of the government, a point that is well developed in Samuel Huntington's classic book, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968).
119. Gilman, 152.
120. Pye, "Introduction: Political Culture and Political Development," 7.
121. Lucian Pye, "The Politics of Southeast Asia," in *The Politics of the Developing Areas*, eds. Gabriel Almond and James Coleman (Princeton: Princeton University Press, 1960), 115, 124.
122. Pye, "The Politics of Southeast Asia," 147.
123. Lucian Pye, "Democracy, Modernization and Nation-Building," in *Self-Government in Modernizing Nations*, ed. J. Roland Pennock (Englewood Cliffs: Prentice Hall, 1964), 7.
124. Pye, *Aspects of Political Development*, 81, 27, 25. This rejection of the idea of neutral institutions had policy implications for the United States, and Pye saw that in the case of Burma and Malaya, a greater suspicion toward American aid programs than the Soviet's, because the latter's explicit partisan motive confirmed the preconception concerning neutrality.
125. Lucian Pye, "Military Development in the New Countries," in *Social Science and National Security*, eds. Ithiel de Sola Pool et al. (Washington, D.C.: Smithsonian Institution, 1963), 151. Of course, from our twenty-first century perspective, we now know that military government is just as likely to devolve into dysfunctional kleptocracy of which Mobutu Sese Seko's 32-year rule of the Congo stands out as an exemplary case in point.
126. Pye, *Aspects of Political Development*, 187.
127. Lucian Pye, "Introduction," in *Communication and Political Development*, 19.
128. Pye, "Democracy Modernization and Nation-Building," 11.
129. Pye, "Introduction: Political Culture and Political Development," 15, 17.
130. Pye, *Aspects of Political Development*, 8, 63.
131. Lucian Pye, *Guerilla Communism in Malaya: Its Social and Political Meaning* (Princeton: Princeton University Press, 1956), 344.
132. Pye, "Democracy, Modernization and Nation-Building," 22.

133. Pye, *Aspects of Political Development*, 105; Pye, "Introduction: Political Culture and Political Development," 8, 10.
134. Pye, *Aspects of Political Development*, 26.
135. Pye, "Democracy, Modernization and Nation-Building," 23.
136. Pye, "Introduction: Political Culture and Political Development," 22, 23.
137. Pye, "Introduction," *in Communication and Political Development*, 13.
138. Pye, *Aspects of Political Development*, 89, 101.
139. Pye, "Democracy, Modernization and Nation-Building," 22; Pye, *Aspects of Political Development*, 102.
140. Pye, "Democracy, Modernization and Nation-Building," 24,23, 25; Pye, *Aspects of Political Development*, 72–73.
141. Pye, *Aspects of Political Development*, 76, 25–26.
142. Karl W. Deutsch, "Social Mobilization and Political Development," *The American Political Science Review* 55 (September 1961): 494.
143. Deutsch then goes on to construct equations from his indicators, see Deutsch, "Social Mobilization and Political Development," passim.
144. Deutsch, "Social Mobilization and Political Development," 501.
145. Deutsch, "Social Mobilization and Political Development," 498.
146. Pye, "Introduction: Political Culture and Political Development," 16.
147. The discussion of Lerner's contribution to modernization theory is taken from Daniel Lerner, *The Passing of Traditional Society: Modernizing the Middle East* (Glencoe, Il: The Free Press, 1958), passim.
148. Lerner also applied an interview methodology to issues associated with the impact of nuclear deterrence on relations in NATO. See Daniel Lerner and Morton Gorden, *European and Atlantic Security in the World Arena* (Cambridge, MA: MIT Center for International Studies, no date supplied.) For analysis of the difficulties associated with the interview methodology, see Leonard W. Doob, "Just a Few of the Presuppositions and Perplexities Confronting Social Psychological Research in Developing Countries," *Journal of Social Issues* 24 (April 1968), 71–81.
149. While economic development may reduce the salience of class cleavages for instability, it may sharpen the salience of ethnic division as a source for instability. This is especially likely if public sector jobs are assigned on the basis of ethnic or other particularistic criteria. See Paul Collier, *Wars, Guns and Votes: Democracy in Dangerous Places* (New York: Harper Collins, 2009), 178, 179.
150. Daniel Lerner and Richard D. Robinson, "Swords into Ploughshares: The Turkish Army as a Modernizing Force," *World Politics* 13 (October 1960): 19–44. Throughout the modernization literature, Turkey served as a virtual poster child for the positive role the military could play in facilitating modernization.

151. For a reminder of how reversible that separation proved to be, see Stephen Kinzer, "Triumphant Turkey," *The New York Review of Books* LVIII (August 18, 2011): 37–40.
152. Lerner, *The Passing of Traditional Society*, 45.
153. Alex Inkeles, "Making Men Modern: On the Causes and Consequences of Individual Change in Six Developing Countries," *American Journal of Sociology* 75 (September 1969): 210, 218.
154. Inkeles, 213, 214, 211, 212.
155. Engerman, 2009.
156. Blackmer, 119.
157. Kenneth E. Boulding, "The Intellectual Framework of Bad Political Advice," *Virginia Quarterly Review* 47 (Autumn 1971), 602.
158. Max F. Millikan and W.W. Rostow, *A Proposal: Key to an Effective Policy* (New York: Harper Brothers, 1957), 1, 114. Some aspects of the analysis contained in this book appeared in earlier Rostow publications. See W.W. Rostow, "The Take-Off into Self-Sustained Growth," *The Economic Journal* 66 (March 1956) 25–48.
159. David E. Lilienthal, "Another 'Korea' in the Making?" *Collier's* 23 (August 4, 1951): 23, 56–58.
160. Millikan and Rostow, 129.
161. W.W. Rostow, *The Stages of Economic Growth: A Non-Communist Manifesto* (Cambridge: Cambridge University Press, 1960), 167. For an effort to apply a stages model to politics that was inspired by Rostow's work see, A.F.K. Organski, *The Stages of Political Development* (New York: Alfred A. Knopf, 1965).
162. W.W. Rostow, *The Stages of Economic Growth*, 1, 2.
163. Rostow, *The Stages of Economic Growth*, 17.
164. Rostow, *The Stages of Economic Growth*, 26, 24 and W.W. Rostow, "Leading Sectors and Take-Off," in *The Economics of Take-Off into Sustained Economic Growth: Proceedings of a Conference Held by the International Economic Association,* ed. W.W. Rostow (New York: St Martin's Press, Inc., 1963), 20.
165. Simon Kuznets in particular voiced this objection to stages. See: Simon Kuznets, "Notes on the Take-Off," in *The Economics of Take-Off into Sustained Growth,* ed. W. W. Rostow, 22–43.
166. Rostow, *The Stages of Economic Growth*, 39.
167. Rostow, "Leading Sectors and Take-Off," 5, 6.
168. Rostow, *The Stages of Economic Growth*, 90.
169. Rostow, *The Stages of Economic Growth*, 36 and W. W. Rostow, *Politics and the Stages of Growth* (Cambridge: Cambridge University Press, 1971), 176.
170. Rostow, "Leading Sectors and Take-Off," 19, 20.

171. Pye, *Aspects of Development*, 54. John Kenneth Galbraith who served in the Kennedy Administration also shared the view that non-economic factors provide an essential precondition for economic development. See Galbraith, 445–446.
172. Rostow, "Leading Sectors and Take-Off," 8.
173. For Huntington's views see, Samuel P. Huntington, "The Change to Change: Modernization, Development and Politics," *Comparative Politics* 3 (April 1971): 283–322; Morgenthau's views on foreign aid were expressed in, Hans Morgenthau, "A Political Theory of Foreign Aid," *The American Political Science Review* 56 (June 1962): 301–309.
174. Quoted in Ron Robin, *The Making of the Cold War Enemy: Culture and Politics in the Military-Intellectual Complex* (Princeton: Princeton University Press, 2001), 189–190.
175. Immanuel Wallerstein, *The Capitalist World Economy*, (Cambridge: Cambridge University Press, 1979), 132–134.
176. W.W. Rostow, "The Challenge of Democracy in Developing Nations," *The Department of State Bulletin* 50 (February 17, 1964): 257.
177. Max Millikan and W.W. Rostow, "Notes on Foreign Economic Policy," in *Universities and Empire: Money and Politics in the Social Sciences,* ed. Christopher Simpson, (New York: The New Press, 1998), 44.
178. W.W. Rostow, *The Stages of Economic Growth*, 2nd edition (Cambridge: Cambridge University Press, 1971), ix.
179. Henry Rosovsky, "The Take-Off into Sustained Controversy," *The Journal of Economic History* 25 (June 1965): 275.
180. Frederick Cooper and Randall Packard, "Introduction," in *International Development and the Social Sciences: Essays on the History and Politics of Knowledge,* eds. Frederick Cooper and Randall Packard (Berkeley: University of California Press, 1997), 15.
181. Stephen Toulmin comes to a contrary conclusion concerning the impact of viewing society as a tightly interrelated system and says that change in one component will not automatically lead the change in others, hence social change requires rejecting the whole system and starting fresh. Toulmin, 129.
182. Millikan and Rostow, 131.
183. Rostow, *The Stages of Economic Growth* 2nd edition, 172–174.
184. Bator, et al., 5.
185. Kimber Charles Pearce, *Rostow, Kennedy and the Rhetoric of Foreign Aid* (East Lansing, MI: Michigan State University Press, 2001), 16.
186. John F. Kennedy, "A Democrat Looks at Foreign Policy," *Foreign Affairs* 36 (October 1957): 54, 59.
187. Packenham, *Liberal American and the Third World: Political Development Ideas in Foreign Aid and Social Science,* 49–59.

188. Roxborough, 756.
189. Verba, 529.
190. Lucian W. Pye, "Political Science and the Crisis of Authoritarianism," *American Political Science Review* 84 (March 1990): 4.
191. Albert O. Hirschman, "The Search for Paradigms as a Hindrance to Understanding," *World Politics* 22 (April 1970): 329–343.
192. Toulmin, 385, 390, 382.
193. Streit and Streit, 62.

CHAPTER 5

Theory Meets Practice: The Case of the Vietnam War

Any assessment of the impact of social science theory on strategy in the case of the Vietnam War must necessarily address two of the key themes found throughout the vast literature on the war. The first theme in the literature is an attempt to identify the particular policy-maker most influential in the development of strategy. As James McAlister put it, the literature on Vietnam can often be reduced to "the search for blame."[1] For example, some scholars portray Lyndon Johnson as a reluctant war leader pushed into choosing escalation by his advisors. Geoffrey Warner is notable in this regard for the extent to which he suggests that President Johnson was a passive pawn in the hands of hawk-like advisors. Indeed, he concludes that without the advice from these men, Johnson's policy toward Vietnam might have been different. Lloyd Gardner is another scholar who shares Warner's view. Other scholars like David Milne single out particular advisors for having an outsized influence on Vietnam policy. Thus, Milne asserts that while Robert McNamara, McGeorge Bundy and Dean Rusk were "managers" of policy, they were never its creators. Rather, it was Walt Rostow who supplied the ideas and blueprint for victory. Andrew Preston notes that McGeorge Bundy can easily be construed as a hawk because he had made a case for a preventive strike against China's nuclear program.[2] Yet Preston goes on to say that both extreme interpretations of Bundy, either as the architect behind the strategy or as a closet dove, are incorrect. Rather, Bundy was merely obeying the direction of the two presidents he served, John Kennedy and Lyndon Johnson.

J. M. Klinger, *Social Science and National Security Policy*,
https://doi.org/10.1007/978-3-030-11251-6_5

Yet such efforts to attribute Vietnam strategy to any single individual is problematic at best, in part, because an individual's real views, though expressed in private, might never appear in the documentary record. This issue is best highlighted by the example of John McNaughton who served as Robert McNamara's deputy. While McNaughton wrote memos to support the bombing campaign and was, therefore, assumed to be an administration hawk, that view is inaccurate. Daniel Ellsberg, who worked for McNaughton, pointed out that official documents are drafted with an eye toward satisfying one's superior and do not necessarily reflect the views of the author. Ellsberg argues that such was the case with McNaughton.[3] Moreover, the hardline memos attributed to McNaughton in *The Pentagon Papers* date from his first year as McNamara's deputy, a time when he would likely be most sensitive of the need to please his boss. A subsequent discovery of McNaughton's personal diary documents his consistent opposition to the conduct of the war and his repeated efforts to convince Secretary of Defense McNamara of the folly of the war and the need for a US withdrawal. McNaughton was, however, careful not to contradict McNamara publicly in meetings.[4] McNaughton's diary entry of April 4, 1966, specified that the near anarchy in the South Vietnamese government at that time would justify American withdrawal.[5] Compounding the difficulty of separating views expressed privately from those expressed in public documents, is the fact that by virtue of culling through material to be passed to a superior, a subordinates' own views might easily be obscured. John Prados believed this to be the case with McGeorge Bundy, and that it created sufficient ambiguity so that historians are able to draw diametrically opposed conclusions concerning Bundy's actual views.[6]

A corollary to the search for blame is the rather sterile debate concerning what might have happened had John Kennedy lived. What might have been in this case is difficult, if not impossible, to ascertain because the record of President Kennedy's views and actions is so mixed. As senator, Kennedy had visited Indochina in 1951 when it was still a colony of France, and the congressional record expressed his views on Vietnam in 1954:

> The hard truth of the matter is…that without the whole hearted support of the people of the associated states, without a reliable and crusading native army with a dependable officer corps, a military victory, even with American support, in that area is a difficult if not impossible achievement.[7]

Perhaps the most famous and often quoted support for the view that President Kennedy would not have escalated the war in the manner that Lyndon Johnson did, is Kennedy's interview with Walter Cronkite on September 2, 1963. In that conversation, President Kennedy suggestively said, "In the final analysis, it is their war. They are the ones who have to win it or lose it." What is more, according to presidential aide Kenneth O'Donnell, President Kennedy told Senator Mike Mansfield that it would not be politically feasible to withdraw from Vietnam until after his reelection in 1964.[8]

Yet Kennedy's statements are difficult to reconcile with his actions. For example, he replaced the Military Assistance Advisory Group (MAAG) established by President Truman with the larger Military Assistance Command, Vietnam (MACV), which expanded the American role from merely advising to training. Kennedy also authorized the augmentation of military advisors to 16,000 upon the recommendation in the trip report by Walt Rostow and Maxwell Taylor, even though the augmentation ran against the Geneva Accords. Further, President Kennedy had been warned in a memo dated February 1961 from National Security Council (NSC) staffer, Robert Komer, of the tenuous legal grounds if the United States met South Vietnamese Premier Diem's request for additional funding, because doing so would "probably require circumvention of the Geneva Accords."[9] To be sure, in a memorandum for the record of January 3, 1962, President Kennedy cautioned the Joint Chiefs of Staff (JCS) and the new military commander in Vietnam, Paul Harkins, that he did not want the United States to become "further involved militarily in the area" and "the US military role there was for advice, training and support of the Vietnamese armed forces and not combat."[10] Moreover, according to William Bundy, when serving as assistant secretary of Defense for International Affairs, even when Kennedy was preparing to reduce US advisors, he did so because he thought the South Vietnamese were growing in strength and not because he was abandoning the commitment. Such preparation was feasible because, as Arthur Schlesinger reports, in 1962, there was some sense of success demonstrated by the fact that the Viet Cong attacks against provincial capitals had stopped. The end of the attacks led Secretary McNamara to declare that the United States was winning the war.[11]

Events in Laos illustrate the conundrum faced by Kennedy that contributed to the ambiguity of his actions in Vietnam. Laos dominated Kennedy's East Asian foreign policy concerns during the first two months of his administration. There, the president sought a political rather than a

military solution that would steer a course between intervention and retreat via a neutralization agreement. In order for successful negotiations on neutralization of Laos, the United States would have to signal its willingness to use force with preliminary moves like putting the task force on Okinawa on alert and sending the Seventh Fleet to the Gulf of Siam.[12] Discussions in the National Security Council also considered sending troops to the Mekong Valley, not to fight but rather to deter the communists and act as a bargaining chip for an international conference. Indeed, Roger Hilsman believed that the landing of American troops in Thailand and the threat it conveyed convinced the communist powers that a political solution for Laos was in their best interest.[13] The Laotion problem opened up a fissure between military and civilian strategists. While Walt Rostow supported a restrictive commitment, the Joint Chiefs opposed it. According to Schlesinger, the Chief's recommendation "was for all or nothing: either go in large scale with 60,000 soldiers, air cover and nuclear weapons, or else stay out."[14]

At best, by taking small steps to support the South Vietnamese, even if intended as temporary until the 1964 election, Kennedy was strengthening a commitment that would make any American withdrawal more difficult and painful. For once American troops became targets of Viet Cong actions, it would be naïve to believe an American withdrawal would be feasible after the 1964 election. A number of observers doubt whether President Kennedy's actions could have differed much from those chosen by his successor because Kennedy faced pressure to demonstrate resolve in Vietnam in response to Nikita Khrushchev's actions in Berlin. While President Kennedy recognized that sending additional troops to Vietnam was likely to lead to demands for more, he believed he could not afford to appear weak. Therefore, as Lloyd Gardner concluded, "a desire to redeem the Bay of Pigs, to stay clear of Laotion entanglements, and to stand fast in Berlin, all drive the New Frontier in the direction of Vietnam."[15] Leaving aside Kennedy's mixed record on Vietnam, one difference between him and Lyndon Johnson concerned their views on the Third World. David Kaiser identifies a major shift between the two presidents and suggests that Kennedy was more willing to tolerate neutrality in the Cold War in developing countries. In contrast, Johnson was more rigid and his approach to the developing world led to a deterioration of American relations with important neutrals like Egypt, India and Indonesia.[16]

Although this chapter will not attempt to gauge the relative weight of the influence of various policy-makers or address the "what if " question,

we do recognize that individual personalities helped shape decisions. Thus, for example, Robert McNamara's well-known preference for scientific, quantitative measures certainly reinforced a reliance on social scientific theories. Also, any difference between Kennedy's policies and Johnson's may well have been affected by Johnson's insecurity and distrust of the advisors he inherited from Kennedy. Such elements are not the central part of this narrative—although the search for blame does include the differences between military and civilian strategists and this difference will be addressed later in the chapter.

The second pervasive theme in the Vietnam literature concerns the debate over the character of the war. Was this simply another version of earlier conventional wars and, like Korea, the result of North Vietnam's aggression? Or was this war really an internal rebellion growing from the weak and illegitimate government in the south? The debate over the character of the war is frequently couched in terms of "the big war" and "the other war." How one answers the question concerning the character of the war, shapes any judgment about the appropriate strategy for the war and the theoretical underpinnings for that strategy as well as conclusions concerning the reasons the strategy failed.

For convenience, the Vietnam literature can be divided into two broad schools of thought on the character of the war. One school, best represented by Harry Summers in his book, *On Strategy: A Critical Analysis of the Vietnam War*, argued that Vietnam was similar to any conventional war and had to be fought as such. Summers believed that the United States failed to recognize this and hence did not mobilize people at home or apply the necessary force abroad. Implicit in Summers' view is the idea that properly applied coercion was necessary. These failures accounted for the outcome of the war. A second school, best represented by Andrew Krepinevich in his book, *The Army in Vietnam*, characterized the war as a classic insurgency against an unpopular government. For him, the strategy failed because it overemphasized the so-called big war and did not take into account the political character of the war. Greater attention, he argues, should have been paid to securing the support of the people through the pacification techniques of nation-building. Behind Krepinevich's analysis lies a view of the need to enhance economic and political development.

The debate over the nature of the war is mirrored in evaluations of two MACV commanders: General William Westmoreland (1964–1968) and General Craighton Abrams (1968–1972). To the counterinsurgency

school focused on the other war, Westmoreland is viewed as too inflexible and wedded to conventional war doctrine that emphasized overwhelming fire power in search and destroy missions to annihilate the enemy. In contrast, Abrams is seen more positively as a "free thinking" soldier who understood the political nature of the war and pursued an approach to win the support of the people.[17] Scholars increasingly recognize that both kinds of war were present and that the real problem with American strategy lay in the lack of coordination between the conduct of the two.[18] From the standpoint of the present chapter, whichever way the war is conceived, it made it amenable to being framed in terms of either deterrence/coercion theory or modernization theory.

Although the Vietnam War strategy is most closely associated with the Kennedy-Johnson Administrations, the US involvement goes back farther and was a problem inherited from previous administrations. So a few observations about this heritage are in order. As with most of the colonial world, World War II helped unleash the forces of nationalism in Southeast Asia. Japanese occupation provided Ho Chi Minh and the Viet Minh leaders with the opportunity to assert their leadership through their resistance against the Japanese while working with the American Office of Strategic Services (OSS). At the same time, the French Vichyite regime in Saigon was collaborating with the Japanese. In the immediate postwar period, the United States enjoyed some good will on the part of the Viet Minh who were counting on the United States to pressure France into granting independence. According to Chester Cooper, who was a member of the American delegation to the 1954 Geneva Conference on Indochina, this was a lost opportunity that would not come again.[19] Instead of granting independence, the French reestablished colonial control and as one observer put it, acted "as if nothing had happened in Indochina between May 1940 and August 1945." A paper prepared by the Army's Special Operations Research Office went so far as to suggest that the economic and social conditions were not sufficient in themselves to generate revolution. Rather, it was the French unwillingness to meet demands for wider political participation that set Vietnam on its revolutionary course.[20]

From the American point of view, however distasteful the French policy in Indochina was, that issue was viewed as subordinate to the more important one of gaining French support in Europe for NATO and the European Economic Community. Indeed, in an interview in 1969, Dean Acheson, President Truman's Secretary of State, noted that the American assistance to France in Indochina grew from the need for their support in Europe.

Acheson observed that "The French blackmailed us. At every meeting when we asked them for greater effort in Europe, they brought up Indochina…"[21] As the Cold War hardened in the wake of Mao Tse-Tung's victory in China in 1949, and the onset of the Korean War in June 1950, the Truman administration believed it had little leeway for withdrawing its support for France in what came to be seen as an effort to halt the spread of communism. Therefore, in June 1950, President Truman established the Military Assistance Advisory Group (MAAG) in Saigon. American assistance to France was substantial. Between 1950 and 1954, the United States supplied between 75 and 80 percent of the cost of France's Indochina war. In addition, the United States supplied the Vietnamese government of Bao Dai with $126 million in direct aid.[22] What is more, a new generation of leaders including John Kennedy and Lyndon Johnson learned a lesson from the way that Truman and Acheson were excoriated for the failure in their Asian policies.[23] Ultimately, however, the United States under President Eisenhower's administration decided not to bail out the French at the battle of Dien Bien Phu. Both Admiral Radford and General Ridgway opposed the use of American ground troops to assist the French. And General Ridgway, in a memo to the Joint Chiefs, presciently argued that air power alone would not win the war and deploying American troops would be "a dangerous strategic diversion of limited US military capabilities…in a non-decisive theater to the attainment of non-decisive local objectives."[24] Although Eisenhower was under pressure to intervene to save France, congressional opinion against intervention was strong. Somewhat ironically, Senator Lyndon Johnson played a central role in the demise of the air strike option to save France.[25]

The French defeat at Dien Bien Phu opened the path to negotiations in Geneva, beginning in April 1954 and concluding in July of that year. Among other things, the Geneva agreement separated Indochina into the separate states of Laos, Cambodia and Vietnam. Laos and Cambodia were granted independence, and Laos was provided with neutrality prohibiting external domination. The agreement provided for a temporary division of Vietnam at the 17th parallel that was to "settle military questions with a view to ending the hostilities. . .the military demarcation line is provisional and should not in any way be interpreted as constituting a political or territorial boundary." National elections under international supervision were to be held in 1956. A National Intelligence Estimate dated August 1954 gave the South Vietnamese regime's chances of survival under the leadership of Bao Dai a "poor" rating. Bao Dai was chosen by default

because at the time, there did not appear to be an alternative. Bao Dai, in turn, chose Ngo Dinh Diem as his prime minister because he believed that the fact that Diem had lived in the United States for two years would mean he would be effective at channeling American aid.[26]

Despite the fact that President Eisenhower did not send military assistance to relieve the French at Dien Bien Phu, he did decide to hold the line against communism by bolstering the South Vietnamese sufficiently to buy time for it to strengthen its government as an alternative to communism. President Eisenhower began with the enunciation of the "falling domino" principle at a news conference in April 1954—even before the conclusion of the Geneva Conference—that suggested a certain inevitable momentum to the spread of communism in Southeast Asia. President Eisenhower countenanced military intervention only under limiting conditions, that is, if such intervention was multinational and included Asian participation. The Southeast Asia Treaty Organization (SEATO) was created in September 1954, and was a central component of Eisenhower's post-Geneva Indochina policy.[27] The extent to which the SEATO agreement committed the United States to defend South Vietnam has been hotly contested. William Bundy asserted that the treaty "created a new and serious obligation extending to South Vietnam." In contrast, Chester Cooper, who participated in the Manila Conference that established SEATO, suggested that rationalizing American intervention in terms of the SEATO agreement "was nonsense."[28] Be that as it may, in the spring of 1955, the White House announced "at the request of the government of Vietnam with the agreement of the government of France, [the United States] had undertaken responsibility for the training of Vietnam national armed forces."[29]

The Eisenhower Administration also saw the first use of social scientists in Vietnam—albeit more for operational work than for providing a theoretical framework for strategy. Thus, Wesley Fishel, a political scientist from Michigan State University (MSU), went to Vietnam in August 1954 to aid the Diem government. The State Department hired Fishel as a consultant, and other MSU faculty followed, serving as instructors for public and police administration. Although these may not have been the most pressing problems facing the Vietnamese government, the presence of MSU made addressing them fairly easy. The International Cooperation Administration (ICA) contracted with MSU for technical and government training from 1955 to 1962. Although the work of MSU contractors in some sense aimed at nation-building, once the group helped establish an

effective police force, Diem used the force as a tool to eliminate his political opponents so that, by 1956, he had arrested and imprisoned 20,000 people. Evidence of the extent to which Diem militarized the police force is the fact that in 1958, a total of 36 percent of province police chiefs were military officers, and by 1960, a total of 87 percent were military officers. In addition, when members of the MSU group criticized Diem publicly, he canceled their contract.[30] Diem's actions here foreshadowed other problems the Americans would encounter working with Diem. In the end, the Eisenhower administration deepened American involvement in Vietnam so that these small steps, in the words of one observer, merely "postponed the day of reckoning in Vietnam."[31]

President Eisenhower may only have set the stage for the deeper involvement in Vietnam, but during the Kennedy-Johnson years, there remained a contrast with Kennedy's approach to Vietnam and US foreign policy that constitutes a sharp break with Eisenhower. David Anderson's final judgment on Eisenhower suggests just such a contrast with Kennedy: "Eisenhower and his advisors made some tactical adjustments to the growing complexity of the world, but their goals remained fixed to an unremitting anti-communism grounded in the moral and material preeminence of America in 1945."[32] Although this chapter stresses the discontinuity between Eisenhower's and Kennedy's policies, some observers saw some continuity to argue that ideas on military strategy and economic development that "blossomed" during the Kennedy presidency had in fact been "incubating" during the Eisenhower administration.[33]

In Chap. 4 we have already noted their differing views concerning aid. Here, we will add that during the eight years of the Eisenhower administration, the International Cooperation Administration (ICA)—the agency charged with managing the technical assistance program—had eight different chiefs, and White House historian Arthur Schlesinger noted that one of these did not believe in foreign aid and had voted against it in the Congress.[34] Indeed, the fact that President Eisenhower, as a fiscal conservative was unresponsive to calls for increased foreign aid, prompted Massachusetts Institute of Technology (MIT) professors Max Millikan and Walt Rostow to write their book on behalf of aid, *A Proposal: Key to an Effective Foreign Policy*, in 1957 that brought Rostow's ideas to the attention of Senator Kennedy. As senator, Kennedy had first met Rostow at the 1956 democratic convention and wrote to Rostow's brother Eugene, that he had "enjoyed and profited from Walt's advice" on how best to exploit the weakness of Eisenhower's policy toward the developing world.[35] Once he was president, Kennedy

organized a task force to examine foreign economic policy, and the task force reported that for fiscal 1960, three-quarters of aid funds went for short-term political and military purposes. In 1961, four-fifths of the aid was allocated in this way. Given such findings, President Kennedy acted to reorganize the aid effort because of his belief that foreign aid needed to serve more than the negative purpose of stopping communism. Rather the goal needed to be broadened and work to improve social and economic conditions in the developing world.[36] The Peace Corps and the Alliance for Progress were two concrete manifestations of this belief. What is more, Kennedy's Secretary of Defense Robert McNamara testified to the Senate Foreign Relations Committee in 1961 of the need for US aid to foster economic progress.[37]

President Kennedy's break with Eisenhower went beyond the issue of foreign aid to include a more staunchly anti-colonial point of view. The Eisenhower administration chose to abstain from the UN General Assembly vote on the 1960 resolution that declared the "right" of colonial countries to independence.[38] President Eisenhower also criticized Senator Kennedy for his 1957 speech on Algeria that faulted France for not granting independence to that country sooner. Eisenhower characterized the speech as irresponsible because the attack had the potential to weaken the NATO alliance. The post-colonial countries, however, found the Kennedy approach appealing, and the view that Kennedy would accept neutralism in the Third World was solidified once, as president, Kennedy agreed to the neutralization of Laos.[39] Perhaps the divergence in their views of colonialism reflected their different experiences. Eisenhower had been, after all, the general that helped lead the allies to victory in World War II, and it should not be surprising that his focus would remain in Europe. Kennedy seemed more conscious of the tectonic shift in world politics to nationalism as a result of the war that required greater recognition of its impact on colonial countries.

Given their contrasting views and experiences, it is not surprising that Kennedy's ideas about military strategy would be consistent with his interest in the Third World and vary from those of Eisenhower. Most notable in this regard was the shift away from the massive retaliation doctrine that relied on nuclear weapons, to a focus on limited war. Theorists and policy-makers alike began to see that the nuclear standoff between the West and the Communist powers meant that the latter were less likely to attack the West directly but would "nibble" the United States to death in local wars on the periphery. Certainly, Nikita Khrushchev's pledge in January 1961 to aid in wars of

National Liberation raised this concern within the Kennedy Administration. Indeed, Secretary of Defense McNamara suggested as much in his June 1961 testimony to the Senate Foreign Relations Committee.[40] Within such a context, analysis of limited war gained greater urgency.

At the heart of ideas associated with limited war lay counterinsurgency which provides a military corollary to modernization theory. One first step for reorienting the military toward counterinsurgency was to bring Maxwell Taylor out of retirement. Taylor was initially brought back to investigate what had gone wrong with the Bay of Pigs operation. Taylor was the perfect choice to help shift the military toward counterinsurgency because he had retired from the post of Army Chief of Staff to protest Eisenhower's emphasis on the Air Force embodied in the massive retaliation doctrine.[41] Despite Taylor's role in fostering "flexible response" as an alternative to massive retaliation and the claims made by Walt Rostow, the real impetus behind the push for counterinsurgency lay with Kennedy himself.[42] Indeed, a participant in changes at the Defense Department during the Kennedy era, Seymour Deitchman dedicated his 1964 book, *Limited War and American Defense Policy*,[43] to JFK "who created radical new directions in US Defense policy with respect to limited war." President Kennedy outlined his view of counterinsurgency to the West Point graduating class of 1962:

> This is another type of war, new in its intensity, ancient in its origins— war by guerrillas, subversives, insurgents, assassins; war by ambush instead of combat; by infiltration, instead of aggression, seeking victory by eroding and exhausting the enemy instead of engaging him…It requires in those situations where we must counter it…a wholly different kind of force, and therefore a new and wholly different kind of military training.[44]

To meet the challenge of this style of war, President Kennedy pushed the Pentagon to focus attention on guerrilla war that led to the upgrading of Special Forces training at Fort Bragg, North Carolina. At the president's urging, the Department of Defense increased its research on the subject, from $10 million in fiscal 1960 to nearly $160 million by 1966.[45] In addition, the Army's Special Operations Research Office (SORO) attached to American University expanded its research on foreign areas and revolutionary war.[46] In 1961, Advanced Research Project Agency's (ARPA's) "Project Agile" was established to perform research on counterinsurgency, and that research was "largely to support American activities

in Southeast Asia."[47] The research fostered as a result of President Kennedy's efforts carried over into the Johnson Administration as well and the military remained interested in the subject. For instance, in March 1965, the Defense Department's Defense Science Board directed the Army to develop a plan for a coordinated program of applied behavioral and social science research in support of counterinsurgency.[48] What is more, Lieutenant General W.W. Dick, Jr., Department of the Army Chief of Research and Development, underscored the importance of social science's contribution to counterinsurgency in his testimony to the US House of Representatives in July 1965. In fact, David Kilcullen has since noted that counterinsurgency itself is nothing more than "armed social science."[49]

Another sharp distinction between Kennedy and Eisenhower revolved around the former's interest in the ideas of academics. Kennedy's attraction to academics was natural considering the fact that as senator, he was accustomed to consulting with the so-called Charles River economists. Roger Hilsman reinforced the point concerning Kennedy's interest in ideas and theories, especially "when the ideas had some practical consequences…if they could make it possible to shape the world, to accomplish something."[50] Therefore, both modernization theory and deterrence/coercion theory would naturally resonate with Kennedy as a way to frame policy choices.

To take full advantage of social science theory, President Kennedy brought academics like Walt Rostow into his administration. His Secretary of Defense, Robert McNamara, recruited a number of RAND academics into the department, including Charles Hitch, Alain C. Enthoven and Daniel Ellsberg. McNamara's intent was to draw on RAND personnel in his effort to rationalize defense management, but the group also engaged in wide-ranging reviews of general war, limited war that left RAND's stamp on counterinsurgency.[51] RAND research made important contributions to insurgency and counterinsurgency reflected in well-known studies of Viet Cong motivation and morale and an attempt to outline a comprehensive theory of insurgency studies that will be discussed in detail later. Further, with his appointment of McGeorge Bundy, a former Harvard Dean, as his national security advisor, President Kennedy inaugurated changes in the national security bureaucracy. Whereas in earlier administrations, the National Security Advisor (NSA) was almost exclusively an administrative position, Bundy elevated the status of the position to "virtually if not officially, equivalent to that of a cabinet secretary." In the end, the changes to the NSA allowed President Kennedy to rely more on the

National Security Advisor and his staff at the expense of the traditional foreign policy bureaucracies. Under Bundy's leadership, the NSC staff began to gain power at the expense of the National Security Council. Consequently, Bundy's staff "could outmaneuver the large unwieldy State Department" so as to marginalize its role in shaping the course of Vietnam strategy.[52]

Beyond the aforementioned differences between President Eisenhower and President Kennedy, each president faced different circumstances in Southeast Asia. Vietnam was still a colony of France when Eisenhower became president, so there was some logic to providing support to France—a key Western ally—in its efforts in Indochina. Even after the conclusion of the Geneva agreement in 1954, a case could be made for supporting an effort to find an alternative government to that offered by the communists. However, the United States during this time relied on Vietnamese leaders that were not up to the task of creating a viable alternative to Ho Chi Minh. Indeed, there were early warnings that effective leadership was lacking. Thus, for example, Robert McClintock, who had served as counselor in Saigon from 1953 through 1954, warned that reliance on Bao Dai was foolish and immoral, and he characterized Diem as "a messiah without a message." In 1954, when the Eisenhower Administration asked the embassy in Saigon about grass-roots support for Diem, the embassy reported that the only leader with grass-roots support was Ho Chi Minh.[53] Furthermore, a National Intelligence Estimate of August 1954 expected that if elections were held, the Viet Minh were certain to win.[54] However, RAND disputed this view of Diem's leadership at a symposium held in 1962. There, Diem was given credit for taking the issue of independence away from the Viet Minh and thereby demonstrating his independence from France.[55]

Although not known at the time, the Fifteenth Plenum of the Central Committee in Hanoi decided at the end of the Eisenhower Administration in 1959, to create a limited number of armed units in the south according to the slogan, "political struggle mixed to the right degree with armed struggle," with a decision, in principle, to begin the armed struggle at some future date.[56] Such a slogan suggested a cautious approach on the part of the North Vietnamese that might mean a modest military effort by the south would be effective in halting the insurgency. However, by the time the Kennedy Administration took office, Hanoi's position on the armed struggle in the south had hardened. The Vietnamese communist party revealed in a history published in 1970 that disagreement over strategy in the south ended by 1960 when preparations for an armed struggle

were launched.[57] Consequently, President Kennedy faced a more serious military challenge than the one Eisenhower had faced in the 1950s. Besides inheriting an imprecise commitment to Vietnam, the United States was linked to the fortunes of Ngo Dinh Diem. In the face of the worsening military situation, President Kennedy received ample warnings concerning the shortcomings of the Saigon government and its leaders. Most important of these warnings came in a memo Vice President Lyndon Johnson had attached to his 1961 trip report. The vice president warned that there was a real danger that the Saigon government would become a "glittering façade" and that:

> [i]t will come to rest in the end, not on its people, but on a modern military establishment and an oriental bureaucracy both maintained for the indefinite future by the United States Treasury. The power which is inherent in the ordinary Vietnamese people will be left to others to organize. The ordinary people of Vietnam, starved for leadership with understanding and warmth, would respond with great enthusiasm. But it cannot be evoked by men in white linen suits whose contact with the ordinary people is largely through the rolled up windows of a Mercedes-Benz.[58]

President Kennedy also received a warning in 1962 from Senator Mike Mansfield that Diem had done little to broaden his base of support and the Saigon government was more dependent on the United States than it had been five years earlier. Similarly, Chester Cooper reported on his 1963 trip to Vietnam that he "was disturbed not only by the Kafkaesque sessions with Nhu and Diem, but even more of my conviction that they were divorced from what was going on outside of Saigon."[59] As if these warnings were not enough, Daniel Ellsberg reports that his work for the McNamara study, which would become known as the *Pentagon Papers*, focused on documents produced in 1961. In his research, Ellsberg says that "not a single one of Kennedy's military or civilian advisors had told him that the program of advisors and support units he announced in mid-November would be adequate to stop the deteriorating trend in South Vietnam, even in the short run, let alone bring ultimate success."[60]

Given these doubts about the South Vietnamese leaders and conditions in the country, a continued American involvement there was not a forgone conclusion. However, two factors seem to have influenced policy-makers to pursue involvement. First, the generation of leaders in the Kennedy-Johnson Administrations was shaped by the failures of isolationism and

appeasement during the interwar years. The success of the allies in World War II and American success in rebuilding Europe and Japan after the war, laid a foundation of self-confidence concerning prospects for social engineering that formed an important context for foreign policy in the early Cold War. Second, what better way to guide that social engineering than with social science that had contributed to the war and appeared so appropriate for dealing with the novel conditions that had emerged. Both deterrence/coercion theory and modernization theory offered guidelines to policy-makers who grappled with the problem of Vietnam.

From the standpoint of operationalizing theory, which was the most relevant and where strategic emphasis should lay, depended on how the character of the war was defined. If the war was conceived as a civil war prompted by ineffective governance in Saigon, then modernization theory provided a framework for pacification. For the most part, civilian strategists defined the war in this way.[61] In fact, McGeorge Bundy, after returning from a trip to Vietnam at the time of the attack on Camp Halloway at Pleiku in 1965, described in the following terms, "the current situations among non-communist forces gives all the appearance of a civil war within a civil war."[62] As an exception, Walt Rostow defined the war as closer to a conventional one driven by aggression from the North and by so doing implied that deterrence/coercion theory might be more useful. Rostow asserted that "the operation run from Hanoi is as certain as a form of aggression as the violation of the 38th parallel by the North Korean armies in June 1950."[63] In 1966, Maxwell Taylor reinforced Rostow's view in his testimony before the senate. When asked directly if the war was a civil war, he responded in the negative: "We have indeed a foreign aggression supported from Hanoi."[64] Once the National Liberation Front (NLF) was cast as agents of a "foreign country," any decision for US withdrawal became more difficult.

How one defined the character of the war, of course, depended in turn on the exact nature of the role played by the North in the insurgency in the South. Leaving aside the fact that the Geneva agreement explicitly stated that division of the country at the 17th parallel did not constitute a political or territorial boundary, so that the North could hardly be described as a "foreign" aggressor, the initial infiltration from the North was negligible during the 1950s. According to data from MACV infiltration studies, infiltration from the North was negligible from 1954 to 1956, and then increased slightly from 1956 to 1959. MACV saw a substantial increase in 1960, which we now know corresponds to the hardening of Hanoi's policy.

From June through December 1961, MACV estimated that 500 to 1000 troops a month went south, with totals estimated to range from 3100 to 6200. For the first half of 1962, a total estimate of infiltration ran between 2000 and 2600. What is more, these troops possessed military training and technical skills from earlier experience with guerrilla war to "ensure the high quality, morale and discipline of fighting units."[65] Roger Hilsman emphasized that the modest number of infiltrators included primarily leaders, with the rank and file recruited in the south. He also goes on to note, "One of the most significant facts of all about the use of infiltration routes was that the cadres that were sent over were all *South Vietnamese* (italics in the original). Ninety thousand communist or pro-communist southerners had taken the opportunity afforded by the Geneva Accords of 1954 to go North, and it was from among these that the infiltrating cadres were picked."[66]

If the rate of infiltration was insufficient to determine whether the United States should focus on the "big war" closely associated with deterrence/coercion theory, or the "other war" drawing more on modernization theory, could the morale within the NLF provide indications of the optimal strategy to pursue? RAND Corporation sought to answer this with studies on the morale and motivation of the Viet Cong. RAND modeled its studies on one conducted by Lucian Pye that interviewed "surrendered enemy personnel" in Malaya to ascertain why people joined that insurgency. In fact, RAND tried unsuccessfully to recruit Pye to lead research on the Viet Cong. Mai Elliott believes that RAND had the most direct influence on Vietnam policy from 1965 through 1967, and its most extensive research was the morale and motivation studies.[67] In Vietnam, Diem had refused researchers' access to captives and defectors, so no study could begin until November 1963 after Diem was removed from power by a coup.

What is most notable about the motivation and morale studies is the extent to which their findings varied under the authorship of different people. The first study, conducted from July through December 1964 by J.C. Donnell, Guy J. Pauker and Joseph Zasloff, drew on interviews of 145 Prisoners of War (POWs) defectors and suspects. Although the authors admitted their sample was small, they believed it to be representative of attitudes throughout the Viet Cong. The authors concluded that the VC were motivated more by nationalist idealism than socialism or communism, and the number of defectors could be increased if the South Vietnamese government improved its treatment of prisoners because defectors were

not motivated by ideology but rather driven by personal reasons like separation from families. Moreover, the study found the VC to have high morale and were an effective fighting force, in part because of the use of a buddy system that divided squads into three men cells to ensure cohesion.[68] Interestingly, one of the report's authors, Guy Pauker, later recalled that he had warned RAND President Frank Collbohm that, after visiting Vietnam, he thought "that an American involvement would be a mistake, and he wanted to keep RAND out of it."[69] Another of the study's authors said that references to prisoner brutalization by the South Vietnamese government "got sanitized" when the report was publicly released in 1968. Nevertheless, when John McNaughton was briefed on the report's findings in December 1964, he commented that, "we're fighting on the wrong side."[70] Despite this observation, McNaughton did not forward the report to McNamara. Furthermore, according to Mai Elliott, McNaughton was unable to interest General Westmoreland in the study, and neither he nor other top military leaders at MACV were interested in it. Consequently, the task of facilitating and monitoring the study fell to a second lieutenant.[71]

A second study of VC motivation and morale was conducted by Leon Gouré, a Soviet specialist who had endeared himself to the Air Force by advocating increased production of planes and missiles to overcome Soviet civil defense. Even before his trip to Vietnam, Gouré was an advocate of air power as a weapon of counterinsurgency.[72] Not surprisingly, the Gouré study, based on 450 interviews, found that air power was indeed weakening the morale of the VC, causing a decline in their belief that they could win the war without outside support. What is more, Gouré asserted that the American bombing campaign had not alienated the local population from the Vietnamese government or the Americans.[73] Perhaps more significantly, the Gouré report was forwarded to Robert McNamara, and McNaughton cited "prisoner interviews" as showing the bombing campaign was affecting VC morale. McNamara, in turn, used Gouré's finding to reassure Johnson that the bombing was effective.[74]

Seymour Deitchman offered a justification for the fact that the Gouré report received greater attention than the earlier report. He argued the first report focused on why people joined the insurgency and how they maintained group cohesion, while the second one focused on VC reaction to US air and ground operations, topics of greater interest to the military evaluating its strategy. The fact that the Gouré report supported the military point of view—particularly the interests of the Air Force—was apparently coincidental. Deitchman also suggested the different treatments of

the reports was "largely a bureaucratic accident." However, Deitchman does say that social scientists talking to villagers concluded that more was being lost in terms of loyalty and support for the South Vietnamese government and the Americans by shelling villages than was being gained in hurting the Viet Cong.[75]

We might also suggest that Gouré proved to be a more effective policy entrepreneur than his other RAND colleagues. Indeed, Mai Elliott asserted that RAND staff became divided into pro- and anti-Gouré factions because his work "received more than the usual attention accorded RAND work," and that it seemed to be "exercising an influence out of proportion to its intrinsic merits." In fact, one of Gouré's co-authors, Anthony Russo, came to the conclusion that Gouré's analysis was aimed specifically to gain favor with the Air Force. In any event, the high point of Gouré's influence came to an end by 1966, when General Westmoreland expressed doubts about the veracity of Gouré's claims concerning the low state of VC morale. At the same time, Senator Fulbright questioned the integrity of RAND work when Gouré's research came to his attention. The senator wrote to Robert McNamara demanding an investigation of the study's methodology be reported to the Foreign Relations Committee.[76]

So, in the end, RAND's research on morale provided a mixed message as to the character of the conflict in Vietnam and the best strategy to pursue. That mixed message had been foreshadowed by the 1961 report of Eugene Staley that found armed insurgency to be a problem that was inseparable from development. Consequently, the Staley Group recommended that ending the war could be accomplished "only by the prompt application of effective military power, coupled with large-scale economic and social action reaching every part of the country."[77] The battle of Ap Bac in January 1963 confirmed a need for a two-pronged approach, because in the battle, the South Vietnamese forces, who had surrounded the VC with a force ten times as large, did not engage. The battle demonstrated two things to strategists. First, the United States would have to increase its military support to Vietnam. Second, while doing so, it had to encourage reform in the government so that the South Vietnamese army could become a professional one, rather than serve as a source for political patronage.[78] Since greater military effort and nation-building reforms seemed to be required, let us now see how both processes were informed by theory.

Chapter 1 recounted the observation made by one social scientist who worked for Office of War Information (OWI) during World War II.

He claimed, in his experience, social science was used less as a prompt to policy decisions and more as justification for decisions already made. May Elliott confirms as much concerning RAND's research on Vietnam.[79] Consequently, it is difficult to trace a straight line directly from theory to policy decisions or outcomes. We have suggested that Leon Gouré's motivation study was well received because it supported the military point of view, particularly that of the Air Force. Indeed, Daniel Ellsberg dismissed the role that limited war theory played in formulating strategy during the Vietnam War. Rather, at a conference at the Adlai Stevenson Institute in Chicago in 1969, Ellsberg asserted that strategy was based on the widespread belief in the efficacy and acceptability of aerial bombardment that gave policy-makers confidence for pursuing this strategy.[80] In contrast to Ellsberg's view, Stephen Peter Rosen places the blame for the mismanagement of the war on a failure to take into consideration military necessity by conceiving of military force merely as a diplomatic instrument. Such thinking, according to Rosen, grew from a reliance on limited war theory whose origins could be traced back to Thomas Schelling.[81]

Ellsberg's disclaimer aside, social science theory did provide a backdrop to policy. Given the ties between policy-makers in the Kennedy-Johnson administrations and the academics from RAND and other institutions, it was inevitable that the theories under discussion "would seep like water through limestone" into policy. Significantly, when Walt Rostow was moved from the NSC to the State Department's Policy Planning Staff, he gradually built his staff with academics rather than drawing them from the diplomatic service.[82] Thus, Seymour Deitchman reported a comment made by "a world renown physicist" in a briefing to Robert McNamara: "[W]hile World War I might have been considered the chemist's war, and World War II was considered a physicist's war, World War III, which we might already be in, might well have to be considered the social scientist's war." Further, Deitchman, as special assistant for counterinsurgency programs in the Office of Defense Research and Engineering, believed that "many of the important solutions to problems such as we were facing in Vietnam would have to be sought through research in the social sciences."[83]

Chapter 3 has already described in the abstract the work of key theorists of deterrence and coercion. Not all of these were convinced that deterrence in the Cold War required a commitment to South Vietnam.[84] However, Thomas Schelling was one theorist who used the Vietnam War to illustrate his ideas. Schelling too had a direct impact on policy through

his connection to John McNaughton, assistant to Robert McNamara, and to Daniel Ellsberg, who was John McNaughton's assistant. Two interrelated aspects of Schelling's discussion of Vietnam are most salient for understanding the strategy chosen. First is his idea that military force be used as a symbol or signal to an adversary, and second that those signals be implemented through a process of escalation. In the preface of *Arms and Influence*, Schelling expressed uncertainty concerning his approval of the bombing of North Vietnam in 1965. Nevertheless, he conceived of the bombing as "coercive pressure" that was an exercise in the power to hurt. Moreover, he expressed the idea—one we will see in explicit statements by policy-makers—that the intention of the bombing was less the damage it would cause and more the psychological impact it would create by providing the North Vietnamese "the prospect of cumulative losses that were more than the local war was worth, more unattractive than concession, compromise, or limited capitulation."[85] What was novel about this conception of military force was that it was thought possible to get an opponent to capitulate without a decisive victory. No longer did you have to defeat his armed forces to change his behavior; threats and a surgical application of force could do so.

Schelling had outlined a view of military action that stressed its symbolic importance over its military impact as early as 1962. In an article on nuclear strategy for *World Politics*, he asserted that the actual destruction of a particular target was incidental to the message it might convey to the Soviets, so that targets should be selected with a view "to signal our intent and not for their tactical importance."[86] In Vietnam, Schelling characterized the American reprisal against the North after the Tonkin Gulf incident as less important for the damage it caused than for the message it conveyed to North Vietnam and China. Here, we see the divorce of military means from military effects when he wrote:

> Equivalent damage inflicted on the North Vietnamese air force, or its army or its military supply lines would not have carried the same meaning… Equivalent damage on other military resources might have made as much sense militarily, but the symbolism would have been different.[87]

And while he had expressed doubts about the bombing campaign in the North, he did approve of retaliation for the Tonkin Gulf incident.[88]

Even though Schelling characterized military means as "coercive pressure" to send signals to the enemy, he was not sanguine concerning success. Schelling goes on to say, and it is worth quoting at length:

> The fact that nations show a tendency to embody their intent in their actions does not mean that this sort of communication is received and interpreted with a high degree of fidelity…the process of diplomacy by maneuver is typically a good deal clumsier, with actions less subject to careful control for the message they embody, subject to background noise from uncontrollable events, and subject to misinterpretation. Even the Gulf of Tonkin events may not have been as plain to the North Vietnamese at the same time as they were shortly afterward to the Armed Services and Foreign Relations Committees.[89]

Schelling recognized the paradoxical impact of the bombing that while it might reduce the North's support for the Southern insurgency, it also increased their costs of doing so.[90] In fact, Schelling was sufficiently cautious about the coercive effect of the bombing campaign that he warned John McNaughton that if the bombing did not succeed within the first three weeks, it was a hopeless strategy.[91]

Whatever the merits of reducing military actions to diplomatic signals, implicit in the idea of coercive pressure is some notion that signals needed to be implemented through gradual escalation. That is, the threat can be ratcheted up to create in the minds of an opponent a dread that worse would come in the absence of compliance. Herman Kahn had this in mind when he labeled the process Type III deterrence or tit-for-tat, graduated or controlled deterrence where acts are deterred because "the potential aggressor is afraid that the defender…will then take limited actions, military or non-military that will make aggression unprofitable."[92] For Schelling, the larger context of the Cold War rivalries between the United States and the Russians and Chinese shaped the nature of the escalation process in Vietnam. He saw at work in Vietnam a certain brinkmanship with the tacit threat and risk of enlarging the conflict, "evidently meant to intimidate the Chinese and the Russians."[93]

As the process of escalation was operationalized in Vietnam, it found expression in the strategy of graduated pressure, which was an option outlined in in a report prepared by the Vietnam Task Force, headed by William Bundy. The report offered three options to President Johnson: one, to continue with the same program of advisors and training already in place,

second, to rapidly escalate and third, for a graduated response referred to as the "slow squeeze." The signaling function behind it was often stated explicitly by policy-makers. Thus, both McGeorge Bundy and Walt Rostow are on record emphasizing the signaling function behind graduated pressure. Bundy in a memo to President Johnson dated May 1964 stated that "a pound of threat is worth an ounce of action" and that American actions needed to be designed to emphasize their deterrent impact. Rostow echoed Bundy's point in a letter to McNamara in November 1964, where he stressed that the important aspect of bombing was not the damage done but "the signal we wished to send."[94] McNamara himself took up the theme of military action as a form of diplomatic communication and said: "At any time, 'pressure' on the DRV depends not upon the current level of bombing but rather upon the credible threat of future destruction which can be avoided by agreeing to negotiate or agreeing to some settlement in negotiations." Rostow explained that such a strategy would work because "Ho [Chi Minh] has an industrial complex to protect: he is no longer a guerrilla fighter with nothing to lose."[95] Even Maxwell Taylor emphasized the signaling value of the bombing campaign, and as ambassador in Saigon cabled President Johnson in January 1965 that: "It would be in our interest to regulate our attacks not for the purpose of doing maximum physical destruction but for producing maximum stresses in Hanoi minds."[96] An added benefit of the bombing campaign was thought to be the psychological impact it would have on the South Vietnamese that would improve their morale and sustain their resistance against the North.[97]

Choosing the option of graduated pressure opened up a fissure between civilian and military strategists, although Walt Rostow and Maxwell Taylor, at times, were outliers in their respective groups. Indeed, Rostow's persistent recommendation of bombing North Vietnam as early as the crisis in Laos and his more hawkish views generally provided President Kennedy with reasons to move him from the NSC staff to the State Department's Policy Planning Staff in late 1961.[98] Arthur Schlesinger recalled that during the Kennedy Administration, the military was "unrelenting in its opposition to limited intervention." In 1961, the JCS had recommended sending 40,000 troops that prompted President Kennedy to send Maxwell Taylor and Walt Rostow on a fact-finding trip to Vietnam.[99] The JCS continued to hold that perspective in the Johnson administration and favored large-scale bombing up to the border of China. In a memo dated May 1964, the Chiefs argued: "We should not waste critical time and more

resources in another protracted series of 'messages', but rather we should take positive, prompt and meaningful action…"[100] Daniel Ellsberg recorded in his memoirs that the sole controversy concerning Vietnam was not whether to intervene, but rather what form and scale the intervention should take.[101] In closed-door testimony to a senate investigating committee, the Chiefs charged that the bombing had not accomplished all its goals because civilian leaders like McNamara imposed too many restrictions, and General Westmoreland in his memoirs attributed the failure in Vietnam to the fact that Washington tightly controlled the conduct of the war.[102]

At the center of the divide between military and civilian strategists lay differences concerning the very purpose of the bombing campaign. For the civilians, the purpose of the bombing was to hurt the North sufficiently that it would cease supporting the insurgency in the South. McGeorge Bundy stated this explicitly in a paper dated May 1964.[103] Robert McNamara also made the argument upon returning from his trip to Vietnam that the bombing would boost the morale of the government in the South.[104] It was thought that targeting industrial sites in the North would be especially important for shaping their actions. Yet, according to the *Pentagon Papers*, the North had very few lucrative industrial targets and the JCS had only identified eight. Even after lowering their standards at the end of 1965, they could only identify 24 such targets.[105] Of course, the assumption behind the bombing was that there existed some level of punishment which would break the will of the North and lead them to discontinue their support of the insurgency in the South. Policy-makers could reasonably argue for the validity of this assumption because American objectives were limited to protection of the South, and not the overthrow of the North's regime, so that its survival was never at stake.[106]

In contrast to the civilian view, the military saw the purpose of the bombing as less an attack against the will of the North and more for the purpose of reducing their capabilities. Doing so meant focusing targeting on infiltration routes into the South as well as industrial war production sites in the North. Robert Pape has pointed out that air power is most likely to coerce opponents successfully when it targets their capabilities, thereby making it unfeasible for them to pursue their goals with military means. In this way, air power comes closer to a traditional conception of military force as the means to achieve a decisive victory. Pape goes on to describe the Air Force strategy on this score as a "genteel" version of the air power theories of Giulio Douhet.[107] Moreover, Stephen Peter Rosen

asserts that the military had been thinking along the lines of destroying Northern war-making capabilities in 1961 and 1962 before it fell under the sway of limited war theorizing.[108] That military strategists in the end acquiesced to "graduated pressure" reflects the fact that disagreements between the Army and the Air Force prevented them from presenting a unified approach and because they entertained a belief that they would get more forceful military measures once graduated pressure failed.[109] In the end, as Maxwell Taylor testified to the Senate Foreign Relations Committee in February 1966, the bombing campaign was to serve all three purposes: to reassure the South, to hamper the ability of the North to supply the South and "to provide a sobering reminder to the leaders of Hanoi that progressively they must pay a mounting price for the continuation of their support of the Viet Cong insurgency."[110] At best, the differences concerning the purpose of the bombing created ambiguity, if not confusion, concerning the ends that were sought. In fact, Chester Cooper is quoted as saying that there was a policy, but no real plan, "that is, it was not clear what to do after the first attack. . .Despite the plentiful planning of targets and lists, and the frequent inclusion of the bombing 'option' in policy planning, what was not in the plan was exactly how bombing was supposed to lead to an outcome."[111]

In addition to the aforementioned confusion over the purpose were four problems with the bombing campaign from the standpoint of both signaling and escalation. First is the practical point brought out by Schelling himself, that the North Vietnamese may not have been able to feasibly withdraw support or exercise influence over the behavior of the Viet Cong even if they wanted to. Such a withdrawal was especially not possible once the hardliners in Hanoi were in charge of the war for, according to one North Vietnamese intelligence officer who defected in 1967, a total of 200 officials of the party and the government were arrested because of their opposition to the direction of the war.[112] Related to this is the fact that policy-makers assumed that the signal would be received instantaneously and interpreted correctly. This, in turn, required treating the government in the North as a single unified actor when, in practice, governments (on both sides) are coalitions so that it is not possible to know whose preferences or interests are embedded in policy or which voice your opponent is listening to. That signals might be misinterpreted is especially likely given the fact that the objectives sought by the bombing were confused and the US public pronouncements were erratic. At times, statements hinted at expanding the war and others denying such plans.[113]

Chester Cooper recognized the problem with signaling that derived from "conflicting statements and just plain static."[114] Schelling, however, dismissed the criticism that President Johnson had not stated his objectives explicitly when he initiated the bombing campaign. Rather, Schelling saw advantages to vague demands because meeting them was less public and therefore less humiliating for an adversary.[115]

Washington too faced the problem of misinterpreting signals from Hanoi's actions. Thus, the attack against the Maddox at Tonkin Gulf was read as a premeditated policy decision in Hanoi, rather than what it was: a local act of retaliation for a raid against a North Vietnamese Island.[116] Similarly, the attack against the American military base at Pleiku in February 1965 that provided an impetus for escalating the war against the North was also a Viet Cong initiative taken without prior knowledge or approval of Hanoi.[117] With its attacks at the US base at Bien Hoa in 1964, and at Pleiku, the Viet Cong miscalculated its use of force, demonstrating the unintended consequences of its use of force because rather than convince the United States to withdraw, these attacks against Americans may have foreclosed the political feasibility of withdrawal.[118]

Second, from an operational point of view, there may have been no way to design a bombing campaign that would *both* persuade the North to desist its support while damaging their ability to infiltrate resources to South. John McNaughton explained the difficulty this way:

> There is a conflict between the objective of 'persuading Hanoi,' which would dictate a program of painful surgical strikes separated by fairly long gaps, and the objective of interdiction, which would benefit from heavy bombings. No program can be designed which optimized the chances of achieving both objectives.[119]

Third, from a conceptual point of view, there was an inherent contradiction in Schelling's notion of coercion as the power to hurt pointed out by Robert Johnson, the author of a 1964 study on measures for applying pressure to North Vietnam. He argued that the very logic of the strategy set an upper limit on how far a bombing campaign could go. Because if the United States destroyed all of the targets in North Vietnam, it would also destroy any incentive the North might have to comply with American demands. Destroying all the major targets would eliminate the North's fear of American power to hurt. Johnson also said that his committee had little confidence that gradual escalation would discourage the North from

using its military assets and rather saw this claim as "a hypothesis to be examined." Johnson goes on to comment on the dynamic at work in escalation: "Escalation raises the stakes by increasing the costs to the United States by increasing the extent to which US credibility is seen to be at risk. It therefore tends to increase demands or at least harden bargaining positions. Similarly, escalation tends to strengthen the unity of America's opponents and stiffen their determination to resist compromise for fear of appearing to cave into duress."[120] Robert Pape also observed that by December 1967, all of North Vietnam's industrial war potential had been destroyed and that still had not created risks of sufficient magnitude to affect its political calculus.[121]

The Pentagon's own SIGMA war games conducted in 1964 refuted the hypothesis of the benefits of gradual escalation and suggested instead that mutual escalation would occur. Curtis LeMay was critical of the results of the games and dismissed their results as unrealistic.[122] RAND's Harry Rowen who played Ho Chi Minh in the games thought that bombing would not alter the North's support, for the insurgency was criticized as representing the North in a very implausible way.[123] Nevertheless, the report from the SIGMA games gave George Ball support for his opposition to escalation, a conclusion he had drawn already from his experience with the Strategic Bombing Survey of World War II.[124] It was not until 1967 that Robert McNamara, in testimony to the Congress, admitted publicly that the bombing had not been effective.[125]

The fourth and final problem with the bombing campaign is that it did not consider the point of view from Hanoi. For, as expressed in the option for the slow squeeze, the purpose of the bombing was to make the North Vietnamese cry uncle and consent to negotiations. McGeorge Bundy believed that the bombing would strengthen the United States' negotiating position, and Robert McNamara's preference for bombing was because it offered a bargaining chip the United States could use. Schelling too saw the bombing "as much in support of negotiations as in support of the military effort."[126] Surely then the North was likely to view the bombing as intended to strengthen the United States' negotiating position, which meant that the United States ultimately aimed for a settlement not victory. If such was the case, the North likely reasoned it would have no incentive to negotiate and could wait for the American public to tire of the war. Roger Hilsman believed that the North would interpret the bombing as an act of desperation *unless* the United States could demonstrate success in the counterinsurgency in the South.[127] This interpretation of Hanoi's

view is credible because, according to the Canadian representative to the International Control Commission James Blair Seaborne, and based on his observations while visiting the North in 1964, he saw no evidence of war weariness there. Nor did he believe that the prospect for material gain would induce the North to seek a settlement because the North saw the air strikes as a last-ditch effort by the United States to improve it bargaining position.[128] In the final analysis, the North's first priority remained unification, and not protecting their industrial base.[129]

Even though the theory of coercion provided flawed reasoning behind graduated pressure, there were still some practical reasons for the strategy to appear so attractive to policy-makers. Two practical considerations seem to be the most important. First was the consideration of domestic political realities. In the 1964 presidential election campaign, Lyndon Johnson faced a challenge on the right from the hawkish Barry Goldwater. Consequently, to please both the left and the right, Johnson tried to avoid charges of either weakness or warmongering. Within such a context, the cautious approach embodied in graduated pressure was bound to appear attractive. In fact, Robert Mann pointed out the domestic political motive behind the Tonkin Gulf Resolution—that it might serve to undercut Barry Goldwater's characterization of Johnson as weak.[130] Second, from the standpoint of international considerations, both Chester Cooper and Daniel Ellsberg pointed out that carefully calibrated restrained pressure was necessary to avoid Chinese or Soviet intervention. If the use of force was so overwhelming that the very survival of the Hanoi regime appeared to be threatened, then communist powers might well be provoked into action.[131] And the concern about Chinese intervention was all the more salient to policy-makers because China acquired nuclear capability in 1964. At the same time, Nikita Khrushchev's removal from power in October 1964 also raised concerns that the era of "peaceful co-existence" was at an end, and could lead to increased Soviet support to Hanoi.

In his discussion of coercion, Thomas Shelling makes a clear distinction between the coercive air campaign against the North and what he saw as the "more straightforward military campaign against the Viet Cong."[132] Military planning for training the South Vietnamese army (ARVN) and the ground campaign assumed that a conventional Korea-type invasion from the North was likely. Roger Hilsman argues that this was certainly on the mind of General Taylor. Similarly, Albert Wohlstetter justified the contours of the conduct of the ground campaign because strategists expected a conventional invasion from the North. Indeed, US Ambassador

Dubrow was critical of the training of ARVN by the Americans as the insurgency began to grow in 1960, precisely because training emphasized "large-unit, road based conventional force to defend against a Korean war type invasion from the North."[133] General Earl Wheeler publicly reconfirmed the big war conventional approach to Vietnam in a speech in 1962: "It is fashionable in some quarters to say that the problems in Southeast Asia are primarily political and economic rather than military. I do not agree. The essence of the problem in Vietnam is military."[134]

This focus on a conventional military approach was buttressed by drawing on notions of coercive pressure. The starting point for applying coercive pressure lay in a cost-benefit analysis predicated on the same assumption found in the bombing campaign—namely, that insurgents could be defeated through the capacity to harm them and their supporters beyond a certain tolerance level. As rational actors, the insurgents and villagers, so the logic goes, could be made to abandon their cause once that tolerance level is reached. Lyndon Johnson apparently opted to view the conduct of the war from a cost-benefit approach which was rooted in Schelling's analysis.[135]

While the logic of coercion in counterinsurgency might ultimately be traced to Schelling's work, it found explicit expression in the work of two RAND consultants, Nathan Leites and Charles Wolf. Their work subsequently published in a book in 1970, titled *Rebellion and Authority: An Analytical Essay on Insurgent Conflicts,* sought to outline a comprehensive theory of insurgency, one that would add the scientific rigor found in nuclear strategy to limited war. Contrary to some conventional wisdom derived from modernization theory, Leites and Wolf did not believe that economic and political development was sufficient to eradicate insurgent movements. In fact, they saw modernization as destabilizing and that economic improvement was just as likely to facilitate as to hinder insurgency. They argued that any additional income generated by economic development would be given to insurgents, allowing people to purchase security from them.[136]

Leites and Wolf based their understanding of peasant behavior on the cost-benefit analysis derived from economics. They saw people as rational actors whose behavior would be influenced by calculations concerning damage limitation and profit maximization. Thus, rather than attempting to alter attitudes toward the government via a "hearts and minds" approach, counterinsurgency operations needed to focus on changing behavior with coercive measures emphasizing the judicious use of punishment. In this way, the limited wars of combatting insurgency

become contests in "the effective management of coercion." Leites and Wolf illustrated this point in a footnote, quoting a former Viet Cong combat leader who joined the Government of Vietnam (GVN) in 1967: "We knew the people wanted nothing but peace for themselves…we had no illusion that they were for us…we knew that when we left they'd serve the GVN…the people would submit to whoever was wearing a gun."[137] Leites and Wolf do admit that the effectiveness of coercion depends on the degree of understanding on the part of the people concerning what is intended and why.[138] In other words, much like the efforts to coerce the North with the bombing campaign, the success of coercion as a signal depended on it being interpreted correctly.

Leites and Wolf downplayed the role of "conscious and conviction" in determining behavior with their focus on cost-benefit reasoning and said that altering preferences or feelings toward the government was less important. In this, they reached a conclusion similar to one made by Bernard Brodie based on his study of the efficacy of bombing on German morale in World War II. Noting the distinction that the German security services made between *stimmung* (attitude or feeling) and *halttung* (behavior), the allies discovered that "the influence of the former upon the latter was much less immediate and direct than had been generally supposed."[139] Within such a context, it was incumbent upon the government to apply coercion that would counter any that the insurgents might use.[140]

It was in the area of measuring the government's coercive capacity that the well-known role of statistics—that emblem of scientific precision—came into play during the war. Defense Secretary McNamara, in particular, was enamored of collecting statistics, and Roger Hilsman observed that the secretary tended "to brush away broad political and strategic arguments, and to concentrate on what could be quantified—money, men, guns and ammunition."[141] Thus, the war saw the gathering of body counts that calculated kill ratios of how many enemy and friendly soldiers were lost as well as an incident account that calculated weapons lost and weapons captured. Seymour Deitchman, in his oversight of "Project Agile," reinforced McNamara's proclivity for quantitative measures and attempted as much as possible to orient research toward them. A report to the secretary of Defense urged that the "soft sciences be hardened by combining operations research with social science techniques to develop indicators to measure progress in counter-insurgency."[142] Chester Cooper commented on the statistical measures: "[T]here were red, white and blue charts designating with deceptive accuracy the localities under Viet Cong,

Saigon, or mixed control. It was all very quantitative, very scientific, and very misleading." Furthermore, despite the apparent precision, according to Alain Enthoven who reviewed captured enemy documents, enemy body counts were overstated by at least 30 percent.[143] Statistics were also used to convince President Johnson of the need to replace a bombing policy that only retaliated against attacks against Americans with a sustained bombing campaign. To that end, Daniel Ellsberg records that John McNaughton asked him to gather "atrocity statistics" of Viet Cong acts.[144]

Yet, despite the best efforts to coerce the insurgents and the North, the results of that effort were disappointing and raise the question of whether the communists in Vietnam could be deterred or coerced. Alexander George believed that Hanoi could not be deterred. That policy-makers thought so was perhaps due to their tendency to overgeneralize from the Cuban Missile Crisis where the Soviets did back down in response to American actions. In his criticism of the simplified form of deterrence theory, George notes that it prevented policy-makers from being able to distinguish between deterrable and non-deterrable threats. Similarly, General Westmorland hinted at the fact that Hanoi might not have been coercible when he remarked that the losses sustained by the North's General Giap would have led to his firing had he been an American Commander.[145] Most notable of all were the observations of Konrad Kellen who read transcripts of interviews with Viet Cong prisoners and defectors—for Kellen had experience with prisoner interrogations in World War II and Korea and had interviewed East European defectors. He said he never saw anything like the Vietnam transcripts and that "[p]risoners and defectors tell you what they think you want to hear. These people, you can't get them to say anything critical of their regime." He concluded that this was one adversary whose leadership and population simply "could not be coerced." They could be annihilated but not coerced.[146]

While the results of the coercive aspect of American strategy were disappointing, counterinsurgency also drew on another technique, namely, pacification via nation-building. This aspect of American strategy aimed to win the loyalty of the people to the government, thereby granting it the "right to rule," that Kalevi Holsti has aptly labeled vertical legitimacy.[147] Seymour Deitchman had recognized the importance of bringing benefits to the people, more attractive than any gains offered by the insurgents.[148] Sometimes referred to as the "hearts and minds" approach, it focused efforts, contrary to Leites and Wolf's view, on rewards rather than on punishment. For this aspect of counterinsurgency, policy-makers drew on

modernization theory as a frame of reference, and several factors illustrate the extent that this was so.

To begin with, Walt Rostow, author of what is perhaps the most famous statement of modernization theory, was a member of President Kennedy's inner circle. As such, he had written Kennedy's speech, calling the 1960s the Decade of Development. Rostow also penned two speeches for the then Senator Kennedy dealing with developing countries.[149] As a long-time champion of aid to developing countries, Rostow wrote a memo to President Kennedy that outlined the political benefit of aid. In the memo, he observed that at the Belgrade meeting of neutrals in September 1961, the 18 moderate states either received most of their aid from the United States or were hoping to obtain increased aid. In contrast, all six of the states he considered extremist received substantially more aid from the Soviets.[150] After Rostow's trip to Vietnam in the fall of 1961, where he interviewed captured NLF soldiers, he concluded that they were attracted to the cause because they "had been caught up for the first time and found various degrees of satisfaction and disappointment—in a modern organizational structure reaching beyond the family" and that these were "dislocated, rootless, young men who wanted above all to become part of a larger, modern institution."[151]

Chapter 4 has already provided a comprehensive analysis of Rostow's contribution to modernization theory. Here, we wish to note that as one author put it, "the communicative clarity of Rostow's theory of the stages of growth led to its widespread circulation in policy circles." Schlesinger underscored the point and enumerated three advantages for policy-makers to Rostow's stages concept. First, it offered specific economic criteria for giving economic aid. Second, it provided a reminder of the non-economic factors that determine growth. Finally, and most importantly, it looked for the long term, so that it conceived of foreign aid not as "a State Department slush fund to influence tactical situations," but rather aimed "at strategic goals of a stronger national independence, an increased concentration of domestic affairs, greater democracy and a long-run association with the West."[152] Armed with Rostow's compelling theory, Kennedy advisors from McGeorge Bundy to Robert McNamara became proponents of economic development. Kennedy and his advisors also solicited proposals for an official "Modernization Institute" where social scientific theory and counterinsurgency doctrine could be effectively disseminated through a series of seminars conducted at the Foreign Service Institute. These seminars would draw on the expertise of people like Walt Rostow and Lucian Pye.

What is more, Rostow's ideas had even penetrated leaders in South Vietnam. Ngo Dinh Nhu, for example, was reported as referring to the stages of growth to government cadres in 1963 and the need to end "traditional" society.[153]

As a basis for strategy, modernization theory was problematic because it contained a paradox that was recognized at the time by some of its theorists. Development policies were often predicated on the notion that modernization was a cure for political instability. Yet, both theorists and practitioners questioned this causal relationship. Thus, for example, Lucian Pye observed that policy would be easier if research demonstrated a direct link between poverty and backwardness and the attractions of communism. Pye went on to warn that although economic aid might help, it also increased demands that would grow faster than the means for satisfying them. Similarly, Roger Hilsman rejected the idea that modernization provided a panacea for insurgency and might be its cause and not its cure because "Modernization inevitably uproots established social systems, produces political and economic dislocation and tension, and cannot deliver results quickly enough to relieve these short-term pressures."[154] In essence, the very process of modernization remains a dual-edged sword in terms of its political impact.

Another aspect of modernization theory made it a problematic basis for strategy. That is, among its theorists, there was a profound disagreement concerning the optimal institutional arrangements for facilitating modernization—a point already raised in Chap. 4. Some theorists believed that authoritarian regimes—especially those led by the military—were more conducive to modernization than democracy. In this regard, Turkey is often cited as the poster child for successful modernization by the military. Jefferson Marquis developed categories for social scientist's varied views of the optimal contours for institutions. He breaks the approaches recommended by social scientists into three categories. The first he labels "conservative populist" that would build Vietnam on the basis of traditional institutions and local autonomy. The second approach Marquis labeled "liberal nationalist" that recommended fostering prosperity and representative institutions granting rewards in the form of land ownership to rural people. The third approach labeled "bureaucratic authoritarian" sought to integrate the country administratively under a strong, central authority. Marquis suggests that aspects of each vision of modernization were used by policy-makers at various times. Given their mutually exclusive recommendations, the disagreements created a certain policy incoherence that help account for failure in nation-building.[155]

Regardless of the theoretical problems with notions of modernization, its ideas and assumptions were attractive for policy-makers. In particular, they fell on receptive ears of Lyndon Johnson. For Johnson, as a southern politician, had witnessed the economic transformation of the American South brought about by Franklin Roosevelt's New Deal. Johnson remained a staunch New Dealer for the rest of his career and held to the belief that the government could play a positive role in economic development. Thus, in preparation for his 1961 trip to Vietnam as vice president, he met with an old New Deal friend Arthur Goldschmidt, then serving as an economic specialist at the UN. Goldschmidt told Johnson about the plans for a development project to finance a series of dams on the Mekong River.[156] This conversation may well have planted the seed for Johnson's proposal to the North Vietnamese for a billion dollar regional development plan contained in his speech at Johns Hopkins University in 1965. The plan promised more than a donation in aid and rather aimed for broader development goals. Undoubtedly inspired by the Depression Era TVA, David Lilienthal, a former head of the TVA, was appointed to head the study and planning team. As announced by Johnson, the plan was viewed as an effort to win over left-leaning critics of the war, but given the legacy of the New Deal, Johnson was likely sincere in making the proposal.[157] Johnson was not alone in his belief of a version of the TVA for Vietnam. Gilbert F. White, while president of the Association of American Geographers, championed the idea in an article for the *Bulletin of the Atomic Scientists* in 1964. White did recognize the importance of providing security for the project and suggested that the UN might supply a blue helmet force for this purpose.[158]

With the theoretical reference of modernization theory reinforced as it was by the legacy of Great Depression programs, American strategists were poised to implement nation-building in Vietnam. The starting point for that effort was an aid program and the extent of American aid to Vietnam before the escalation of the war was substantial. From 1955 to 1960, gross US aid to the country was $220 million per year, or roughly 22 percent of South Vietnam's GNP.[159] From the outset, the aid program had problems. For example, four-fifths of the South Vietnamese labor force was in agriculture, but no more than 20 percent of American aid reached the countryside.[160] Aid was misdirected in other ways. For example, the Commodities Import Program (CIP), where the United States supplied financing for the import of manufactured goods, was used by the Vietnamese to finance military or consumer goods rather than investment

goods that might enhance economic development. The Vietnamese also imported rice to keep the price low, which discouraged domestic production of its major crop. Typical of projects that benefited the military rather than the broader economy was the major road project of the Saigon-Bien Hoa Highway constructed specifically to handle heavy military traffic.[161] Adding to this dysfunction was the tension between the United States and the South Vietnamese concerning the best vehicle for industrial development. The American advisors, not surprisingly, preferred the private sector, while the Vietnamese favored government ownership at a time when the Vietnamese government had a "conspicuously inefficient and inexperienced bureaucracy." The Diem regime saw government ownership of industry as offering a "third way" between communism and capitalism.[162]

The aid program in its overall effect was deeply flawed and anti-development in its impact because only a small amount of aid was directed for the long-run development of the economy. AID administrator David Bell admitted as much in his written testimony to the Congress in 1963. Although the Johnson Administration, by 1966, began to demonstrate greater concern for long-term economic development, by then it may have been too late to salvage American strategy. Milton Taylor, who as a member of the Michigan State University Advisory Group in Vietnam and who served as taxation advisor to the Vietnamese government from January 1959 to July 1960, described American aid as "a large-scale relief project more than economic development." He went on to observe: "After six years of large-scale American aid, Viet-Nam is becoming a permanent mendicant." Taylor even expressed doubts that the South Vietnamese had reached Rostow's "take-off" stage when aid was supposed to be the most effective.[163] The situation with American aid had not improved by the time escalation of the war was underway. Daniel Ellsberg illustrated the corruption associated with American aid with an anecdote from his stay in Vietnam from 1965 to 1967. In one example, the United States supplied bags of cement intended to be used for building schools. Only 30 of the 75 bags supplied were so used, and the rest were diverted to district chiefs for sale on the black market for private housing in Saigon. Under such circumstances, few people would see the benefits of US aid or be convinced to view the South Vietnamese government more favorably.[164] American aid also had insidious consequences for politics in Vietnam. The ready availability of aid allowed the Vietnamese government to avoid facing up to their internal economic problems. In addition, as Douglas Dacy pointed out, the Vietnamese government by taking all the aid that they could, lost

a chance to enhance its political credibility to its citizens. For the greater the aid, the more the government looked like an American puppet.[165]

American aid did not generate Rostow's "take-off " phase in the Vietnamese economy, and modernization theorists had explicitly recognized that the stage would not be reached in the absence of changes in social structure and institutions. The structural changes found concrete manifestations in the "strategic hamlet" program. Strategic hamlets had a predecessor in Diem's agroville program announced in July 1959. The scheme had been borrowed from the successful Malayan example where jungle squatters had been moved away from centers of guerrilla activity. The plan called for half a million peasants in the southern delta region to be moved into secure agrovilles. But while the agrovilles did provide greater security, their "fortress like" quality had a demoralizing effect on the peasants. For besides being moved from their native hamlets, the agrovilles demanded forced labor without remuneration, so that, in practice, the program "dissolved into large scale conscription of reluctant peasants" providing the Viet Cong with an issue they could exploit. Weakness and problems with the agrovilles led to the abandonment of the program after only 40,000 peasants had been resettled. In the end, the failure of the program can be attributed to the fact that drawing on the Malayan example was misplaced because Vietnamese farmers were *not* jungle squatters and therefore resented being moved.[166]

The subsequent effort of strategic hamlets by Diem, and taken up by the United States, sought to learn from the mistakes in the agrovilles, thereby improving on the idea of protecting the peasants from the Viet Cong. From its inception, the program set an unrealistic pace for construction of the hamlets. Because of the emphasis on speedy construction, provincial subordinates were overwhelmed, which was conducive for using coercion to produce resources and results quickly. Such behavior, of course, met resistance from peasants while giving the NLF an issue to exploit.[167] The goals of the strategic hamlets did not meet reality in the countryside. The plan for strategic hamlets did not envision large-scale relocation, but rather a strengthening of security in existing hamlets. The idea was that strategic hamlets had the potential to provide institutional "modern" links between the peasants and the central government so that Rostow's ideas contributed part of the rationale behind the program.[168] However, the problem that had plagued the agrovilles emerged in the strategic hamlets: that is, the obligatory labor required and the costs the peasants bore in material contributions and the sacrifice of removing land

from cultivation for earthworks. A report by RAND in 1962 identified and detailed these problems and noted that the work method of the strategic hamlets was reminiscent of the inequitable features of the agroville program. An anthropologist with expertise on Vietnamese culture reported that in his visits to farmers, they told him that forced labor in the strategic hamlet took them from their own fields for 45 to 90 days.[169]

Other problems became apparent in the use of strategic hamlets as a means of pacification to win the "other war." Chester Cooper commented that despite their formidable fortifications, Viet Cong sympathizers could and did open the gates for the Viet Cong to gain access to supplies and weapons. By the time the troops arrived in response to the VC incursions, they would be gone. Part of the problem with easy VC access was due to the fact that strategic hamlets were located too close to Viet Cong strongholds. Roger Hilsman had even reported on the shortcomings of strategic hamlets to President Kennedy after a trip to Vietnam.[170]

Finally, the problem with strategic hamlets, and pacification in general, lay with a disinterest that General Westmoreland expressed toward the effort. In his memoirs, Westmoreland outlined what he saw as the appropriate division of labor between American and Vietnamese forces. The latter were to be responsible for pacification, while the US Army would clear main force units. His logic was that the goals of pacification were to provide people with social justice, education, medical care and economic opportunity, tasks he believed only the South Vietnamese could accomplish.[171] While in the abstract it might be true that indigenous forces are best able to perform such tasks, in practice, in the case of Vietnam, this might have been an unwarranted assumption. For the first RAND morale and motivation study had reported that the South Vietnamese army brutalized its prisoners. If such was the case, could they really be expected to perform reliably the tasks that Westmoreland thought them most suited for?

Like the body counts and atrocity statistics that were to provide evidence for measuring coercion, the pacification program too devised a "scientific" method for measuring progress in the strategic hamlets. The Hamlet Evaluation System (HES) was a reporting device initiated at the request of the secretary of Defense in 1967. It sought to collect objective evaluations of the strategic hamlets by using contractors instead of government personnel. Ithiel De Sola Pool saw HES as an example of the positive potential impact that social science methods had on policy. Because HES required district advisors to fill out monthly reports, it forced them "to get deeply involved with what was on the minds of the villagers." Prior to the

implementation of this reporting system, district advisors had very little contact with hamlets in the area. The final report of a study of HES concluded that while it could provide a "reasonably reliable" method of estimating security trends, it was not clear that the same could be said for measuring development which was the sine qua non for winning peasant support for the government.[172]

Other views of HES reinforced concerns raised in the study group's conclusion. For example, William Lederer commented on HES that "it represented the underlying problems of measuring success in counter-insurgency: that data entered into the system were suspect because US advisors, ninety percent of whom did not speak Vietnamese, were unable to interview the local population." Anthropologist Gerald Hickey noted that he and John Donnell were asked by RAND in 1962 to go to Vietnam to study the strategic hamlet program, and the Pentagon tried to attach a rebuttal to their report because its conclusions were too negative. Hickey also told an interviewer for *The New Republic* that it was not possible to devise a single set of indicators applicable to the whole of Vietnamese society. In particular, Hickey noted that the highlanders represented a distinct group, and their revolt in September 1964 was indicative of profound ethnic cleavages within South Vietnam. In fact, the cleavages were so pronounced between the Vietnamese and the people of the central highlands that the former referred to the latter as "savages." Given such a divided society, it would be difficult to forge a unity that could provide South Vietnam with what Holsti labeled "horizontal legitimacy," a necessary foundation to undergird vertical legitimacy.[173] Here it is useful to remember Karl Deutsch's warning, noted in Chap. 4, that the process of social mobilization in a setting with deep ethnic cleavages was likely to exacerbate those divisions rather than heal them.

Whatever the flaws in the strategic hamlet program and the system for measuring progress, perhaps the greatest error to pacification lay in the timing of its efforts at economic reform. Thus, Douglas Dacy suggests that the time for progress on economic development needed to come before 1960 and the formation of the NLF. Dacy observed that Diem in early 1956 had initiated a promising effort to foster development when he promulgated an ordinance to limit ownership of rice land to 100 hectares and established administrative capacity for the transfer of land, giving first priority for ownership to those tilling the land.[174] Similar bad timing occurred later when the secretary of Defense identified the problem with pacification as one of mismanagement due to the failure to coordinate

civilian and military efforts. Lyndon Johnson then signed National Security Action Memorandum 343, intended to correct these management problems. He centralized control with the creation of the Civil Operation and Revolutionary Support (CORDS) in 1967 under the stewardship of Robert Komer.[175] This bureaucratic arrangement may well have come too late to salvage pacification.

Perhaps the greatest obstacle of all that overshadowed attempts at pacification was the weak leadership foundation for building vertical legitimacy. Its very absence meant that the prerequisites for modernization were not met. We have already noted doubts expressed by Robert McClintock concerning the Diem regime. Diem's subsequent actions and policies underscore his weakness and illustrate the extent to which he proved to be a slender reed to organize vertical legitimacy around. To begin with, Diem's refusal to hold elections required by the Geneva Accords did not bode well for his willingness to broaden his base of support, although Diem justified this action by the fact that Bao Dai had signed the agreement. Then, Diem's policy of forcibly assimilating ethnic minorities set off discontent among the ethnic minority communities. In August 1956, Diem issued a decree affecting ethnic Chinese that required them to register as aliens, thereby making them ineligible to engage in some economic activities.[176] Diem also broke with a tradition that villages were autonomous and elected their own officials by replacing village chiefs and municipal councils with people appointed by his hand-picked province chiefs and district advisors. Diem alienated the population in another way when, in January 1960, he broke up a major strike using the military and intimidated and arrested labor leaders. As if these errors were not enough, he weakened his own army by dismissing 6000 experienced noncommissioned officers, ensuring that personal loyalty to Diem became the criteria for the army.[177]

All of these actions by Diem were mere preludes to the event that shattered American confidence in his leadership. That event was the Buddhist crisis in the summer of 1963, and the harsh response of the regime to it. During that summer and into the fall, seven monks set themselves on fire to protest the regime and brought Vietnam to what journalist Staley Karnow in the *Saturday Evening Post* vividly described as "the edge of chaos." If anything underscored the weakness of a regime whose narrow base of support rested with Catholics, it was this crisis, because Buddhism was genuinely Vietnamese and as such provided a vehicle for a variety of political aspirations.[178]

Yet the Buddhist crisis took the Kennedy Administration by surprise, and the president is reputed to have asked during the crisis: "Who are these people? Why didn't we know about them before?"[179] To find an answer to this question and an evaluation of the impact that Diem's attack against the pagodas would have on the war, President Kennedy sent another fact-finding mission to Vietnam. This mission consisted of Marine General Victor Krulack and the State Department's Joseph Mendenhall. Krulack interviewed the US advisors and reported to the president that they did not believe the crisis would affect the war. In contrast, Mendenhall interviewed Vietnamese and reported their view that there was a virtual breakdown of the government in Saigon. Upon hearing these contradictory statements, the president asked: "You two did visit the same country didn't you?"[180] Perhaps no one captured the essence of the problem with Diem's leadership better than North Vietnamese Premier Phan Van Dong, who observed in an interview with Bernard Fall that Diem was not popular, and the more unpopular he was the more American aid he would need to stay in power. And the more American aid he received, the more he looked like a puppet of the Americans and less likely he was to win popular support.[181]

It was against the backdrop of these problems with Diem that policymakers began to consider—what proved to be a futile effort—to find more effective leadership for the South. As a first effort to do something about Diem, the United States cut aid to the Commodity Import Program. Chester Cooper asserted that this action suggested to Vietnamese generals that the United States might look favorably on a coup against Diem.[182] That suggestion was reinforced by a cable sent on August 24, 1963, to the American embassy that the administration was willing to support the anti-Diem faction. The cable explicitly stated that if Diem remained obdurate and refused to remove his brother, then the United States "must face the possibility that Diem cannot be saved." The cable subsequently generated controversy because it was sent while key principles (President Kennedy, Robert McNamara and Dean Rusk) were out of town. Maxwell Taylor in his memoirs characterized the approval process for this cable as "an egregious end run." While Roger Hilsman, to whom the cable has been attributed, justified the action because he saw the choice in 1963 as one "between lesser evils, a high probability of political instability if the generals moved against the regime, and more or less certain disaster if the Diem-Nhu regime continued as it was."[183] Muddying the waters surrounding the coup further was the view of former Vice President Nguyen Ngoc who

told Gerald Hickey in an interview that Diem was making overtures to the NLF for accommodation to form a coalition government which was the primary motive for the United States to seek alternative leadership.[184]

Be that as it may, President Kennedy himself was assassinated three weeks after the coup against Diem, and Lyndon Johnson assumed the presidency against the backdrop of chaos in Vietnam. Shortly after the junta led by Durong Van Minh assumed power, Minh himself was replaced in a subsequent coup in January 1964 led by Ngoyen Khanh. Robert McNamara in his memoirs reports that at the time of the coup, policy-makers judged that the generals stood only a 50/50 chance of bringing some improvement in governance. William Bundy later recalled that the Khanh coup was a "disastrous event" that "removed for a long time to come any chance of a true government of unity. . .or with any claim to the crucial element of legitimacy." Maxwell Taylor noted in a telegram in 1964 that the "best thing that can be said about the present Khanh government is that it has lasted six months."[185] After a trip to Vietnam as Chairman of the Joint Chiefs and accompanied by Robert McNamara in February 1964, Taylor made the following assessment:

> The enemy was clearly making the most out of the political turbulence and reduced military effectiveness resulting from the November and January coups. The political structure linking the central government with the provinces had virtually disappeared. Thirty-five of the forty-one province chiefs were new appointees, and most of the senior military commands had changed hands twice since the previous October. The desertion rate in the South Vietnamese forces was high and increasing…[186]

Taylor's analysis was confirmed in August 1964, a special National Intelligence Estimate was issued and, much like the one in 1954, did not hold out much hope for stability in South Vietnam: "[A]t present the odds are against the emergence of a stable government capable of effectively prosecuting the war."[187] So chaotic were conditions in South Vietnam in 1964 that at the time of the attack against the Brinks Hotel in December of that year, it took the US Embassy several days to determine if the attack had come from the Viet Cong or been the result of intramural squabbling among South Vietnamese factions.[188]

Within the context of a dysfunctional government and political instability exacerbated by multiple coups, some observers believed that it was feasible for the United States to withdraw its support of South Vietnam

without any loss of prestige. Chester Cooper certainly thought so, especially in light of Lyndon Johnson's landslide electoral victory over Barry Goldwater in November 1964.[189] Walt Rostow disagreed, however, and argued that withdrawal after the Diem coup was impossible because the United States was so closely associated with the overthrow of his regime.[190] Rostow, as Lyndon Johnson's National Security Advisor, held these hardline views until the end.

Subsequent changes of government did not improve governance in South Vietnam, and a second Buddhist crisis erupted in 1966. In a meeting of NSC principals in November 1967, Maxwell Taylor assessed the situation in Vietnam as bleak and that the United States still had to establish adequate government there.[191] Continued instability also led John McNaughton in his diary to conclude: "Since the big issue is US reputation, the time to disengage is when the blame is on someone else—in this case the South Vietnamese government whose total incapacity to behave themselves should amount to a justification for our dumping them."[192] General Westmoreland in his memoirs seconded the view that the inefficiencies, bickering and divisiveness among the South Vietnamese would justify US withdrawal with no harm to the reputation of the United States.[193] When power came to rest in the hands of Nguyen Van Thieu and Nguyen Cao Ky in 1965, it was solidified further with their election in 1967 when they ran on a joint ticket that Robert Komer and Walt Rostow believed would make the election "appear as a sham."[194] William Bundy described Thieu and Ky as the "bottom of the barrel, absolutely the bottom of the barrel."[195] Even as late as 1970, Robert Komer, who had been assigned responsibility for centralizing American pacification efforts, described the condition of the South Vietnamese government as "feeble."[196] What might be the appropriate epitaph for the Thieu-Ky regime was written by Michigan State University professor, Wesley Fishel, the long-time advisor to the South Vietnamese government. Writing in an editorial for *The New York Times*, Fishel observed: "After seventeen years of total involvement in Vietnamese internal affairs, the United States has sanctified in power a polished and ruthless military regime, authoritarian, institutionalized in its corruption and lacking support among the people."[197] Such was the foundation on which the United States had hoped to build a nation.

In evaluating the strategy in Vietnam and placing blame for the outcome, how much weight should be ascribed to the ideas behind the strategy and how much weight placed on the implementation of the ideas?

George Ball as the head of president elect Kennedy's task force on foreign economic policy worried at the time about New Frontier fascination with "that intriguing new invention of the professors, nation-building." Numerous commentators since have criticized social science for its role in Vietnam. May McCarthy has been quoted as denouncing the "gross stupidities and over confidence of the Kennedy-Johnson advisors, not to mention their moral insensitivity issued from a sectarian faith in the factuality of the social sciences." Stanley Hoffmann was similarly critical and said that it was a myth "to believe that what we were engaged in was nation-building. This was an illusion fed by a social science imbued with engineering pretensions and an ideological justification for the less savory aspect of our role." Walter McDougall reinforced Hoffmann's point and noted there was no case where political strategies based on social science theories were more outrageously exposed than in the pacification effort in South Vietnam.[198]

While there is certainly some truth to these criticisms, several caveats are in order. First, as already noted, policy-makers are inclined to draw on social science in support of decisions already made. But there is another side to the coin, and that is they are equally inclined to ignore research when it fails to confirm their preconceptions. One example of this is the fact that the outcomes of the SIGMA games were ignored or dismissed as "unrealistic." Similarly, the negative evaluation of the strategic hamlet program by Gerald Hickey and John Donnell elicited strong objections from strategists and was ignored. Robert Johnson makes a similar claim about his 1964 report on alternatives for pressuring North Vietnam. He said that although the study was a model of foreign policy planning, it was ignored because its conclusions did not fit with the preconceptions of policy-makers.[199]

Second, as stated at the start of this chapter, there is a certain futility to fixing blame for a failed strategy on any single individual. The same might be said about attributing blame to any single social science theory. Yet, in one sense, deterrence/coercion theory was, in fact, more systematically employed in Vietnam strategy than was modernization theory. For all the modernization theorists—Walt Rostow included—saw political and social structures as a precondition for successful economic development and modernization. As the description of South Vietnamese leaders demonstrates—these preconditions did not exist. Expecting a narrowly economic approach to succeed was never realistic even though its implementation may have seemed straightforward enough to strategists. What is more, modernization

theorists, as suggested in Chap. 4, were more inclined than their colleagues espousing coercion theory, to warn of the difficulty of the process.

In contrast, the logic of coercion theory was explicitly embedded in graduated pressure with little acknowledgment of its shortcomings or special constraining conditions. In consultations with John McNaughton, Thomas Schelling pondered his questions concerning what the United States could ask the North Vietnamese to stop doing that would be easy to verify and could not be quickly undone after a bombing halt. Schelling offered no answer to these questions and "was stumped" with "no idea of where to begin."[200] Despite this experience, Schelling was adamant in his defense of coercion. When Daniel Ellsberg was asked to prepare options for Vietnam to Richard Nixon and Henry Kissinger in 1968, his mentor Thomas Schelling pointed out then that his options contained no "threat tactic." Ellsberg then raised the question at the heart of the logic of coercion: "Why would a threat of escalation work when the actual practice of bombing the North had not?"[201] While coercive diplomacy may not be inherently doomed to fail, there may, in fact, be special limiting circumstances when it can prevail. Thus, Wallace Thies concluded from his study of Vietnam that there was no direct relationship between the rate of escalation and the coercer's chances of success. However, coercive diplomacy has the greatest chance of success when the target state's government is not yet firmly committed to the action that the coercing state wishes to stop.[202] Such detailed knowledge of factions within the North Vietnamese government was unavailable to policy-makers which limited their chance to apply coercion successfully.

The limitations of theory in the case of Vietnam were bound to disillusion social scientists to the extent that it ended their post-World War II optimism concerning the American ability to change the world. At the same time, even scholars who had worked for the Pentagon came to distrust the defense establishment because Vietnam called into question both direct government support for scholarship and scholarly ethics.[203]

Even recognizing the shortcomings of social science theories in this case, one cannot dismiss the role of policy-makers' strategic blunders in implementation. Policy-makers often ignored advice of their colleagues who opposed the escalation of the war. George Ball was notable in his persistent opposition to the war. He was appalled at Taylor and Rostow's recommendations in their report after their 1961 trip. Ball did not see the Vietnam case as one of overt military invasion, but rather as a revolutionary situation with strong anti-colonialist overtones. In a cabinet meeting

in July 1965, when Ball argued against escalation, McGeorge Bundy responded that the United States could always withdraw later after it gave military force a good try. To which Ball presciently replied: "We won't get out, we'll double our bet and get lost in the rice paddies."[204] Other policymakers like Roger Hilsman resigned from the State Department's Far East Bureau in 1964 because of his disagreement over the military and political strategy that was being pursued.[205] Still others with the ability to influence policy, like Senators Mansfield and Fulbright, agreed to keep their doubts about Vietnam to themselves so that the public had no opportunity to hear opposition viewpoints from experts like George Kennan and Hans Morgenthau until the televised Vietnam hearings in 1966.[206] In fact, Hans Morgenthau expressed his views in the pages of *Look Magazine* in 1966 where he described the war as a civil war whose global significance was remote and "that, far from containing China and communism, it opens the gates to both—by destroying the social fabric of Vietnamese nationalism, which is implacably hostile to China; and that in consequence, the risks we are taking in the pursuit of victory are out of all proportion to the interests at stake."[207]

Finally, of course, was the fact that various advisors deliberately limited information received by both President Kennedy and President Johnson that narrowed their choices to when and how to intervene but not whether to intervene in Vietnam. McGeorge Bundy and the NSC staff, from 1961 to 1965, were inclined to move quickly to squelch any dissent from a hawkish approach to Vietnam.[208] Walt Rostow too, in his role as National Security Advisor to President Johnson, was accused by both Arthur Schlesinger and Robert McNamara as only forwarding views on the war that corresponded to his own. In 1965, Rostow claimed that the bombing campaign had the Viet Cong near collapse. Thus, he buried CIA reports that questioned the degree of progress being made.[209] Even President Johnson bears some responsibility for confining discussion of Vietnam strategy within acceptable parameters. Thus, Chester Cooper observed that Johnson had a "compulsion to keep as many people in the dark about as many things for as long as possible." Johnson's approach then trickled down and influenced every layer of Washington's Vietnam policy community.[210]

In the final analysis, the strategy failure in Vietnam reflects the unfortunate confluence of problems with theory and practice as the strategy was implemented. Failure in this case illustrates the consequences of flawed strategic judgment. David Kaiser cites a letter written by William Bundy to American ambassadors in South Korea, Laos and Japan dated June 1965. In it, Bundy listed three unpleasant choices for strategy in Vietnam:

1. Expand the bombing campaign and risk Chinese intervention.
2. Mine Haiphong Harbor.
3. Deploy more ground troops to raise the total to 300,000.

The letter then noted that not one of these options would raise the chance of success much above 30 percent. That the United States pursued the war against such odds speaks not only to flawed theory with a misplaced sense of its scientific basis, but also to the folly of strategic judgment.

Notes

1. James McAlister, "Who Lost Vietnam? Soldiers, Civilians and Military Strategy," *International Security* 35 (Winter 2010/2011): 121.
2. Geoffrey Warner, "Review Article: Lyndon Johnson's War? Part 1 Escalation," *International Affairs* 79 (2003): 853. Lloyd C. Gardner, *Pay Any Price: Lyndon Johnson and the Wars in Vietnam* (Chicago: Ivan R. Dee, 1995), 141–142. David Milne, "Our Equivalent of Guerrilla Warfare: Walt Rostow and the Bombing of North Vietnam, 1961–1968," *The Journal of Military Affairs* 71 (January 2007): 172–173. Andrew Preston, *The War Council: McGeorge Bundy, the NSC and Vietnam* (Cambridge, MA: Harvard University Press, 2006), 62, 2.
3. Daniel Ellsberg, *Secrets: A Memoir of Vietnam and the Pentagon Papers* (New York: Viking, 2002), 56.
4. Benjamin T. Harrison and Christopher L. Mosher, "John McNaughton and Vietnam the Early Years as Assistant Secretary of Defense, 1964–1965," *History* 92 (2007): 498.
5. Benjamin T. Harrison and Christopher L. Mosher, "The Secret Diary of McNamara's Dove: The Long-Lost Story of John T. McNaughton's Opposition to the Vietnam War," *Diplomatic History* 35 (June 2011): 532, 507, 521–522.
6. John Prados, *Keepers of the Keys: A History of the National Security Council from Truman to Bush* (New York: William Morrow and Company, Inc. 1991), 211.
7. Quoted in Chester L. Cooper, *The Lost Crusade: America in Vietnam* (New York: Dodd, Mead and Company, 1970), 167.
8. Roger Hilsman, *To Move a Nation: The Politics of Foreign Policy in the Administration of John F. Kennedy* (New York: Doubleday and Company, 1967), 501. Robert Mann, *A Grand Delusion: America's Descent into Vietnam* (New York: Basic Books, 2001), 262. Dean Rusk later refuted the idea of withdrawal after the 1964 election because it cynically suggested that American troops were being committed for domestic political purposes—something he believed no president would do. Mann, 283.

9. Brian VanDeMark, *Into the Quagmire: Lyndon Johnson and the Escalation of the Vietnam War* (Oxford: Oxford University Press, 1995), 8. Mann, 254, 228.
10. Gardner, 65.
11. Mann, 283. Arthur M. Schlesinger, *A Thousand Days: John F. Kennedy in the White House* (Boston: Houghton Mifflin, Co. 1965), 549.
12. Hilsman, 130.
13. Hilsman, 146.
14. Schlesinger, 332.
15. Schlesinger, 547. Gardner, 47.
16. David Kaiser, *American Tragedy: Kennedy, Johnson, and the Origins of the Vietnam War* (Cambridge, MA: the Belkap Press, 2000), 312–313.
17. Andrew Brittle, "PROVEN, Westmoreland and the Historian," *Journal of Military History* 72 (October 2008): 1263–1264. McAllister, 95–97. McAllister also distinguishes a third revisionist school of thought that sees Westmoreland's tactics as necessary because the presence of North Vietnamese main force units left him no choice.
18. Dale Andrade and James H. Whilbanks, "Cords/Phoenix: Counterinsurgency Lessons from Vietnam and for the Future," *Military Review* (October 2006) Supplemental Special Edition: 80.
19. Cooper, 40, 49.
20. George A. Carver, Jr. "The Real Revolution in South Vietnam," *Foreign Affairs* 43 (April 1965): 392. Bert Cooper, John Killigrew and Norman LaCharité, *Case Studies in Revolutionary Warfare, 1941–1954* (Washington, D.C.: Special Operations Research Office, American University, 1964), 8.
21. Cooper, 56.
22. Figures on the amount of American aid to France vary. Those supplied by Chester Cooper, 62, are slightly higher than those provided by Roger Hilsman, 100.
23. Mann, 49.
24. Quoted in David L. Anderson, *Trapped by Success: The Eisenhower Administration and Vietnam, 1953–1961* (New York: Columbia University Press, 1991), 29.
25. Prados, 227. Alexander George and Richard Smoke, *Deterrence in American Foreign Policy: Theory and Practice* (New York: Columbia University Press, 1974), 255.
26. Anderson, 67, 45, 53.
27. Anderson, 32, 70.
28. William Bundy, "The Path to Vietnam: Ten Decisions," *Orbis* 11 (Fall 1967): 650. Cooper, 112–113.
29. Cooper, 146.

30. James M. Carter, *Inventing Vietnam: the United States and State Building, 1954–1968* (Cambridge: Cambridge University Press, 2008), 64, 88. Anderson, 75,140–141. Christopher T. Fisher, "Nation-Building and the Vietnam War: A Historiography," *Pacific Historical Review* 74 (August 2005): 448.
31. Anderson, xiii. Later, when Lyndon Johnson consulted with Eisenhower about the bombing campaign, Eisenhower was willing to express more hawkish views than he did when he was president. See VanDeMark, 78.
32. Anderson, 207–208.
33. See: Seymour J. Deitchman, *The Best-Laid Schemes: A Tale of Social Research and Bureaucracy* (Cambridge, MA: MIT Press, 1976), 4.
34. Schlesinger, 586.
35. David Milne, *America's Rasputin: Walt Rostow and the Vietnam War* (New York: Hill and Wang, 2008), 56.
36. Schlesinger, 591, 593.
37. William Kaufmann, *The McNamara Strategy* (New York: Harper and Row Publishers, 1964), 62.
38. Schlesinger, 510–511.
39. Hilsman, 363.
40. Kaufmann, 61, 62,
41. Hilsman, 78.
42. John Lodewijks, "Rostow, Developing Economies and National Security Policy," in *Economics and National Security: A History of Their Interaction*, ed. Craufurd D. Goodwin (Durham: Duke University Press, 1991), 297.
43. Seymour J. Deitchman, *Limited War and American Defense Policy*, 2nd ed. (Cambridge, MA: MIT Press, 1969).
44. Hilsman, 415.
45. The House of Representatives, subcommittee on International Organizations and Movements, of the Committee on Foreign Affairs, Report no. 4 "Behavioral Sciences and the National Security," with part ix of the hearings on winning the cold war (Washington, D.C.: U.S. Government Printing Office, December 1965), 72.
46. Deitchman, *The Best Laid Schemes*, 65. For an example of SORO research see: Bert Cooper, et al., *Case Studies in Insurgency and Revolutionary Warfare, 1941–1954.*
47. Deitchman, *The Best Laid Schemes,* 67.
48. This was reported to the House Subcommittee on International Organizations and Movements, 30. Quoted in Report of the House Subcommittee on International Organizations and Movements, 5r.
49. George Parker, "Knowing the Enemy: A Reporter at Large," *The New Yorker* 82 (December 18, 2006): 60–69. Downloaded on August 19, 2017 from, www.newyorker.com/magazine/2006/12/18/knowing-the-enemy
50. Schlesinger, 590. Hilsman, 47.

51. Mai Elliott, *RAND in Southeast Asia: A History of the Vietnam War Era* (Santa Monica, CA: the RAND Corporation, 2010), 14. Elliott also notes that because RAND personnel joined the Defense Department, they were able to serve RAND's own interest to broaden its contacts beyond the Air Force.
52. Preston, 7, 47. Prados, 99.
53. Anderson, 60–61, 106.
54. Carter, 54.
55. Stephen T. Hosmer, Sibyelle O. Crane, "Counter-Insurgency: A Symposium, April 16–20, 1962" RAND Corporation, R-412-1. Downloaded on June 5, 2017 from www.rand.org/content/dam/rand/pubs/reports/2006/R-412-1
56. Wallace J. Thies, *When Governments Collide: Coercion and Diplomacy in the Vietnam Conflict, 1964–1968* (Berkeley: University of California Press, 1980), 232–233.
57. Thies, 233. See also, Anderson, 176.
58. Gardner, 54.
59. Mann, 274. Cooper, 115–117. For a more sympathetic appraisal of Diem's leadership, see Philip E. Catton, "Counter-Insurgency and Nation-Building: The Strategic Hamlet Program, 1961–1963," *The International History Review* 21 (December 1999): 918–940. RAND's analyst, Charles Wolf Jr. also had a positive view of Diem. See Elliott, 7–8.
60. Ellsberg, 192.
61. Frank Leith Jones, *Blowtorch: Robert Komer, Vietnam and American Cold War Strategy* (Annapolis, MD: Naval Institute Press, 2013), 139.
62. Preston, 170–171. Robert McNamara, *In Retrospect: The Tragedy and Lessons of Vietnam* (New York: Random House, 1995), 170.
63. Walt Rostow, "Guerrilla Warfare in Underdeveloped Areas," in *The Guerrilla and How to Fight Him*, ed. T.N. Greene (New York: Fredrick Praeger, 1962), 59.
64. *The Vietnam Hearings* with an introduction by J. William Fulbright, Chairman of the US Senate Committee on Foreign Relations (New York: Random House, 1966) 180.
65. Joseph J. Zasloff, "The Role of North Vietnam in the Southern Insurgency," RAND Research memorandum, RM 4140 July 1964. Downloaded on June 8, 2017 from www.rand.org/content/dam/rand/pubs/research_memorandum/2008/RM4140.
66. Hilsman, 428.
67. Elliott, 49, vi, vii. Unless otherwise noted, analysis of the RAND motivation and morale studies is drawn from Mai Elliott's history of RAND. Elliott also notes that RAND's work on Vietnam was closely linked to its institutional interest in expanding its influence by broadening its client base beyond the Air Force.

68. J.C. Donnell, Guy J. Pauker, and Joseph Zasloff, "Viet Cong Motivation and Morale, 1964: A Preliminary Report," RAND, March 1965, RM-4507/3 ISA. Downloaded on June 12, 2017 from www.rand.org/content/dam/rand/pubs/research_memorandum/2006/RM4507.3
69. Elliott, 46.
70. Ellsberg, 255–256.
71. Elliott, 64.
72. Elliott, 89–93. Elliott also suggests that there were rumors that Gouré was chosen to continue the motivation and morale studies because he would get the results the Air Force wanted, 99.
73. Leon Gouré, A.J. Russo and D. Scott, "Some Findings of the Viet Cong Motivation and Morale Study, June–December 1965," RAND Memorandum RM-491102 ISA/ARPA February 1966. Downloaded on June 14, 2017 from www.rand.org/content/dam/rand/pubs/research_memorandum/2006/RM4911-2
74. Elliott, 126, 157.
75. Deitchman, *The Best Laid Schemes*, 235–237, 340–341.
76. Elliott, 164,169, 172.
77. Quoted in Ekbladh, 202.
78. Cooper, 199.
79. Elliott, viii.
80. Daniel Ellsberg, "Some Lessons From Failure in Vietnam," RAND Corporation, July 1969. Downloaded on June 17, 2017 from www.rand.org/pubs/papers/p.4036.html
81. Stephen Peter Rosen, "Vietnam and the American Theory of Limited War," *International Security* (Fall 1982): 83–113.
82. Prados, 161.
83. Deitchman, *Best Laid Schemes,* 28, 46.
84. See for example, William Kaufmann, "The Requirements of Deterrence," in *Military Policy and National Security*, ed. William Kaufmann (Princeton: Princeton University Press, 1956), 132.
85. Thomas Schelling, *Arms and Influence* (New Haven: Yale University Press, 1966), vii, 169.
86. Thomas Schelling, "Nuclear Strategy in Europe," *World Politics* 14 (April 1962): 427.
87. Schelling, *Arms and Influence,* 144–145.
88. Schelling, *Arms and Influence,* vii.
89. Schelling, *Arms and Influence,* 151.
90. Schelling, *Arms and Influence,* 83.
91. Harrison and Mosher, "John T. McNaughton and Vietnam: The Early Years as Assistant Secretary of Defense, 1964–1965," 505.

92. Herman Kahn, *On Thermonuclear War* (Princeton: Princeton University Press, 1961), 126.
93. Schelling, *Arms and Influence*, 166.
94. Fred Kaplan, *The Wizards of Armageddon* (New York: Simon and Schuster, 1983), 333. Prados. 214.
95. Robert A. Pape Jr., "Coercive Air Power in the Vietnam War," *International Security* 15 (Fall 1990): 114.
96. Gardner, 162.
97. Rosen, 93.
98. Milne, 9. Although both Hilsman, 50 and Schlesinger 445, claimed the motive for moving Rostow was to "re-vitalize" the State Department.
99. Schlesinger, 337. Gardner, 57.
100. Ellsberg, *Secrets: A Memoir of Vietnam and the Pentagon Papers,* 48. Thies, 55.
101. Ellsberg, *Secrets: A Memoir of Vietnam and the Pentagon Papers,* 48.
102. Mann, 555. Gardner, 42–43. For a current military view that argues the case of civilian interference, see H.R. McMaster, *Dereliction of duty: Lyndon Johnson, Robert McNamara, the Joint Chiefs of Staff and the Lies that Led to Vietnam* (New York: Harper Collins Publishers, 1997). Robert Pape dismisses charges of civilian constraints as a myth. Pape, 121.
103. Preston, 147.
104. Gardner, 118.
105. Thies, 75.
106. For a discussion drawing on historical data that suggest that policy-makers may have been on firm ground in making this assumption, see John E. Mueller, "The Search for the 'Breaking Point' in Vietnam: The Statistics of a Deadly Quarrel," *International Studies Quarterly* 24 (December 1980): 497–519.
107. Pape, 104, 115.
108. Rosen, 88.
109. Elliott, 67.
110. Cooper, 263.
111. Prados, 219.
112. Schelling, *Arms and Influence*, 85. Thies, 218–219.
113. Thies, 12–13, 39.
114. Cooper, 228.
115. Schelling, *Arms and Influence,* 84.
116. Cooper, 239.
117. So claimed an advisor to the NLF in 1997. Preston, 172.
118. Cooper, 261.
119. Quoted in Thies, 129.
120. Robert H. Johnson, "Escalation Then and Now," *Foreign Policy* 60 (1985): 137–138, 134, 143.

121. Pape, 123–124.
122. Prados, 205. William Bundy later claimed the outcome of the games was dismissed because they were run by theorists and not policy-makers. Kucklick, 186.
123. Elliott, 69.
124. Johnson, 141. Milne, *America's Rasputin*, 32.
125. Prados, 241.
126. Mann, 308. Ellsberg, *Secrets: A Memoir of Vietnam and the Pentagon Papers,* 52. Schelling *Arms and Influence*, 172.
127. Hilsman, 139.
128. Thies, 39, 85. Bernard Fall drew a different conclusion than Seaborne, and suggested that the North Vietnamese did fear US bombing because it posed a danger of Chinese intervention and occupation. See Bernard Fall, "The Master of the Red Jab," *The Saturday Evening Post* (November 24, 1962): 21.
129. Milne, *America's Rasputin*, 138.
130. Mann, 357.
131. Cooper, 5. Ellsberg, *Secrets: A Memoir of Vietnam and the Pentagon Papers,* 60.
132. Schelling, *Arms and Influence,* 174.
133. Hilsman, 422–423. Richard Pfeffer, ed., *No More Vietnams? The War and the Future of American Foreign Policy* (New York: Harper and Row Publishers 1968), 4. Anderson, 186.
134. Hilsman, 424.
135. So argues Richard Schultz, "Coercive Force and Military Strategy: Deterrence Logic and the Cost-Benefit Model of Counter-insurgency Warfare," *Western Political Quarterly* 32 (December 1979), 445–446.
136. Nathan Leites and Charles Wolf, Jr., *Rebellion and Authority: An Analytical Essay on Insurgent Conflicts* (Chicago: Markham Publishing Company, 1970), 30, 150, 19.
137. Leites and Wolf, 155, 149.
138. Leites and Wolf, 155.
139. Bernard Brodie, *Strategy in the Missile Age* (Princeton: Princeton University Press, 1959), 132.
140. Ron Robin, *The Making of the Cold War Enemy: Culture and Politics in the Military Industrial Complex* (Princeton: Princeton University Press, 2001), 193–195.
141. Hilsman, 43.
142. Deitchman, *Best Laid Schemes*, 296, 115–116.
143. Cooper, 202. Mueller, 504.
144. Ellsberg, *Secrets: A Memoir of Vietnam and the Pentagon Papers,* 68.
145. Alexander L. George, David K. Hall and William E. Simons, *The Limits of Coercive Diplomacy: Laos, Cuba, Vietnam* (Boston: Little Brown and

Company, 1971), xi. Alexander George and Richard Smoke, *Deterrence in American Foreign Policy: Theory and Practice* (New York: Columbia University Press, 1974), 79. Elliott, 68.

146. Ellsberg, *Secrets: A Memoir of Vietnam and the Pentagon Papers,* 290.
147. Kalevi Holsti, *The State, War and the State of War*, (Cambridge: Cambridge University Press, 1996), 84–87.
148. Deitchman, *Limited War and American Defense Policy*, 40.
149. Milne, *America's Rasputin*, 79 and footnote on 264.
150. Schlesinger, 521–522.
151. Michael E. Latham, *The Right Kind of Revolution: Modernization, Development, and US Foreign Policy from the Cold War to the Present* (Ithaca: Cornell University Press, 2011) 137–138.
152. Kimber Charles Pearce, *Rostow, Kennedy and the Rhetoric of Foreign Aid* (East Lansing: Michigan State University Press, 2001), 118. Schlesinger, 588.
153. Pfeffer, 29. Johnathan Nashel, "The Road to Vietnam: Modernization Theory in Fact and Fiction," in *Cold War Constructions: The Political Culture of United States Imperialism, 1945–1966,* ed. Christian G. Appy (Amherst, MA: University of Massachusetts Press, 2000), 151–152. Michael E. Latham, *Modernization as Ideology: American Social Science and 'Nation-Building' in the Kennedy Era* (Chapel Hill NC: University of North Carolina Press, 2001), 167. Catton, 935.
154. Lucian W. Pye, *Guerrilla Communism in Malaya: Its Social and Political Meaning* (Princeton: Princeton University Press, 1956), 343–345. Hilsman, "Internal War: The New Communist Tactic," 31. The assumption that modernization can eliminate political instability and extremist violence still permeates US foreign policy as is evident by President Bush's response to the September 11 attacks. See Lael Brainard, "The Millennium Challenge Account and Foreign Assistance: Transformation or More Confusion?" Downloaded on July 6, 2016 from http://www.brookings.edu/research/articles/2003/03/spring-development=brainard
155. Jefferson P. Marquis, "The Other Warriors: American Social Science and Nation Building in Vietnam," *Diplomatic History* 24 (Winter 2000), passim.
156. Gardner, xii, 52.
157. Albert P. Williams, Jr. "South Vietnam's Development in a Post War Era: A Commentary on the Thuc-Lilienthal Report," *Asian Survey* 11 (April 1971): 353.
158. Gilbert F. White, "Vietnam: The Fourth Course," *Bulletin of the Atomic Scientists* (December 20, 1964), 9.
159. Douglas C. Dacy, *Foreign Aid, War and Economic Development: South Vietnam, 1955–1975* (Cambridge: Cambridge University Press, 1986), 3.
160. Carter, 94.

161. Milton C. Taylor, "South Viet-Nam: Lavish Aid, Limited Progress," *Pacific Affairs* 34 (Autumn 1961): 246. Dacy, preface. Carter, 89.
162. Taylor, 251. Carter, 92.
163. Dacy, 28. Taylor, 243, 256, 253.
164. Ellsberg, *Secrets: A Memoir of Vietnam and the Pentagon Papers,* 129–130.
165. Dacy, 237.
166. Dacy, 7. Cooper, 157–158. Catton, 921. Joseph J. Zasloff, "Rural Development in South Vietnam: The Agroville Program," *Pacific Affairs* 35 (Winter 1962–1963): 331, 334, 338. Zasloff based his assessment of the agrovilles on investigations in 1960 as part of a project organized by the Michigan State University Group.
167. Catton, 938–939.
168. Milne, *America's Rasputin*, 104.
169. John C. Donnell and Gerald C. Hickey, "The Vietnamese 'Strategic Hamlets': a Preliminary Report," RAND Corporation, RM-3208 September 1962. Downloaded on June 6, 2017 from www.rand.org/content/dam/rand/pubs/research_memorandum/2006/RM-3208 Gerald C. Hickey, *Window on a War: An Anthropologist in the Vietnam Conflict* (Lubbock, TX: Texas Tech University Press, 2002), 93.
170. Cooper, 227. Hilsman, 440–441, 464.
171. McAllister, "Who Lost Vietnam? Soldiers, Civilians and U.S. Military Strategy," 111.
172. Pfeffer, 205. Final Report on Hamlet Evaluation Study, May 1, 1968. Downloaded on June 1, 2017 from www.dtic.mil/dtic/tr/fulltext/02/839821
173. Jones, 164. Hickey, 9, 209, 151, 6. Holsti, 87–89. For an elaboration of the extent of divisions within Vietnamese society, see Gerald C. Hickey, "Accommodation in South Vietnam: The Key to Socio-political Solidarity," RAND. October, 1967. Downloaded on June 28, 2017 from www.rand.org/content/dam/rand/pubs/papers/2008/p3707.pdf
174. Dacy, 7, 112.
175. Jones, 102.
176. Gardner, 48. Hickey, *Window on a War*, 7, 69.
177. Cooper, 160. Carter, 113. Mann, 197.
178. Stanley Karnow, "The Edge of Chaos," *The Saturday Evening Post* (September 23, 1963): passim. Hilsman, 471, 475.
179. Quoted in Preston, 120.
180. Hilsman, 502. This mixed message on conditions in Vietnam was mirrored in other reports as well. In January 1963, the CIA characterized the Viet Cong as increasing the effectiveness of its forces, while General Harkins and Ambassador Nolting reported them as weakening. Mann, 276.
181. Fall, 19–21.

182. Cooper, 216–217.
183. Preston, 123. Hilsman, 486–487.
184. Hickey, *Window on a War,* 104.
185. McNamara, 78. Gardner, 114. Warner, 837.
186. Thies, 26.
187. Warner, 838.
188. Logevall, 293.
189. Cooper, 253.
190. W.W. Rostow, "The Case for the Vietnam War," *Parameters* (Winter 1996–1997): 39–50. Downloaded on February 11, 2013, from www.Carlisle.army.mil/USAWC/parameters/articles/96 winter/rostow
191. Thies, 62.
192. Harrison and Mosher, "The Secret Diary of McNamara's Dove: the Long Lost Story of John T. McNaughton's Opposition to the Vietnam War," 520–521.
193. Gardner, 164.
194. James McAllister, "'A Fiasco of Nobel Proportions': the Johnson Administration and the South Vietnamese Elections of 1967," *Pacific Historical Review* 73 (November 2004): 634.
195. Van Der Mark, 150.
196. Robert Komer, "Impact of Pacification on Insurgency in South Vietnam," RAND Corporation 1970, p-4443. Downloaded on June 28, 2017 from www.rand.org/pubs/papers/p4443
197. Quoted in Carter, 232.
198. Gardner, 42. Kai Bird, *The Color of Truth, McGeorge Bundy and William Bundy, Brothers in Arms: A Biography* (New York: Simon and Schuster, 2000), 146. Pfeffer, 133. Walter A. McDougall, "Commentary: The Cold War Excursion of Science," *Diplomatic History* 24 (Winter 2000): 124.
199. Elliott, 26–27. Johnson, 145.
200. Kaplan, 335.
201. Ellsberg, *Secrets: A Memoir of Vietnam and the Pentagon Papers,* 234, 259.
202. Thies, 282.
203. Engerman, *Know Your Enemy*, 236.
204. Prados, 127. Van Der Mark, 190.
205. Cooper, 228–229. Hilsman's resignation is disputed, and Dean Rusk said he fired him because LBJ blamed him for the coup against Diem.
206. Preston, 397.
207. *Look Magazine*, "What Should We Do Now? Five Experts Give Their Answers," (August 9, 1966): 24–31.
208. Prados, 6–7.
209. Ellsberg, *Secrets: A Memoir of Vietnam and the Pentagon Papers,* 184. Milne, *America's Rasputin*, 166.
210. Cooper, 415.

Bibliography

Books

Abella, Alex. 2008. *Soldiers of Reason: The RAND Corporation and the Rise of the American Empire*. New York: Harcourt, Inc.

Adas, Michael. 1989. *Machines as the Measure of Men: Science, Technology, and Ideologies of Western Dominance*. Ithaca: Cornell University Press.

Almond, Gabriel A. 1960. Introduction. In *The Politics of Developing Areas*, ed. Gabriel A. Almond and James S. Coleman, 3–64. Princeton: Princeton University Press.

Almond, Gabriel A., and G. Bingham Powell Jr. 1966. *Comparative Politics: A Developmental Approach*. Boston: Little, Brown and Co.

Almond, Gabriel A., and Sidney Verba. 1965. *The Civic Culture: Political Attitudes and Democracy in Five Nations*. Boston: Little Brown and Company.

Anderson, David L. 1991. *Trapped by Success: The Eisenhower Administration and Vietnam, 1953–1961*. New York: Columbia University Press.

Anderson, Lisa. 2003. *Pursuing Truth, Exercising Power: Social Science and Public Policy in the Twenty-First Century*. New York: Columbia University Press.

Appy, Christian G., ed. 2000. *Cold War Constructions: The Political Culture of United States Imperialism, 1945–1966*. Amherst: University of Massachusetts Press.

Apter, David E. 1965. *The Politics of Modernization*. Chicago: University of Chicago Press.

———. 1968. *Some Conceptual Approaches to the Study of Modernization*. Englewood Cliffs: Prentice-Hall.

J. M. Klinger, *Social Science and National Security Policy*,
https://doi.org/10.1007/978-3-030-11251-6

Apter, David E., and Carl G. Rosberg, eds. 1994. *Political Development and the New Realism in Sub-Saharan Africa*. Charlottesville: University Press of Virginia.

Arndt, H.W. 1987. *Economic Development: The History of an Idea*. Chicago: University of Chicago Press.

Ayson, Robert. 2004. *Thomas Schelling and the Nuclear Age: Strategy as Social Science*. London: Frank Cass.

Ball, Terrence. 1989. The Politics of Social Science in Postwar America. In *Recasting America: Culture and Politics in the Age of Cold War*, ed. Lary May, 76–92. Chicago: University of Chicago Press.

Bator, Francis., et al. 1960. *Economic, Social and Political Change in the Underdeveloped Countries and Its Implications for United States Policy*. A Study Prepared at the Request of the Committee on Foreign Relations United States Senate. Washington, DC: U.S. Government Printing Office, March 30.

Bell, Daniel. 1982. *The Social Sciences Since the Second World War*. New Brunswick: Transaction Books.

Bender, Thomas, and Carl E. Schorske, eds. 1997. *American Academic Culture in Transformation: Fifty Years, Four Disciplines*. Princeton: Princeton University Press.

Benedict, Ruth. 1946. *The Chrysanthemum and the Sword: Patterns of Japanese Culture*. New York: New American Library.

Bernal, J.D. 1965. *Science in History*. New York: Hawthorne Books, Inc.

Bienen, Henry, ed. 1971. *The Military and Modernization*. Chicago: Aldine.

Bird, Kai. 2000. *The Color of Truth, McGeorge Bundy and William Bundy, Brothers in Arms: A Biography*. New York: Simon and Schuster.

Black, Cyril E. 1971. Theories of Political Development and American Foreign Policy. In *The Role of Ideas in American Foreign Policy*, ed. Gene M. Lyons, 47–65. Hanover: University Press of New England.

Blackmer, Donald. 2002. *The MIT Center for International Studies: The Founding Years, 1951–1969*. Cambridge, MA: MIT.

Boulding, Kenneth E. 1962. *Conflict and Defense: A General Theory*. New York: Harper and Row Publishers.

———. 1966. *The Impact of the Social Sciences*. New Brunswick: Rutgers University Press.

Bowers, Raymond V. 1967. The Military Establishment. In *The Uses of Sociology*, ed. Paul F. Lazarsfeld, William H. Sewell, and Harold L. Wilensky, 234–274. New York: Basic Books.

Brodie, Bernard, ed. 1946. *The Absolute Weapon: Atomic Power and World Order*. New York: Harcourt, Brace and Company.

———. 1959. *Strategy in the Missile Age*. Princeton: Princeton University Press.

———. 1973. *War and Politics*. New York: Macmillan Publishing Company, Inc.

Brooks, Stephen, and Alain-G Gagon, eds. 1990. *Social Scientists, Policy and the State*. New York: Praeger.

Bundy, McGeorge. 1964. The Battlefields of Power and the Searchlights of the Academy. In *The Dimensions of Diplomacy*, ed. E.A.J. Johnson, 1–15. Baltimore: The Johns Hopkins Press.

Buxton, William. 1985. *Talcott Parsons and the Capitalist Nation-State: Political Sociology as a Strategic Vocation*. Toronto: University of Toronto Press.

Carter, James M. 2008. *Inventing Vietnam: The United States and State Building, 1954–1968*. Cambridge: Cambridge University Press.

Collins, Martin J. 2002. *Cold War Laboratory: RAND, the Air Force and the American State, 1945–1950*. Washington, DC: Smithsonian Institution Press.

Cooper, Chester L. 1970. *The Lost Crusade: American in Vietnam*. New York: Dodd, Mead and Company.

Cooper, Frederick, and Randall Packard, eds. 1997. *International Development and the Social Sciences: Essays on the History and Politics of Knowledge*. Berkeley: University of California Press.

Cooper, Bert, John Killigrew, and Norman LaCharite. 1964. *Case Studies in Insurgency and Revolutionary Warfare: Vietnam, 1941–1954*. Washington, DC: Special Operations Research Office, American University.

Cullather, Nick. 2004. Modernization Theory. In *Explaining The History of American Foreign Policy*, ed. Michael J. Hogan and Thomas G. Paterson, 212–220. Cambridge: Cambridge University Press.

Curd, Martin, J.A. Cover, and Christopher Pincock, eds. 2013. *Philosophy of Science: The Central Issues*. 2nd ed. New York: W.W. Norton and Company.

Dacy, Douglas C. 1986. *Foreign Aid, War, and Economic Development: South Vietnam, 1955–1975*. Cambridge: Cambridge University Press.

Davison, W. Phillips. 1967. Foreign Policy. In *The Uses of Sociology*, ed. Paul F. Lazarsfeld, William H. Sewell, and Harold L. Wilensky, 391–417. New York: Basic Books.

Deitchman, Seymour J. 1969. *Limited War and American Defense Policy: Building and Using Military Power*. 2nd Revised ed. Cambridge, MA: The MIT Press.

———. 1976. *The Best-Laid Schemes: A Tale of Social Research and Bureaucracy*. Cambridge, MA: MIT Press.

Deutsch, Karl W. 1964. Integration and the Social System: Implications of Functional Analysis. In *Integration of Political Communities*, ed. Philip E. Jacob and James V. Toscano, 179–208. Philadelphia: J.B. Lippincott Company.

Diamond, Sigmund. 1992. *Compromised Campus: The Collaboration of Universities with the Intelligence Community*. Oxford: Oxford University Press.

Downing, Brian M. 1992. *The Military Revolution and Political Change: Origins of Democracy and Autocracy in Early Modern Europe*. Princeton: Princeton University Press.

Eisenstadt, S.N. 1973. *Tradition, Change and Modernity*. New York: John Wiley and Sons.

Ekbladh, David. 2006. From Consensus to Crisis: The Postwar Career of Nation-Building in US Foreign Relations. In *Nation-Building Beyond Afghanistan and Iraq*, ed. Francis Fukuyama, 19–41. Baltimore: The Johns Hopkins University Press.

———. 2010. *The Great American Mission: Modernization and the Construction of an American World Order*. Princeton: Princeton University Press.

Elliott, Mai. 2010. *RAND in Southeast Asia: A History of the Vietnam War Era*. Santa Monica: The RAND Corporation.

Ellsberg, Daniel. 2002. *Secrets: A Memoir of Vietnam and the Pentagon Papers*. New York: Viking.

Engerman, David C. 2009. *Know Your Enemy: The Rise and Fall of America's Soviet Experts*. Oxford: Oxford University Press.

Engerman, David C., Nils Gilman, Mark H. Haefele, and Michael E. Latham, eds. 2003. *Staging Growth: Modernization, Development and the Global Cold War*. Amherst: University of Massachusetts Press.

Farr, James, and Raymond Seidelman. 1993. *Discipline and History: Political Science in the United States*. Ann Arbor: University of Michigan Press.

Fisher, Donald. 1993. *Fundamental Development of the Social Sciences: Rockefeller Philanthropy and the United States Social Science Research Council*. Ann Arbor: University of Michigan Press.

Gardner, Lloyd C. 1995. *Pay Any Price: Lyndon Johnson and the Wars for Vietnam*. Chicago: Ivan R. Dee.

Geertz, Clifford, ed. 1963. *Old Societies and New States*. New York: Free Press of Glencoe.

Geiger, Roger L. 2004. *Research and Relevant Knowledge*. New Brunswick: Transaction Publishers.

Gendzier, Irene. 1985. *Managing Political Change: Social Scientists and the Third World*. Boulder: Westview Press.

George, Alexander L., and Richard Smoke. 1974. *Deterrence in American Foreign Policy: Theory and Practice*. New York: Columbia University Press.

George, Alexander L., David K. Hall, and William E. Simons. 1971. *The Limits of Coercive Diplomacy: Laos, Cuba, Vietnam*. Boston: Little Brown and Company.

Gilman, Nils. 2002. Involution and Modernization: The Case of Clifford Geertz. In *Economic Development: An Anthropological Approach*, ed. Jeffrey H. Cohen and Norbert Dannhaeuser, 3–22. New York: Altamira Press.

———. 2003. *Mandarins of the Future: Modernization Theory in Cold War America*. Baltimore: The Johns Hopkins University Press.

Goulden, Joseph C. 1969. *Truth Is the First Casualty: The Gulf of Tonkin Affair—Illusion and Reality*. Chicago: James B. Adler, Inc.

Green, Philip. 1966. *Deadly Logic: The Theory of Nuclear Deterrence*. Columbus: Ohio State University.

Green, Donald, and Ian Shapiro. 2005. Revisiting the Pathologies of Rational Choice. In *The Flight from Reality in the Human Sciences*, ed. Ian Shapiro, 51–99. Princeton: Princeton University Press.

Greene, John C. 1981. *Science, Ideology and World View*. Berkeley: University of California Press.

Hall, A.R. 1954. *The Scientific Revolution 1500–1800: The Formation of the Modern Scientific Attitude*. 2nd ed. Boston: Beacon Press.

Hickey, Gerald C. 2002. *Window on a War: An Anthropologist in the Vietnam Conflict*. Lubbock: Texas Tech University Press.

Higgs, Louis D., and Robert Weinland, eds. 1964. *Project Michelson: Status Report 1, 1 February 1964*. China Lake, CA: Naval Ordnance Test Station.

Hilsman, Roger. 1962. Internal War: The New Communist Tactic. In *The Guerrilla and How to Fight Him*, ed. T.N. Greene, 22–36. New York: Frederick A. Praeger.

———. 1967. *To Move a Nation: The Politics of Foreign Policy in the Administration of John F. Kennedy*. New York: Doubleday and Company.

Horowitz, Irving Louis, ed. 1967. *The Rise and Fall of Project Camelot: Studies in the Relationship Between Social Science and Practical Politics*. Cambridge, MA: The M.I.T. Press.

Jervis, Robert, Richard Ned Lebow, and Janice Gross Stein. 1995. *Psychology and Deterrence*. Baltimore: The Johns Hopkins University Press.

Jones, Frank Leith. 2013. *Blowtorch: Robert Komer, Vietnam, and American Cold War Strategy*. Annapolis: Naval Institute Press.

Kahn, Herman. 1961. *On Thermonuclear War*. Princeton: Princeton University Press.

———. 1965. *On Escalation: Metaphors and Scenarios*. New York: Frederick A. Praeger, Publishers.

Kaiser, David. 2000. *American Tragedy: Kennedy, Johnson and the Origins of the Vietnam War*. Cambridge, MA: The Belknap Press.

Kaplan, Fred. 1983. *The Wizards of Armageddon*. New York: Simon and Schuster.

Kaufmann, William W., ed. 1956. *Military Policy and National Security*. Princeton: Princeton University Press.

———. 1964. *The McNamara Strategy*. New York: Harper and Row Publishers.

Kevles, Daniel J. 1978. *The Physicists*. New York: Alfred A. Knopf.

Killian, James R., Jr. 1964. Science and Foreign Policy. In *The Dimensions of Diplomacy*, ed. E.A.J. Johnson, 57–87. Baltimore: Johns Hopkins Press.

Kindleberger, Charles P., and Guido di Tella, eds. 1982. *Economics in the Long View: Essays in Honor of W.W. Rostow*, Models and Methodology. Vol. 1. New York: New York University Press.

Kissinger, Henry A., ed. 1965. *Problems of National Strategy: A Book of Readings*. New York: Frederick A. Praeger, Publishers.

Klausner, Samuel Z., and Victor M. Lidz, eds. 1986. *The Nationalization of the Social Sciences*. Philadelphia: University of Pennsylvania Press.

Kornhauser, William. 1964. Rebellion and Political Development. In *Internal War*, ed. Harry Eckstein, 142–156. New York: The Free Press.

Kuhn, Thomas S. 1970. *The Structure of Scientific Revolutions*. 2nd ed. Chicago: University of Chicago Press.

Kuklick, Bruce. 2006. *Blind Oracles: Intellectuals and War from Kennan to Kissinger*. Princeton: Princeton University Press.

Latham, Michael E. 2000. *Modernization as Ideology: American Social Science and "Nation-Building" in the Kennedy Era*. Chapel Hill: University of North Carolina Press.

———. 2011. *The Right Kind of Revolution: Modernization, Development, and US Foreign Policy From the Cold War to the Present*. Ithaca: Cornell University Press.

Leighton, Alexander H. 1949. *Human Relations in a Changing World: Observations on the Use of the Social Sciences*. New York: E.P. Dutton and Company Inc.

Leites, Nathan, and Charles Wolf Jr. 1970. *Rebellion and Authority: An Analytical Essay on Insurgent Conflicts*. Chicago: Markham Publishing Company.

Lerner, Daniel. 1958. *The Passing of Traditional Society: Modernizing the Middle East*. Glencoe: The Free Press.

———. 1961. Toward a Communication Theory of Modernization. In *Communications and Political Development*, ed. Lucian W. Pye, 327–350. Princeton: Princeton University Press.

Lerner, Daniel, and Morton Gorden. n.d. *European Community and Atlantic Security in the World Arena*. Cambridge, MA: MIT Center for International Studies.

Levy, Marion J. 1966. *Modernization and the Structure of Societies: A Setting for International Affairs*. Princeton: Princeton University Press.

Lindblom, Charles E., and David K. Cohen. 1979. *Usable Knowledge: Social Science and Social Problem Solving*. New Haven: Yale University Press.

Lipset, Seymour Martin. 1964. Democracy and the Social System. In *Internal War*, ed. Harry Eckstein, 267–333. New York: The Free Press.

Lodewijks, John. 1991. Rostow, Developing Economies and National Security Policy. In *Economics and National Security: A History of Their Interaction*, ed. Craufurd D. Goodwin, 285–310. Durham: Duke University Press.

Logevall, Fredrik. 1999. *Choosing War: The Lost Chance for Peace and the Escalation of the War in Vietnam*. Berkeley: The University of California Press.

Long, Austin. 2006. *On "Other War": Lessons from Five Decades of RAND Counterinsurgency Research*. Santa Monica: The RAND Corporation.

Lynd, Robert S. 1939. *Knowledge for What? The Place of Social Science in American Culture*. Princeton: Princeton University Press.

Lyons, Gene M. 1969. *The Uneasy Partnership: Social Science and the Federal Government in the Twentieth Century*. New York: Russell Sage Foundation.

Mann, Robert. 2001. *A Grand Delusion: America's Descent into Vietnam*. New York: Basic Books.

McClintock, Robert. 1967. *The Meaning of Limited War*. Boston: Houghton Mifflin Company.

McMaster, H.R. 1997. *Dereliction of Duty: Lyndon Johnson, Robert McNamara, the Joint Chiefs of Staff and the Lies that Led to Vietnam*. New York: Harper and Collins Publishers.

McNamara, Robert. 1995. *Retrospect: The Tragedy and Lessons of Vietnam*. New York: Random House.

Milburn, Thomas W. 1969. Intellectual History of a Research Program. In *Theory and Research on the Causes of War*, ed. Dean G. Pruitt and Richard C. Snyder, 263–283. Englewood Cliffs: Prentice Hall Inc.

Millikan, Max F., and Donald L. Blackmer, eds. 1961. *The Emerging Nations: Their Growth and United States Policy*. Boston: Little, Brown and Company.

Millikan, Max F., and W.W. Rostow. 1957. *A Proposal: Key to an Effective Policy*. New York: Harper Brothers.

Mirowski, Philip. 1991. When Games Grow Deadly Serious: The Military Influence on the Evolution of Game Theory. In *Economics and National Security: A History of Their Interaction*, ed. Craufurd D. Goodwin, 227–255. Durham: Duke University Press.

Moore, Wilbert E. 1967. Sociology in Developing Areas. In *The Uses of Sociology*, ed. Paul F. Lazarsfeld, William H. Sewell, and Harold L. Wilensky, 643–654. New York: Basic Books.

Morgenthau, Hans J. 1970. Kennedy and Foreign Policy. In *Truth and Power: Essays of a Decade, 1960–1970*, 139–145. New York: Praeger Publishers.

Naroll, Raoul. 1969. Deterrence in History. In *Theory and Research on the Causes of War*, ed. Dean G. Pruitt and Richard C. Snyder, 150–164. Englewood Cliffs: Prentice Hall, Inc.

Naroll, Raoul, Vern L. Bullough, and Frada Naroll. 1974. *Military Deterrence in History: A Pilot Cross-Historical Survey*. New York: State University of New York Press.

Nash, Manning. 1984. *Unfinished Agenda: The Dynamics of Modernization in Developing Nations*. Boulder: Westview Press.

Organski, A.F.K. 1965. *The Stages of Political Development*. New York: Alfred A. Knopf.

Packenham, Robert A. 1973. *Liberal America and the Third World: Political Development Ideas in Foreign Aid and Social Science*. Princeton: Princeton University Press.

Paret, Peter, and John W. Shy. 1962. Guerilla Warfare and US Military Policy. In *The Guerilla and How to Fight Him*, ed. T.N. Greene, 37–53. New York: Frederick A. Praeger.

Parsons, Talcott. 1951. *The Social System*. New York: The Free Press.

———. 1960. *Structure and Process in Modern Societies*. New York: The Free Press.

Pearce, Kimber Charles. 2001. *Rostow, Kennedy and the Rhetoric of Foreign Aid*. East Lansing: Michigan State University Press.

Pfeffeer, Richard M., ed. 1968. *No More Vietnams? The War and the Future of American Foreign Policy*. New York: Harper and Row Publishers.

Pool, Ithiel de Sola, et al. 1963. *Social Science and National Security*. Washington, DC: Smithsonian Institution, Report prepared by the Research Group in Psychology and Social Sciences.

Prados, John. 1991. *Keepers of the Keys: A History of the National Security Council from Truman to Bush*. New York: William Morrow and Company, Inc.

Preston, Andrew. 2006. *The War Council: McGeorge Bundy, the NSC and Vietnam*. Cambridge, MA: Harvard University Press.

Price, Don K. 1954. *Government and Science: Their Dynamic Relation in American Democracy*. New York: New York University Press.

Price, David H. 2016. *Cold War Anthropology: The CIA, the Pentagon and the Growth of Dual Use Anthropology*. Durham: Duke University Press.

Pye, Lucian. 1956. *Guerilla Communism in Malaya: Its Social and Political Meaning*. Princeton: Princeton University Press.

———. 1960. The Politics of Southeast Asia. In *The Politics of the Developing Areas*, ed. Gabriel A. Almond and James S. Coleman, 65–152. Princeton: Princeton University Press.

———. 1963. Introduction. In *Communications and Political Development*, ed. Lucian Pye, 3–23. Princeton: Princeton University Press.

———. 1964a. Democracy, Modernization and Nation-Building. In *Self-Government in Modernizing Nations*, ed. J. Roland Pennock, 6–25. Englewood Cliffs: Prentice Hall.

———. 1964b. The Roots of Insurgency. In *Internal War*, ed. Harry Eckstein, 157–179. New York: The Free Press.

———. 1965. Introduction: Political Culture and Political Development. In *Political Culture and Political Development*, ed. Lucian Pye and Sidney Verba, 3–26. Princeton: Princeton University Press.

———. 1966. *Aspects of Political Development*. Boston: Little Brown and Company.

———. 1968. Description, Analysis, and Sensitivity to Change. In *Political Science and Public Policy*, ed. Austin Ranney, 239–261. Chicago: Markham Publishing Company.

———, ed. 1975. *Political Science and Area Studies: Rivals or Partners?* Bloomington: Indiana University Press.

Rattansi, P.M. 1972. The Social Interpretation of Science in the Seventeenth Century. In *Science and Society 1600–1900*, ed. Peter Mathias, 1–32. Cambridge: Cambridge University Press.

Rich, Robert F. 1981. *Social Science Information and Public Policy Making*. San Francisco: Jossey-Bass Publishers.

Rist, Gilbert. 1997. *The History of Development: From Western Origins to Global Faith*. London: Zed Books.

Robin, Ron. 2001. *The Making of the Cold War Enemy: Culture and Politics in the Military-Intellectual Complex*. Princeton: Princeton University Press.

Rostow, W.W. 1960. *The Stages of Economic Growth: A Non-Communist Manifesto*. Cambridge: Cambridge University Press.

———. 1962. Guerilla Warfare in Underdeveloped Areas. In *The Guerilla and How to Fight Him*, ed. T.N. Greene, 54–61. New York: Frederick A. Praeger.

———, ed. 1963. *The Economics of Take-Off into Sustained Growth: Proceedings of a Conference Held by the International Economic Association*. New York: St. Martin's Press, Inc.

———. 1964. The Planning of Foreign Policy. In *The Dimensions of Foreign Policy*, ed. E.A.J. Johnson, 41–55. Baltimore: John Hopkins Press.

———. 1971a. *Politics and the Stages of Growth*. Cambridge: Cambridge University Press.

———. 1971b. *The Stages of Economic Growth*. 2nd ed. Cambridge: Cambridge University Press.

———. 1972. *The Diffusion of Power: An Essay in Recent History*. New York: The Macmillan Company.

———. 1985. *Eisenhower, Kennedy and Foreign Aid*. Austin: University of Texas Press.

Schelling, Thomas C. 1959. Surprise Attack and Disarmament. In *NATO and American Security*, ed. Klauss Knorr, 176–208. Princeton: Princeton University Press.

———. 1960. *The Strategy of Conflict*. Cambridge, MA: Harvard University Press.

———. 1964. The State of the Arms Race. In *The Prospects for Arms Control*, ed. James E. Dougherty and John F. Lehman Jr., 50–58. New York: Macfadden Books.

———. 1966. *Arms and Influence*. New Haven: Yale University Press.

———. 1967. The Strategy of Inflicting Costs. In *Issues in Defense Economics*, ed. Roland N. McKean, 105–127. New York: Columbia University Press.

Schelling, Thomas C., and Morton H. Halperin. 1969. Pre-emptive, Premeditated and Accidental War. In *Theory and Research on the Causes of War*, ed. Dean G. Pruitt and Richard C. Snyder, 43–48. Englewood Cliffs: Prentice Hall, Inc.

Schlesinger, Arthur M. 1965. *A Thousand Days: John F. Kennedy in the White House*. Boston: Houghton Mifflin Company.

Scott, James C. 1998. *Seeing Like a State*. New Haven: Yale University Press.

Seitz, Thomas R. 2012. *The Evolving Role of Nation-Building in U.S. Foreign Policy: Lessons Learned, Lessons Lost*. Manchester: Manchester University Press.

Shils, Edward. 1961. Demagogues and Cadres in the Political Development of the New States. In *Communications and Political Development*, ed. Lucian W. Pye, 64–77. Princeton: Princeton University Press.

———. 1962. *Political Development in the New States*. Dordrecht: Mouton and Co.

Simpson, Christopher, ed. 1998. *Universities and Empire: Money and Politics in the Social Sciences During the Cold War*. New York: The New Press.

Skocpol, Theda. 1985. Bringing the State Back In: Strategies of Analysis in Current Research. In *Bringing the State Back In*, ed. Peter B. Evans et al., 3–37. Cambridge: Cambridge University Press.

Smelser, Neil J. 1967. Sociology and the Other Social Sciences. In *The Uses of Sociology*, ed. Paul F. Lazarsfeld, William H. Sewell, and Harold L. Wilensky, 3–44. New York: Basic Books.

Smith, Mark C. 1994. *Social Science in the Crucible: The American Debate Over Objectivity and Purpose 1918–1941*. Durham: Duke University Press.

Snow, C.P. 1959. *The Two Cultures and the Scientific Revolution*. New York: Cambridge University Press.

Sutton, Francis X. 2006. Nation-Building in the Heyday of the Classic Development Ideology: Ford Foundation Experience in the 1950s and 1960s. In *Nation-Building Beyond Afghanistan and Iraq*, ed. Francis Fukuyama, 42–63. Baltimore: The Johns Hopkins University Press.

Thaler, Richard. 1987. The Psychology of Choice and the Assumptions of Economics. In *Laboratory Experimentation in Economics: Six Points of View*, ed. Alvin E. Roth, 99–130. Cambridge: Cambridge University Press.

The Vietnam Hearings with an Introduction by J. William Fulbright. New York: Random House, 1966.

Thiers, Wallace J. 1980. *When Governments Collide: Coercion and Diplomacy in the Vietnam Conflict, 1964–1968*. Berkeley: University of California Press.

Toulmin, Stephen. 1961. *Foresight and Understanding: An Enquiry into the Aims of Science*. Westport: Greenwood Press, Publishers.

———. 1972. *Human Understanding: The Collective Use and Evolution of Concepts*. Princeton: Princeton University Press.

VanDeMark, Brian. 1995. *Into the Quagmire: Lyndon Johnson and the Escalation of the Vietnam War*. Oxford: Oxford University Press.

Verba, Sidney. 1965. Comparative Political Culture. In *Political Culture and Political Development*, ed. Lucian Pye and Sydney Verba, 512–560. Princeton: Princeton University Press.

Wallerstein, Immanuel. 1979. *The Capitalist World Economy*. Cambridge: Cambridge University Press.

Walt, Lewis W. 1970. *Strange War, Strange Strategy: A General's Report on Vietnam*. New York: Funk and Wagnalls.

Weiss, Carol H., ed. 1977. *Using Social Research in Public Policy Making*. Lexington: Lexington Books.
———. 1980. *Social Science Research and Decision-Making*. New York: Columbia University Press.
Wilson, William Julius, ed. 1993. *Sociology and the Public Agenda*. Newbury Park: Sage Publications.
Wohlstetter, Albert. 1958. *The Delicate Balance of Terror*. Santa Monica: The RAND Corporation.

Journals

Acemoglu, Daron, Simon Johnson, James A. Robinson, and Pierre Yared. 2009. Re-evaluating the Modernization Hypothesis. *Journal of Monetary Economics* 56: 1043–1058.
Achen, Christopher H., and Duncan Snidal. 1989. Rational Deterrence Theory and Comparative Case Studies. *World Politics* 41: 143–169.
Albaek, Erik. 1995. Between Knowledge and Power: Utilization of Social Science in Public Policy Making. *Policy Sciences* 28: 79–100.
Almond, Gabriel A. 1946. Politics, Science and Ethics. *American Political Science Review* 40: 283–293.
———. 1963. Political Systems and Political Change. *The American Behavioral Scientist* 6: 3–10.
———. 1965. A Developmental Approach to Political Systems. *World Politics* 17: 183–214.
———. 1969. Political Development: Analytical and Normative Perspectives. *Comparative Political Studies* 1: 447–468.
Almond, Gabriel A., and Stephen J. Genco. 1977. Clouds, Clocks and the Study of Politics. *World Politics* 29: 489–522.
Alpert, Harry. 1958a. Congressmen, Social Scientists, and Attitudes Toward Federal Support of Social Science Research. *American Sociological Review* 23: 682–686.
———. 1958b. The Knowledge We Need Most. *The Saturday Review*: 36–38.
Andrade, Dale. 2008. Westmoreland Was Right: Learning the Wrong Lessons from Vietnam. *Small Wars and Insurgencies* 19: 145–181.
Appleby, Paul H. 1950. Political Science the Next Twenty-five Years. *American Political Science Review* 44: 924–932.
Arjomand, Said Amir. 2004. Social Theory and the Changing World. *International Sociology* 19: 321–353.
Bendix, Reinhard. 1967. Tradition and Modernity Reconsidered. *Comparative Studies in Society and History* 9: 292–346.

Berger, Mark T. 2003. Decolonisation, Modernisation and Nation-Building: Political Development Theory and the Appeal of Communism in Southeast Asia. *Journal of Southeast Asian Studies* 34: 421–448.

Berghahn, Volker R. 1999. Philanthropy and Diplomacy in the 'American Century'. *Diplomatic History* 23: 393–419.

Bessner, Daniel. 2015. Organizing Complexity: The Hopeful Dreams and Harsh Realities of Interdisciplinary Collaboration at the RAND Corporation in the Early Cold War. *Journal of the History of the Behavioral Sciences* 51: 31–53.

Binder, Leonard. 1986. The Natural History of Development Theory. *Comparative Studies in Society and History* 28: 3–33.

Boulding, Kenneth E. 1971. The Intellectual Framework of Bad Political Advice. *Virginia Quarterly Review* 47: 602–607.

Bracken, Paul. 2010. Scholars and Security. *Perspectives on Politics* 8: 1095–1099.

Brecht, Arnold. 1947. Political Theory: Beyond Relativism in Political Theory. *American Political Science Review* 41: 470–488.

Brodie, Bernard. 1949. Strategy as a Science. *World Politics* 1: 467–488.

———. 1957. More About Limited War. *World Politics* 10: 112–122.

———. 1958. The Meaning of Limited War. RM2224 The Rand Corporation, July 30.

———. 1965. The McNamara Phenomenon. *World Politics* 17: 672–686.

Brodie, Janet Farrell. 2011. Learning Secrecy in the Early Cold War: The RAND Corporation. *Diplomatic History* 35: 643–670.

Bundy, William P. 1967. The Path to Viet Nam: Ten Decisions. *Orbis* 11: 647–663.

Campbell, Virginia. 2004. How RAND Invented the Postwar World. *Invention and Technology* 20: 50–59.

Carver, George A. 1965. The Real Revolution in South Viet Nam. *Foreign Affairs* 43: 387–408.

Catton Philip, E. 1999. Counter-Insurgency and Nation Building: The Strategic Hamlet Programme in South Vietnam. *The International History Review* 21: 918–940.

Cohen, Eliot. 1980. Systems Paralysis. *The American Spectator* 13: 23–27.

Cohen, Patricia. 2009. Field Study: Just How Relevant is Political Science. *The New York Times C* 1: 7.

Coleman, James S., and C.R.D. Halisi. 1983. American Political Science and Tropical Africa: Universalism VS Relativism. *African Studies Review* 26: 25–62.

Cowen, Tyler. 2014. Crimea, Through a Game-Theory Lens. *The New York Times*: B6.

Crowther-Heyck, Hunter. 2006. Patrons of the Revolution: Ideals and Institutions in Postwar Behavioral Science. *The History of Science Society* 97: 420–446.

Cullather, Nick. 2000. Research Note: Development? It's History. *Diplomatic History* 24: 641–653.

———. 2002. Damming Afghanistan: Modernization in a Buffer State. *The Journal of American History* 89: 512–537.
———. 2003. Bombing at the Speed of Thought: Intelligence in the Coming Age of Cyber War. *Intelligence and National Security* 18: 141–154.
———. 2006. 'The Target is the People': Representations of the Village in Modernization and US National Security Doctrine. *Cultural Politics* 2: 29–48.
Cumings, Bruce. 1997. Boundary Displacement: Area Studies and International Studies During and After the Cold War. *Bulletin of concerned Asian Scholars* 29: 6–27.
Czyzak, John J., and Carl F. Salans. 1966. The International Conference on Laos and the Geneva Agreement of 1962. *Journal of Southeast Asian History* 7: 27–47.
Daddis, Gregory A. 2013. Mired in a Quagmire: Popular Interpretations of the Vietnam War. *Orbis* 57: 532.
Dahl, Robert A. 1961. The Behavioral Approach to Political Science: Epitaph for a Monument to a Successful Protest. *American Political Science Review* 55: 763–772.
Deutsch, Karl W. 1961. Social Mobilization and Political Development. *American Political Science Review* 55: 493–514.
Dinerstein, Herbert S. 1958. The Revolution in Soviet Strategic Thinking. *Foreign Affairs* 36: 241–252.
Doob, Leonard W. 1947. The Utilization of Social Scientists in the Overseas Branch of the Office of War Information. *American Political Science Review* 41: 649–667.
———. 1950. Goebbels' Principles of Propaganda. *The Public Opinion Quarterly* 14: 419–442.
———. 1968. Just a Few of the Presuppositions and Perplexities Confronting Social Psychological Research in Developing Countries. *Journal of Social Issues* 24: 71–81.
Downing, Brian M. 1988. Constitutionalism, Warfare and Political Change in Early Modern Europe. *Theory and Society* 17: 7–56.
Easterly, William. 2007. The Ideology of Development. *Foreign Policy* 161: 31–35.
Easton, David. 1951. The Decline of Modern Political Theory. *The Journal of Politics* 13: 36–58.
———. 1969. The New Revolution in Political Science. *The American Political Science Review* 63: 1051–1061.
Eckstein, Harry. 1988. A Culturalist Theory of Political Change. *American Political Science Review* 82: 789–804.
Eisenstadt, S.N. 1974. Studies of Modernization and Sociological Theory. *History and Theory* 13 (3): 225–252.

Ekbladh, David. 2002. 'Mr. TVA': Grass-Roots Development, David Lilienthal and the Rise and Fall of the Tennessee Valley Authority as a Symbol for US Overseas Development, 1933–1973. *Diplomatic History* 26: 335–374.

Engerman, David C. 2000. Modernization from the Other Shore: American Observers and the Costs of Soviet Economic Development. *The American Historical Review* 105: 383–416.

———. 2004. The Romance of Economic Development and New Histories of the Cold War. *Diplomatic History* 28: 23–54.

———. 2007. Bernath Lecture: American Knowledge and Global Power. *Diplomatic History* 31: 599–622. Downloaded from Wiley online library on April 3, 2012.

———. 2010. Social Science in the Cold War. *Isis* 101: 393–400.

Engerman, David C., and Corinna R. Unger. 2009. Introduction: Towards a Global History of Modernization. *Diplomatic History* 33: 375–385.

Evans, Peter, and John D. Stephens. 1988. Studying Development Since the Sixties: The Emergence of a New Comparative Political Economy. *Theory and Society* 17: 713–745.

Fall, Bernard B. 1962. Masters of the Red Jab. *The Saturday Evening Post* 235: 18–21.

Fisher, Christopher T. 2005. Nation Building and the Vietnam War: A Historiography. *Pacific Historical Review* 74: 441–456.

Frieden, Jeffry A., and David Lake. 2005. International Relations as a Social Science: Rigor and Relevence. *The Annals of the American Academy* 600: 136–156.

Friedrich, Carl J. 1947. Instruction and Research: Political Science in the United States in Wartime. *American Political Science Review* 41: 978–989.

Fursenko, Aleksandr, and Timothy Naftali. 1998. The Pitsunda Decision: Khrushchev and Nuclear Weapons. *Cold War International History Project Bulletin* 10: 223–227.

Galbraith, John Kenneth. 1961. A Positive Approach to Economic Aid. *Foreign Affairs* 39: 444–457.

Geiger, Roger L. 1988. American Foundations and Academic Social Science, 1945–1960. *Minerva: A Review of Science, Learning and Policy* 26: 315–341.

George, Alexander L. 1972. The Case for Multiple Advocacy in Making Foreign Policy. *American Political Science Review* 66: 751–785.

Golinski, Jan. 2011. Science in the Enlightenment, Revisited. *History of Science* 49: 217–231.

Goodwin, Richard N. 1968. Unthinkable and the Unanalyzable. *The New Yorker*, February 17.

Gunnell, John G. 1988. American Political Science, Liberalism, and the Invention of Political Theory. *American Political Science Review* 82: 71–87.

Gusfield, Joseph R. 1967. Tradition and Modernity: Misplaced Polarities in the Study of Social Change. *American Journal of Sociology* 72: 351–362.

Gusterson, Hugh. 2007. Anthropology and Militarism. *Annual Review of Anthropology* 36: 155–175.

Haas, Mark L. 2001. Prospect Theory and the Cuban Missile Crisis. *International Studies Quarterly* 45: 241–270.

Hallowell, John H. 1944. Politics and Ethics. *American Political Science Review* 38: 639–655.

Halperin, Morton H. 1961. The Gaither Committee and the Policy Process. *World Politics* 13: 360–384.

Harrison, Benjamin T., and Christopher L. Mosher. 2007. John T. McNaughton and Vietnam: The Early Years as Assistant Secretary of Defense, 1964–1965. *History* 92: 496–514.

———. 2011. The Secret Diary of McNamara's Dove: The Long-Lost Story of John T. McNaughton's Opposition to the Vietnam War. *Diplomatic History* 35: 505–534.

Heaney, Michael T. 2007. The Chicago School that Never Was. *PS: Political Science and Politics* 40: 753–758.

Herken, Gregg. 2000. Commentary: In the Service of the State: Science and the Cold War. *Diplomatic History* 24: 107–115.

Herring, Pendleton. 1953. On the Study of Government. *American Political Science Review* 47: 961–974.

———. 1958. Expand the School. *The Saturday Review*, February 1: 39–40.

Heuser, Beatrice. 1998. Victory in a Nuclear War? A Comparison of NATO and WTO War Aims. *Contemporary European History* 7: 311–337.

Hirschman, Albert O. 1970. The Search for Paradigms as a Hindrance to Understanding. *World Politics* 22: 329–343.

Hitch, Charles J. 1958. Economics and Military Operations Research. *The Review of Economics and Statistics* 40: 199–209.

———. 1960. National Security Policy as a Field for Economic Research. *World Politics* 12: 434–452.

Holsti, K.J. 1998. Scholarship in an Era of Anxiety: The Study of International Politics During the Cold War. *Review of International Studies* 24: 17–46.

Howard, Michael. 1992. Brodie, Wohlstetter and American Nuclear Strategy. *Survival* 34: 107–116.

Hughes, Geraint. 2003. A 'Missed Opportunity' for Peace? Harold Wilson, British Diplomacy and the Sunflower Initiative to End the Vietnam War, February 1967. *Diplomacy and Statecraft* 14: 106–130.

Huntington, Samuel P. 1967. Introduction: Social Science and Vietnam. *Asian Survey* 7: 503–506.

———. 1971. The Change to Change: Modernization, Development, and Politics. *Comparative Politics* 3: 283–322.

Inglehart, Ronald. 1988. The Renaissance of Political Culture. *American Political Science Review* 82: 1203–1230.

Inkeles, Alex. 1969. Making Men Modern: On the Causes and Consequences of Individual Change in Six Developing Countries. *American Journal of Sociology* 75: 208–225.

Isaac, Joel. 2007. The Human Sciences in Cold War America. *The Historical Journal* 50: 725–747.

———. 2009. Tangled Loops: Theory, History and the Human Sciences in Modern America. *Modern Intellectual History* 6: 397–425.

Jakobsen, Peter Viggo. 2010. Pushing the Limits of Military Coercion Theory. *International Studies Perspectives* 12: 153–170.

Jentleson, Bruce W. 2002. The Need for Praxis: Bringing Policy Relevance Back In. *International Security* 26: 169–183.

Jervis, Robert. 1989. Rational Deterrence: Theory and Evidence. *World Politics* 41: 183–207.

Joas, Hans. 1999. The Modernity of War: Modernization Theory and the Problem of Violence. *International Sociology* 14: 457–472.

Johnson, Robert H. 1985. Escalation Then and Now. *Foreign Policy* 60: 130–147.

Jorgensen, Joseph G., and Eric R. Wolf. 1970. A Special Supplement: Anthropology on the Warpath in Thailand. *The New York Review of Books*, November 19: 26–35.

Kahan, Jerome H., and Anne K. Long. 1972. The Cuban Missile Crisis: A Study of Its Strategic Context. *Political Science Quarterly* 87: 564–590.

Karnow, Stanley. 1963. The Edge of Chaos. *The Saturday Evening Post*, September 28.

Kaysen, Carl. 1954. The Vulnerability of the United States to Enemy Attack. *World Politics* 6: 190–208.

Kennedy, John F. 1957. A Democrat Looks at Foreign Policy. *Foreign Affairs* 36: 44–59.

Kevles, Daniel J. 1977. The National Science Foundation and the Debate Over Postwar Research Policy, 1941–1945: A Political Interpretation of Science-The Endless Frontier. *Isis* 68: 4–26.

Krasner, Stephen D., Josephe S. Nye Jr., Janice Gross Stein, and Robert O. Keohane. 2009. Autobiographical Reflections on Bridging the Policy-Academy Divide. *Cambridge Review of International Affairs* 22: 111–126.

Lanier, Lyle H. 1949. The Psychological and Social Sciences in the National Military Establishment. *American Psychologist* 4: 127–147.

Lasswell, Harold P. 1956. The Political Science of Science: An Inquiry into the Possible Reconciliation of Mastery and Freedom. *The American Political Science Review* 50: 961–979.

Lasswell, Harold D. 1965. The Policy Sciences of Development. *World Politics* 17: 286–309.

Latham, Michael E. 1998. Ideology, Social Science, and Destiny: Modernization and the Kennedy-Era Alliance for Progress. *Diplomatic History* 22: 199–229.
Lebow, Richard Ned. 2006. Reason Divorced from Reality: Thomas Schelling and Strategic Bargaining. *International Politics* 43: 429–452.
Lerner, Daniel, and Richard D. Robinson. 1960. Swords and Ploughshares: The Turkish Army as a Modernizing Force. *World Politics* 13: 19–44.
Lilienthal, David E. 1951. Another 'Korea' in the Making? *Colliers's* 23 (23): 56–58.
Lipset, Seymour Martin. 1959. Some Social Requisites of Democracy: Economic Development and Political Legitimacy. *The American Political Science Review* LIII. Reprint in the Bobbs Merrill Series in the Social Sciences, #175.
Mahnken, Thomas G. 2010. Bridging the Gap Between the Worlds of Ideas and Action. *Orbis* 54: 4–13.
Marglin, Stephen A. 2003. Development as Poison: Rethinking the Western Model of Modernity. *Harvard International Review* 25: 70–75.
Marquis, Jefferson P. 2000. The Other Warriors: American Social Science and Nation Building In Vietnam. *Diplomatic History* 24: 79–105.
Maxwell, Stephen. 1968. Rationality in Deterrence. *Adelphi Papers* 50: 1–19.
Mazrui, Ali A. 1968. From Social Darwinism to Current Theories of Modernization: A Tradition of Analysis. *World Politics* 21: 69–83.
McAllister, James. 2004. A Fiasco of Noble Proportions': The Johnson Administration and the South Vietnamese Elections of 1967. *Pacific Historical Review* 73: 619–651.
———. 2010. Who Lost Vietnam? Solders, Civilians and US Military Strategy. *International Security* 35 (11): 95–123.
McDougall, Walter A. 2000. Commentary: The Cold War Excursion of Science. *Diplomatic History* 24: 117–127.
McFate, Montgomery. 2005. Anthropology and Counterinsurgency: The Strange Story of Their Curious Relationship. *Military Review* 85: 24–38.
McNaughton, John T. 1963. Arms Restraint in Military Decisions. *The Journal of Conflict Resolution* 7: 228–234.
Milburn, Thomas W. 1959. What Constitutes Effective Deterrence? *The Journal of Conflict Resolution* 3: 138–145.
Milne, David. 2007. 'Our Equivalent of Guerrilla Warfare': Walt Rostow and the Bombing of North Vietnam. *The Journal of Military History* 71: 169–203.
Mirsky, Jonathan. 2003. Wartime Lies. *The New York Review of Books*, October 9: 42–47.
Morgenthau, Hans. 1962. A Political Theory of Foreign Aid. *The American Political Science Review* 56: 301–309.
Morse, Wayne. 1966. Dangers in Government Sponsorship of Research on Foreign Policy and Foreign Areas. *Background* 10: 123–130.

Mueller, John E. 1980. The Search for the 'Breaking Point' in Vietnam: The Statistics of a Deadly Quarrel. *International Studies Quarterly* 24: 497–519.

Needell, Allan A. 1993. 'Truth Is Our Weapon': Project TROY, Political Warfare, and Government-Academic Relations in the National Security State. *Diplomatic History* 17: 399–420.

Nelson, Robert H. 1987. The Economics Profession and the Making of Public Policy. *The Journal of Economic Literature* 25: 49–91.

Neustadt, Richard E. 1964. Kennedy in the Presidency: A Premature Appraisal. *Political Science Quarterly* 79: 321–334.

Nye, Joseph S., Jr. 2008. Bridging the Gap Between Theory and Policy. *Political Psychology* 29: 593–603.

Packenham, Robert A. 1964. Approaches to the Study of Political Development. *World Politics* 17: 108–120.

Packer, George. 2006. Knowing the Enemy: A Reporter at Large. *The New Yorker* 82: 60–69.

Pape, Robert A. 1990. Coercive Air Power in the Vietnam War. *International Security* 15: 103–146.

Parsons, Talcott. 1946. The Science Legislation and the Role of the Social Sciences. *American Sociological Review* 11: 653–666.

———. 1964. Evolutionary Universals in Society. *American Sociological Review* 29: 329–357.

Pool, Ithiel de Sola. 1966. The Necessity for Social Scientists Doing Research for Governments. *Background* 10: 111–122.

Popp, Roland. 2008. An Application of Modernization Theory During the Cold War? The Case of Pahlavi Iran. *The International History Review* 30: 76–98.

Porter, Patrick. 2007. Good Anthropology, Bad History: The Cultural Turn in Studying War. *Parameters*: 45–58.

Powell, Robert. 2003. Nuclear Deterrence theory, Nuclear Proliferation, and the National Missile Defense. *International Security* 27: 86–118.

Price, Don K. 1962. The Scientific Establishment. *Science* 136: 1099–1106.

Pye, Lucian W. 1979. Political Modernization: Gaps Between Theory and Reality. *The Annals of the American Academy of Political and Social Science* 442: 28–39.

———. 1990. Political Science and the Crisis of Authoritarianism. *American Political Science Review* 84: 3–19.

Rivlin, Alice M. 1987. Economics and the Political Process. *The American Economic Review* 77: 1–10.

Rohde, Joy. 2009. Gray Matters: Social Scientists, Military Patronage, and Democracy in the Cold War. *The Journal of American History* 96: 99–122.

Rosen, Stephen Peter. 1982. Vietnam and the American Theory of Limited War. *International Security* 7: 83–113.

———. 1984. Systems Analysis and the Quest for Rational Defense. *The Public Interest* 76: 3–17.

Rosenberg, David Alan. 1983. The Origins of Overkill: Nuclear Weapons and American Strategy, 1945–1960. *International Security*: 3–71.

Rosenstein-Rodan, P.N. 1944. The International Development of Economically Backward Areas. *Royal Institute of International Affairs* 20: 157–165.

Rosovsky, Henry. 1965. The Take-Off into Sustained Controversy. *The Journal of Economic History* 25: 271–275.

Rostow, W.W. 1956. The Take-Off into Self-Sustained Growth. *The Economic Journal* 66: 25–48.

———. 1963. The Third Round. *Foreign Affairs* 42 (1): 10.

———. 1964. The Challenge of Democracy in Developing Nations. *The Department of State Bulletin* 50: 251–260.

———. 1965. The Chapter that Keynes Never Wrote. *The Department of State Bulletin* 52: 454–459.

———. 1966. The Sharing of the Good Life. *The Department of State Bulletin* 54: 803–811.

———. 1996–1997. The Case for the Vietnam War. *Parameters* 26: 39–50. Reprinted with Permission from The Times Literary Supplement, June 9, 1995.

Roxborough, Ian. 1988. Modernization Theory Revisited: A Review Article. *Society for Comparative Studies in Society and History* 30: 753–761.

Russo, Anthony. 1972a. Inside the RAND Corporation and Out: My Story. *Ramparts*: 45–55.

———. 1972b. Looking Backward: RAND Vietnam in Retrospect. *Ramparts* 40–42: 52–54.

Rustow, Dankwart A. 1968. Modernization and Comparative Politics: Prospects in Research and Theory. *Comparative Politics* 1: 37–51.

Sagan, Scott D. 1987. The Nuclear War Plan Briefing to President Kennedy. *International Security* 12: 22–51.

Schelling, Thomas C. 1957. Bargaining, Communication, and Limited War. *Conflict Resolution* 1: 19–36.

———. 1958. The Strategy of Conflict Prospectus for a Reorientation of Game Theory. *Journal of Conflict Resolution* 2: 203–264.

———. 1959. Surprise Attack and Disarmament. *Bulletin of the Atomic Scientists* 15: 413–418.

———. 1960a. Reciprocal Measures for Arms Stabilization. *Daedalus* 89: 892–914.

———. 1960b. The Retarded Science of International Strategy. *Bulletin of the Atomic Scientists* 16: 103–106.

———. 1960c. The Retarded Science of International Strategy. *Midwest Journal of Political Science* 4: 107–137.

———. 1961. Experimental Games and Bargaining Theory. *World Politics* 14: 41–68.

———. 1962a. Nuclear Strategy in Europe. *World Politics* 14: 421–432.

———. 1962b. The Role of Deterrence in Total Disarmament. *Foreign Affairs* 40: 392–406.
———. 1963. Strategic Problems of an International Armed Force. *International Organization* 17: 465–485.
———. 1964. Review of Strategy and Conscience, by Anatol Rapoport. *American Economic Review* 54: 1082–1088.
———. 1976. Who Will Have the Bomb? *International Security* 1: 77–91.
———. 1993. Nuclear Strategy in the Berlin Crisis, Washington, July 5, 1961. Document 56 in *Berlin Crisis, 1961–1962*, vol. 14 of *Foreign Relations of the United States, 1961–1963*, 170–172. Washington, DC: United States Government Printing Office.
———. 1959. Nuclear Weapons and Limited War. *U.S. Air Force Project RAND Research Memorandum, RM 2510.*
Schlesinger, Arthur. 1969. Vietnam and the End of the Age of Superpowers. *Harper's*: 41–49.
Seers, Dudley. 1962. Why Visiting Economists Fail. *Journal of Political Economy* 70: 325–338.
Sent, Esther-Mirjam. 2007. Some Like It Cold: Thomas Schelling as a Cold Warrior. *Journal of Economic Methodology* 14: 455–471.
Shils, Edward. 1955. The End of Ideology? *Encounters* 5: 52–58.
———. 1960a. Political Development in the New States. *Comparative Studies in Society and History* 2: 265–292.
———. 1960b. Political Development in the New States II. *Comparative Studies in Society and History* 2: 379–411.
———. 1970. Tradition, Ecology, and Institution in the History of Sociology. *Daedelus* 99: 760–825.
Shils, Edward A., and Morris Janowitz. 1948. Cohesion and Disintegration in the Wehrmacht in World War II. *The Public Opinion Quarterly* 12: 280–315.
Shultz, Richard. 1979. Coercive Force and Military Strategy: Deterrence Logic and the Cost-Benefit Model of Counter-Insurgency Warfare. *The Western Political Quarterly* 32: 444–466.
Simon, Herbert A. 1985. Human Nature in Politics: The Dialogue of Psychology with Political Science. *The American Political Science Review* 79: 293–304.
Skocpol, Theda. 2000. Commentary: Theory Tackles History. *Social Science History* 24: 669–676.
Smith, David Horton, and Alex Inkeles. 1966. The OM-Scale: A Comparative Socio-Psychological Measure of Individual Modernity. *Sociometry* 29: 353–377.
Social Science Research Council. 1961. The Comparative Roles of Groups in Political Systems. *Items* 15: 18–21.
Solovey, Mark. 2001. Project Camelot and the 1960s Epistemological Revolution: Rethinking the Politics-Patronage-Social Nexus. *Social Studies of Sciences* 31: 171–206.

———. 2004. Riding Natural Scientists' Coattails onto the Endless Frontier: The SSRC and the Quest for Scientific Legitimacy. *Journal of the History of the Behavioral Sciences* 40: 393–422.

Speier, Hans. 1957. Soviet Atomic Blackmail and the North Atlantic Alliance. *World Politics* 9: 307–328.

Steininger, Rolf. 1997. 'The Americans are in a Hopeless Position': Great Britain and the War in Vietnam, 1964–1965. *Diplomacy and Statecraft* 8: 237–285.

Streit, Peggy, and Pierre Streit. 1956. Lessons in Foreign Aid Policy. *The New York Times Magazine*, March 18: 15, 56, 58, 60 and 62.

Taylor Milton, C. 1961. South Viet-Nam: Lavish Aid, Limited Progress. *Pacific Affairs* 34: 242–256.

Tilly, Charles. 1992. Prisoners of the State. *Historical Sociology* 133: 329–342.

———. 1999. Survey Article: Power—Top Down and Bottom Up. *The Journal of Political Philosophy* 7: 330–352.

Tipps, Dean C. 1973. Modernization Theory and the Comparative Study of Societies: A Critical Perspective. *Comparative Studies in Society and History* 15: 199–226.

Tiryakian, Edward. 1991. Modernisation: Exhumetur in Pace (Rethinking Macrosociology in the 1990s). *International Sociology* 6: 165–180.

Tyvela, Kirk. 2011. 'A Slight But Salutary Case of the Jitters': The Kennedy Administration and the Alliance for Progress in Paraguay. *Diplomacy and Statecraft* 22: 300–320.

Walt, Stephen M. 2005. The Relationship Between Theory and Policy in International Relations. *Annual Review of Political Science* 8: 23–48.

Ward, Robert E. 1974. Culture and the Comparative Study of Politics, or the Constipated Dialectic. *American Political Science Review* 68: 190–201.

Warner, Geoffrey. 2003. Review Article: Lyndon Johnson's War? Part 1, Escalation. *International Affairs* 79: 829–853.

Webb, Eugene J. 1968. A Review of Social Science Research in Vietnam with Procedural Recommendations for Future Research in Insurgent Settings. Research Paper P-450, Institute for Defense Analyses, Science and Technology Division, December.

Weiss, Carol H. 1977. Research for Policy's Sake: The Enlightenment Function of Social Research. *Policy Analysis* 3: 531–545.

White, Thomas D. 1963. The Defense Intellectuals. *The Saturday Evening Post* 236: 10–12.

White, Gilbert F. 1964. Vietnam: The Fourth Course. *Bulletin of the Atomic Scientists* 20: 6–10.

White, T.H. 1967a. The Action Intellectuals: Part 1. *Life* 62: 43–76.

———. 1967b. The Action Intellectuals: Scholarly Impact on the Nation's Past Part 2. *Life* 62: 56–74b.

Whiting, Allen S. 1972. The Scholar and the Policy-Maker. *World Politics* 24: 229–247.
Whyte, William Foote. 1943. Instruction and Research: A Challenge to Political Scientists. *American Political Science Review* 37: 692–697.
Wiesner, Jerome B. 1960. Comprehensive Arms-Limitation Systems. *Daedalus* 89: 915–950.
Williams, Albert P. 1971. South Viet-Nam's Development in a Postwar Era: A Commentary on the Thuc-Lilienthal Report. *Asian Survey* 11: 352–370.
Willner, Ann Ruth. 1964. The Underdeveloped Study of Political Development. *World Politics* 16: 468–482.
Wohlstetter, Albert. 1959. The Delicate Balance of Terror. *Foreign Affairs* 37: 211–234.
———. 1963. Scientists, Seers and Strategy. *Foreign Affairs* 41: 466–478.
Wohlstetter, Albert, and Roberta Wohlstetter. 1965. Controlling the Risks in Cuba. *Adelphi Papers* 5 (17): 3–24.
Zasloff, Joseph J. 1962–1963. Rural Resettlement in South Viet Nam: The Agroville Program. *Pacific Affairs* 35: 327–340.
Zilsel, Edgar. 2000. The Sociological Roots of Science. *Social Studies of Science* 30: 935–949.

Electronic Sources

(no author) Final report of 1 May 1968 Hamlet Evaluation System Study, downloaded on June 1, 2017 from www.dtic.mil/dtic/tr/fulltext/u2/839821.pdf
Donnell, John and Gerald C. Hickey. 1962. The Vietnamese 'Strategic Hamlets' a Preliminary Report, RAND Corporation, RM-3208, September. Downloaded on June 6, 2017 from www.rand.org/content/dam/rand/pubs/research_memorandum/2006/RM3208
Donnell, John, Guy J. Pauker and Joseph J. Zasloff. 1965. Viet Cong Motivation and Morale, 1964: A Preliminary Report," RAND Corporation, March. RM-4507/3ISA. Downloaded on June 12, 2017 from: www.rand.org/content/dam/rand/pubs/research_memoranda/2006/RM4507.3
Ellsberg, Daniel. 1968. The Theory and Practice of Blackmail, July. Downloaded on February 22, 2014 from http://www.rand.org/pubs/papers/P3883
———. 1969. Some Lessons From Failure in Vietnam. RAND Corporation, July. Downloaded on June 27, 2017 from www.rand.org/pubs/papers/p4036.html
Goure, Leon, A.J. Russo and D. Scott. 1965. Some Findings of the Viet Cong Motivation and Morale Study. RAND Corporation, June–December. Downloaded on June 14, 2017 from www.rand.org/content/dam/rand/pubs/research_memorandam/2006/RM4811-2

Hickey, Gerald C. 1967. Accommodation in South Vietnam: The Key to Sociopolitical Solidarity. RAND Corporation, October. Downloaded on June 28, 2017 from www.rand.org/content/dam/rand/pubs/papers/2008/P3707.pdf

Hosmer, Stephen T. and Sibylle O. Crane. 1962. Counterinsurgency: A Symposium, April 16–20. RAND Corporation. Downloaded on June 5, 2017 from www.rand.org/content/dam/rand/pubs/reports/2006/R412-1

Komer, Robert. 1970. Impact of Pacification on Insurgency in South Vietnam. RAND Corporation, 1970, RAND Corporation 1970. Paper Prepared for the American Political Science Association Meeting of September 1970. Downloaded on June 2, 2017 from www.rand.org/content/dam/rand/pubs

———. 1972. The Malayan Emergency in Retrospect: Organization of a Successful Counterinsurgency. RAND Corporation, February. Downloaded on June 2, 2017 from www.rand.org/content/dam/rand/pubs/reports/2005/R957

Maddux, Thomas, ed. 2006. Bruce Kuklick, Blind Oracles: Intellectuals from Kennan to Kissinger. H-Diplo Roundtables. h-diplo@h-net.msu.edu

Mastny, Vojtech. 2014. Planning for the Unplannable. from the Parallel History Project on Cooperative Security. Downloaded on April 9, 2014 from: http://www.php.isn.etnz.ch/collections/colltopic.cfm?ing=en&id=15365

McCone, John. 1964. Memo to McGeorge Bundy, January 17 from https://history.state.gov/historicaldocument/frus1964-68v01/d209

Morgenstern, Oskar. 1961. The Cold War Is Cold Poker. *New York Times Magazine*, February 5. Downloaded on March 6, 2014 from NYTimes.com/mem/archive

Putnam, Robert D. 2003. APSA Presidential Address: The Public Role of Political Science. June. http://www.apsanet.org

Schelling, Thomas C. 1979. A Tribute to Bernard Brodie and (Incidentally) to RAND, July. Downloaded on March 11, 2014 from http://www.rand.org/content/dam/rand/pubs/papers/2006/p6355

———. Interview. http://www.gametheorists.com/interviews/schelling.html

Wagner, R. Harrison. 2001. Who's Afraid of 'Rational Choice Theory'?, October 2001 Paper Downloaded on March 28, 2011 at http://hw.webhost.utexas.edu/papers/rct.pdf

Wolf, Charles Jr. 1956. Economic Development and Mutual Security: Some Problems of U.S. Foreign Assistance Programs in Southeast Asia. RAND Research Memorandum, RM-1778-RC, 14 August. Downloaded on June 8, 2017 from www.rand.org/content/dam/rand/pubs/research_memorandum/2007/RM 1778

Zasloff, Joseph J. 1964. The Role of North Vietnam in the Southern Insurgency. RAND Corporation, Research Memorandum, RM 4140, July. Downloaded on June 8, 2017 from www.rand.org/content/dam/rand/pubs/research_memoradum/2008/RM4140

Government Documents

National Academy of Sciences. 1968. Advisory Committee on Government Programs in the Behavioral Sciences. *The Behavioral Sciences and the Federal Government*, Publication 1680, Washington, DC.

U.S. Congress. House. 1965. Committee on Foreign Affairs. Subcommittee on International Organizations and Movements. *Winning the Cold War: The U.S. Ideological Offensive*. Report No. 4. 89th Cong., 1st Sess.