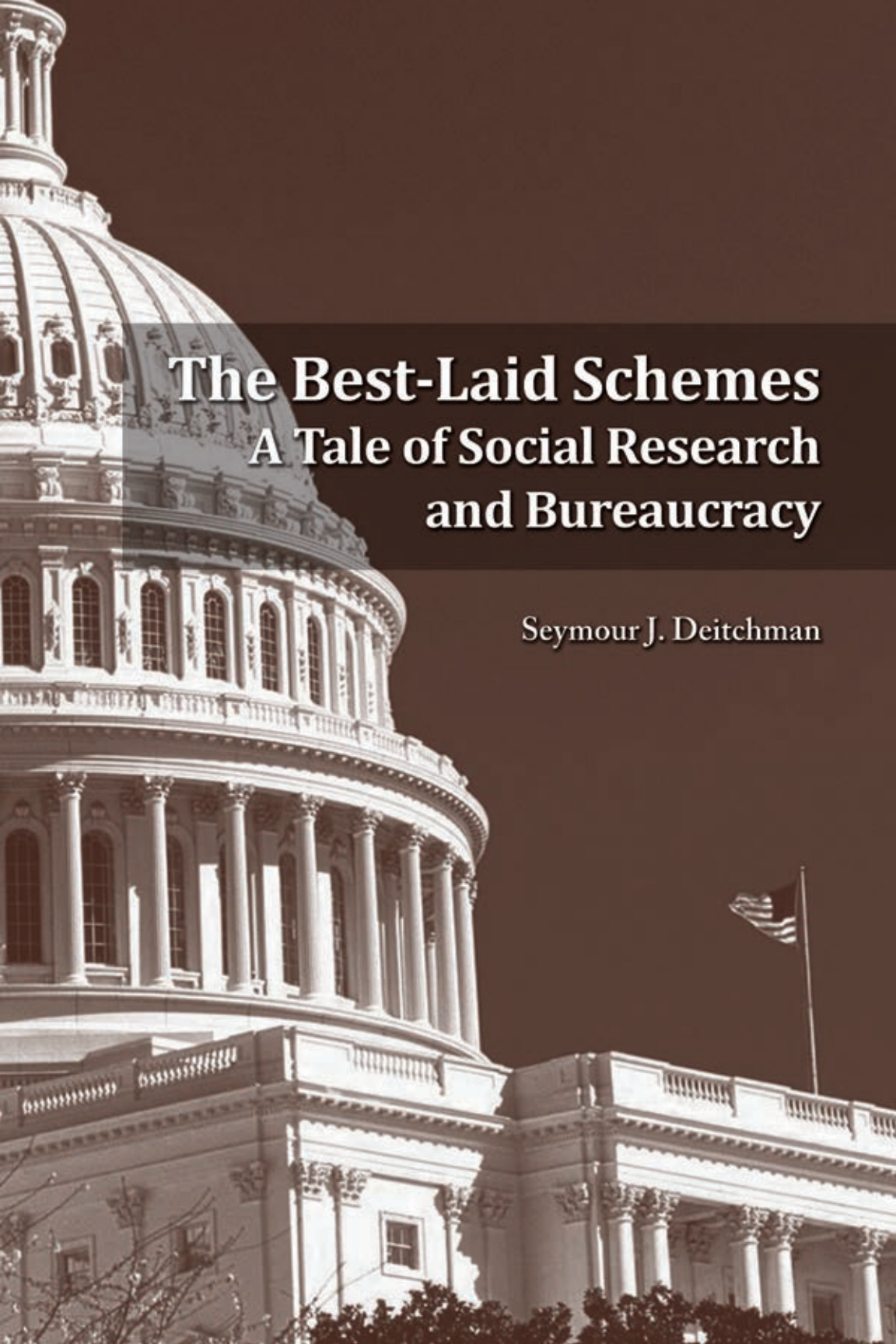

The Best-Laid Schemes
A Tale of Social Research and Bureaucracy

Seymour J. Deitchman

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by
Seymour J. Deitchman

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The views expressed in this book are solely those of the authors. They do not necessarily reflect the opinions of the organizations for which they work, Marine Corps University, the U.S. Marine Corps, the Department of the Navy, or the U.S. government.

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To Rains

"But Mousie, thou art no thy lane
In proving foresight may be vain;
The best-laid schemes o' mice and men
Gang aft agley,
An' lea'e us nought but grief an' pain,
For promised joy!"

~Robert Burns, 1785

Author's Acknowledgements

My purpose for writing this book is to tell the story of a great experiment in social research in which I participated and whose events were important and instructive for American social science in the mid-1960s and later.

I have attempted to limit what I have written to that which I experienced or observed, and to limit personal aspects of the narrative to those which are required to portray the context and the atmosphere of the events. It was necessary, nevertheless, to describe some of what others with whom I have had little contact experienced and expressed. Such descriptions cannot but be tempered by my own views. Thus, I am afraid the reader will find those views pervading everything, even though I have made a conscious attempt to minimize my intrusion. I regret any resulting distortion of "truth" that other participants may find, but at the same time I will defend opinions and conclusions expressly identified (and some that are implicit).

In the interest of saving time and effort, I have taken some liberties with the usual practices of scholarship in annotating references and supporting material. Such annotation is given to support statements of fact that may be generally known or that may be controversial with two important exceptions. Official records, correspondence, or documents that are described and discussed can be presumed to exist, but I have not listed them as explicit references if they have not been published or otherwise made generally available. Newspaper articles identified in the text by source, author, and date did not seem to warrant the embellishment of formal citation.*

Some of the events described in this narrative are "recorded" only in the memories of the individuals involved, including my own. In such cases, the responsibility for errors of fact or differences of perception is purely my own.

You will find that, in many cases, I have gone to some lengths of circumlocution to avoid naming scientists, civilian or military officials, and even countries in connection with particular aspects of the events or particular research projects. As the book will show, the material has in it, often at unexpected turns of subject and time, the capacity to stimulate controversy and cause embarrassment. This applies especially to many members of

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the academic community who are still (as this preface was originally written for the 1976 edition) feeling the effects of these adventures that touched them and who have, in some cases, been subject to attack and vilification because of the role they played. It has definitely *not* been my purpose to write an exposé, and I have tried to avoid mentioning names wherever I judged that specters from the past would be unwelcome and would add little of importance to the narrative.

My heartfelt thanks go out to many individuals. They include those who supported the efforts that, if events had taken a different turn, might have affected more history than simply that of social research; those whose comments and assistance were invaluable both during the events described and in shaping the manuscript; and those without whose help on workday matters I could not have finished it.*

Among the first are Drs. Harold Brown, John McLucas, Charles M. Herzfeld, and Eberhardt Rechtin; all or whom perceived potential value in the efforts that I undertook to stimulate and guide, and they gave me the freedom and the strong support within Department of Defense (DOD) bureaucracy to make and sustain the attempt despite the problems created and the diversions from other more pressing business. The late Dr. S. Rains Wallace and Drs. Jesse Orlansky and Eugene Webb were sources of knowledge and understanding about the intricacies of the world of social science. Without their help and advice, I would indeed have been lost in a strange forest. Their knowledge of the events made their comments on the manuscript especially helpful and valuable. Mrs. Edna Majors deserves a special note of thanks for having borne the tedium of typing and retyping the 1976 manuscript for what must have seemed endless time. It would have been impossible to track down references lost in the pressure of events without the able assistance of Mrs. Evelyn Fass and Mrs. Thomasina Jones. My wife observed the passing events from a ringside seat and shared with me the strain of long hours and stressful conflicts. Finally, to all those other scientists, managers, and "target populations" who helped, who participated, and who bore suffering they did not seek, I must express my gratitude and, where appropriate, my sympathy.

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* Some of the people listed here have passed away since this book was originally published (and, as the dedication page shows, even before in one critical case). Rather than repeating "the late," the text has been left as written in 1976.

It should be obvious that, although I have worked for the government and worked for an organization that serves the government at the time this was written, I am speaking for neither of them in any part of this book. The book is about government programs. But the descriptions and interpretations of those programs, of the events surrounding them, and of the policies that supported or affected them are mine alone. It must be remembered, also, that the programs and policies I have thus described existed at another time and in circumstances different from those that apply today. It is de rigueur in modern bureaucratese to admonish that nothing in this book should be taken as implying that the programs and policies described still exist in the DOD or any other agency, or that anything like them will be espoused in the future.

Having said that, however, I must bring my reaction to the events in this book up to date by commenting on what I observed as I reviewed the text for this U.S. Marine Corps University Press edition. In a word, I found its relevance to events and circumstances of today's world in the national security sphere uncannily unchanged. As we wind down our participation in two major wars—in Iraq and Afghanistan—and face the potential for another conflict should Iran persist in its apparent pursuit of nuclear weapon capabilities, the need to understand the culture and thought processes of our actual and potential opponents is as urgent and important as ever. For example, we may have understood enough about the Soviet mindset to tolerate the deterrence standoff known as "mutual assured destruction." But what do we know about their "deterrability" for a nation (Iran) with rulers that have explicitly stated that another nation (Israel) should be wiped off the map, even if that means much of their own population would be killed by a last-gasp Israeli return strike, and that they are willing to sacrifice a generation of young men by having them act as human minesweepers as it happened in the Iran-Iraq war of the 1980s?

Thus, it seems eminently reasonable to ask the social sciences, once again, to give us insight into the mindsets of the peoples in the broad swath of land from Egypt through Afghanistan and Pakistan. And indeed that was done, but with a very different outcome from the one described in this book. The facts and reasons for that are discussed in context at the end of chapter 3.

In that connection, I should note here the more extended rationale for the way I chose to bring the modern world (of 2013) into this narrative. I chose to do so by adding comments in footnotes when context suggested that an update to today's times would be in order, leaving the original text

untouched. The reason for this was that I found that if I attempted to add changes to the main text to bring the events up to date, the need for such changes would cascade through the chapters, before and after the specific additions to update to modern times. Updates of this nature would have meant major rewrites of some sections of the book, adding apparent disconnects to the original narrative. In particular, it would have added comments about matters where, unlike those in past events, I have no personal knowledge through participation. And, finally in this connection, I am indebted to Sharon Weinberger for her reminders that the events of bureaucratic infighting described at the end of the last chapter were as momentous, or more so, than those described in this book—ironically, involving the same departments of the federal government that also figured into past events.

Credits

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Abbreviations and Acronyms

AFOSR	Air Force Office of Scientific Research
AID	Agency for International Development
ANZUS	Australia, New Zealand, U.S. Security Treaty
ARO	Army Research Office
ARPA	Advanced Research Projects Agency
CI	Counterinsurgency
CIA	Central Intelligence Agency
CINCSO	Commander-in-Chief, Southern Command
CINCPAC	Commander-in-Chief, Pacific
CINFAC	Counterinsurgency Information and Analysis Center
DCSOPS	U.S. Army, Deputy Chief of Staff for Operations, and his "office"
DDR&E	Director of Defense Research and Engineering
DOD (DoD)	Department of Defense
DSB	Defense Science Board
FAR	Foreign Area Research Coordinating Group
FCRC	Federal Contract Research Center
GAO	General Accounting Office
GVN	Government of Vietnam (refers to South Vietnam)
HumRRO	Human Resources Research Organization
IDA	Institute for Defense Analyses
INR	U.S. State Department, Bureau of Intelligence and Research
ISA	International Security Affairs (refers also to the Assistant Secretary of Defense for International Security Affairs and his "office")
JUSPAO	Joint U.S. Public Affairs Office
MAAG	Military Assistance Advisory Group
MACV	Military Assistance Command, Vietnam

MIT	Massachusetts Institute of Technology
NAS	National Academy of Sciences
NATO	North Atlantic Treaty Organization
NLF	National Liberation Front (South Vietnam)
NRC	National Research Council
ODDR&E	Office of the Director of Defense Research and Engineering (i.e., the director's staff)
ONR	Office of Naval Research
OSD	Office of the Secretary of Defense
RAC	Research Analysis Corporation
RAND	The RAND Corporation
R&D	Research and Development
SAC	Strategic Air Command
SECDEF	Secretary of Defense
SORO	Special Operations Research Office
SRI	Stanford Research Institute
USAPRO	U.S. Army Personnel Research Office
USIA	United States Information Agency
VC	Viet Cong

Foreword

By Kerry Fosher*

> "The administrator uses social science the way a drunk uses a lamppost, for support rather than illumination."[1]
>
> ~Alexander Leighton

When I first began to work with the military in 2006, helping a colleague start the U.S. Air Force's cross-cultural competence effort, I was already involved in the debates about the way national security organizations try to use social science research and social scientists. I had done my field research on homeland security, beginning a few months before the attacks of 9/11, and continued the work at a research center at Dartmouth Medical School. Because of my background, I was asked to serve as a member of the American Anthropological Association's (AAA) commission looking into the ethics of conducting work related to national security. In graduate school, the problematic relationship between anthropology and both the Department of Defense (DOD) and the intelligence community (IC) during the First and Second World Wars and the Vietnam War became clear. My time on the AAA commission only deepened my understanding of the cyclic dance of engagement, misunderstandings, betrayals of trust, frustration, and distancing that have played out at various points in U.S. history. As I worked across military training and education, intelligence, and policy, I could see the dance starting again.

However, anthropology's memory is long and DOD's is short. Most of the things I read were from an anthropological viewpoint. I knew I should be getting other perspectives, but they were hard to find. DOD personnel sometimes acknowledged past efforts to deal with "the culture problem." Military and civilian personnel would point to the U.S. Marine Corps' *Small Wars Manual* or the national character studies done during WWII and wonder why those efforts had ended. Aside from some articles and theses written by military personnel between the 1960s and 1990s, my fellow anthropologists and I found few accounts of past cycles from the perspective of DOD personnel, and almost none that describe the institutional contexts of the problems.

---

* Kerry Fosher is the director of research for the Translational Research Group at the Center for Advanced Operational Culture Learning, Marine Corps University.

Several of us working with DOD stumbled across Seymour (Sy) Deitchman's book, *The Best-Laid Schemes*, at roughly the same time.[2] Although *Schemes* was out of print, we quickly purchased the used copies still in circulation, only loaning them out with the threat of dire consequences to a borrower who did not return the book in a timely fashion. Here, at last, was an account from someone who could offer a different perspective on the tangled bureaucratic and discursive reasons for why things went wrong in the 1960s. These cautionary tales might help us anticipate obstacles and, perhaps, avoid intractable quagmires. We did not expect the book to be a roadmap for success, and it is not. It does, however, chronicle how easily such efforts can become twisted by and mired in the institutional orientations and habits of large bureaucracies.

Between 2006 and 2011, DOD developed a ravenous appetite for both knowledge about culture and social science expertise, especially from anthropologists. They were willing to expend great sums—astonishing sums to an academic sensibility—in the pursuit of this knowledge and expertise. Unfortunately, as with past cycles I had read about, the pursuit often was indiscriminate, based on outdated fantasies about what culture is and what anthropologists do. Social sciences, perhaps especially the fieldwork-focused sciences like anthropology, are not an easy fit with how DOD wants to engage with the world. Scientific developments since the 1960s have moved social science away from tidy ethnographies that characterize the activities of groups of people in terms of Western categories, such as politics, economics, and religion. Anthropologist Tim Ingold wrote that it is "more realistic to say that . . . people *live culturally*, rather than that they *live in cultures*."[3] It is more accurate to look at how people go about in the course of their daily lives, creating, maintaining, and changing the patterns we once would have referred to as "their culture."

This scientific shift is not a fad; it is based on the fundamental realities of human social existence. These realities have relevance both in operations and military planning. A classical ethnography of Fallujah written in 2000 would have done little to help a Marine understand the complexities of engaging with that same population a few years later as the people coped with the changes brought by the conflict. Likewise, a description of Pashtunwali written for the whole of Afghanistan would not help a planner anticipate the diverse and operationally significant ways this "code" was understood and used in communities across that country. Yet in trying to integrate contemporary social science into DOD, we knew we were up against strong

preconceptions and an equally strong desire for particular types of solutions. These organizations wanted databases, models, simulations, systems, field guides—all things DOD and the IC know how to acquire and integrate, but that contradict what we know about culture. It was difficult to make headway with more practical approaches, such as teaching military personnel how to figure out complex, changing cross-cultural situations on the ground. As Sy writes in chapter 20, "Part of this 'impact' problem, of course, was the exact manner in which often unexpected results struck the preconceptions of the recipient who was responsible for the action."

For the group of social scientists with whom I collaborate, quotations from *Schemes* often had us laughing and cringing at the number of passages that could have been lifted from an email written the day before instead of a book dated 1976. We knew we were facing a seemingly monolithic structure and deeply entrenched ideas about how the world worked in DOD institutional environments that were unfamiliar for many of us. Yet, we also knew—from the same social science so uncomfortable for DOD—that such structures are never as impenetrable as they seem. Every institution is created and then either maintained or gradually transformed through the daily actions of individual people. Change often takes a long time, longer than the career of any one person, but we knew it was possible. We hoped to help create or steer small changes that might have significant effects down the line.

Consequently, *The Best-Laid Schemes* made me angry when I first read it. I already knew we would repeat many of the mistakes the book outlines. Yet, as somebody trying to effect change without falling into the same sad dance of past generations, it was frustrating to recognize the patterns Sy described, as some within the national security-supporting establishment began to resist the changes we sought to make. Also, as an anthropologist, I disagreed strongly with how the concerns of anthropologists were characterized and with many of the programs described. Still, I was able to learn much from the book. I can spot re-emerging patterns more quickly, and I have often used examples from it to point out the negative long-term consequences of current efforts, such as those that try to employ anthropologists for field research in conflict zones.

Beyond insights on organizational politics and cautionary tales, *The Best-Laid Schemes* contains two overarching lessons for current and future efforts. First, social scientists and defense personnel failed to communicate their constraints and capabilities sufficiently for integration to happen. So-

cial science cannot do everything DOD wants. Some of what military organizations want is not scientifically possible or violates the ethical codes necessary for scientific enterprise. Likewise, DOD, especially the supporting establishment, is not a blank slate onto which scientists can layer current theory, methods, and information. Scientists new to DOD have to build their understanding of the existing context, including preconceptions about social science, budgetary and human resource processes, time constraints on training and education, and powerful discourses about the desirability of technology-based solutions. All of these issues hinder the quick uptake of new approaches.

Second and, I believe, the most important lesson is the danger of indulging in crisis mode behavior rather than building something sustainable. It may appear easier to build something new and separate than to graft a new approach onto existing processes. Work without the constraints of bureaucracy seems more exciting, but whatever you build that way is ephemeral. Stand-alone programs can be cut easily or simply stripped of resources and left to die with little or no effect on other parts of the institution. Truly integrating social science means fully entangling that science in a bureaucratic labyrinth, while simultaneously keeping the institution from altering the science to fit its comfort zone. New approaches must be turned into business-as-usual, so that they are difficult to excise.

Even after all this time, *Schemes* still creates a strong impression as I watch DOD retreat from promising (but decidedly unshiny), long-term training, education, and professional development efforts and instead place their faith in technology-based solutions. I wince as leaders advocate a "break glass in case of stability operations" approach to academic outreach that, in the face of nearly a century of contrary evidence, assumes social science expertise will be available and willing whenever and however DOD needs it. Despite some successes, DOD is indeed on the verge of making many of the same mistakes Sy documented, with consequences that will be borne by future junior military personnel and the people they encounter. There is still time to make course corrections. It is not quite time for one of us to write a sequel to *The Best-Laid Schemes*.

When we first approached Sy about reprinting this book, he modestly questioned its value in the current environment. Fortunately, we were able to convince him to proceed, and Marine Corps University Press came on board as the publisher. Even though the author watched as many in DOD ignored his first effort, I am deeply grateful that he agreed. While its im-

pact on the current cycle is uncertain, it is still a critical work to get back into circulation for the future. This will not be the last time social science and DOD get out on the dance floor.

So, to the reader who is seeing *The Best-Laid Schemes* for the first time in 2014, 2024, or 2034, before you get too far into your work, stop and read. Read this book, read accounts of the same issues by social scientists, read everything you can get your hands on. Pay special attention to accounts that make you uncomfortable about what you are doing. Be willing to accept that you did not invent the dance and that these lessons do apply to you. Accept that you will not change everything and that real change may mean sacrificing personal accolades. If you want to create long-term change, you must learn about both current scientific practices and the institutions with which you work. Remember that, even though you are not the *first* person to do this work, you also are not the *last*, so consider what you want to leave behind. Finally, you will have to make compromises, but you will also have opportunities to rattle people's preconceptions. Make good use of them.

Introduction

By Jeff Bearor*

The military is all about action and success—and the straighter the line to a successful conclusion the better. One of my favorite sayings to those I work with is that "I don't want to chase down rabbit holes and find no rabbits." When we started the U.S. Marine Corps Center for Advanced Operational Culture Learning in 2005, our purpose was to train and educate Marines and Marine units deploying for combat in Iraq and Afghanistan and, ultimately, to support successful outcomes on the battlefield. We did not fully realize that the onion we were attempting to peel had so many and varied layers.

The premise was simple—extract from academics, social scientists, regional experts, and the intelligence community anything and everything they knew about the impact of culture and human interaction on our current battlefield problem sets, distill it down "Barney-simple," and provide that data as a training resource to Marines and their leaders so they can successfully plan for and operate in the "human terrain" they navigate. What we found was that for every question we asked, there were multiple answers—it was never going to be "Barney-simple." Many times, we realized were not even asking the right questions.

Peering at this aspect of operational and training challenges through a strictly military lens and expecting to find easy-to-understand answers to complex, inter-related problems was not going to happen. Planners and commanders would have to get used to more ambiguity and complexity than our robust training and education programs supported.

Sitting across the table from highly educated subject matter experts, it often seemed that we might as well have been from different planets. I heard what they said, I could make sense of most of it, but I could not figure out how to make it useful down at the lance corporal level. Our academic partners patiently schooled us about the need to understand the context of intercultural interactions; the need to recognize the very basic

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* Jeff Bearor created and was the first director for the Marine Corps Center for Advanced Operational Culture Learning. Bearor was the driving force behind the standup of the Center for Irregular Warfare (now the Small Wars Center and Irregular Warfare Integration Division at Quantico). He most recently served as the executive deputy at the USMC Training and Education Command and was responsible for all formal training and education programs for Marines around the world. Currently, he serves as the senior director for security for the Department of the Navy.

differences that frame the American approach from the Arab or Afghani approach; and the absolute requirement to correctly identify the "powerful" in the conversation from the "powerless" and to establish early on that "power-distance" hierarchy. But also, how to make it most useful in the least amount of time—that was the immediate dilemma.

We focused on an approach that took into account our view of the abilities of Marines and Marine Corps leadership based on the training time crunch. We would provide some training for all Marines, a small measure of training for corporals, a larger measure for captains, etc. There was no time for much more.

What happened with that minimum of training was astounding. Most Marines not only "got it" but they chased our instructors down for more. They wanted more language training; and they wanted to read histories and textbooks to deepen their understanding of both the particular cultural aspects of the area they would operate in and the underlying concepts of cultural interaction. So we provided more. We supported multi-day seminars and month-long language training sessions. We wrote textbooks, and we incorporated everything we learned into professional military education.

Marines took that training and education and applied it to their specific operational challenges and, with typical Marine genius, made it work. We redistributed those hard-won lessons back into the training pipeline and, over time, Marines and their units became fairly proficient in understanding and utilizing their knowledge of "operational culture."

I tell this story for a couple of reasons. Marines will adapt and generally understand very quickly when they have gaps in their abilities. It is incumbent on the Corps to forecast what those new or expanded abilities may need to be and to provide the means to fill the gaps.

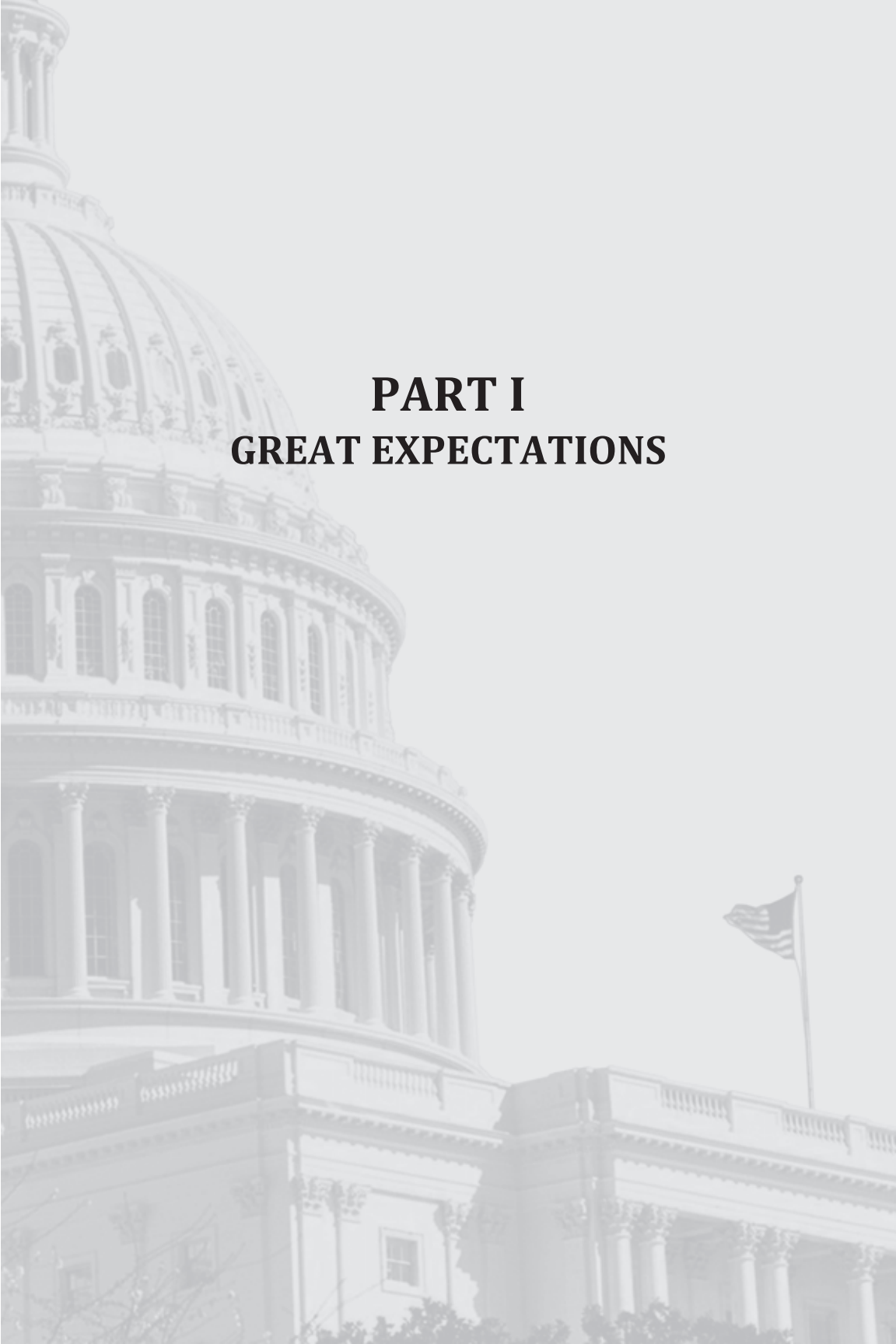
In my estimation, the Corps also needs to focus on leveraging the power of social science to understand the innate abilities, strengths, and weaknesses of Marines so that strengths are maximized and weaknesses diminished. And, by fully utilizing the power of social science research, Marines may be better prepared to do the things we ask them to do—particularly when approaching the complex problem sets associated with foreign operations where the interaction with the local people and their cultural imperatives are more important than tactical maneuvers and firepower.

Math, physics, and technology we get—those disciplines usually provide a straight-line answer and end state. Social science outputs may provide a deeper and more nuanced understanding of the problem, but many times does not lead us to "the" answer and certainty we seek. Military leaders want certainty in the midst of ambiguity, and we want "science" that gets us to that happy place.

The Best-Laid Schemes illustrates that disconnect between what military leaders seek and what social scientists can provide. That does not mean that social science is not significantly useful at every level. This is particularly true for the Corps because each and every Marine is a complex social being seeking his or her place amongst peers and leaders.

Have we fully leveraged the social sciences so we understand the human capital that marches into our ranks each year? Do we understand the youth now in high schools around the country so that we can lay the foundation in the Corps' training and education establishment to take advantage of their strengths? Do we fully understand what "soft skills" we will need to apply in the next fight? I would argue that we do not—that we are not yet ready to ask the right questions much less leverage the outputs for a better grasp of social science.

I believe there is an answer. The Marine Corps requires the ability to ask the right questions and frame the *translational research* to make findings from basic social science useful for practical applications in our training and education institutions and ultimately in the field. We have created a nascent capability to do that. However, we need to expand it so we can ask the right questions now, which will prepare us to apply the answers to our current problem sets while we reset the Corps and prepare for what is coming next. Otherwise, we will not find any rabbits down those rabbit holes.



PART I
GREAT EXPECTATIONS

Chapter 1
Antecedents

In the early 1960s, while the United States was becoming involved in a long and difficult war in a strange and far-off corner of the world, the Department of Defense (DOD) undertook what turned out to be a painful research experiment intimately connected with and driven largely by that war. That experiment, which is the subject of this book, would have profound implications, though still not fully recognized, for social research in the services of the federal government.

The research and the war grew together from America's increasing involvement, after World War II, in the affairs of the former European colonial empires. Conditioned by our deep cultural and historical interrelationships with the nations of Western Europe, and reinforced by the ebb of their power in Asia and Africa, we believed it essential to our own safety and theirs to block the advance of a new and hostile ideology that seemed to be on the verge of sweeping the world with the same force as that of Islam. We were involved in the Berlin blockade, the Korean War, the "loss" of China and Cuba, and the lengthy conflicts in Greece and several countries of Southeast Asia. The increasingly strident Soviet and Chinese promises to support "wars of national liberation" were taken to be a euphemism for a new and (for us) dangerous approach to "conquest" of regions where we deemed our influence, and therefore our ultimate survival, to be at stake. While there were obviously many nonmilitary facets—political, economic, cultural, and informational—to the defenses we had constructed over the years, the military aspects of the worldwide "cold war" conflict also loomed large to successive American governments. By the late 1950s, a large, worldwide American military presence had become an integral part of our policy and of our attempts to contain expansion of the major communist powers—powers that were not then viewed as a group holding diverse and subtly different creeds. But by the early 1960s, it was becoming clear that, despite this military presence and our aid programs, we were not succeeding as we hoped in transmitting military capability for local self-defense to developing countries over whom we had thrown our protective mantle.

Enter, now, into the ambience of the early Kennedy era. An uneasiness had grown in America about our image and our position in the world. Nixon had been mobbed in South America; Eisenhower had been prevented by mob action from visiting Japan; and the embarrassment of the

U-2 affair was still palpable. The essence of Kennedy's election campaign was that he would "get America moving again." When he took office, he brought with him intellectuals and intellectualism from the northeastern universities—especially Harvard and MIT. Some of the defense thinkers of the RAND Corporation—Charles Hitch, Alain Enthoven, and Henry Rowen—entered the DOD. Theories of strategy, economic development in the new world, the formation and use of military power, and foreign policy that had been incubating during Eisenhower's presidency blossomed, and those who held them were placed in positions where they could apply them. The atmosphere was one of change, of ferment, of self-confidence—of "knowing" what had to be done and of not questioning "can do." It would all lead to a better world. It was the time of Camelot.

President Kennedy took office to face a crisis over Berlin; the beginning of active guerrilla warfare in Vietnam; a Laos where their shaky neutrality had been upset in favor of a Western-oriented government whose Communist-supported forces were in the process of attacking and defeating; and a government in Cuba that was openly hostile to the United States and declared both its ties to the Soviet Union and its intention to carry *Fidelismo* to the rest of Latin America. All this was duly noted from the start. In his inaugural address Kennedy said:

> Let every nation know, whether it wishes us well or ill, that we shall pay any price, bear any burden, meet any hardship, support any friend, oppose any foe, to assure the survival and the success of liberty. . . . In the long history of the world, only a few generations have been granted the role of defending freedom in its hour of maximum danger. I do not shrink from this responsibility—I welcome it.

His speech would become the call to resistance against the "wars of national liberation" for which Nikita Khrushchev, in January 1961, promised "resolute" Soviet support.[1] Kennedy met with Khrushchev in Vienna, Austria, and left the meeting feeling that he was up against a tough opponent; one who would not "give" easily in Europe or elsewhere.[2] Kennedy found at the Bay of Pigs that guerrilla warfare is not an easy instrument to use; and the experiences of China, Indochina, Greece, Malaya, the Philippines, Algeria, and Laos showed that when it was used by a tough, well-organized and resourceful enemy it was not easy to counter. Taking seriously the threat to American power and influence implicit in Khrushchev's words, and in the later elaboration on the theme by Khrushchev and the Chinese Com-

munists,[3] he set about building our military and government instruments to meet an obvious and serious threat. That challenge may appear shadowy and full of braggadocio from the vantage point of the bitter experience of all parties in the late 1960s. But who can deny that it was uttered seriously and was meant to succeed, if it could, 10 years earlier?

To meet it, Kennedy built the U.S. Army up from 11 to 16 active divisions, with corresponding increases in the strategic tactical naval and air forces. He added to airlift and sealift forces to improve our ability to deploy overseas—the prospect and the propriety of intervention to meet the threat were foreseen and accepted. The U.S. Army Special Forces became a favorite of the president, and were expanded and oriented toward training foreign troops in counter guerrilla warfare.[4] The president stressed the need for the military to learn about what seemed to be a new kind of warfare. He said in an address at West Point in June 1962:[5]

> It is a form of warfare uniquely adapted to what has been strangely called "wars of liberation," to undermine the efforts of new and poor countries to maintain the freedom that they have finally achieved. It . . . preys on economic unrest and ethnic cultures. . . . These are the kinds of challenges that will be before us in the next decade if freedom is to be saved, a whole new kind of strategy, a whole different kind of force, and therefore a new and wholly different kind of military training. . . . The mission of our armed forces . . . is to master these skills and techniques and to be able to help those who have the will to help themselves . . .

The justification for this interest was at once simple and lofty. A year earlier, Walt Rostow, then in the State Department, had said[6] ". . . it will permit American society to continue to develop along the old human lines which go back to our birth as a nation—and which reach deeper into history than that—back to the Mediterranean roots of Western life. We are struggling to maintain an environment on the world scene which will permit our open society to survive and to flourish." There was a pervading sense of the importance and of the urgency of undertaking the task of assistance overseas. In his statement in spring 1962, supporting the fiscal 1963 defense budget, Robert McNamera said:[7]

> There was come into prominence, in the last year or two, a kind of revolt which Mr. Khrushchev calls wars of national liberation or popular revolts, but which we know as insurrection, subversion, and covert armed aggression. I refer here to the kind of war

> which we have seen in Laos and which is now going on in south Vietnam. Actually, it is not a new Communist technique. We have seen it in many other parts of the world since the end of World War II. . . . We have a long way to go in devising and implementing effective countermeasures against these Communist techniques. But this is a challenge we must meet if we are to defeat the Communists in this third kind of war. It is quite possible that in the decade of the 1960s, the decisive struggle will take place in this arena.

At the same time, McNamera recognized that "to meet successfully this type of threat will take much more than military means alone. It will require a comprehensive effort involving political, economic, and ideological measures as well as military."[8]

President Kennedy started to organize and interconnect the DOD and other parts of the government to undertake the task. The State Department was assigned diplomatic and political roles and the responsibility for coordinating the efforts of other key government agencies. The Agency for International Development (AID) would continue its task of economic assistance, but also was given the responsibility to tailor this assistance, where necessary, to help recipient nations build their strengths against subversion and insurgent forces. This included helping build police and other paramilitary internal security forces. The United States Information Agency (USIA), through its overseas offices, was supposed to carry out not only its traditionally assigned duties of gathering and disseminating information and studying the U.S. image overseas, but it was also to render assistance to countries under insurgent attack by helping them learn to undertake and counter psychological warfare.[9] (The Peace Corps was also organized at this time with a purely people-to-people role that was the obverse of the coin for security-related assistance.) It is clear from later news stories and books about American involvement in Southeast Asia that the CIA was given an operational role in supporting combat and irregular forces that went beyond its intelligence mission alone.[10]

All of these agencies and the DOD were instructed about the parts they were to play in a coordinated program, with the president telling them what they were supposed to do and how and in what areas they were to work with each other. The DOD's role was made broader than simply undertaking military operations; it was to work with other agencies at the "inter-faces" between civil and military operations—psychological opera-

tions, searching for guerrillas and protecting local populations against them and forms of small-scale and local economic assistance that could be clustered under the rubric of "military civic action."

To coordinate all these activities from the top, the president established what came to be called the Special Group (CI).[11] This included the second ranking men in the State and Defense Departments; the chairman of the Joint Chiefs of Staff; the directors of the CIA, AID, and USIA; and a representative from the office of McGeorge Bundy, the president's special assistant for National Security Affairs. The attorney general, the president's brother, was also a member of the group. He could obviously help keep the president closely informed about its activities and carry the president's direct authority and interest into the group and its deliberations.

In the spring of 1962, the Laotian government army moved against the Pathet Lao but was routed at Nam Tha by what had been said to be an inferior force that seemed to be on the verge of crossing into Thailand. The president moved American troops into Thailand near its northern border.[12] Then, in difficult and sensitive negotiations at Geneva, Switzerland, a treaty was patched together that brought Souvanna Phouma back into power in a troika of incompatibles, temporarily stabilizing a very shaky situation. There had been one obvious fact: the Laotian army, which we [the United States] had helped equip and train, had folded and could not withstand an attack. There were other signs of similar problems. Even earlier, the Draper Report had stated that our military training programs in developing nations were not achieving their objectives.[13] In the critical area of military logistics—simply maintaining equipment and keeping the armed forces supplied—our training efforts were not succeeding. These armies were therefore not very effective and, instead of operating in the field, they clustered around a few areas near the national capitals and regional centers of population and power. The Draper Report stated that their effectiveness for conventional wars left much to be desired. It was worse still for the counterguerrilla warfare, where these armies needed extreme flexibility, good intelligence, and the ability to disperse into small groups against bands of a tough and resourceful enemy who used a secret, cellular, and hierarchical organization that was very hard to penetrate. Additionally, in many countries, the armed forces served a political purpose by being the muscle that kept existing regimes in power. This, too, kept them near the population centers where they could be watched and used as political instruments.

Moreover, the developing countries faced severe economic problems, and their societies were characterized by a lack of national cohesion, as well as a political elite usually separated by wealth, education, and a degree of Westernization from the population they governed. Thus, it appeared that for most of our allies outside NATO and ANZUS the political climate and military capability were not well suited to fighting a tightly organized conspiratorial group able to play on local disaffections and to take advantage of both the political and geographical remoteness of most people from the influence of the central government. There were, during this period, about 30 internal wars or conflicts at various stages of maturity in various parts of the world.[14] Such problems were coming to the fore in Southeast Asia, in both Laos and Vietnam. They were not recognized very early, but they became less obvious and insistent with the events that ultimately led to the overthrow of Diem and the consequent series of military coups and countercoups in South Vietnam.

Drawing on some of the research results that I will discuss later, we can now appreciate some of the underlying reasons for the increasing difficulties American policy and its implementation were facing in these areas. We did not really understand the countries we had undertaken to help. We learned ultimately that the guerrillas in South Vietnam did their work in the villages, gaining the confidence of the population or using terror to separate them from the government and to wipe out the government's sparse cadre of trained administrators. But, we came to recognize early that it was a war for the sympathy and support of the people—largely a rural people—in which we were trying to play the important role of guiding and assisting the central government. We then found that we did not really understand the relationships between that government and its people, or among the various factions who might be involved, both within the government and without. Their history and their political and cultural motivations were mysterious to most Americans involved; it was not clear what divided all these groups from each other, much less how they might be drawn together. All this was proof, if any was needed, that the process of social development, revolution, and internal warfare in a country with which we had undertaken a sort of marriage contract were not very clearly understood by those who were most deeply involved in the events. The philosophical underpinnings of the political and economic theories on which we based our actions were complex and more specific to our culture than to that of the Vietnamese—or, for that matter, the culture of other countries with which we had become involved. Experts differed on the nature of the

problems, on who the key players were and what motivated them, and on what should be done or how. Few of the experts were consulted, nor was their advice often followed, as policies were made at the highest levels and orders given to carry them out. At the same time, the Vietnamese Communists and Marxist revolutionary groups in other countries had a firmly based social theory—founded on Marx, Lenin, Mao, Giap, and Che—which, whether it might be right or wrong about processes of social evolution and revolution, lent cohesiveness and the discipline of dogma. So the other side held this important advantage, while we moved pragmatically from one step to the next, without fully understanding the players on both sides or the implications of our moves, their moves, or the interactions between them.

Much of the burden fell on American advisors overseas at all levels. When all was said and done, policy was made in Washington, DC, incorporating the advice of our ambassadors and sundry other high officials. It then had to be implemented in the field. While ambassadors and theater commanders and mission chiefs—the chiefs of the Military Assistance Advisory Groups, the AID missions overseas, the U.S. Information Service and others—worked with the top levels of the local government, they were assisted by staffs which, in the military's case, could number in the hundreds and in Vietnam even before the entry of American combat troops came to be numbered in the thousands. These staffs had the "nuts and bolts" job of making the policy work by interacting with lower-level officials and officers in the foreign armed forces. While this job may have held a certain glamour, it eventually emerged as difficult and frustrating. There were a few Americans who had long local experience and were intimately acquainted with the local cultures and people, but the advisory staffs and the staffs responsible for administering American programs were composed primarily of short-timers who had had a minimum of training to prepare them for the nontechnical parts of their tasks. It was difficult for most of them to understand a people and a culture having the ingrained patterns of thousands of years of evolution, totally different from their own. In general, staff personnel did not speak the local language. The orientations of the Americans and those they came to assist toward getting a job done were poles apart.

In Indochina, for example, the South Vietnamese officer corps were likely to have been politically appointed and, thus, politically motivated. They were not aggressive and job-success-oriented, but rather survival-oriented. Many were foreign trained—French or American—and were not

culturally in rapport with their enlisted troops. Some had been fighters with or against the Viet Minh, and the instruction they received from their American advisors often was not compatible with their understanding of how to organize and fight in Vietnam. The U.S. Army Special Forces, in the job given to them, had to deal with primitive tribesmen, the Montagnard of the Vietnamese Highlands, and in the process they became involved in the long-standing animosity between the Montagnard and the Vietnamese. The latter looked on the mountain tribesmen as little better than animals, while the tribesmen viewed the lowland, agriculturally oriented Vietnamese as enemies who were trying to steal their land and stifle their freedom. In one case, when an anthropologist asked a Montagnard village chief what he would do with the weapons he had asked for—ostensibly to fight the Viet Cong—the chief answered very frankly, "If we have guns, we can drive all those Vietnamese out of here."

In other areas of the world, the United States was making policies about its relations with and giving aid to countries in transition from colonial to modern status. These were fragmented societies just coming into the twentieth century. Age-old differences and antagonisms existed among many groups. The few Americans assigned in these countries became deeply involved with the inhabitants after long periods of time and came to understand the issues, but they were not usually known to those in Washington who were making policy or to those serving a conventional "three-year tour" in the country. Politically, these countries were generally not oriented to or familiar with democratic government as we know it and could be sympathetic with as a people, and yet our aid had to be justified at least partly on the basis of building defenses for Western democracy. On an economic level, these countries were struggling, with small, capitol-oriented and well educated elites and masses of relatively primitive peasant populations, to make the transition from suppliers of raw materials to the West to states undertaking modern commercial and industrial activities.

All of this led to social stresses that must, in any case, create conflict, violence, and revolutionary forces. It was not surprising that the military often could not learn to maintain and use modern equipment and its required forms of organization, since they had no basis of technology and industry on which to build. These conditions existed everywhere in varying degrees, but they were not always obvious to high-level American policy makers or low-level advisors and trainers. Yet judgments based on far-from-adequate knowledge had to be made in an environment of communication across

cultures where even simple acts or words in the wrong pattern could destroy a man's usefulness for the remainder of his "tour," be he ambassador or simple soldier.

This, then, is how the situation had evolved as American foreign policy led to increased American involvement in world affairs. Historically, Western nations in colonial times had a lode of data deriving from and relevant for the master-slave relationship between governors and governed. Such data were often not germane, and the learning problem was much more severe in the more egalitarian relationship we had undertaken with the Vietnamese. We had insufficient knowledge to do the job as well as we wanted to and, while this may be typical of the international efforts of all nations, growing awareness led to a strong feeling at the highest levels of American government that we would have to do better. By 1964, the kind of option held by some who had key roles in participation with or observation of the American advisory effort in Vietnam, that only about 15 percent of the American advisors could establish effective relationships with their counterparts, was beginning to be heard. And questions about how this performance could be improved were being asked.

From our current view, these issues could play into argument that we should not have become involved. But, however we might view Vietnam now in rueful retrospect, the point at the time was not whether the job should be done or even whether it could be done, but rather how to do it better.

Chapter 2
Social Science To the Rescue

In its action-oriented view, the government had much reinforcement from an important part of the social science community. Among its other efforts, that community had rendered past service on difficult human problems for the DOD. Now it saw a new set of problems, and it made recommendations to undertake research to solve them, fully confident that research would provide many of the answers. Some of these social scientists had been involved in studies of international problems with government support or otherwise. But the bulk of the research that had proven so useful for the Armed Forces was performed in the areas of selection and training, human factors engineering, and performance evaluation. A summary of the history of these contributions and the evolution of what needed to be done next was prepared for the formal testimony given before the House Foreign Affairs Subcommittee under Congressman Dante Fascell[1] as a consequence of events that will soon be described. This presentation still appears to be the best concise description available; it is repeated here verbatim:

> The accomplishment of the DOD mission depends basically upon how well the people in the Armed Forces do their jobs. This implies that they must have the knowledge and skills required for the jobs and must want to perform them as well as they can. The behavioral sciences (which includes psychology, sociology, economics, anthropology, and political science) are devoted as basic disciplines to the understanding of how and why people act as they do. As applied sciences, they are directed to improving man's effectiveness and so have a direct input to the DOD mission.
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> The number and variety of jobs in the Armed Forces is almost as great as in the country at large. They range from simple, low-level tasks such as washing vehicles to those of the highest complexity such as commanding large combat organizations.
>
> The Defense Department, through recruiting and the draft, must staff these jobs with capable people at a minimum waste of time, money, and human talent. Individuals who cannot do even the simplest of the Armed Forces tasks become a financial and managerial burden. They must be identified and rejected before entering the service. Beginning with World War I, this process has been performed with ever-increasing accuracy through the use

of tests and procedures developed by mental measurement psychologists. The savings made by these are certainly in the millions of dollars per year.

Among those who were admitted to the Armed Forces there are, of course, people capable of doing a large number and variety of jobs. Some can do even the most complex and it is of primary importance to identify these and make sure that their potential is used. But practically everyone is better suited for one kind of job than another. He* is better off and the efficiency of the Department is increased if he is trained for and assigned to the kind of job he can do the best. It was a standing, only half-joke for decades that the army makes cooks out of college professors and instructors out of high school dropouts. This charge has substantially lost its basis as the result of classification test batteries now used in all of the Departments. The worth of these was demonstrated dramatically during World War II by the Air Force Air Crew Classification Battery. Studies performed at the end of the war conclusively demonstrated, that without this aid for separating men who were well-qualified for pilot training from those whose chances of ever qualifying for wings was low, the Air Force would have been required to put close to twice as many men into training in order to produce the same number of pilots obtained from the smaller but qualified group . . .

Once the individual has been properly screened and classified, he must be given the knowledge and skills necessary to do his job. It is probable that the Department of Defense is the largest training establishment in the world.

Estimates vary but no one would deny that $3 billion is a minimum figure for the annual DOD expenditure in education and training. The efficient investment of these funds is possible only if our training methods and procedures produce proficient people for the least amount of time and money. Research on how to identify the knowledge, skills, habits, and attitudes required to perform a particular job competently and to instill these quickly and permanently has paid off in many ways. Courses have been shortened or completely altered by removing unrealistic, "gingerbread" content which was not really required for job performance.

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* Use of the male pronoun throughout is an artifact of the ethos of the time, before women were admitted to the Armed Forces. Today we would say "he or she" or the euphemistic "they."

Large sums of money have been saved by substituting inexpensive training simulation for expensive and rare operational equipment. The demand for hard-to-get instructors has been kept in bounds through the use of new and more effective aids and methods. Most important is the fact that the training end product in doing the real world job has been improved.

There are outstanding examples of payoffs of research in this large field. One is the Army's modernized rifle marksmanship course, Trainfire I, which trains the soldier to shoot in combat rather than on a rifle range. Another is the Air Force work in programmed instruction which has produced time savings in the neighborhood of 33 percent in a wide variety of courses with no loss and frequently a gain in graduates' proficiency. Here again, business and industry have capitalized on this pioneering work by the Defense Department.

Understanding what a job requires and training men for it, demands knowledge of how men use their senses and their muscles to get it done. Furthermore, some jobs place very high demands on the senses and may even damage them. This has produced a requirement for increased understanding of sensory and motor processes acquired through psychophysics and psychophysiology. The recent walk in space would not have been possible had it not been for the studies of weightlessness performed by the Air Force psychologists at Wright Field and studies of balance made by the Navy psychologists at Pensacola. Navy work on night vision and dark adaptation increased our effectiveness on land and sea long before the physicist made his contribution of infrared scopes. The Army's studies of the effects of loud noise and blasts have saved the hearing of large numbers of men.

In addition, psychophysiological information coupled with job analysis and the study of how various motor skills are developed and maintained has made it possible to design jobs and, indeed, entire systems to ensure the best use of man's sensory and motor capabilities. With the increased complexity of our weapons systems, has come a recognition that men and machine must be fitted together if they are to operate effectively. The design of equipment ranging from shovels to display and control panels on our most sophisticated weaponry is now performed with the help of the research generated by human factors engineering psychologists in all of the military departments. Indeed, without this kind of know-how, some of our present systems could not possibly be manned.

An example of the kinds of savings resulting from this type of work is the $3 million saved through early design changes in the Pershing missile system and a 50-percent reduction in the number of operators required. Another is the reduction of 25 percent in the loading time of the Sergeant missile while reducing the required number of men from 11 to 6.

Having the right jobs and equipment correctly designed and the right men properly trained is not enough to guarantee efficient manpower management. It is also necessary to assign the properly classified and trained men to the jobs they were trained for. The importance of this step was highlighted by the recent study of the draft that showed even slight decreases in assignment "slippage" could result in substantial improvements in the overall effectiveness of our forces. With the availability of computers and the development of highly sophisticated statistical theory about ways of achieving maximum use of personnel, the Departments are now in a position to insure against malassignment or costly and harmful pooling of idle men. An example of the kind of breakthrough that can result from this type of research is the Pensacola Personnel Appraisal System in which, at any desired time during a pilot's training, his progress and achievements can be reviewed and his chances of successfully completing the course can be determined. The resulting elimination of further investment in "bad bets" has already resulted in savings in the millions of dollars.

Men forget as well as learn. The Department must, therefore, concern itself with the maintenance of proficiency after formal training is completed. It is also vital to have a continuing and accurate appraisal of the competence level of our forces. For both these purposes, job proficiency measures are necessary. Such measures have become more numerous and more realistic because of the years of research on how performance can be most accurately and reliably assessed.

We are basically interested in performance when the chips are down. Frequently this means performance under the kinds of extreme stress generated in combat. We know that effectiveness is greatly reduced by stress. What we must know more about is how to design jobs and equipment and train men to be more resistant to stress effects. Our success in this basic research effort may be crucial for our national strength when it counts.

A large proportion of the jobs in the Armed Forces involve teamwork. Furthermore, some of the newer and most important tasks must be performed by groups of men who are isolated under difficult conditions for considerable periods of time. The astronauts in a manned-orbiting laboratory or men in an armored personnel carrier during nuclear warfare are cases in point. These conditions and, indeed, any conditions that demand cooperation introduce a number of problems.

Research is, therefore, being directed to improving teamwork, lessening frictions, and increasing the compatibility of men assigned to a group. Some of the earliest work of this kind was done in composing SAC air crews and resulted in clear gains in their efficiency. Subsequently, social psychologists and sociologists have been working on similar problems for submarine crews, infantry squads, missile teams, etc.

Our personnel must not only work together but, under that present concept of worldwide Defense Department activities, they must also work with both military and civilian foreign nationals . . .

Since World War II, the foreign relations of the United States have increasingly involved the developing nations of Asia, Africa, and Latin America. In all these areas countries have been struggling against great odds to establish stable governments, to maintain their independence, and to improve their standards of living—efforts the United States has supported. The major Communist powers have, on the other hand, sought to exploit the instability and economic problems in these nations to expand their control over large parts of the world.

Khrushchev's January 6, 1961, statement that the Soviet Union will support so-called "national liberation wars" has been often reaffirmed by the Soviet Union, even as recently as June 18, 1965, in Pravda. On December 31, 1962, the *Peking People's Daily* stated that: "The Communists of all countries . . . must . . . resolutely support wars of national liberation . . ." Evidence of this "support"—which includes instigation—has been obvious in Laos, Vietnam, and recently in Thailand. Communist machinations, directly or through proxy countries such as Cuba, have also threatened many countries in Africa and Latin America, and have disrupted their internal affairs.

The State Department, the Defense Department, and key agencies such as the CIA, AID, and USIA have increasingly had to

> turn their attention to meeting this threat. Because of its involvement in military assistance activities in these nations, and because of the all-encompassing nature of the threat—in the political, economic, social as well as military spheres—the Defense Department's mission in this area have been viewed as broader than the traditional mission of providing the U.S. Armed Forces for the national defense . . .
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> Events in Vietnam and elsewhere have made it clear, however, that while improved military hardware can make a very important contribution to the defense against Communist subversive warfare, this by itself is not enough. In fact, proper use of "nonmaterial" tools represented by sound knowledge and actions in the nonmilitary sphere can obviate the need to involve large military forces.
>
> Moreover, whether the military is involved in direct conflict or pre-insurgency military assistance, U.S. military people all over the world must work with and implement the counterinsurgency programs. The war itself revolves around the allegiance and support of the local population. The Defense Department has therefore recognized that part of its research and development efforts to support counterinsurgency operations must be oriented toward the people, United States and foreign, involved in this type of war; and the DOD has called on the types of scientists—anthropologists, psychologists, sociologists, political scientists, economists—whose professional orientation to human behavior would enable them to make useful contributions in this area . . .

There had been gradual buildup to this latter view, with accompanying evolution of the details. In the few years prior to 1960, a research group examining the problems of social science applied to Defense problems, under the auspices of the Smithsonian Institution, had made recommendations for work in an area that they called "persuasion and motivation." In a later publication, the leader of this study explained it as follows:[2]

> The topics of persuasion and motivation refer to the methods of influencing people by means that are short of force, or authoritative command, or other direct incentives, on the one hand and short of formal education or training, on the other hand.
>
> Persuasion is exercised for the purpose of motivation; it shapes expectations, molds opinions and attitudes. . . . Persuasion characterizes the normal, everyday means of social intercourse, engaged in by everyone. The military services constantly use

> persuasion to motivate and influence their own people and those people outside the services with whom they interact. The military services are regularly used in indirect ways by the government to influence foreign peoples and foreign states . . . In any future war of significant length, there will be "special warfare," guerrilla operations, and infiltration. Subversion of our troops and populations will be attempted and prisoners of war will be subjected to "brainwashing." The military establishment must be prepared to assist promoting recuperation and cohesiveness within possibly disorganized civilian populations, while attempting to shift loyalties within enemy populations. . . . The military establishment needs to know all that can be known about persuasion (including) the processes of persuasion; group relations and persuasion; the relation of cultural differences to persuasion; persuasion and social change . . .
>
> Another way to approach the interaction of values and persuasion is to study in greater detail the methods of persuasion and indoctrination used by the Chinese and other Asian people in the comparison with our own attempts to build favorable attitudes in these countries. Still another is to study the effects of our own military postures in different countries. From such testing of theories of persuasive processes can emerge a systematic classification of the value systems, stereotypes, and national images of the chief cultures with which military establishments are likely to have extensive contact . . .
>
> On several occasions, our representatives have seemed to be caught unaware by revolutions, when, with no warning, rioting mobs have boiled up and around our bases reflecting a state of public opinion and anger which previously seemed not to exist . . .
>
> The primary need is to create a small number of stable, permanent centers of research on persuasion as related to politico-military needs. . . . Military support should seek to integrate basic and applied research in the pursuit of a technology of persuasion . . .
>
> In the present structure of research support, and because of its deep technological needs, the Department of Defense is the logical source of this new type of support for the systematic, long-range study of human behavior.

Shortly after the issuance of this report, Dr. Harold Brown became the Director of Defense Research and Engineering (DDR&E). At President Kennedy's request in 1961, Congress had appropriated approximately $120

million to expand research and development programs having to do with limited war. To obtain some better perspective than then existed about how the money could usefully be spent, Dr. Brown established a committee of eminent scientists and high-level DOD civilians and military people, including those on his own staff chiefly responsible for this area of work, to explore the nature of the limited-war problem and recommended how these subsequent funds should be spent. In addition to hearing briefings in Washington from both the Defense and the State Departments about strategy, tactics, the military forces, and the relevant research and development programs, the group traveled extensively to developing countries—where the United States was involved in military assistance programs, and where we were attempting to help the local governments prepare to face the approaching conflicts.

This study group, which included physicists, engineers, generals, and admirals, was largely concerned with problems of military hardware, weapons systems, and military tactics and strategy. But it recognized that there were broader human problems involved, and a subcommittee, including some of the members of the Smithsonian group who had worked on the report quoted previously, was convened to look at these questions. The subcommittee's recommendations reinforced observations made by the main group in its travels and briefings overseas. A substantial portion of the final study report was devoted to issues related to what came to be called "non-material research." It was recognized that, although weapons systems could and needed to be created to help the United States match its own military forces and its military assistance to the kinds of problems then emerging in the world, these would not provide all of the answers. How governments could organize to combat guerilla warfare and insurgency, how to meet the insurgents' psychological offensives, how the "static" armies of the developing countries could be motivated to fight, how these governments could better relate to their people were pointed out as the most important questions facing the national military assistance programs. A number of specific recommendations based on the contribution of the social science subcommittee were made for expanding research in this area. When briefing the results of the main study group's work to Secretary McNamara, the chairman of the limited-war study group and a world renowned physicist commented that, while World War I might have been considered the chemist's war and World War II was considered the physicists' war, World War III, which we might already be in, might well have to be considered the social scientists' war.

After its first report, the Smithsonian group reconstituted a group of politically oriented social scientists to explore in greater detail national defense problems in terms of America's issues overseas. A study under Ithiel de Sola Pool of MIT led to another report in 1963, which became much more specific about revolutionary warfare, insurgency, and counterinsurgency, and the research was needed to help the United States cope with such problems in its overseas relations. This later report[3] set the tone for much of the effort that followed.

It is worth quoting extensively from it, because it illustrates many important aspects of the advice the Defense Department was getting. It is, first and foremost, quite lengthy; ideas are explored in depth and at leisure in a sometimes abstruse language that, later, would make it difficult for the action-oriented parts of the Defense bureaucracy to understand, accept, and implement the results of studies by social scientists. Second, the report illustrates the kind of thinking about the problems that, if it may have been subject to disagreement on the part of some social scientists, was at least widely enough accepted to evoke no great hostility. And third, it demonstrates plainly one of the lines of thinking that died only over a long period of time: that the Defense Department *should* study problems of social change very broadly and that this was a perfectly legitimate activity. The following paragraphs may seem closely connected, but they are drawn widely, although in sequence, from the 250-odd pages of the report.

> The objective of this book is to consider what social science can contribute to more effectively conduct research of the free world's defense effort. It does not aim to be exhaustive but rather to spot significant topics on which social science has heretofore been too little used.
>
> Our chapters skip over those fields in which the military establishment has already made extensive use of the new technology of human behavior . . .
>
> As the Advisory Panel reviewed the outcome of its efforts, it came increasingly to realize that it had inadvertently focused on one aspect of the Defense Department's problems, namely the management of its own establishment.
>
> With the partial exception of the report on persuasion and motivation, which concerned itself extensively with psychological warfare, all of the reports dealt with the expansion of social science knowledge of a kind that could be used to make the manpower of the military establishment more effective . . .

> But there is also an entirely different domain of Defense Department problems which, the research group recognized, had not yet been considered. *To it, too, the social sciences might make a contribution*. [emphasis added]
>
> This other domain of problems may be roughly characterized as the operations of the Defense Department in relation to the eternal world . . .
>
> It is thus not in criticism but simply in candid observation that one must recognize that the defense establishment was eager to accept the aid of social science on its vast management problems but less willing to concede that social scientists might have something to contribute to those decisions, which constitute the crux of military planning and operations . . .
>
> In many places, the military job can only be accomplished by a process of nation building. . . . Success in counterinsurgency outflanks a stalemate in the field by concentrating on actions which will in two to five years' time establish stable communities in a progressing nation, with the surviving guerrillas quietly returned from their hide-outs . . .
>
> Until 1961, when this study was written, the United States has communicated through its postures and strategies a resolve to deter and contain—to deter the Soviet Union from a nuclear attack and to contain the Communist forces within their borders. At the same time, it communicates an intention to help the new and poor countries make a free choice politically and economically . . .
>
> These questions also arise with respect to the policy of commitment. Where are we prepared to draw the line? Where do we use our troops? Where, if at all, and how do we defend against the *internal* subversion and revolution which is the Communist pattern? . . . It is entirely possible that the Soviet blueprint calls for no *invasion* at all by communist troops of one of these countries . . . if we defend only against invasion, we may never have a chance to strike with *either* the left or the right hand.
>
> This is one implication of our military posture. . . . Another is the fact that our opponents seem so free to exploit all the revolutionary movements and expressions of mass discontent in the developing countries and the colonial states. We, on the other and, have been cast in the role of defending the status quo . . .

There is another reason for making a special plea on behalf of the study of internal wars at this time . . .

Much is in doubt about the causes of internal war, as we shall see, but of one proposition we may be certain: internal war is closely connected with social change . . . from this basic assumption it follows that one should expect internal political violence to persist, perhaps to increase. . . . The tremendous number of internal wars in the period 1946–1959 . . . —and in other "transitional" periods, such as late antiquity, the Renaissance and Reformation, and the early nineteenth century in Europe—is evidence of this . . .

It is particularly likely today that the Communist states will use internal wars as tools of international politics. They have already done so frequently in the postwar period and are likely to do so frequently in the future. Communism—especially the present-day Chinese version—is a militant messianic creed, and such creeds always tend to produce expansionist zealots. If the use of conventional warfare is closed to the Communists as a means of exporting their utopia, they will try other means to the same end, and the means most readily available is the instigation and clandestine support of internal violence. Furthermore, Communism places no immediate normative prohibitions on political violence; on the contrary, Communism justifies and glorifies violence. In communist doctrines of course, violence is purely instrumental to other ends . . . Communists have available in other countries much material for fashioning internal wars: much desperate discontent, many sublime and frustrated hopes, and much anachronistic hatred of non-Communist Western systems. Most important of all, the Communists have an enormous lead in the experience of and reflection upon internal war. If we are far ahead of them in deterrence theory, they are immeasurably farther ahead of us in revolutionary theory . . .

At present, the most probable kinds of internal war, once started, are difficult, if not impossible, to win by those on the defensive. Above all, this is likely to be true of guerrillas fighting in favorable terrain . . . if the noncombatant population is well-disposed toward the guerrillas and the incumbents have any sort of scruples . . .

All this gives added point to the frequently repeated statement that internal war adds a new dimension, a political dimension, to the problems of warfare. Internal war is a struggle for political loyalties no less than military victories, a struggle requiring in-

> tense political consciousness on both sides. Indeed, *the political art of detecting internal war potential must have priority over the military art of fighting it. This applies to military policy makers no less than civilian ones*.* In a world of alliances, foreign bases, and far-flung power blocs, detecting in advance the instability of regimes and knowing how to shore them up with fair chances of success are among the most urgent imperatives of the military as well as the political arts . . .

Under the discussion of internal wars, a number of topics for research were presented and elaborated:

> . . . **Analysis of the uses of internal war situations.** Since internal war situations are often largely inchoate in their initial stages; since they are very difficult to anticipate correctly under the best of circumstances, owing to the role of ephemeral precipitants in bringing them about and the complexity of their preconditions; and since they are particularly difficult to anticipate correctly with the knowledge presently available; nothing would seem to be more urgently necessary than knowing how to use them for one's own policy purposes once they have occurred. We live in a revolutionary world in which internal war is a basic fact of life. In such a world, even conservative powers need conspiratorial theories; they can hardly hope to contain the tide of revolutions everywhere, especially while being in the dark about the forces causing it. Studies of techniques by which internal wars can be molded and channeled are therefore of the utmost importance. The Machiavellian overtones, the apparent cynicism, may make such studies repellent but that cannot be helped.
>
> . . . **Studies of Communist theories of internal war.** No doubt we can learn a good deal about using internal wars from the Communists, who are masters of that unattractive art. But this is only one of the many reasons for studying Communist ideas about internal war. Without subscribing to the theory that Communists can make internal wars under any and all conditions, or the view that modern internal wars are all Communist-inspired, one can nevertheless argue that knowledge of Communist ideas about the preconditions of internal war is indispensable at present for anticipating particularly crucial internal wars—those in which Communists are in fact involved . . .

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* Emphasis added. From this statement, as will be seen, the genesis of Project Camelot can be traced.

. . . **Studies of the efficiency of certain policy responses to internal war potential.** Here the most important subjects are to determine the optimum uses of repression, diversion, and concession, or combinations of the three. Each of these responses to internal war potential is potentially useful and potentially dangerous. Each has worked and failed in certain situations . . . Under what conditions, structural and behavioral, are they likely either to work or to fail? How far should they be carried? What kinds of repression, diversion, or concession work best in what sort of situations? How can one policy response best be combined with another—repression, for example, with concessions or diversions?

. . . **Studies of the internal war potential of critically sensitive areas.** In general, the systematic study of internal war potential must be a long drawn-out process; there are no shortcuts to knowledge as inherently complicated and difficult to acquire as knowledge of this subject. In certain countries, however, we can hardly await fully adequate theoretical knowledge to carry out concrete appraisals. Such countries are those in which the security of important military bases might be imperiled by internal war or countries in which internal war might imperil our fundamental international designs . . .

As for other projects relating to the problem of anticipating internal war these can easily be derived from the text of the study . . .

. . . [there is] one other topic for research, to illustrate the kind of general-purpose inquiry requiring large resources which might be particularly useful at this stage of inquiry. This topic involves inquiring very broadly into what might be called "symptoms" (or "indicators") of internal war potential . . .

In the next few years, it can be expected that there will be a growing interest among social scientists in the problems of guerrilla warfare and counter-subversion. It is, indeed, not impossible that this area may prove to be a more fruitful one for social scientists than many other aspects of military strategy. This is because the problems posed by such forms of warfare and violence are intimately related to questions about the social structure, culture, and behavior patterns of the populations involved in such conflicts. *Without question, social science research is in a strong position to contribute useful knowledge in designing and developing internal security forces.* [emphasis added] Indeed most of our understanding about communist strategy and tactics in guerrilla war-

> fare and subversion and of their basic appeal in underdeveloped countries has come out of the works of social scientists . . .
>
> In the last few years, social scientists have becoming increasingly interested in the political administrative roles which armies can play in the nation-building process. . . . There is, in addition, a long tradition of historical analyses of the domestic political role of military leaders, but unfortunately most of the work in the field of civil-military relations in the West has only marginal application for understanding current problems in the newly emerging nations . . .

Clearly, the research administrator who wanted to base a specific program on these recommendations had some work to do. Later, recommendations by social scientists as to the research that was needed would become more precise. But it is obvious that the government and important members of the social science community were converging on both the definition of an important problem for research and on acceptance of the premise that it was "researchable."

Chapter 3
A System of Ethics and Values

In both the government's and the scientists' views, there were some implicit assumptions about the legitimacy of the activities in question.

First, there was the question of counterinsurgency (or "internal war") as a matter of interest for scientific investigation. In the current view of much of the intellectual community, as a result of our problems in Vietnam and the opposition of that community to the war, the term "counterinsurgency" has been taken on the connotation of suppressing "legitimate" revolutionary social developments in the third world (or even at home). This interpretation of the term was stressed by Senator Fulbright, as well as by many of those in our universities who increasingly voiced their opposition to American overseas involvements in such conflicts. But in 1960–64, "insurgency" was viewed in relationship to the Soviet and Communist China's approaches to protracted warfare, for influencing countries and changing their governments to be more sympathetic with the "socialist" countries and "progressive forces all over the world." We in the United States viewed "wars of national liberation" as a polite term for "Communist takeover," using terror, guerrilla warfare, coercion, and suppression of freedom. We were still sensitive about the problem of "brainwashing" with Communist techniques generally—for example, Viet Cong methods of indoctrinating their adherents, thereby making them dangerous and effective insurgents. Such terms as "unconventional warfare" and "special warfare" were used to describe both what the revolutionary forces and the defenders against them were doing, but these terms either had specialized military definitions or did not seem to cover the complete gamut of activities—military, civil, political, and economic—that had to be undertaken to defeat "wars of national liberation."

Thus, while those who were defining what research needed to be done in this area were not unaware of the other possible connotations of the term, at this time, the term "counterinsurgency" came to serve as a convenient shorthand for American resistance to Communist takeovers of weak countries through "wars of national liberation." The term did not change its complexion until our efforts in Vietnam faltered and turned the American intellectual community sour on the idea of America being "the world's policeman."*

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* But some things never die, they just get redefined. Today (2012), the "counterinsurgency" term is applied with public support to such things as countering the Taliban's attempt to recapture government functions in Afghanistan. Note, however, that it still applies to what amounts to irregular warfare.

A related semantic-cum-philosophical problem, which was to come on stronger later, was whether research on another country's problems constituted undue political interference in that country's internal affairs. Even in a time of growing Defense Department interest in such research, there was sensitivity on this point. It is well known how the arguments about Vietnam began around the issue of whether this was, in truth, a civil war or an attack from the outside, and the United States government took great pains to prove the latter view through State Department White Papers[1] and even remarks by the president at his press conferences. A later article in *Foreign Affairs* by George Carver[2] traced North Vietnamese involvement in the south from the beginning. But it seems, in retrospect, that none of this convinced anybody, and the opposing sides held to their views. Of course, well before this issue became important and gained added poignancy from direct American participation in the war, military and economic assistance programs had acted as instruments of foreign policy through which we hoped to influence the path of economic and political development of many nations of the world. Even then, we were confusedly aware of the problems of reconciling the many uses of such assistance with our public policy.[3] In addition, as illustrated by the many arguments and discussions in Congress and the general sensitivity, at home and abroad, on the same issue of aid "with strings attached," we were torn between the motives of altruism and extraction of maximum value from the expenditure of American dollars.

As far as the Defense Department was concerned in the early days of the Vietnam War (and since, for that matter), not much further rationale was needed than that the United States had been asked for help by a beleaguered "allied" government, and the whole official American policy from the president on down was oriented toward providing it. So this was not viewed as interference so much as assistance and, from the research point of view, there was even less concern because the research supported the quest for knowledge, understanding, and greater effectiveness in the overseas activities of the United States government. The main question that was asked by the researchers was whether these could be provided. The Harvard research groups studying the USSR and the Peoples' Republic of China in the early fifties, as well as others, had shown ways of doing research about countries on the opposite side without access to much more than was published by those countries or could be gleaned by visiting scholars. As for research on the problems of friendly countries, the question "is it polite to study friends?" was raised in the second Smithsonian report (see, no. 3, ch. 2), and was answered as follows:

> Research on political matters is a neighbor of intelligence. Perhaps this is the reason why a reluctance to study political conditions in friendly nations, or to be studied by friendly nations can be detected. . . . Another reason for reluctance to conduct certain types of research in friendly nations may be that this kind of activity can be taken as indicating distrust of the government of that nation. . . . There is often something faintly ridiculous about the inhibitions applied to research among allies. Furthermore, the United States in particular, and to an only slightly lesser degree, Great Britain, France, and West Germany publish about themselves large amounts of information in categories that are useful in connection with the functioning of alliances. It is not, therefore, as though any of these nations were seeking information about others that they are not willing to disclose about themselves. Nevertheless, the existence of suspicion about some kinds of research, especially among emerging nations without fully democratic governments, should be recognized and faced as one of the problems besetting researchers in this field . . .
>
> Two measures for dealing with this suspicion are suggested for further exploration. One would involve having alliances themselves sponsor research. . . . It is probable that alliance-sponsored research would not only overcome a large part of the resistance to studies of friendly countries but would also make it possible to benefit from better cooperation on the part of significant groups in the country being studied.
>
> A related suggestion is that greater use be made of cooperation between private American social science research organizations and research groups in friendly countries. . . . Many of them, both within and without the universities, have traditions of cooperating with American research institutions and have been willing to cooperate in sponsoring and executing studies of mutual interest.

That is, the general idea was put forward that, by enlisting the support and collaboration of researchers and government in the country under study, the work could be undertaken and would, in fact, be welcome. We were to learn over the years that the problem was not so simple for many reasons, but that, too, lay in the future.

It is clear, however, that as the Defense Department, in its efforts to learn more about its military problems in the developing countries, followed the strong and consistent advice given by the scientific community.

The members of that community who were involved and the government, in general, held a group of premises and a set of values in common, without which the research efforts could never have been initiated. In 1963 and early 1964, they were, in summary, as follows:

- The problems of the United States' relations with developing countries were important and contained many unknowns of a cultural, philosophical, strategic, and operational character.

There was deep concern about Communist expansion via Communist groups within developing countries that were supported by China or the USSR and were exploiting the difficulties faced by these countries. If this expansion were successful, it would be detrimental to American interest everywhere.

- The Communist instrument of expansion was the "war of national liberation"—internal war or insurgency.
- Assisting developing nations with counterinsurgency was the American national strategy for preventing this expansion, and was a legitimate form of expression and implementation of American foreign policy.
- The U.S. Armed Forces played a major role in counterinsurgency, not only in the military sphere but in such areas as local economic development, psychological warfare, and internal security.
- Research was needed to help solve the many problems associated with providing counterinsurgency assistance to foreign countries. The unknowns that needed to be researched included the processes of social change, revolutionary organization, guerrilla and counter guerrilla warfare, the role of the military in the development of new nations, the uses of "persuasion and motivation" in the advisory processes, and cultural factors that would increase the effectiveness of advisors.

Research in the social sciences had helped solve "internal" problems for the Defense Department, and it could help solve these "external" problems as well.

- It was acceptable and desirable for the Defense Department to provide the funds for this research because it had the resources, the mission, and the necessary extensive contacts within the countries concerned.

- It was acceptable to perform research in allied foreign countries about their problems, and the sensitivities involved in such research could be overcome by enlisting the support and participation of local foreign governments and scholars in the research.

It would be an exaggeration to say that these premises and value orientations were universally held. To the extent that the key figures in the Defense Department thought about social science research in this context, there was a good mixture of suspicion, or skepticism, about its value. The social scientists involved would have attributed this to a lack of understanding rather than pointed hostility. These social scientists did not feel especially inhibited by ethical considerations or the possibility of sanctions by their peers for their involvement. The professional organizations were permissive if not apathetic; only the American Psychological Association had a code of professional ethics, and this dealt with personal behavior on the part of psychologists rather than with the kind of work they might undertake or their relationships with or choice of clients.[4] The whole was pervaded by an atmosphere of intellectual excitement and inquiry stimulated by the Kennedy presidency, an atmosphere whose momentum was to continue for another year, still. It would not have been difficult to obtain a majority opinion within the government community and the community of scholars most concerned with the pertinent research in the social sciences that this was a valid set of premises on which to build government research and actions programs. We shall examine, later, the transformation that was to overtake these premises. The reader who is skeptical of them now might note how difficult it is to project backward from the mid-1970s through the bitterness of the late sixties to that earlier, more innocent, time.*

As could be expected (see chapter 15), there were objections from the anthropological community about the performance of such work by the DOD, but these did not flare up into a major "Camelot-like" imbroglio. In retrospect, I concluded that the difference between then and now was, first, that the effort initiated in 2008 was proposed and "owned" by Secretary of

* It is of interest to note that the idea of performing research by social scientists to understand our enemies better was revived in the context of the wars in Iraq and later in Afghanistan. I took the trouble, in 2008, of writing to the person named in an article in *Science* as the one in OSD responsible for getting this effort, termed the Minerva Initiative, off the ground (For a brief description of the initiative, see http://en.wikipedia.org/wiki/Minerva_Initiative). This led me into several meetings with people involved in management of the effort and with the staff chief of Representative Ike Skelton, a key member of the House Armed Services Committee. And, all this activity led me to give much further thought to understanding the differences between "then (1964–65)" and "now (2008)."

Defense Robert Gates rather than by an official in the bowels of the organization who might then be viewed as having made trouble for the secretary of defense that the latter could have done without. Second, the 2008 presidential election cycle was heating up amongst much antagonistic rhetoric within the primary campaigns and then during the party conventions and the candidates' election campaigns. This political period thus provided much more grist for the media mill than the secretary of defense's efforts, which the media essentially ignored, to enlist social scientists' help in understanding our opponents in the ongoing wars and related matters.

What's the same, I think, is the sensitivity of the subject. As soon as a social scientist breaks out and does or says something that the media sees as newsworthy and would make a news article of interest or amusement to the general public. The media will jump on it, mainly as something to make fun of or to be snide or accusatory about, either vis-à-vis the government because "why didn't you understand what this guy is saying" or vis-à-vis the social scientist because "why did you probe into that sensitive area?" The sensitivity of what social scientists might find and how that would strike the media, and how that might backfire on policy, I think, is much the same now as it was then. An alert reader can detect the signs of such sensitivity in much of the discussion about the Taliban in Afghanistan and its role in disturbances in the tribal areas of Pakistan—for example, the floating of the story that a polio vaccination campaign was purportedly used to gain entrée to the compound where Osama Bin Laden was hiding, and the sequelae in punishing the doctor who helped in that campaign. Similar sensitivities can be identified, with some thought, to the Israeli/Arab conflict or to the apparent Iranian attempts to move toward acquiring nuclear weapons capability. The recent media-congressional circus that led UN Ambassador Susan Rice to withdraw from possible nomination as Secretary of State by President Obama offers a similar, related example of the kinds of media-congressional interactions that characterized the Camelot affair, as will be seen.

PART II
THE BUREAUCRACY

Prefatory Note: A Personal Prologue

How did an engineer become deeply involved with the social sciences? As these things usually happen, almost imperceptibly. Much of my career had been spent on defense problems, and I had developed a strong personal and professional interest in military affairs, strategy, and the problems of the operation of American military forces on the world scene. I joined the Institute for Defense Analyses in 1960, had become involved in studies of tactical weapons systems and limited war, and participated as executive secretary of the group performing the limited-war study for Harold Brown, mentioned in chapter 2. These studies all showed the difficulty and complexity of the military problems inherent in our foreign policy, and led to the conviction that most of the contemporary theories of military strategy developed by the early 1960s did not lend sufficient structure and clarity to the difficult tasks of creating military forces able to meet the realities of the world political scene outside the nuclear standoff. This appeared especially important at the time, since President Kennedy and his administration seemed, finally, to have discarded the unworkable doctrine of massive retaliation and were changing the form, functions, doctrines, and missions of the Armed Forces. I expressed my views in a book, Limited War and American Defense Policy, *written during 1962-63. Through the work on this book and my work at IDA, I became acutely aware of the growing problems the United States faced in Vietnam, and of the difficulties of building the bridges between American and Vietnamese cultures and politics that would be essential if those problems were to be solved.*

The problems of military equipment, tactics, and strategy in the particular kind of warfare fought by the Viet Minh seemed important (and they still do), but I felt that, as I had observed in my book, "the application of hardware and strictly military techniques . . . can obviously be of great assistance, but is equally obvious that (they) cannot offer the entire solution. The technical parts of the problem emerge as ancillary elements of the entire process of societal revolution that reflects itself in the outbreak of subversion and guerrilla war." When I was asked in October 1963 to join the Office of the Director of Defense Research and Engineering as his special assistant for counterinsurgency programs, I told Harold Brown and his deputy for tactical warfare programs, Dr. John McLucas (to whom I would report), that I believed many of the important solutions to such problems as we were facing in Vietnam would have to be sought through research in the social sciences. Dr. Brown agreed and also indicated his desire that I draw together and create an orderly research and development program out of many scattered and, in some cases, duplicative and conflicting efforts having to do with counterinsurgency in general and Vietnam in particular that were being undertaken by the military services and the Advanced Research Projects Agency. I therefore had a charter to try to start an effort that appeared important and interesting, and I accepted the position offered in the expectation that I would be able to help the Defense Department in ways that I thought would make the help count.

Chapter 4
The Scene

The position of the director of Defense Research and Engineering (DDR&E) was established by the Department of Defense Reorganization Act of 1958. Having a rank just below that of the service secretaries,* the DDR&E was charged to "supervise all research and engineering activities in the Department of Defense; and . . . direct and control (including their assignment and reassignment) research and engineering activities that the secretary of defense deems to require centralized management." Thus, DDR&E was responsible for initiating, reviewing, passing judgment on, and assuring the budget for any research within the DOD's mission that appeared essential to national interest. There was, of course, a staff organization reporting to the DDR&E (known as his "Office," or ODDR&E). While the precise form of this organization might vary over time, it generally included a principal deputy (who in 1964 and 1965 had the rank of assistant secretary of Defense) and a number of subordinate deputies for areas, such as strategic weapons, "tactical warfare programs," (i.e., attack aircraft, artillery, tanks, aircraft carriers—virtually all the major weapons systems in the Armed Forces except such nuclear strategic offensive and defensive systems as Polaris, Minuteman, or the Anti-Ballistic Missile System), communications and electronics, and "science and technology" or basic and applied research programs. The deputy for science and technology had, reporting to him, a director of behavioral science research programs. The director of the Advance Research Projects Agency (ARPA)** also reported to the DDR&E; but the latter had no cognizance over the assistant secretary of defense, International Security Affairs (ISA), who reported directly to the secretary of defense. ISA was responsible for reviewing the international implications of military affairs and for coordinating Defense with State Department activities; such coordination on State's part was effected through the director of the Bureau of Political-Military Affairs.

The work of ODDR&E centered around the annual budget cycle. Starting in the summertime, the defense budget was prepared for review by the

* Strictly speaking, the Army, Navy, and Air Force are "military departments," while their operating arms (the troops in the field) are "military services." As a matter of convenience, the term "Services" will be used to refer to either.

** Established by Congress after the first Sputnik to accelerate the research and development necessary to improve America's position in what became the "space race." ARPA later came to perform many other tasks assigned by the secretary of defense or the DDR&E.

secretary of defense in mid-fall. This preparation included all necessary coordination with the military services, whose inputs were requested, discussed, argued, and sent to the secretary of defense (Robert S. McNamara, at that time) and issues to be resolved, if necessary. The secretary decided what major items or groups of items would be included in the budget and the level of funds that should be assigned to them. The "presidential budget" would then be sent to the White House at the end of December for incorporation into the federal budget and presentation to Congress the following spring.

Another part of the budget cycle occurred in June in connection with congressional appropriation for the Defense Department's operations based on previous budget requests. The DOD then went through an "apportionment review" in which further decisions were made about fund allocation, taking into account the actual monies appropriated rather than those requested and incorporating program changes that might have been found desirable in the meantime or been instructed by Congress during its deliberations. Between these two periods (the fall budget preparation and the spring apportionment review), there were, of course, the congressional budget hearings.

For each part of the budget cycle, the deputies in ODDR&E reviewed all of the service programs, decided whether to recommend approval of the service budget proposals or to propose alternative allocations, and negotiated with the services to resolve the large number of issues that would inevitably arise. Under the law, the DDR&E had the responsibility for ultimate approval of service research and development (R&D) expenditures. This meant that final approvals of projects or programs and instructions to the services were signed by the DDR&E, although the necessary documents might be prepared and recommendations made by the deputies and their immediate staffs. Generally, each deputy would be responsible for a large block of the entire $6–8 billion work of Defense R&D, with each block adding up to hundreds of millions, or billions, of dollars worth of effort. Each subordinate staff member would be responsible for a group of programs decided partly on the basis of how large a program an individual could effectively oversee, and partly on the basis of coherence of projects within a functional area. Only rarely were programs arranged such that one program, or one group of projects in a particular subject area, was handled by an individual solely because of its importance if the associated funding were very small. The size of the organization simply did not permit this as a standard procedure, however desirable it might be. But the position of

"special assistant" offered this kind of flexibility, and such a position could be created when, as in this case, the DDR&E deemed it necessary.

DDR&E staff (including the deputies) had both no real power over the services and yet a great deal of power at the same time. While only DDR&E could legally give official instructions to the services, the services knew very well that if a member of the DDR&E staff were negotiating for a certain budget level on a project, or to have a certain project established, this could become an "instruction" if DDR&E wished it to become so. Much of the staff work revolved around the preparation of such instruction documents with attendant discussion, negotiations, and coordination almost *ad infinitum*.

Lyons,[1] in an important book on government support of the social sciences, has noted that the Defense Department at the DDR&E level had only part-time help to pay attention to the social sciences. This is true, but it tells only part of the story. My own work with the social sciences required only a fraction of my time. Most of my time was spent on problems of military technology, systems research and development, analysis of operations in a developing war, and organizing and coordinating a fast-growing Defense Department program that included all manner of things, ranging from studies of Viet Cong motivation to the M-16 rifle, new helicopters, aerial ordinance, or night-viewing equipment for infantry. As will be seen, the nature of getting any one task done at that level of bureaucracy means to initiate an action, talk with many people, hold meetings, write papers, and then wait; much time is spent waiting for other people to do or decide something in any one program area. Therefore, I found that I had all the time necessary for the social science research efforts I wanted to initiate when the time was needed.

The part-time nature of DDR&E's attention to social science as *science* lay in the intermittent occupation of the position of responsibility for the social science programs under the deputy for science and technology, rather than in the intentional assignment of the responsibility to someone who held another job simultaneously. This was due to the reluctance of social scientists to give up their academic positions and freedom to take on this important position, rather than to the Defense Department's intent to give the area short shrift, although the conditions under which the job would have to be undertaken—reporting to hardware-oriented engineers and physical scientists—made for an interesting social dynamic not calculated to encourage social scientists' interest. But during the critical first two years of my involvement, a full-time social scientist worked in the Office of the

DDR&E,* responsible for overseeing all of the Defense Department's work in the social sciences, and I worked with him very closely. In addition, the military departments and ARPA had full-time social scientists responsible for their social and behavioral research programs.**

As special assistant for counterinsurgency, I did not interact with the social scientists in the DOD totally as a novice. In the course of my work at IDA, I had come to know some of the social scientists in the outside community who were advising the Defense Department on how social research could be helpful relative to the problem at hand. I had come to know the service and ARPA social science program directors, as well. Therefore, in addition to a personal, avocational interest of long standing in social research, I had had rather extensive exposure to the pertinent social research problems and to the people involved in them. I had had many discussions with these people, and my close friend, Dr. Jesse Orlanskly,*** at IDA about what social research might be expected to accomplish in this area. When I entered the Pentagon, Orlanskly pointedly reminded the social science program directors in the services and ARPA that, despite the fact that my background and responsibilities dealt primarily with hardware, someone in ODDR&E with my orientation to and hope for the social sciences presented a rare opportunity for them to implement some of the things "the community" had been urging on the Defense Department. I did not feel that my own membership in a different discipline would interfere, since there were obviously many sources of expertise and help.

So the atmosphere in the bureaucracy was hopeful, and it appeared that the chance had come for the social sciences to help with the difficult problems of overseas conflict facing the Defense Department and the country.

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* Dr. S. Rains Wallace, a psychologist whose career was devoted to various aspects of measuring human performance, selection and training of personnel for specific job skills, and related matters. In addition to his work for the DOD, Wallace had been chairman of the psychology department at Tulane University, vice president for research for the Life Insurance Agency Management Association, president of the American Institute for Research, and chairman of the psychology department at the Ohio State University. Dr. Wallace died of cancer in August 1973.

** At the time of the events described, these positions were held by Dr. Lynn Baker and Dr. Kenneth Karcher in the Army Research Office; Dr. Richard Trumbull and Dr. Luigi Petrullo in the Office of Naval Research; Dr. Charles Hutchinson in the Air Force Office of Scientific Research; and Dr. J. C. R. Licklider and Dr. Lee Huff in the Advanced Research Projects Agency.

*** Dr. Jesse Orlansky is a psychologist who has specialized in human factors engineering, measurement of human performance, and other problems in the application of the behavioral sciences to defense systems. He was one of the founders of the behavioral research firm of Dunlap and Associates, and has been a member of IDA staff and management since 1960. He has also been a member of many scientific advisory committees for the military services and NASA.

Chapter 5
The Foundation

When the Defense Department—which from here on will be called by its familiar acronym, the DOD—undertook in 1964 to enlist the social sciences in support of its expanding foreign operations, it was not starting from a zero level of effort. There were already a number of research programs underway that intended to improve the U.S. government's knowledge about foreign countries and their peoples, the behavior of foreign governments, causes of revolutions, and cultural factors affecting relationships between the United States and other countries. The thrust of recommendations previously described, when translated in action, was to build on this base, to add work seeking the particular knowledge and understanding for which the need had become apparent, and to increase the level of effort to meet newly emerging and newly emphasized problems.

Before describing this existing research base, it will be useful to digress into a brief discussion about what is meant by "a million dollars worth of research," in terms of effort applied, since the question of money available to the DOD but not to others, such as the State Department, for such research was to dominate later discussion of the propriety of the work. By the time the researcher was paid his salary, his* "fringe" benefits were added, his secretarial help and travel to Washington and overseas were paid for, and other "overhead" expenses (e.g., general administrative support and the mortgage or rent on their buildings/facilities) were covered, it cost (during the 1964–69 period covered by this book) approximately $50,000 per year for one professional researcher, outside the government, to work on a problem for the government.[1] This amount might vary, depending on the organization. The cost at some universities might be as low as $35,000–$40,000 per year; but often many overhead items were covered in other accounts encompassed by a variety of grants from many sources. Moreover, the university researcher received his teaching salary from the university, and the research grant was used to cover his time only when he was not teaching. He could be assisted by graduate students who received minimal compensation. Thus, a man-year

* The previous comment about the gender of researchers applies here as well, and will be adhered to henceforth without comment. It is appropriate to also note that the cost figures for a person-year of effort have also been overtaken by the upward drift of all costs, expressed in current dollars, over the time from the events described here (1960s) and the present (2012). No attempt has been made to escalate the dollar costs to the present or to convert the costs given here to constant dollars, as that would not change the thrust of the argument that costs for research bear overhead attachments well beyond the salaries of the researchers.

of effort in a university could be stretched further than in a contract organization. On the other end of the scale, some nonprofit or profit-making organizations who had to charge all their expenses to contracts, in addition to a management fee, which might vary from 4 to 10 percent and was used for organizational development, such as exploratory research in new areas or continuing staff education, might require $50,000–$60,000 per year for a professional researcher in the continental United States. The cost could go as high as about $70,000 per year for a researcher who moved overseas and lived there with his family for a period of two, three, or four years.

Since most DOD work in this subject area was done by contract, the money to support this research had to be shown in the Defense budget explicitly under the particular research subject "line item" and appropriated by Congress each year. Therefore, it was highly visible. Some work by social scientists in other subject areas, such as the studies of human resources and performance undertaken for the Armed Forces, might be performed "in-house," and the budget would be covered by a general appropriation for in-house laboratories. Although the items might be distributed differently and the accounting categories might not be the same (e.g., such research might be carried under manpower, operations and maintenance funds, or facilities, and all might be scattered through different, aggregated budget line items), the total cost for a year of work by a professional researcher within the government was not very different. Even the State Department, which had at the time only about $150,000 for "external" contract research, did much related analysis in its Bureau of Intelligence and Research, which had a budget and staff whose overall "professional man-year" costs did not depart far from this average figure.

Thus, regardless of the source, whether the research was contracted out or performed inhouse, "a million dollars worth of research" meant about 20 professional scientists or analysts were working on the government's problems and were paid for by the government. A DOD budget of about $5–$8 million for this work (about the range that was covered, although many higher figures were quoted incorrectly, as we shall see) meant increasing the number of professional social scientists engaged in this area of research from about 100 to about 160. In the DOD case, since most of the pertinent research was performed by contract, these researchers were largely outside the bureaucracy. The analytical staff of the State Department's Bureau of Intelligence and Research came to roughly the same numbers, although they were almost exclusively within the organization.[2] The additional 60-odd

people would be drawn from a professional social science community that, including all social scientists, numbered over 50,000 during that period of time.[3] At the same time, thousands of professional engineers and physical scientists were working on research and development to solve weapons and equipment problems associated with or related to the Vietnam War alone, and the number working on all DOD R&D was obviously far higher. While it later became obvious and was, in fact, understood at the same time that very few people performing research related to foreign social problems could be in a very sensitive position, the numbers talked about were never very large and only a few of these people—perhaps 10 percent—ever went overseas to do research on the problems in the field under DOD auspices.

The sponsorship of DOD programs in the areas associated with overseas conflict and counterinsurgency was divided, in 1964 and subsequently, among the three services, the Advanced Research Projects Agency (ARPA), and the Office of the Assistant Secretary of Defense for International Security Affairs (ISA). The pattern of work among these varied. Such research in the Air Force and the Navy was funneled through or initiated and monitored by the Air Force Office of Scientific Research (AFOSR) and the Office of Naval Research (ONR), respectively. They worked largely through a system of research grants to university scholars. The offices were under the general supervision of the military, but essentially operated with a fair degree of independence under the control of civilian division directors within the offices (see notes, p. 48). The grantees worked as individuals and had great freedom to travel, contact American and foreign officials, and to undertake their work and publish their results with no restraints on academic freedom. The work was usually not classified, although the scholars might need access to classified sources. But the work might be quite sensitive, especially if the scholars had to contact American or foreign government officials or study government papers. AFOSR and ONR relied upon the good sense and circumspection of the scholars to protect their sources and not to upset the delicate matters of foreign relations.

Work for AFOSR and ONR was undertaken largely though the mechanism of unsolicited proposals sent to these organizations by the scholars. Generally, relationships existed between the service organization and the universities, which would allow the scholars to learn what problems were of interest. Except for specifying general areas of interest to encourage proposals, problem definition and specific research subject matter and format were performed by the grantees. The research projects then were molded

into final form through discussion between the scholars and the government "program managers." Some work for the Air Force in this area was also performed by the RAND Corporation—a nonprofit Federal Contract Research Center (FCRC) that had a style and reputation for scholarliness that rendered it not much different from the university community in important respects and gave it the same kind of access and freedom. Some studies oriented primarily toward naval strategy but possibly having social science components that might also be performed by another FCRC,[4]—the Center for Naval Analyses (CNA)—again under similar conditions.

The Army's research operation in these areas was quite different. Behavioral and social science research at that time was conducted for the Army primarily through one inhouse and two contract organizations—respectively, the U.S. Army Personnel Research Office (USAPRO), the Human Resources Research Office (HumRRO), and the Special Operations Research Office (SORO). USAPRO concentrated largely on the kinds of human factors and testing problems described in the statement for the Fascell subcommittee (pp. 23–28). The other two organizations, which had been established for the Army by George Washington University (HumRRO) and American University (SORO), were at that time captive Federal Contract Research Centers, obtaining their funds exclusively from the Army and working on problems largely prescribed by the Army. These organizations were supervised by the Army Research Office and, in some cases, parts of their programs were monitored by a representative of the Army's Deputy Chief of Staff for Operations (DCSOPS), a primary "user" of the results. ARO was essentially a *military* organization, although it employed civilians. Its Human Factors and Operations Research Division, headed in 1964 and 1965 by the late Colonel William Sullivan, had two civilians Dr. Lynn Baker and the late Dr. Kenneth Karcher—both psychologists—who provided technical supervision over the work of SORO and HumRRO and who were to play an important part in later events.*

Each year SORO and HumRRO had to work out the detailed agreements for the individual work programs. While management responsibility was vested in The Army Ressearch Office, DCSOPS made its formal

* This office also supervised the work of the Research Analysis Corporation (RAC), another FCRC that performed operations and systems analysis work for the Army. RAC became deeply involved in counterinsurgency research under the auspices of ARPA; social scientists were involved in some of this work, but it was operational rather than social-system oriented. The Army supported little counterinsurgency research at RAC.

"requirements" for outputs known and played an important part in shaping the program. The organizations were assigned formal tasks by, and prepared reports for, the Army. The reports were published only after Army (e.g., DCSOPS) review and approval. There was close supervision by the Army not only of the work but of the salary structure, travel, contacts with Army officers in the field, and contacts with others for the purpose of gathering data. Thus, whereas the Air Force and the Navy hired top talent at universities (i.e., supported scholars in their own areas of expertise, largely doing work that the scholars proposed to do), Army-supported organizations had tightly structured programs representing largely what the *Army* wanted them to do and for which professional researchers were hired to work as assigned, first on one task and then on another, away from any close contact with university colleagues.

It is implicit, first, that there was a division among the services between "basic" and "applied" research. The university scholars supported by the Air Force and the Navy were searching out and exploring basic phenomena—why did certain human events or interactions take place; what were the pressures for social change in a society; what were the underlying mechanisms of human behavior and interaction therein; what were the differences in behaviors in different cultures? Thus their work was along the lines of the fundamental studies called for by Smithsonian reports. But Army contractors worked on applications of social science knowledge and followed more closely the applied directions recommended in the report or the limited-war advisory group for DDR&E. HumRRO, staffed largely by psychologists, was oriented toward problems of troop training and behavior and troop performance under stress. This included some study of language training and of cultural factors in contacts between American Army personnel and foreign military personnel. Beyond this, HumRRO's program did not get involved much in counterinsurgency questions. Rather, this cross-cultural research fed and was picked up by others.

SORO was the organization that concentrated on studies of foreign areas and revolutionary war. SORO had been established by the Army in 1957 to serve the needs of the Army's psychological warfare directorate, which did not find the special skills needed either at HumRRO or the Operations Research Office (Research Analysis Corporation's predecessor organization). Its mission and level of effort were expanded in 1962, with increased interest in the Army Special Forces and counterinsurgency operations. This was part of the Army's response to President Kennedy's call for increased effort in such areas.

SORO's work included analyses of how Communist organizations worked; case studies of revolutionary warfare, such as Cuba, Algeria, and Vietnam; and for DCSOPS who wanted to use them in preparation for Special Forces operations, descriptions of tribal groups in various countries. Also under DCSOPS auspices, they engaged in preparation of "area handbooks"—unclassified books that described, in detail, the history, culture, and societal structure of diverse foreign countries in the "Third World"—to educate officers assigned to overseas duty. These handbooks were also in great demand by other parts of government, such as the State Department or Agency for International Development, who regularly sent people on overseas assignments. By the nature of the research or study assignments, while the people supported by ONR and AFOSR used basic data sources or performed field and laboratory research, SORO used secondary sources and did much library research. As an organization, they were inexperienced in fieldwork, although (with HumRRO) they had small field offices working under the respective military commanders in Korea and Panama on specific problems of the relationships between American troops and the local populations.

The research programs of the services were thus different in quality and responsiveness to service needs. It was my view then that the Air Force and Navy programs could be undertaken by the best people, but the work was generally oriented to subjects the scholars wanted to explore, while the Army could much more easily initiate work on problems that appeared important to the Services but it could not always have such work performed by the best available expertise. This dichotomy underlay much of the later difficulty that the entire DOD program encountered.

The ISA- and ARPA-supported work (see p. 51) followed a different pattern still. ISA needed strategic and policy-oriented studies—what, for example, might happen in various areas, such as NATO or Communist China, under international conditions, and what were the policy implications of such events for the defense of the United States? ISA, often working through ARPA for administrative arrangements (since both were part of the Office of the Secretary of Defense) let contracts with Federal Contract Research Centers, such as RAND or the Institute for Defense Analyses (IDA); with nonprofit organizations such as the Stanford Research Institute (SRI); and on occasion, they might be able to undertake a study contract with a well-known university scholar. The work supported by ISA, essentially a civilian organization, was under much less rigid control than the Army's, but research differed from that at universities in that much of

it was classified. However, at this time, before the campus turmoil engendered by Vietnam, the universities had no trouble accepting classified research; often it was embraced as a source of steady support. Thus, since such work was usually undertaken by mutual agreement between the organizations and their sponsors, the conditions for ISA's contractors were generally similar to those at universities, with the additional advantage for the government that teams of two or more researchers could work full-time on a problem without diversion for teaching or to administer other grants necessary to support graduate students. These research organizations could attract scholars and experts from universities and government and turn out studies of high quality, depth, and perception more or less at the predicted time, which was not always the case with university research.

RAND and a number of other organizations, such as RAC and Stanford Research Institute, also worked for ARPA (sometimes, as in the RAND studies of Viet Cong prisoners described later, jointly with ISA). In 1963, ARPA had initiated a basic research program in the behavioral sciences associated with advanced information processing technology, under the direction of Dr. J. C. R. Licklider, a well-known psychologist who had taken leave from the consulting firm of Bolt, Beraneck, and Newman, to help implement some of the recommendations of the first Smithsonian report. ARPA, after helping to start a U.S. space program, had by this time become more generally applied to DOD instruments for starting new, experimental research and development programs that entailed more technical risks than the service budgets could tolerate. Licklider's program represented the kind of advanced work that ARPA could undertake; at the same time he was available, as a member of the agency, for consulting on the counterinsurgency studies just being initiated under ARPA's Project Agile.*

Project Agile, along with "counterinsurgency" and "the CIA," came to symbolize all that the Students for a Democratic Society and others found reprehensible about American overseas operations[5] and had been established by ARPA in 1961 in response to DDR&E's instruction, which resulted in turn from recommendations contained in the limited-war study group's report. The name "agile" was selected to signify the project's ability to respond rapidly to urgent requests for research. Its assignment was to perform research and development in the counterinsurgency area, largely in

* It is not generally recognized that Licklider's work for ARPA during this period laid the foundation for the continuing work that later led, first, to ARPANET and subsequently to what we now know as the Internet.

support of American activities in Southeast Asia. It had, early in its history, concentrated on counter-guerrilla hardware systems and equipment. In late 1963 and early 1964, it was beginning to become concerned with the human problems of such warfare.

After Licklider's departure from ARPA in 1964, and under stimuli that will be described in detail shortly, Agile increased its efforts in the so-called "soft" sciences related to Vietnam and counterinsurgency. In 1965, this area of work and some basic social science research that had been initiated by Licklider, both came under the direction of Dr. Lee Huff, a political scientist who had worked for ARPA in Thailand. One of the first such efforts undertaken by Agile, under a contract to the RAND Corporation and jointly sponsored with ISA, was a program of interviewing Viet Cong and Northern Vietnamese prisoners and defectors in Vietnam to explore the factors of motivation and social cohesion that bound cadres to the Communist side in the Vietnam War. Also through RAND, ARPA initiated a study of the relationships between U.S. military advisors and their "counterparts"—those they advised in a one-to-one relationship—in Vietnam. Agile had had studies performed of the effectiveness—or reasons for the lack of it—of the strategic hamlet program in Vietnam and, in 1964, Michael Pearce, a young RAND social scientist under AGILE sponsorship who spoke Vietnamese and lived in a village some 30 miles from Saigon, was studying the dynamics of the interactions among the Viet Cong, Vietnamese government officials, the Vietnamese army, and the people. A study of village security in Thailand was also initiated in 1963, and an evaluation was soon to be made of the Thai Government's Mobile Development Units, which were designed, as part of an effort to counter Communist infiltration and propaganda, to help integrate the villagers in Northeast Thailand better into central Thai society. The ARPA program was much freer than that of the Army because individual contracts were let with many different organizations, so that the best available research quality could be sought.

The total DOD programs of nonhardware research that could be considered directly relevant to counterinsurgency programs in 1964 came to about $5 million, divided as shown in table 1. Table 2 lists the titles of typical study projects from each of the sponsor's subprograms. The breakdown into four categories was derived later when we started to work at expanding the program; it will be described in some detail in chapter 7.

We have already alluded to the processes by which "quality control" was exercised over the work. AFOSR and ONR generally maintained contact

Table 1 • Support for work in the social sciences related to counterinsurgency, FY 1964.

	Amount (thousands of dollars)	Equivalent professional researcher*
By sponsoring DOD component:		
Army	$2,305	46
Navy	280	7
Air Force	393	8
ARPA	2,200	44
TOTAL	$5,178	105
By orientation of work:		
Political studies	562	11
Operations research, systems analysis, economics	1,841	36
Persuasion and motivation, psychological operations	1,617	34
Manpower, selection, and training research	1,158	24
TOTAL	$5,178	105
By organization conducting the research:		
Universities	455	9
Nonprofit organizations:		
SORO	1,180	24
RAC	691	13
RAND	542	11
HumRRO	845	18
Other	529	11
Inhouse, government	455	9
Industry	472	9
Foreign universities	9	1
TOTAL	$5,178	105

*Based on $50,000/man-year.

Table 2 • Illustrative projects supported by sponsor, program area, and type of contractor, FY 1964.

Army	Navy	Air Force	ARPA
Political studies			
None	USSR-Chinese relations (university)	Patterns of national development and implications for military planning, Africa, and Latin America (university)	Studies of Northeast Thailand (nonprofit)
Operations research, systems analysis, and economics			
Studies in counterinsurgency (nonprofit)	Research implications of naval counterinsurgency and unconventional warfare operations (nonprofit)	None	Republic of Vietnam Air Force after-action study (nonprofit) Railroad security (nonprofit) Ambush patterns and counteraction techniques (inhouse)
Persuasion and motivation, psychological operations			
Psychological operations guides (nonprofit)	Delineation of the naval role in psychological operations (for-profit research organization) Inducing cooperation between adversaries (university) Small group coalitions (university)	Military power and persuasion (nonprofit) Measurement of attitudes and attitude change (university)	Analysis of mobile development unit operations (nonprofit)
Manpower, training, and selection research			
Development of concepts and techniques in area training (nonprofit) Procedures for increasing the effectiveness of small infantry-type units	Development of culture-free tests (industry)	None	Advisor-counterpart communications (nonprofit)

Note: All projects not included; represents a sampling to convey the flavor of work supported by each sponsor.

with their grantees and reviewed their work periodically with them, but the drive toward high quality was largely those of scholarship exercised within the university community. The Army meticulously reviewed the products of the work it supported and, through this review process, affected both its quality and its orientation toward the Army's areas of concern—at least as interpreted by the reviewing officers. The program directors in the Office of the Secretary of Defense—ISA and ARPA—generally worked through "agents" in the services; that is, their money was transferred to the services, which let the contracts for research. But responsibility for technical review and supervision of the work was retained by the initial sponsoring organizations, and quality control was left at least as much up to the contract organizations as to the sponsors. The sponsors could, of course, exercise the sanction of not extending, renewing, or giving further contracts if work was found to be below par.

This, then, was the basic program material to which the DOD, while I was in the Office of the DDR&E, had decided to add, and the organizational structure we had to work with. It might be commented about the shape of this initial program, that its size and the studies and subjects included in its description reflect, to a certain extent, judgments that were made at the same time about what information to include in assembling the program data. The counterinsurgency problem was so complex and touched on so many different aspects of human behavior and social structure, varying from the military to the economic to the political and the cultural, that a separate decision had to be made for almost each individual project regarding whether it should be included or not. Since the intent was to understand the phenomena in all their broad aspects, and since the boundaries of the DOD role were quite fuzzy, the judgments were almost invariably made to include more that might appear relevant rather than less. In the words used at the time, we tended to "cast the net wide." These judgments were subsequently refined by a study undertaken for the purpose; but this is jumping ahead. After the events of Project Camelot, with the attending reaction by Congress and the press, this tendency to encompass as much as possible became more and more constrained. Ultimately, with Senator Mansfield's amendment to the 1969 Defense Authorization Bill requiring all DOD work to be directly and obviously relevant to purely military matters, almost everything that the DOD might do in relation to foreign areas that would use the contributions of social scientists came to be included, so that, ironically, the act of making judgments about relevance became irrelevant.

Chapter 6
Bureaucratic Interactions and Attitudes

The report to the government by a group of experts who make program or action recommendations in a particular area, which then languishes because the recommendations are conceptually or bureaucratically difficult to implement, is a familiar phenomenon in American government life. Given a set of recommendations that read, roughly, "increase social sciences research on counterinsurgency, and here are some general subject areas of interest," and assuming that the government is positively motivated to follow such recommendations, how does one go about doing it? There are no rules except that one has to work within the bureaucracy. The task becomes purely a matter of matching the elements of the bureaucracy and their inter-relationships to the components of the problem and *their* inter-relationships, and trying to launch new ideas and new budget assignments over the usual bureaucratic inertia and resistances. We needed to learn what was going on currently in the DOD's programs, to establish a measure of control over those programs, to decide where to go next, and to start the process of instruction and coordination that would initiate the travel of the new program from conception to implementation. All this came to be encompassed in a set of five actions, which evolved as the effort went along as much as they were planned long in advance: the establishment of an inhouse DOD working group; a more or less regular series of meetings with the research directors of the other departments of government; a review of past history, new program proposals, and the DOD's research instruments by the Defense Science Board; negotiation and issuance of appropriate instructions for budget actions and program changes; and, subsequently, meeting with the staffs of the responsible congressional committees to explain the new funding requests.

From all the previous work of the social science community, we had a fair idea of what needed to be done generally. But the need now was to make the ideas very much more specific. An ad hoc committee of those responsible for this area of research and development in the services and ARPA was convened, with myself as chairman. The committee first met on February 7, 1964, and agreed that it would serve as a channel for informal communication of ideas and "trial balloon" instructions between DDR&E and the services, and as a vehicle to discuss and agree on purposes, definitions, and specific projects. The question of defining counterinsurgency and

judging the relevance of individual projects were among the first discussed. The work of drawing together and collating lists of "relevant" ongoing projects was initiated. It was agreed that those would be grouped in the four categories (which, by themselves, illustrate the scope of the research being considered) mentioned earlier: political studies; operations research, systems analysis, and economics; persuasion, motivation, and psychological operations; and manpower training and selection research. In addition, a first step was to be taken toward listing additional work that the participants desired to undertake for two years ahead, assuming that additional funds would be available.

From these discussions, I gained the impression that not all the members of this ad hoc committee were exactly eager to bring their work under one set of headings for later exposure. As is natural in bureaucratic affairs, the coming together for a common task began the exposure of divergent points of review and self-protection for the different agencies. The resulting conflicts, friendly and low-key though they were in committee, had much to do with the shape of the final program plan. Since the director of ARPA reported directly to DDR&E, he would have no trouble in protecting his efforts; but ARPA was concerned about whether the committee's deliberations would lead to an altered pattern of control. The Army viewed the subject as very much within its mission and responsibilities, and it was anxious to get started; but the committee's work would shake established patterns and exacerbate the then-quiescent conflict between the Army's R&D and DCSOPS organizations about what SORO should be doing.

The Navy and Air Force representatives indicated more by attitude than words, that they foresaw a number of problems in the offing. They had had enough trouble obtaining support for their programs from the professional military officers who were really more concerned with hardware, weapons systems, and studies of tactics and operations, and they anticipated more critical reviews as visibility and pressure to change were increased. The relationship with the university community would always include some sensitivities, and it would need protection. Gathering a list of all their projects, which would then become part of a different sort of program than the basic research efforts within which they were then imbedded, made those projects in the aggregate much more visible and therefore much more vulnerable to adverse action, both within the services and by Congress. Thus, it took some time over several meetings and a considerable amount of discussion and negotiation to agree that all the information would be drawn together and that

the program would be considered in the aggregate for this particular purpose. But all of this was quite straightforward in concept and did not present more than the expected number of bureaucratic difficulties.

At almost the same time I initiated an effort to examine—in relation to each other and, if possible, to tie together—the efforts of the DOD and the other interested government departments. This was a much more complex problem, since there was not, among the various departments, nearly as much unity of purpose and of methods as there was among the disparate parts of the DOD—and even the latter were different enough from each other. A meeting was held on March 20, 1964 with representatives of State's Bureau of Intelligence and Research; the research director of USIA; one of the men in charge of a part of AID's research and evaluation program (there was more than one program); and a representative of the CIA's R&D community.

The purpose of the meeting was stated, first, as information exchange on "non-material research of interest to all the departments with respect to the counterinsurgency area," and then to explore the possibility of improving coordination of this research among the departments. Again, we ran into the definition problem and, again, decided to leave it broad and up to the judgment of the individual as to what might be included in such a program. But it was decided that the scope should encompass research in the "political, behavioral, economic, life sciences, and operations research associated with areas of psychological operations and civic action."

I outlined the developing DOD program objectives and gave a preview of our intent to expand this area of research. These plans received the universal blessing from those attending the meeting. They agreed that the problems were of important and universal interest. Of course, shadings of differentiation within this general agreement were to appear soon enough. We discussed the potentiality of conflicts between this particular attempt at interdepartmental coordination and the efforts of other coordinating groups, such as the Federal Council of Science and Technology and the State Department's Foreign Area Research Coordinating Group (FAR), which was just being formed. The latter was especially important because all the principals in the DOD ad hoc committee would eventually become members of FAR (along with representatives of any other government department that might be supporting work having to do with a foreign area—even the National Science Foundation and the Department of Agriculture). FAR was to play an important role in the later bureaucratic infighting between State and Defense Departments over Project Camelot.

The group agreed to meet again soon and to exchange information on the departments' respective research programs, following which we would discuss where to go from there. The possibility of exchanging funds, whereby one agency, such as DOD, could contribute to the support of work more appropriate to the mission of another, was also explored. It was noted that this might not sit well with Congress, but that the idea warranted exploration in any case. We did meet again on May 8, 1964, with some new faces as interest and responsibility were sorted out. In particular, William Nagle, Director of External Research in the State Department's Bureau of Intelligence and Research, came, and thus began a year-long interaction that was to have important consequences for the DOD's program. The respective research programs were discussed and for the first time each department obtained a coherent and comprehensive view of work done by the others in this area and of the relationship of its work to that of the others. None of the departments—even the DOD, at the time, except for the Army and ARPA-Agile work—had as specific an orientation to the subject of counterinsurgency as was being contemplated by the DOD for the future. Thus, the work bore generally on internal economic and political development of the developing nations, but not specifically on the human problems of revolutionary events that were boiling up in American consciousness and foreign policy.

AID's program included such things as country-oriented economic modeling, studies of the diffusion of innovation, studies of the economic aspects of cultural change, and studies of socioeconomic problems, such as land tenure (but, in the last case, not as used by revolutionaries, such as the Chinese Communists, the Viet Minh, or the Viet Cong). Despite the large AID responsibility in Vietnam and elsewhere, there was no research associated with the economic problems of the country at war, or with AID's public safety program and its operations in Vietnam. (This came much later, about 1967, when AID established a Vietnam bureau.) It became apparent also, from this and other "two-sided" discussions, that the "formal" research program of AID was not all of AID's research, since evaluations of overseas economic programs, a key part of AID's job, were performed by the "desks"—those offices responsible for administering and overseeing action programs in specific countries or regions—using AID program funds. These studies were not under the control of AID's research director. The AID research program, exclusive of the "desk" evaluation programs, came to about $12 million annually.

USIA generally had about $300,000 per year devoted to survey research—the impact of information programs overseas, views of the United States by foreign populations, attitudes and aspirations of populations in foreign countries toward whom our information programs were directed. The USIA representatives made the point that they had the facilities and the know-how for such work, which could make an important contribution to understanding the overseas social and attitudinal problems associated with counterinsurgency, and that these could be available to other agencies for their use if appropriately funded. Again, they had no research supporting their operations in Vietnam, although USIA had the responsibility for all information and psychological operations programs in Vietnam and were soon to be joined with the military in a USIA-directed organization in Saigon (the Joint U.S. Public Affairs Office, JUSPAO). They agreed that much work was needed in Vietnam to understand the outlooks and aspirations of people on both sides in the conflict, and the impact of the war on the Vietnamese people—a need ARPA was later to fill. USIA either could not get, or did not ask for, the research budget—I never found out which, but in their case as well as State's, the two possibilities obviously interacted to establish a pattern.

The CIA stated that they had no general research program in the social sciences, but that they performed ad hoc, inhouse studies in support of their immediate operational needs. The meeting and the DOD work (current and proposed) were of great interest to them for what they could learn from them.

In this and in many subsequent meetings, I learned much about the State Department's research structure and attitude. They took a dim view of contract research. They felt that research into foreign affairs or studies that might support policy making in foreign affairs had great sensitivity and was best done in an atmosphere of secrecy with respect to "outsiders." Their attitude was that they would rather not have the outside world know they were studying subjects that might be of immediate operational importance, and clearly this could not be avoided if they hired people on contract. Ergo, outsiders could simply not study important affairs of state. The State Department did have about $150,000 for contract work, which they tied to the problems of the "country desks" and which might be used, for example, on studies of elites in Africa or estimation of the long-term consequences to Panama of a second canal. But most of the State's study or research was performed inhouse and was not discussed by this group. Later

views of this work that I obtained showed such results as narrative descriptions of events like the rebellions in the Congo in the early 1960s, which might contain some "inside" information but little analysis of causes and effects, and which struck me—a purely personal view, and, of course, I do not claim to have seen all of their work—as not much different from the analytical dispatches by well-informed correspondents that one could have read in the newspapers.

By the time of our second meeting, State had also formally established the Foreign Area Research Coordinating Group, which was to serve as a clearinghouse for all information on foreign area studies—drawn from all government-supported contract work, summarized, and disseminated to all departments. The impression I had at the time was that this effort represented the primary occupation of the External Research Group in the Bureau of Intelligence and Research. The State attitude toward research became important because it affected how the DOD and the social scientists interested in work overseas were to view the State Department when State was given government-wide responsibility for reviewing research on foreign areas.

State's attitude toward counterinsurgency was, at the time, also interesting and especially frustrating in view of our involvement in Laos and Vietnam. They appeared to consider problems of internal conflict a diversion from their normal areas of concern, outside the main interest of foreign policy and diplomacy, and something that would, if played down long enough, eventually be resolved in the normal course of international relations. This was not because of any expressed aversion to the conception of counterinsurgency as a method of countering Communist activities, such as those in Vietnam, or because of any unhappy connotation that the term might have—possibly some such attitude existed, but I did not detect it at the time. Rather, the attitude of those in the State Department with whom I spoke was that events in foreign countries were all interrelated, and it was not State's "style" to single out some sequence of these events for special consideration. Events in Vietnam would be handled in due course by traditional methods, and it did not seem that even a burgeoning war would disrupt their system, cause them to establish a special effort, or require coordination and research planning outside the "normal," diffuse channels they had established.

State's attitudes toward research at the time are summarized by Lyons' statement that "the Foreign Service . . . leans almost exclusively on the tra-

ditional historical and institutional analyses favored by the political realists, when it does not proceed simply on the basis of intuition. . . . The conservative attitude . . . its recruitment policies, its emphasis on short-term goals, its limited research system—all tend to discourage acquaintance with recent trends in social science."[1] Their response to the war evoked the following from a wholly different source. Chester L. Cooper, who worked on the problems of Vietnam in the CIA, the White House National Security Staff, and later, on Ambassador Harriman's negotiating staff, has written:[2]

> . . . But time after time during 1964 and 1965, McNamara and his subordinates seemed to be crying for political guidance and leadership from the State Department. It was slow in coming.
>
> The State Department seemed resigned to playing a reactive, even peripheral, role during the early 60s. The war in Vietnam, it was felt, was Pentagon business. . . . By and large, the non-defense elements of the government were neither psychologically nor organizationally able to come to grips with an insurgency that was quickly getting out of hand. None of the courses given at the Foreign Service Institute, and none of the experiences of AID specialists and Foreign Service officers elsewhere, seemed relevant to what was going on in Vietnam.

Thus, Nagle, as a member of State Department bureaucracy, was reflecting what seemed to be the general climate of opinion and attitude at State.

My own graphic illustration of these attitudes, other than that received during the Camelot crisis, occurred in two related incidents. Early in 1965, after the Defense Science Board's report recommending certain research was sent to the State Department, I suggested to Nagle that some subjects had arisen during the course of that work that appeared to be of interest and importance to the United States but were really beyond the scope of DOD's direct concern. Among these was the question of understanding the political groups we undertook, implicitly or explicitly, to support or oppose by our policies in the developing countries. Another subject which seemed important was that, since events in Vietnam were obviously not going as smoothly as the early predictions might have led us to wish (this was the period of unstable government under the "generals" after Diem's overthrow), one should undertake some studies leading to understanding of feasible "fallback" positions in Southeast Asia in case things in Vietnam did, in fact, not work out as they had planned.

I offered to have the funds made available and the research undertaken in such a way that State could formulate the problems and participate in the general direction of work. State's answer, as I remember it, was that the State Department did not believe that such work should be done under contract. I later probed quite deeply to find out whether the work was being done by State inhouse. Eventually I came to believe that these were long-range, "iffy" sorts of problems, which, since they did not present themselves for immediate decision, were simply deferred in the analyses State was performing to support its own policy making. State was concerned with much more immediate futures and problems. One can suppose, also, that the thought of seeking alternatives in Vietnam was bureaucratically and politically unattractive in 1965.

In another case a year later, I asked a high State official whether any study was underway on the problems of the growing rebellions in Portuguese Africa to help build an understanding of the forces at work there so that, if we were forced into a policy crisis by some future events, we would have the knowledge needed to formulate the policies wisely. The response was that this subject was too sensitive to study—"If it even became known that this was under study . . . " They may have been right. The subjects were certainly sensitive ones to study; and there are similar ones today. On the other hand, I have not observed recently that we are necessarily developing the best knowledge and expertise for formulation of policies about such problems (although it must be admitted that our policy of *no* action in that area seems to be working reasonably well, thus far).*

It became apparent, as a result of these interchanges, that with a serious war building up in Southeast Asia, the DOD rightly or wrongly, appeared to be the only government organization that saw the need to undertake research on problems associated with that war and was gearing up to try to meet them. But the others were interested, and because they faced budget problems and political problems, they were glad to let the DOD take the lead, and they encouraged us to do so.

At the May 8, 1964 meeting, an idea was born that was to persist or be rediscovered often in various forms. The idea was to establish a formal, high-level interagency coordinating group to review research needs in this general area, propose projects, and see that they were undertaken. It was

* The wars of separation of these Portuguese colonies from their colonial masters and establishment of their independence came later, during the 1970s and beyond.

suggested at the time that this group might report to the Special Group (CI) and it was agreed that this would be worth further exploration. Again, problems of budget exchanges were discussed. We confirmed our earlier intuition that neither the Budget Bureau nor Congress would be especially happy to have money that was appropriated for one department transferred to another. On the other hand, it seemed that each department could let contracts in its mission area at the others' request, and that each could work with and advise the DOD if the DOD with its larger funds were to let such contracts. Thus, the problems were not viewed as insurmountable, even though "clean" exchanges did not appear to be in the cards.

And so, while Congress and the news media were later and continually to deplore the lack of interdepartmental coordination on research having to do with foreign policy matters, and were to call for more and more coordination, those most deeply and directly involved had initiated the coordination at an early date. It is ironic, too, that at these meetings AID and DOD, rather than State, were the ones who raised questions about the sensitivity of performing research in and about foreign countries, and initiated discussions about ways in which these sensitivities might be respected and the research prevented from causing problems between those countries and the United States. Of course, while "the DOD" on one level had such concerns, on another level and independently, "the DOD's" actions were to lead before too long to just the kind of problems that were feared.

Chapter 7
The DSB Study

It is almost axiomatic in the American system that an idea welling up from the bowels of a bureaucracy needs the recommendation and approval of an outside, respected advisory group before it can receive the official imprimatur. Despite the fact that these new ideas originated virtually at the top of the DOD research leadership, and that they were based on recommendations contained in a series of studies by recognized experts in the field, the decision to increase social research on counterinsurgency problems, which would involve budget changes and specific contracts, started a whole new ballgame. It is perhaps proper that the opinions of the experts who were responsible for social research within the DOD could not be accepted at face value, since the nonexperts in the Department could not know how these opinions were colored by past prejudices and current bureaucratic constraints. And when the work is to begin "at eight o'clock on a Monday morning," the specific problem is not the same as that contained in the reports of the more generally concerned study groups. So a new study group was formed.

To obtain expert advice on specific work to be done in the expanded program and about what would seem to be a reasonable rate of expansion, and to obtain the advice from a level and source that would carry the weight and authority of "the best people in the field" with the upper structures of the DOD and the Services, and with Congress, DDR&E turned to the Defense Science Board. The Defense Science Board was created in 1956 in response to a recommendation of the Hoover commission "that the Assistant Secretary of Defense (Research and Development) appoint a standing committee, reporting directly to him, of outstanding basic and applied scientists. This committee will canvass periodically the needs and opportunities present by new scientific knowledge for radically new weapons systems."[1] From this beginning, the scope of the board's membership and studies came to encompass all of the DOD's technical, scientific, and associated management efforts. In 1964 and 1965, it consisted of 28 members, 8 of whom were drawn from public bodies, such as the Atomic Energy Commission, the National Academy of Sciences, National Science Foundation, National Bureau of Standards, and NASA. The others were selected on the basis of their eminence in any of the fields of scientific research and engineering with which Defense R&D might have to be concerned. Study

tasks were assigned by DDR&E, who might be acting on his own or on behalf of the secretary of defense. The board's chairman and vice chairman were appointed by the secretary of defense on the recommendation of the DDR&E, and very often its reports were addressed to the secretary of defense "through" the DDR&E. Eminent scientists, not members of the board proper, could be added to special subcommittees or "panels" of the board to carry out specific studies.

At the time of these events, Dr. Frederick Seitz, who was the president of the National Academy of Sciences, was also the chairman of the Defense Science Board. (Although this connection between the National Academy and the DSB no longer exists, it was not as tenuous as might be thought at first glance. The academy was formed by an act of Congress in 1863, in part because eminent scientists of the day were seeking an organized way to contribute to the Union war effort.)[2] A Behavioral Sciences Panel was appointed, under the chairmanship of Dr. Lyle H. Lanier, a member of the board and the executive vice president and provost of the University of Illinois. The panel included, besides Lanier, Ithiel de Sola Pool, who had participated in the Smithsonian studies; Dael Wolfle, the executive director of the American Association for the Advancement of Science; and Thomas Caywood, one of the founders of the Operations Research Society of America and a leader in the field. On April 21, 1964, Dr. Harold Brown initiated the work of the panel with the following charge:

> Conduct a study and evaluation of research and development programs and findings related to ethnic and other motivational factors involved in the causation and conduct of small wars among the peoples of Southeast Asia.

It was not unusual for a member of the DDR&E staff to work with a Defense Science Board panel performing a study requested by DDR&E. In this case, I worked with the Behavioral Sciences Panel as executive secretary, and Rains Wallace, who joined the Pentagon as director for behavioral and social sciences under the Deputy DDR&E for Science and Technology in August 1964, participated in the panel's meetings and helped write the report.

The work of collecting information on ongoing programs and planning for the future, which had been initiated with the ad hoc DOD committee, met an immediate need; the DOD committee became, in effect, a support group for the DSB panel. The panel heard from the services and ARPA

about what work was being supported and what they thought the needs were. They met with the major contractors and scholars who were doing the work. In addition, they were briefed by the military commands from overseas and Washington, who represented potential "users" of the research results, on how they viewed the developing problems overseas and on the problems they were encountering trying to provide military assistance in Vietnam and elsewhere. Particular attention was given to the special needs and problems arising in the course of the Vietnam War. Rather than taking the position that they would solve the problems with tactics and hardware, and yet without dismissing those as unnecessary, the military "users" reiterated their view of the importance of understanding people in the countries where they were working; of understanding the motivations of troops to fight or not to fight on our side and on the other side; and of what makes a good advisor able to relate to and work with his counterpart.

I had written to Brigadier General John K. Boles Jr., a thoroughly professional soldier who had gone to Vietnam to head the Military Assistance Command's Joint Research and Test Activity early in 1964, and with whom I had a very close working relationship, about the work of the committee, describing the kinds of problems it was concerned with and posing a list of questions, including the following:

- What capability exists in Vietnam for such research now?
- Do opportunities and data exist to explore the psychological strengths and weaknesses of the Viet Cong?
- Is it possible to work on such studies through the medium of prisoner-of-war interrogation, as was done in Korea?
- Has the South Vietnamese government ever carried out opinion surveys in the cities and villages?
- What studies of the kind I had outlined did he feel would be necessary and feasible?

In response, Boles said "Generally, I think that the studies cited in your letter are excellent, and if performed successfully, would go far toward helping us get a handle on defining our CI problems—something which we really have not succeeded in doing yet. Unfortunately, the war here greatly complicated the problems involved in conducting such research; it is a great pity that these studies were not carried out in Vietnam five years or so ago when it was more peaceful, because the results would certainly have been

most helpful then and now. . . . It is generally acknowledged that insurgent movements can be successful only where the established government has lost, or failed to gain, the support of its people. If . . . research techniques could be applied in a given country to determine the degree to which the people . . . support its established government, we would have an extremely valuable tool for evaluating the real danger represented by an insurgent movement. This tool undoubtedly would help us in advising a friendly country . . . " He then proceeded to answer the questions, generally to the effect that there was no research capability in Vietnam; that the South Vietnamese government had been very sensitive about the performance of research there, but that this sensitivity was evaporating under the increasing pressure of the war; that MACV (U.S. Military Assistance Command, Vietnam) and the Vietnamese government would therefore cooperate in the performance of research, but that research would be difficult because of the insecurity in the countryside. Of the possible subjects for research mentioned (to be described shortly), the two most important were viewed as the problems of advisor/counterpart relationships, and the psychology and motivation of the Viet Cong as they might be explored through interviews with prisoners and defectors.

Boles later visited Washington while the panel was in session and spent a day discussing with them the problems and needs of research on the insurgency and counterinsurgency in Vietnam. He stressed the relative failure of the "hardware approach" in solving the problems of insurgency. Although not deprecating those efforts and recognizing the contributions that they were making, Boles reported that it was generally recognized by the military in Vietnam that hardware research would not provide the answers to the struggle in Vietnam. He felt this general recognition had improved the climate for research, and he indicated that this was reflected in, among other things, General Westmoreland's frequent comments in staff meetings on the need for and desirability of social science research.

Other high-ranking military visitors to the panel—men such as Air Force Major General Anthis, who had been in command of U.S. air operations and advisory efforts to the South Vietnamese Air Force—added to the list of specific questions. These encompassed such problems as teaching American advisors more about the nature of their task, especially to keep it from being viewed as solely a military job; assessing what aspects of American counterinsurgency advisory programs should be emphasized for the greatest effect; and studying the problem of grassroots leadership in Viet-

nam to advise the government on how to strengthen such leadership, which seemed at the time to be the exclusive property of the Viet Cong. The problems of knowing *how* one was doing against the adversary in the counterinsurgency conflict and of obtaining data for evaluation and planning, loomed very large in all of these discussions. Further, a number of difficulties in performing research in the field, including those of assuring the understanding of the American military below the very top levels; of obtaining the support and cooperation of the South Vietnamese government; taking the time, against political pressures for great speed, to evaluate long-term social programs properly; and the sheer difficulty of being able to take systematic data in the field under adverse conditions—all problems that were to plague the DOD's field research efforts later—were brought out.

At the same time that it was concerned about what work needed to be done, the panel started to explore the capability of the research community to undertake that work. It was concerned with what "state of the art" might be. A study by the Institute for Defense Analyses of the materials being gathered by the services was commissioned to ascertain the then-current status of the ongoing programs and the availability of qualified research organizations and personnel. What had previously been in the category of rather vague generalities now received much more careful definition. The work categories that the ad hoc committee of the DOD had decided to use for organizing the project listings were defined more precisely:

> (1) Political studies: analyses of national or international political interrelationships, looking toward the qualitative elucidation of counterinsurgency problems.
>
> (2) Operations research, systems analysis, and economics: operations-research or systems-evaluation studies related to specific operational problems in Vietnam or elsewhere; generally, but not necessarily military oriented; and possibly including problems of tactics and doctrine, but not selection or evaluation of hardware equipment or systems.
>
> (3) Persuasion and motivation, psychological operations: studies of attitudes; social, economic and political behavior; motivation of individuals and groups; interpersonal and intergroup relationships and responses to various stimuli in such relationships.
>
> (4) Manpower, training and selection research: research into requirements for training programs; preparation of standards and tests for personnel selection; and development of criteria for measuring the effectiveness of selection and training procedures.

The problem relevance, which had earlier been dealt with but not resolved, was explored much more deeply. Still another group of research experts* was convened and asked to review the ongoing and proposed DOD studies, expressing their opinions as to their relevancy to counterinsurgency in two categories: *directly relevant* studies and *supporting studies*, which were not specifically relevant to counterinsurgency but would give useful information concerning methodology or general behavioral processes. Thus, while the problem of relevancy was always a troublesome one, there came to be some expert judgment about the applicability and utility of individual projects from outside the operational community that was responsible for undertaking the studies. Detailed information on who sponsored work within the DOD, the kind of work sponsored, contracting patterns, and the research organizations was compiled, and the dollar allocations within the program were listed according to subject, sponsor, country of interest, organization performing the work, etc. For the first time, there was a clear view of the DOD's efforts and immediate plans in this area. Some of the results are shown in tables 3, 4, and 5, which follow. Note that the plans for fiscal year (FY) 1965, even at this time, included higher expenditures than FY 1964, partly as a result of the services' responses to the urgings of the Smithsonian reports and partly as a result of the work of the ad hoc DOD committee that I had convened prior to the Defense Science Board panel's study.

The DSB panel report was positive and reflected the acceptance and elaboration of previous ideas on what was needed and how it should be obtained. Further, there was some very specific advice on where to go next. The assignment to study "small wars among the peoples of Southeast Asia" had been stretched considerably, but no one objected. After reviewing the post-World War II world situation, the report stated:

> This world situation has added an essentially new dimension to the responsibilities and requirements of the Department of Defense (DOD). In addition to dealing with the problems associated with the confrontation of nuclear powers in this space age, the Department must now assess the potentialities for internal conflict and subversive revolution in underdeveloped countries and take appropriate steps to help prevent insurgent movements in those areas from growing into communist dominated governments hostile to the United States. Obviously, this broad mis-

* Joseph E. Barmack, City University of New York; Alex Bavelas, Stanford; Launor Carter, Systems Developent Corporation; Max Milliken, MIT; and Jesse Orlensky and Alfred Blumstein of IDA.

Table 3 • FY 1964 program distribution and planned increases for the following year.

	FY 1964			FY 1965			Change		
	Direct	Support	Total (thousands of dollars)	Direct	Support	Total	Direct	Support (percent)	Total
By DOD component									
Army	2,305	1,890	4,204	3,106	1,798	4,904	35	-5	17
Navy	280	280	560	560	313	873	100	12	56
Air Force	393	209	602	724	382	1,106	84	83	84
ARPA	2,200	437	2,637	3,477	437	3,914	58	0	48
TOTAL	5,178	2,825	8,003	7,867	2,930	10,797	52	4	35
By area of work									
Political studies	562	145	707	813	195	1,008	45	34	43
Operations research, systems analysis, economics	1,841	1,788	3,629	3,301	1,869	5,170	79	6	5
By technical area									
Persuasion motivation, and psychological operations	1,617	574	2,191	2,151	649	2,800	33	13	28
Manpower, training, and selection research	1,158	318	1,476	1,602	217	1,819	38	-32	23
TOTAL	5,178	2,825	8,003	7,867	2,930	10,797	52	4	35

sion must be carried out within the general framework of the U.S. foreign policy, which is established by the president and implemented primarily through the Department of State.

In recognition of the growing seriousness of the internal-war problem in developing countries, in August 1962, overlapping operational responsibilities were assigned to the government departments concerned. Thus, it is required that DOD activities be closely coordinated with those of the State Department, espe-

Table 4 • Average size of studies.

	FY 1964		FY 1965	
Contractor	Number of Studies	Average Number of People	Number of Studies	Average Number of People
Universities	30	1	34	1
Nonprofits:				
SORO	20	1	22	1.5
RAC	15	3	15	3+
RAND	7	1+	4	2.5
HumRRO	13	1.5	13	1.5
Other	7	1.5	6	2+
Inhouse government	4	2.5	9	2
Industry	5	2	4	3
Foreign universities	1	1	1	1
Unknown			6	3
TOTAL	102	1.5	114	2

cially with programs of the Agency for International Development (AID). With regard to the collection, evaluation and dissemination of information, the DOD shares the mission with the State Department's regular foreign-service agencies and also with the U.S. Information Agency and the Central Intelligence Agency. Effective interaction among all these agencies is essential to the successful conduct of this country's foreign relations—in the broadest sense of that term.

It is significant that the Department of Defense is the only agency assigned explicit responsibility for carrying out research and development in support of the internal defense of developing countries friendly to the United States. This does not mean, of course, that the other agencies are prohibited from doing research bearing upon this general problem. AID, for example, has research funds and is planning a considerable expansion of its research effort; but its studies would not be concentrated upon so-

Table 5 • Distribution of funds, by country studied.

	FY 1964				FY 1965			
Country	Direct	Support	Total	Direct (percent)	Direct	Support	Total	Direct (percent)
South Vietnam	1,211		1,211	24	1,571		1,571	20
Southeast Asia	388		388	8	617		617	8
Asia	25	15	40	0.5	66	16	82	0.9
Latin America	417		417	8	685		685	9
Africa								
Middle East	80	156	236	2	30	181	211	0.4
Multiple countries	1,716	256	1,972	34	2,305	180	2,485	30
Overseas, not specified	399	91	490	8	1,068	98	1,166	14
USSR and Communist China	160	260	420	3	160	275	435	2
Europe		105	105			110	110	
United States		544	544			544	544	
Not country-oriented	642	1,398	2,040	13	679	1,446	2,125	9
Unknown	140		140	3	686	80	766	9
TOTAL	5,178	2,825	8,003		7,867	2,930	10,797	

> cial conflict and insurgency. An adequate research program with this focus would have to be conducted mainly by the Department of Defense.

The ongoing DOD programs were evaluated and criticized quite severely:

> (1) The overall level of effort is seriously inadequate to meet the DOD's need for knowledge about incipient and active insurgency in critical areas of Africa, Asia, and Latin America. The small amount of work being done overseas is oriented toward Southeast Asia, chiefly South Vietnam. There is little research regarding Latin America and other parts of Asia, and there is none on Africa. Accentuating the meagerness of this Defense effort is the

> dearth of overseas research in the behavioral sciences by other organizations. University research, for instance, by no means fills the gap.
>
> (2) Specifically, there is a great need for up-to-date basic information about the major cultural and political groupings within the developing countries that are of special significance to the United States. The ethnic, religious, economic, and political conditions conducive to social conflict can be adequately understood only through intensive, systematic research performed within each country . . .
>
> (3) Since most of the current research consists of comparatively small, unrelated projects, the overall program needs better focus, greater continuity, and a more systematic structure in all its aspects—including basic research on relevant behavioral and social processes, applied research on the phenomenology of insurgency and counterinsurgency, policy studies, and an evaluation of the effectiveness of counterinsurgency operations.
>
> (4) Most of the programs are of an unidisciplinary nature, although an adequate attach upon the problems of social conflict and insurgency would involve several different disciplines. The major programs, at least, should be multi-disciplinary—involving not only different kinds of behavioral scientists but also specialists from the information sciences and operations research.

Specific areas of work were recommended in some detail:

> 1. **Political Studies**
>
> . . . Over the range of research in this area, from policy studies dealing with the effects of alternative governmental actions to the examination of the characteristics of insurgent populations, there is an urgent need for substantive information to improve the planning and execution of policy. Political action must be planned, coordinated, and integrated with military action; otherwise, there is a risk that counterinsurgency operations . . . will fail.
>
> Research on fundamental sources of stability and instability in emerging nations, as well as more nearly applied research on the characteristics of specific political systems, is desperately needed. . . . In a case of current importance in Southeast Asia, we appear to be in deep trouble partly because there has been no systematic political research in Vietnam.

> In politics of revolution, overt political behavior is drastically different from that in stable democratic or totalitarian systems. . . . Experience gained in a more stable political environment is probably not directly applicable. Suitably posed research questions, on the other hand, can lead to the successful development of improved descriptive and analytical information for operational problems . . .
>
> Systematic research is needed, not only on the internal politics of revolutionary political behavior but on the international analogue . . . the conflict between the Soviet Union and Communist China may well have important implications regarding future Communist support for wars of national liberation . . . there is a need to explore the political alternatives open to the United States regarding international intervention of a different type than was used in Vietnam, along with implications of the possible success of national liberation movements in other areas with respect to U.S. policy.

Examples of the types of studies suggested are as follows:

- Exacerbation of insurgency by Communist countries
- Vulnerabilities of political systems to infiltration and exploitation
- Characteristics of insurgent populations
- Types of insurgency and internal violence
- Impact of civic action on local politics
- Impact of large-scale foreign support of indigenous governments on the structure of local support
- Differences between urban and rural insurgency
- Political implications of military counterinsurgency operations
- Political role of indigenous military institutions
- Political effects of alternative government policies
- U.S. foreign-policy equivalents to communist national liberation movements
- Political indoctrination and organization in Viet Cong-controlled villages

2. **Operations Research, System Analysis, and Economics**

. . . Since extensive commitments in the field of counterinsurgency are relatively recent undertakings by the Department of Defense, not much prior work is directly applicable. Little of the research reported reflects a systematic program in operations research related to counterinsurgency. A more serious deficiency is that inadequate consideration is given to variables of behavior . . . the character of counterinsurgency operations is such that behavioral aspects are often critical . . . behavioral variables can be used to a greater extent in operations research and systems analysis.

In addition to the economic analysis of production and distribution systems under conditions of insurgency, research on the operational and economic consequences of civic-action programs is needed. . . . We need to devise basic strategies concerning the application of civic-action resources with a view to . . . desired effects and the relationship . . . to long-term programs of political and economic development.

. . . it is necessary to study paramilitary forces supporting the internal-security mission of the regular military services. Most of the pertinent research done so far has emphasized the regular military mission, to the exclusion of the parts played by the police and the militia . . .

The following list illustrates the types of research problems that the subcommittee believes are appropriate topics for systematic operations research:

- Information requirements for counterinsurgency planning
- Modeling of this counterinsurgency environment
- Analysis of indicators of potential insurgency
- Analysis of behavior as an indicator of attitude
- Use of Vietnam data base for indication of progress in the war
- Studies of food-distribution system in Vietnam and how it feeds the Viet Cong (or other insurgents elsewhere)
- Analysis of Viet Cong logistics
- Analysis of Viet Cong tactics and operations

- Research on the telecommunications in counterinsurgency environment
- Research on the economic infrastructure of village environments

3. **Persuasion and Motivation, Psychological Operations**

. . . The direct measure of success is the extent to which the control of people, not the destruction of armies, is achieved. With this object in view, the motivation aspects of the conflict became far more important in counterinsurgency than conventional warfare.

In the existing programs, it is apparent that there is little systematic research in (1) the sources of political power in rural environments, (2) the types of motivation for granting or withholding political support, (3) traditional patterns of communications in general, and (4) the sources of, and reasons for, behavior destructive of the existing society. This information might be obvious in a totally repressive society, but in the mixed milieu of a typical underdeveloped country it is not so easy to determine . . .

. . . There is an important need for quantitative data on the anthropological, sociological, political, and psychological aspects of societies in which insurgency is a threat. In Vietnam, we now appear to be in a difficult situation because our decision to commit ourselves to a certain kind of counterinsurgency action was based on insufficient knowledge of the sort of people we were dealing with and the way they might react to our efforts and those of their own internal groups.

The following list represents only a small proportion of the studies that would compose an adequate research program:

- Dynamics of village counterinsurgency: bases of the village support for one side or the other
- Viet Cong motivation
- Use of traditional religious beliefs by counterinsurgents
- Systematic study of elite and mass beliefs, behavior, and interactions
- Potential sources of support for insurgency
- Outlets for aggression and antisocial behavior

- Responses of the passive villager to insurgent terror
- Patterns of insurgent recruitment
- Rural intervillage and intravillage communication and influence patterns
- Influence of traditional elite-peasant relationships on feasibility of successful rural counterinsurgency

4. **Selection and Training of U.S. Advisors**

Until very recently, the primary emphasis of social science research on our military operations in developing nations has been on the indigenous peoples, despite the fact that an interaction between the indigenous population and the U.S. military personnel is involved and the success or failure of this interaction may facilitate or prevent the attainment of U.S. objectives. It seems probable that we have concerned ourselves least with the side of this interaction we should know most about—that the performance of many advisors has been dramatically unsuccessful.

. . . Psychology has contributed greatly to the selection and training of many kinds of people in widely varying jobs. There is every reason to expect that the use of psychological research techniques would considerably improve the military advisor's effectiveness.

. . . In brief, it will be necessary to:

(1) Study the relationships of U.S. and indigenous people and the factors that may increase or lessen the effectiveness of cross-cultural interchange. This may be done through studies in the field, the systematic debriefing of returned advisors, and the study of research findings already in existence.

(2) Define the objectives of military-advisor system, and set up methods for determining the degree to which these objectives are being achieved in specific situations.

(3) Develop proficiency measures that may be applied to military advisors who are still in the field or have recently left it. Then combine these measures to serve as criteria in evaluating various selection instruments and alternative training procedures.

It should be noted that these tasks imply a need for closely and continuously observing military advisors in the field—in this case, *not* for the purpose of rating individual performance. . . .

> Opposition to allowing this observation will be great,* particularly on the part of higher-level personnel in foreign areas who tend to perceive such evaluations as invidious or threatening.
>
> . . . research that is not based on (realistic performance) criteria may actually result in selection and training procedures worse than those that have grown up informally.
>
> As soon as some reasonably reliable criteria are available, work should begin to develop a procedure for selecting individuals who are likely to perform well and rejecting those whose chances of performing successfully are low . . . **
>
> It is unfortunately true that procedures for training military personnel who must develop relationships with foreign nationals have been constructed in an informal, hit-or-miss, inconsistent manner . . .
>
> It is generally assumed, for example, that the highest priority should be given to teaching the potential advisor the language of the country in which he will serve. This assumption may be correct, but it is possible that a man who has only the rudiments of the language but possesses other useful knowledge, skills, or attitudes might, in fact, be more effective than someone who can fluently reveal his inadequacies or prejudices . . .
>
> Finally, there is little point in having effective selection and training procedures if they are inappropriate to the personnel-assignment system or if they are emasculated by a bureaucratic system. For this reason, an immediate and continuing operational analysis of the system by which personnel are assigned to counterinsurgency activities is required . . .

In addition to its specific research recommendations, the report took account of some other special areas of concern. First, the suggestion was made that the "soft" sciences be "hardened" by combining operations research and social science research techniques. Particular study areas where it appeared that such combination could be useful included the development of indicators of potential insurgency, indicators of progress in counterinsurgency, and, particularly in the last case, improving the operational database being built in Vietnam (work in this area was later to lead to the

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* As, indeed, it was later found to be.

** Note the intended extension to this area of performance of a technique that was eminently successful in connection with hardware (pp. 23–27).

Hamlet Evaluation System, which became, for good or ill, one of the foundations of the government's assessment of progress in war). The report recommended that in areas of active or threatened insurgency the military commands organize operations research units with behavioral scientists as participants to help in analysis of ongoing operations. (There was a precedent for this in earlier British operations in Malaya.) It also pointed to the Army's Camelot Project, which had started some time before the report was written, as an example of research in which systems research techniques could be applied. It noted also that ARPA was thinking tentatively about a feasibility study to find whether it might be possible to obtain sufficient data on a social system to be able to describe it quantitatively and simulate its behavior on a computer. (This program never started, having come to a halt when Licklider left the Pentagon in 1965.)

Secondly, the report recognized that few people were appropriately trained to perform competent research in the area of counterinsurgency. It recommended that the underlying foundation of behavioral sciences research personnel be built up through support of multidisciplinary centers for basic research in selected universities. These centers, of which perhaps five were visualized, would have both an "area" focus (e.g., Asia, Africa, Latin America) and a "comparative international" orientation (e.g., the world Communist movement and its methods in developing countries, and comparative studies of stability and internal conflict in developing countries). Specific criteria to make such centers as effective as possible were prescribed. These included commitment to a college "degree program"; avoidance of the usual loose conglomeration of separate departmental efforts under a "study center" label; availability of interested and suitably oriented faculty; and willingness to base the program primarily on field research.

Thirdly, the report noted that two specially oriented programs merited particular attention and support. The first was that of SORO, which was viewed as the "principal large-scale effort supported by the Department of Defense in the field reviewed." The second was Project Agile, which was recognized as the DOD's only program specifically and wholly oriented toward the problems of counterinsurgency. With respect to Agile, the report had this to say:

> These small-scale AGILE projects have been essentially exploratory in nature; for example, studies have been made of advisor-counterpart relations, mainly interviews with U.S. advisors; urban insurgency; and the anthropology of the Montagnards,

> tribal groups in Vietnam. They have yielded much useful information and opened up promising areas for investigation, but, with regard to the solution of these important, complex problems, they have barely scratched the surface.

Regarding SORO, the report said:

> It is recommended that the Department of the Army continue its strong support of the Special Operations Research Office: and that SORO be encouraged in its effort to shift research emphasis from small, library-based projects to more comprehensive programs of empirical research conducted, at least in part, in overseas locations. It should be recognized, however, that an organization whose capability was developed for the first type of work will not necessarily be adequate for the broader research. SORO's staff as a whole is seriously deficient with respect to mathematical-statistical capability, and it includes no professional economists . . .
>
> Assuming that SORO can expand and strengthen its staff in the directions indicated, it should probably be recognized as the principal DOD agency for behavioral science research directed specifically toward the Defense mission of counterinsurgency and special warfare. Furthermore, SORO should probably become the primary point of focus within the DOD for studies of socioeconomic and political conditions in the developing countries. By its contract, SORO should be permitted to conduct a reasonable amount of basic research related to its area of cognizance (e.g., in the form of "institutional" funds) without having to secure specific Army approval of each project. More generally, with regard to terms of reference and administration, SORO's contract should be appropriately broadened so as to allow the kind of flexibility in planning and conducting research that seems essential to ensure productivity.

Thus the DSB panel entered directly into the conflict that had been going on within the Army regarding whether SORO should undertake research or continue to provide the library-search-service kind of activity that DCSOPS felt was necessary to support its training and orientation programs. The panel recognized the essential weakness of this key link in the entire research program that it was proposing and made sensible recommendations for strengthening it; these, as will be seen, could not be carried out in time to avert disaster. And it subtly exacerbated the conflict between the Army and the ARPA views of the role each should play. But the recommendations that had emerged from the internal and interacting dy-

Table 6 • Summary of impetus toward increased social research by the DOD on problems of counterinsurgency.

Study or planning effort	Date	Auspices
First Smithsonian report	1960	DOD (Office of Naval Research) funding to Smithsonian Institution
Limited War Task Group	1961	DDR&E, "inhouse"
Second Smithsonian report	1963	DOD (ONR) funding to Smithsonian Institution
Defense Science Board	1964	DDR&E
Internal DOD programming efforts through		
Modest budget increases and "getting organized"	1962–64	Self-generated by services with guidance from ODDR&E
Ad hoc Committee on Counterinsurgency Research	1964–65	DDR&E, Special Assistant (CI)
Increased budget proposals	1964–65	Self-generated by services and ARPA
Ad hoc interdepartmental coordinating committee	1964–65	Initiated by DDR&E, Special Assistant (CI)

namics of both the DDR&E "shop" and the panel's study efforts were viewed in ODDR&E as exactly the right prescription for the further development of the program.

The Defense Science Board effort consolidated the results of all the previous studies and planning efforts that had recommended and started to implement an expansion of social research to help the United States in its growing struggle against the "war of national liberation" strategy of the Soviet Union and Communist China (table 6). It critiqued the ongoing DOD program, isolating inadequacies and opportunities. It surveyed the capability available, what it could do and what it needed to be made to do, and it made very specific recommendations regarding subjects and areas for study, who should undertake the work, and how they should go about doing a creditable job of it. The report accepted the idea that the Defense Department needed and had the mission to do the work, and that it would do the work because it had the dollars to do so; the change in values that would

subject the whole premise to question had not come about yet. In general, this panel made an effort to relate its recommendations to the DOD mission, and it noted the need for coordination with and dissemination of results to other departments of government who were concerned with the same problems. This report was thus the culmination of several years' evolution of a set of ideas. It focused them and turned vague and shifting conceptualizations into concrete program ideas that could be funded.

Chapter 8
The Wheels Start Turning

The DSB report was published and distributed within the DOD and the other departments on January 20, 1965. While the panel was doing its work, the normal cycle of program review, preparation of budgets, and planning ahead for new programs was continuing with its inexorable momentum. In order to incorporate the new program in the budget for FY 1966 (July 1, 1965 to June 30, 1966), we had to instruct the services regarding what was needed, receive their initial proposals, negotiate differences of opinion and conception, incorporate the results in the draft of the budget and the written justification for the budget requests, re-coordinate the draft, and send a final draft of the "presidential budget" to the secretary of defense by the end of November 1964. Then, he would review it as necessary with the offices of the comptroller and the systems analysis group (the latter, who originally earned the name "whiz kids" when Secretary McNamara established the group in 1961, played no role in the events described here; the money involved was too small for their notice, although several years later, in 1967, they became involved in analyzing "progress data" from Vietnam). The comptroller and the systems analysis group would have been working to clarify various major issues for decision throughout the year; the secretary would make decisions involving budget issues; he would iron out any remaining differences with the services and DDR&E (who could appeal, or "reclama," an initial decision); prepare his "posture statement" (which had also been "in work" for some months); and send the budget to the White House in time for review and incorporation of major new ideas (this one was not "big" enough to receive such separate attention) in the State of the Union message on January 1.

It sounds slow, involved, and tedious, and it was. If we wanted to increase the social research program associated with counterinsurgency by additional funds to be expended during the following July 1, 1965, we had to start thinking in August 1964—or a year earlier—about how much money we wanted, for what it was to be spent, and how it would be distributed among the services and ARPA. The time lag until there were actually people working on the problems with the new funds might be as much as two years, since once money was made available it might take most of FY 1966 to define individual projects precisely enough to specify contract work statements and go through the red tape of soliciting proposals, eval-

uating them, and negotiating and signing contracts. The bureaucratic process thus required that, if we wanted to see any new effort at all before July 1966, we had to start in the middle of 1964 to pave the way for it. Some small starts could be made earlier through modest "reprogramming," but it would be difficult to rob Peter to pay Paul when both were already doing much of what was wanted but had insufficient funds in the first place.

The expanded DOD program was intended to rely heavily on the DSB report. The DSB panel, which had started its periodic meetings in April 1964, was planning to complete its work and write its report in the fall and winter of 1964–65. Since we in ODDR&E had to start working on the budgeting problem in August 1964, we were in the position, not for the first or last time in government program planning, of having to start a program before we had done our homework, and with ideas only half formulated.

But this was not as bad as it sounds. By late summer 1964, the DSB panel, and therefore Rains Wallace and I, had a fairly clear view of what was in the existing programs and the shape of the recommendations that would be made, although the details and specific issues remained to be worked out. In August 1964, the preparation of an "interim" guidance memorandum from DDR&E to the service assistant secretaries for R&D and the director of ARPA was initiated. This became the first formal document relating to the expanding program and was issued on September 2, 1964. The "covering brief" transmitting the memorandum to DDR&E for signature explained its purpose and summarized its contents: provision of guidance to the services and ARPA; affirmation of the Defense Department's mission for such work; asking that the military departments and ARPA make a coordinated review of their plans and programs with a view toward expanding the effort; asking that appropriate resources be provided in the respective FY 1966 budgets; and indicating that more specific and detailed guidance would be forthcoming upon completion of the DSB study. The services and ARPA were asked to submit their preliminary plans by October 15, 1964.

The pattern of responses differed only in program details from the more definite plans that were made after the DSB report was published, and it followed the positions that began to emerge in the early deliberations of the ad hoc DOD committee. The Air Force proposed to almost double its relatively small program of research at universities. Since their budget for such work had in the past been consistently reduced from the AFOSR requests

by the upper echelons of the Air Force, this amounted essentially to the very modest expansion from the work they were actually trying to undertake in any case. The Navy was cautious, expressing the opinion that with some very small expansion they were doing about the right amount and kind of work. ARPA proposed to expand behavioral sciences research, including a separate counterinsurgency-oriented behavioral research program in Project Agile, from about three-quarters of a million dollars per year to $5 million over the following five years. This included a substantial ARPA contribution to the Counterinsurgency Information and Analysis Center at SORO (on which more will be said shortly), which would absorb about three-quarters of a million of this budget. ARPA's proposal included a combination of individual research projects; regional research enters based on universities overseas to undertake research in those areas with locally trained social scientists; and the attempt to assess the feasibility of computer simulation of behavioral patterns for predictive purposes.*

ARPA anticipated major problems in the latter program arising from the scarcity of reliable data and from differences between the value systems of the builders and interpreters of the computer programs and results and the groups being simulated. More generally, they anticipated that it would be difficult to identify individuals both qualified and willing to do original fieldwork. The host nations would be sensitive about such research and would have to agree to it before any could be done. They foresaw administrative, security, and language barriers to conducting research in such areas as Vietnam. In all this ARPA was prescient; their reservations were not unfounded. However, the reservations were presented in terms of problems to be solved, rather than reasons not to proceed.

The Army proposed roughly a $2-million-per-year increase in their programs for FY 1965 and FY 1966, but noted that it would be very difficult to increase expenditures at a rapid rate, so that the expansion might be delayed. They proposed a detailed research plan which included what appeared to be carefully thought out programs in six areas: research studies of counterinsurgency policy and planning; research on requirements for social science information and area studies; research on military psychological operations and the information and communication process within

* It has come to be both unfashionable and even gauche to think about computer simulation of something so subtle, delicate, and complex as a society. The reader must remind himself that consideration was not being given here to *doing* it, but to exploring whether it should even be thought about seriously. In another context, society accepts simulation of urban evolution (by Forrester of MIT)[1] with equanimity and even interest, even though this is at least as difficult and uncertain as what we were considering.

developing nations; research on the design and impact of civic action and military assistance efforts for counterinsurgency; research on relationships between Americans and local officials and populations in military operations in developing nations; and "counterinsurgency single-country studies." They expected their total program to reach a level of about $5 million per year in about three years. Other than their concern about the rate of expansion, they expressed no reservations.

By the beginning of 1965, the DSB report was near completion, and we started to work on the draft-coordination-redraft cycle of the final guidance memorandum that would launch the expanded program. This memorandum was issued by the DDR&E on March 1965. It tried to deal with all of the problems highlighted by the DSB report and followed closely from the recommendations in that report. The general nature of the work desired was reviewed. It was noted that areas, such as Africa and Latin America, about which there appeared currently to be little knowledge of the kind needed, and virtually no research, required further attention in the DOD's study programs. Problems associated with military-civic action, internal security, and constabulary operations (all of these in countries receiving U.S. military and security assistance) were identified as areas virtually neglected in the existing research programs.

Attention was given to the problem of research quality. It was pointed out that behavioral and social science research applied to Defense's problems other than personnel management and human factors engineering were still viewed with suspicion and had yet to prove their worth conclusively. There followed recognition that the creation of high competence on the scale demanded would have to be a long-term proposition, with an admonition not to support work of poor quality with unqualified researchers solely for the purpose of expansion. For specific program guidance, the DSB report was transmitted to the Services and ARPA. The Navy and Air Force were instructed to elaborate the individual university and contract research studies that were characteristics of their current research efforts. ARPA was assigned the responsibility of creating a university-center program having the general characteristics and objectives described in the DSB report. The work it had proposed to undertake through Project Agile was encouraged.

The Army was singled out for major responsibility. This followed from the nature of the Army's program and from the magnitude of its responsibilities overseas. The Army had been responsible for much of the applied behavioral sciences work that had to do with troop performance in the field

and troop training and proficiency. Of all the Services, the Army was most heavily engaged in Vietnam (at that point, with Special Forces and many thousand additional military advisors) and had the most intimate contact with Vietnamese forces as well as with the Vietnamese people in the countryside. Major responsibilities for advisory assistance in the use and maintenance of equipment; for training large numbers of troops; and for counterinsurgency oriented programs, such as psychological operations and military-civic action, as well as counter-guerrilla warfare, fell on the Army's shoulders. The problem of transforming research results into policy, training programs and curricula, and doctrine to which the Services adhered was always a difficult one, and to the Army as a user would be in a far better position to do this than ARPA, which had no "troops in the field" and was viewed with suspicion by those who did. It was therefore logical that the Army should be assigned the responsibility by DDR&E, who supervised all DOD research under the law, for a major applied research effort in the counterinsurgency area.

From the research plan that they had proposed the previous fall, the inference that they understood the problems and could undertake the work seemed reasonable. They were directed to establish in the Washington, DC area, a "centrally coordinated applied research effort" that would build on their five-year program plan. They were specifically assigned responsibility for the major aspects of research on selection and training problems and those associated with "special warfare." Their responsibility included the establishment of a coordinating office, which would not strictly supervise but would help keep related to each other all of the other service and ARPA programs. In addition, the guidance memorandum pointed out that coordination would be necessary with the State Department, AID, USIA, and CIA. The Army was instructed to invite representatives of those agencies to work in residence with the headquarters of their effort and to work with participating government organizations and research contractors. The general funding levels that had been proposed in the response to the earlier guidance were accepted, and the services and ARPA were instructed to adjust their FY 1966 budgets accordingly.

As I worked closely with ARPA and the Services to try to ensure a coherent program whose parts were properly interlocked and coordinated, it became apparent that ARPA, which had planned expansion of the work under Project Agile, and the Army, which was moving forward with its own expansion plans revolving initially around Project Camelot, were coming

into conflict. In response to the March 24 memorandum, the Army had proposed to assume the total responsibility for at least the applied research part of the overall program, and this had been granted by DDR&E. ARPA was unhappy that the responsibility for coordination was assigned to the Army, because it appeared to them that this infringed on ARPA's independence as an agency reporting directly to DDR&E; and they feared it would retard the expansion of Agile's work in Southeast Asia, which was already underway, while they might have to wait for the Army to come up to speed. The proposed Agile work was not in conflict with what the Army was planning from the duplication point of view, but it was clear that the Army and ARPA, if they both expanded rapidly and simultaneously, would compete for the same limited talent, and this would be no help at all to an orderly expansion of the program. It became apparent, therefore, that further guidance was necessary to try to resolve the conflict.

We built on a proposal that had been made by the Army in response to the March 24 memorandum by preparing another much more specific instruction to which ARPA and the Army agreed. The Army would create a new office reporting directly to the Director of Army Research (a "short-circuit" of the bureaucratic system not often used in the service bureaucracy). It would be staffed jointly by representatives from all the Services and ARPA, as well as representatives of the outside agencies having an interest in the work. It would be responsible for direct supervision of the Army's work, but only for coordination of the efforts of other agencies. It would also be the responsible point of contact with overseas commands having to clear the work a priori and for translating research results into language and plans appropriate for the "user" community. This joint office, which would in fact be little more than a coordinating committee chaired by the Army, would be responsible for future planning of research efforts and, through recommendations to its constituent agencies, helping to ensure that a coherent and useful program was undertaken. It was explicitly stated that, while the military departments and ARPA might continue to undertake individual applied research efforts, these would be coordinated within the joint organization and would therefore become an integral part of planning done by that body. At the same time that this memorandum was prepared, letters were drafted to the secretary of State, the administrator of AID, the director of the USIA, and the director of the CIA, explaining what was being planned and soliciting their support. The "package" was to go out formally in June, but this progress was interrupted by the explosion over Project Camelot.

Chapter 9
The Army Moves Out Smartly

At this point, we had entered the indeterminate region between planning and implementation. At some time in any effort, such as we are describing, some of those who have been involved in the preparation of plans, being aware that they will have implementation responsibility, shift their efforts gradually from planning to action. Events develop slowly, even under pressure, so that the exact point in which the boundary between talk and action has been crossed is difficult to define, although it can often be identified in retrospect. In this case, the shift was in two stages: from relatively generalized DOD planning involving all the relevant agencies to specific plans made by one of the action agencies—the Army—for its own efforts; and thence to action by the Army, undertaken before either its own or the DOD's plans had been fully formulated.

This transition took place in a complex bureaucratic environment with developing crosscurrents of interrelationship and conflict among parts of disparate agencies, and between some of those agency "offices" and the individuals who served them under contract or as advisors. The external environment was also in a dynamic state, with the increasing war in Southeast Asia lending to a sense of urgency while associated tensions at home and abroad began to intensify. Let us recapitulate, briefly, before proceeding.

The DOD offered the essential lure, in effect a pot of gold in the form of resources for potential support of social research in connection with Vietnam and other "wars of national liberation" and it lent to the attraction of the resources a sense of mission reinforced by the president and many of those he had brought into his administration—in the White House, the Defense Department, and elsewhere. A number of influential members of the social science community, who were aware of the past successes with DOD-supported research in the behavioral sciences, encouraged the DOD to believe that the kind of research they advocated could be equally useful in the world of international affairs—a world partly outside the DOD, but connected with its assigned tasks. Those tasks, in Southeast Asia, were related to an unfamiliar kind of war, of which the only apparently "known" quality was that it posed a danger to the United States because it was being encouraged and supported by nations we considered our adversaries. Members of the DOD hierarchy, civilian and military, who exerted influence and control over the available resources accepted the advice of the social scien-

tists. They believed it had merit since they perceived that much of the problem of Vietnam and similar conflicts lay in understanding societies, cultures, and relationships among people, rather than in improving weapons and military equipment (or, at least, in addition to those more familiar activities).

The president had assigned responsibilities for counterinsurgency operations overseas to several agencies. These were overlapping responsibilities, presenting opportunities for both productive cooperation among the agencies and conflict between them. The DOD, having become accustomed as a general matter to undertaking research and development to solve its problems and to expanding the relevant R&D when the problems became pressing, turned its attention with the same reflex to the specific problems developing in Vietnam and Southeast Asia. In this, it was encouraged by the presidential assignment of responsibilities, which included "relevant research and development." The other agencies, some without research resources of their own, reacted positively to the invitation to participate with the DOD, but with no resources and, in the State Department's case, with reservations. State met the DOD's offer of research support and cooperation with overt cordiality but, it turned out later, deep misgivings as well as an apparently irresistible bureaucratic interest in the opportunity to expand its span of control in the research field. The response seemed to reflect State Department attitudes that included reluctance to accept the character of the Vietnam conflict as it was perceived by the DOD, and fear of studies that were not performed by their own personnel, because such studies were sensitive. While State had virtually no resources to support contract research, they had a substantial inhouse study staff. However, the linkage with DOD was left to the part of the Bureau of Intelligence and Research that was responsible for spending State's minuscule contract research resources rather than the part that did the substantive research.

Within the DOD, the situation was similarly mixed. Early on, there had been no especially strong support for social research beyond that essential to improve the functioning of the Services' men and equipment. Now, the horizon was being expanded, and this expansion, pressed by a few individuals on his staff, was being supported by the third-ranking official under the secretary of Defense. In the Navy and the Air Force, those responsible for such research were cautious and saw mainly the opportunity to expand the work of individual scholars that they supported in several universities. Wanting a more massive and centrally coordinated effort with

greater orientation to operational problems rather than underlying social and political phenomena, the ODDR&E staff turned to the Army and ARPA. Both responded positively, but they came into conflict with each other and with ODDR&E as a result of the Army's ambition and ARPA's desire to preserve the independence of its control over the work it funded. An uneasy compromise was worked out, and it was also decided that the interested agencies outside DOD would be invited to join in the coordination and planning. Thus, any coordinating group, when it was established, would be composed of individuals having mixed motives and organizational loyalties and having diverse understandings and attitudes about research and the real-world problems that were to be the subjects of research.

Those problems, during March–May 1965, were becoming more urgent. The United States was bombing North Vietnam and Marines were landing in Danang, South Vietnam. The United States had rejected U Thant's offer to help make peace. The antiwar movement in the United States was becoming more vocal; "teach-ins" and demonstrations centered in and around the universities were growing in number and intensity. Many social scientists were involved and helping to organize the antiwar protests, and their numbers would increase. The war protest movement was strong overseas, as well, exacerbating sensitivity to "American foreign intervention"; and there was tension in Latin America over America's counterinsurgency policies as reflected in Vietnam, and over America's expressed perception of rising Communist revolutionary agitation in Latin America (which was believed by many in the United States government to originate in Cuba).

Into this maelstrom, the Army's early start toward implementing the new DOD social research polices gradually began to intrude.

About August 1964, the Army had started to plan a large expansion of its counterinsurgency-related study effort. Its nature was described in the research plan submitted in response to the September 2 guidance memorandum from DDR&E. Initiating some aspects of the plan, the Army joined with ARPA to establish a Counterinsurgency Information and Analysis Center (CINFAC) at SORO. This center would draw together available information about tribal groups, developing societies, and social systems in various parts of the world, using library sources generally, but adding first-hand information culled as needed from consulting experts who had performed research in these areas. It started with the information base that already existed to support the preparation of SORO's Area Hand-

books and would grow from there. Any government agency that needed information about a foreign country or area in the "Third World," for purposes of training, policy making, or what-have-you, could query the center and expect, within a few days or weeks, depending on the amount and complexity of the information that had to be assembled, an answer reporting the current state of knowledge. The center was not a new idea, but rather the extension of an existing pattern to a new subject area. Other such centers operated by appropriately qualified organizations, such as the Battelle Memorial Institute, had been established by the Defense Department to make available to the scientific community the accumulated knowledge of years of research in a variety of technical fields. The pattern was part of an attempt to deal with the well-known "information explosion."

The work of the center was to include reports on such diverse subjects as the progress of economic development under the AID program in South Korea and the history of the village council in South Vietnam. Another question that the center's workers answered, which we shall discuss in due course, figured large in Senator Fulbright's later attacks on DOD social science research. Typically, CINFAC might receive 100–200 questions a month, about half of them from DOD components and the others from scattered agencies of government.

While CINFAC was being established, a document began, also August 1964, to float up into the Army "system" for approval, requesting allocation of funds to SORO for a project which was later named Camelot and which had as its objective:

> . . . to test in one country the feasibility of designing and developing, for strategic planning and other Army use, an advanced system of early warning of internal conflict or its increased likelihood in foreign nations, together with concepts for early Army reaction systems requirements.

The scope of the project was to develop the means for measuring "conflict potential," estimating "posture effects," and establishing information collection and handling systems to feed data into the first two efforts. The request for funds noted that "the study here proposed is a high risk, high pay-off feasibility study."

The genesis of the idea of measuring "internal war potential" is clear in the words of the second Smithsonian report (see chapter 2), and elaboration of those ideas continued in the Camelot documents, as will be seen.

However, this statement of a desired project seemed a far cry from the subtle kind of social research that was called for in the reports by the scientists. It illustrates well what happens when an operating organization tries to incorporate results from research into its action capabilities. It was not so much that the action part of the Army did not understand the subtleties of the research or the scholarly aspects of obtaining results; it was, rather, that generally the Army had to interpret the knowledge gained in terms of its mission, as it understood that mission.

If the policy of government, illustrated by our entry into Vietnam and by the organizational changes and mission assignments that had continued throughout the Kennedy and the beginning of the Johnson administrations, was to lend assistance to countries threatened by "wars of national liberation," and the U.S. Army was assigned a role as one of the instruments of this policy, then the Army had to glean what knowledge it could to prepare itself to carry out this mission. In fairness, it might be noted that the popular conception of the Army going off on its own to carry out nefarious foreign policy activities far beyond the scope of its military assignments, which became common after the Army was attacked for the Camelot Project, was simply false. The Army was preparing itself to carry out a mission that the president had ordered it to be ready to carry out, with the approval and knowledge of Congress. The fact that important segments of Congress later acted to change its approval does not obviate the fact that, at the time, the Army was operating within the context of national policy agreed upon between the executive and legislative branches.

However, the Army was certainly in sympathy with the assignment. The terse and insensitive military language of its request for funds reflected the beginning of this "user's" translation of the language of the scientists into the operational language necessary for its application of the research results, and the translation reflected the Army's view of what the scientists' language was all about.

The project funding request was signed by Colonel Sullivan, chief of the Human Factors and Operations Research Division of the Army Research Office, whom we met earlier. Colonel Sullivan was an infantry officer with a "can-do" outlook. In my years of working closely with him, I came to feel that he understood full well the broader sensitivities and implications of the social research that was being proposed; but he was not one given to questioning the aspects of the program that might cause difficulty. From his point of view as a soldier, if the job was assigned, he expected that there would be

problems and troubles (every assignment had them), and his overriding motivation was to overcome them in the best way he could. Because we had worked together, he had been aware of the directions being taken by DDR&E's thinking in this program area, and he had had a preview of instructions to come. He concluded that the Army should "move out smartly," without waiting for explicit direction from above. He received tentative approval from the director of Army Research, Brigadier General Walter E. Lotz (an electrical engineer) and from the chief of Army Research and Development, Lieutenant General William W. Dick Jr. By about December 1964, the project idea described in the August memorandum had evolved into Project Camelot, which was to be assigned to SORO.*

From later task statements and documents describing the evolution of the research plan, the idea that the predicting mechanisms would be developed through research in one country had developed into a much broader study plan. There would first be an attempt to gather in many countries various kinds of data describing the nature of the societies and how violence might erupt and affect social change in those societies. These data would then be used to develop a "model" of a society in conflict and to select from among the various existing but untested theories of social change those that appeared to be valid. The consultants working with SORO had catalogued from the literature about 800 hypotheses about internal war. Almost all of them appeared plausible, but many appeared to be in direct opposition to each other. For example, some economic theories stated that (a) internal wars are generated by growing poverty, and others stated that (b) internal wars result from rapid economic progress. Social theories postulated that (a) internal war is a reflection of disorder resulting from *great* social mobility, and also that (b) internal war is a reflection of frustration arising from little social mobility. Political theories said that (a) internal wars are responses to oppressive government, or that (b) internal wars are due to excessive toleration of alienated groups. It was first seen as necessary to sort out some of these theories and to decide which ones had real validity based on historical data. Possibly many did, depending on the culture and circumstances, and it would be necessary to establish these correlations empirically. Then a detailed description, or model, of social change in diverse societies would be

* SORO had the habit of naming all of its projects. This made subsequent attacks on them easier and, in the case of Camelot, added a new connotation to an old concept in the English language. In the words of Dr. Vallance, the director of SORO, later testifying before the Fascell Subcommittee: "The label 'CAMELOT' simply emerged from the basic intent of the story . . . that is the development of a stable society with domestic tranquility and peace and justice for all."[1]

constructed. From this model—and there could be more than one—the "indicators of internal conflict potential" would be developed. Finally, using the models, the events in a single country would be examined in depth to ascertain whether the predictors were, in fact, valid ones—that is, whether they would indeed predict. From here, it might be relatively straightforward to apply the verified theories to strategic planning.

To run the project, SORO hired the late Dr. Rex Hopper, an expert on Latin America from Brooklyn College, New York,* and assigned a few staff people. I met Hopper only once; he appeared to stay very much in the background. He did not, for example, testify before the Fascell Committee. Aside from Vallance and Bill Sullivan, our main contact with the project was through a young political scientist, Ted Gude, who was very bright and who appeared to do the organizing, much of the writing, briefing, and contacting of external consultants. SORO planned the work so that it would remain unclassified and could involve many members of the scholarly community. They planned to rely heavily on outside experts and enlisted many consultants who were well known in such fields as anthropology, psychology, and political science. During spring 1965, a number of these consultants formed a working group that met one day a week with the internal staff, helping to draft preliminary documents, research plans, and generally to describe what the project was supposed to do and how it would go about doing it.

SORO had planned to complete in the United States its preliminary thinking and library research on theories of conflict, revolutionary warfare, and processes of change in diverse social systems, during the winter and spring of 1964 and 1965, focusing on several societies at a time. Overseas research was planned to begin—again, largely library research—by September 1965. It was intended that such research would be undertaken almost simultaneously in some 21 countries—an expansion of overseas research of unprecedented ambition and scope. Following from SORO's previous work on case studies of revolution, they built their planned research around such conflicts and planned to examine such historical or current events as the Argentina revolution of 1943; the Venezuelan revolution of 1945; the Peruvian coup of 1963; Colombia since World War II; the Egyptian coup of 1952; the Iranian coup of 1953; the Korean revolution of

* Dr. Hopper was chairman of the Sociology Department. He had explored the sociology of social movements, particularly of Latin America, and was coauthor of *The Seizure of Power: A Century of Revolution*. He died of a heart attack not long after the Camelot events described in the following chapters.

1960; and Greece in the context of the Cold War. As rather remote possibilities that might later be found interesting, they also considered, in a very tentative way, the Algerian independence struggle, the Congo since 1960, the French-Canadian separatist movement, and several others. Chile was not mentioned as one of the possibilities, although it figured very much in the news later on. The amount of money to be spent was never very certain. Ultimately, the Army allocated about half a million dollars for the first year (only about half of which was spent), and then planned to spend about a million dollars per year for three to four years thereafter.

Despite these ambitious plans, the project never really got off the ground during fall 1964. The full project staff was never assembled or fully organized. There were many meetings, but the output consisted of generalities, never becoming specific about research hypotheses and how they would be tested; what data would be gathered; or what the social system models would consist of. That is, the "research plan," without which a study in the social sciences (or in any other scientific area) cannot be undertaken, remained vague and formless. In its report, the DSB took note of Project Camelot, saying:

> . . . Presumably, as Project CAMELOT gets under way, there will be such (i.e., systems-oriented) staff additions (at SORO) and a corresponding methodological reorientation, but this matter should be kept under continuing review by the ODDR&E.

By January 1965, Rains Wallace and I were quite concerned about, and discussed with Lyle Lainer and Harold Brown, the increasing evidence that SORO management might not be up to the broader tasks being planned for it. We considered the possibility of asking the Army to change the management of SORO, but this seemed inappropriate at the time. The evidence was still largely in the form of random observations, unformed fears, impressions, and hunches; it was insufficient to justify broaching this delicate question to the Army, and it was never raised outside the circle of those most concerned in ODDR&E. Several times later during that spring, I had talks with social scientists who were not involved in Camelot but who visited SORO and discussed the project with its staff and management. Uniformly, they reported that they believed that SORO did not know what it was doing. These inputs served to reinforce the concerns reflected in our subsequent actions.

We had several talks with Colonel Sullivan, who then put more pressure on SORO to prepare and present a firm research plan. About the time we were preparing the March 24 guidance letter (in February), we received a document from SORO entitled "Project CAMELOT: Design and Phasing." Among the most important problems the document presented were these:

- Diffuse wording, still not pointing toward specific research tasks and problems. For example,

  > Since it is not possible to specify a priori the exact form of possible models of internal conflict and internal war, it is necessary to develop a broad range of information requirements for the case studies so that many types of models can be developed and tested with the data.
  >
  > The initial models developed in the theoretical design effort will have as a primary objective the specification of the types of models that may prove valuable to the project.
  >
  > The research design for the individual analytical case studies and the individual social systems studies will be planned to include the information requirements for all of the various models.

  Clearly, the thinking was circular. Which models? What would they look like? How could they plan to get data to construct models of which they had not yet conceived? What data would they seek? Would they randomly amass data on all conceivable subjects and then try to process it? A disastrous end for such an undertaking could be readily predicted.

- This led, of course, to more specific concerns about the data problem per se. The general tone of the discussion was that they would have more data than they would know what do with; but there was no indication that they would know or try to determine a priori what data were needed, whether they might exist or be available, how they would obtained, how SORO would obtain access to sources. None of this had yet been explored after six month's "work" nor did it even appear to have been recognized as a problem.

- There was a section that talked about small group experimentation "particularly as concerned with concepts, such as cohesion, control, socialization, goal development, and motivation in

primary groups." The purpose of such experimentation, the validity of its results in terms of large social systems, or the possibility of doing it in the cultures and situations they would be concerned with, and how it would relate to the objectives or final results, were not dealt with.

- The plan still read as though the program would be done primarily by outside consultants, with little effort by the SORO inhouse staff except loose coordination. In our view, systems analysis of questions of this kind required that the technical work be tightly coordinated, carefully planned, and divided into subtasks to be performed by a cohesive, inhouse research staff. Consultants could help, but they could not do the job themselves. Thus, this was not shaping up as a coordinated program, but rather as a loose conglomeration of somewhat related studies by individual scholars who had their own diverse interests in the general area, and who were, it seemed, to do Camelot tasks when those were defined as spare-time jobs. This moved opposite to the DSB recommendation for improving social science research methodology.

- Finally, the rush to go overseas about six or eight months after publication of this "plan," in view of the state disarray of the plan, was frightening.

Wallace and I called for a meeting on the subject and met with Sullivan, Gude, and Vallance at ARO in early March. We discussed in detail the problems we saw, and Gude responded by describing the thinking of the group of consultants then meeting periodically with the staff and promised to pay more attention to the specific research questions in future writing. (They later did prepare a very detailed document of information requirements which, when it surfaced in Chile, made it look as though this were a strictly intelligence-oriented operation by the U.S. Army. The relationship between intelligence and research in this area had been recognized by the Smithsonian panel and was a troublesome one, which I will come back to in some detail much later.) Gude noted that there was to be a meeting of the key consultants and a number of distinguished scholars, forming a technical advisory group, in August. Then, the detailed and final research plan would be worked out. The staff would be on board and, he assured us, we would be satisfied with the project. After the meeting, Wallace and I decided privately that, if the August convocation did not produce these results, the project would be cancelled.

We were now in a dilemma. DDR&E was preparing to issue a guidance paper giving the Army major responsibility for the total program, and the Army capability seemed to be weak, indeed. But all of the programmatic results of the previous year's bureaucratic work had evolved in directions desired by both the DOD and many members of the social science community. The Services and ARPA, despite their diverse concerns, were responding positively, making plans and allocating budgets. At various points in previous years, the organizations directly responsible for research in the social sciences within the Services had also responded to recommendations for increasing the work by requesting the budgets, which then had been turned down at the top service levels. This was not happening now. The question was, should we at this point call a halt until the Army could solve its problems of management and research capability and risk losing credibility and the ability to restart afterward; or should we go ahead and try to fix the Army's problems as we went along? Inevitably, bureaucratic momentum won out.

Although, in retrospect, this was obviously a mistake, it seemed a reasonable risk at the time. SORO was still working inhouse and unobtrusively (we thought). Money was not being spent at a high rate because of the small staff. We had the ultimate control of the Army's budget to use as a club. Therefore, we decided to go ahead with the guidance, but we retained our private determination to have Camelot cancelled, while the Army might be allowed to go ahead with the other parts of its six-point program, if the project did not shape up in August.

After the flurry of activity involved in getting the March 24 guidance letter out, I had another inconclusive discussion with Gude and decided to put on paper what was bothering me most. Since we were still trying to get the Army turned around rather than turned off, the situation appeared to bear handling with kid gloves. Being aware of the power of an official complaint to a Service or contractor from a member of the DDR&E staff, I wrote a personal letter to Gude rather than a formal letter to Sullivan or Vallance. In it, I reviewed the situation regarding Camelot as we saw it; I pointed out the novelty and difficulty of taking a systems approach to the study of a major social problem; and I dwelt on the problem of getting the idea accepted, both by the social science community and the operational community. I noted that just doing the project could affect the social system that the project was about, if the latter were defined to include the ambassador, the military commanders, and the whole U.S. decision-making

apparatus as well as people and government in the country in question. In effect, just by the act of undertaking the research we were effecting social change, and we had to tread very gingerly. Their plans were as yet imprecise and uncertain. Therefore they should not plan to be in 21 countries overseas by September, but rather should hope to start by then to arrange to do research in one country over the winter; they could then learn to test data sources, the climate of acceptance, etc. If successful, they could plan, some months later, to start preparing the way for research in two or three additional countries. That is, the problem should be approached more slowly, and the schedule should be extended considerably. I did not know when I wrote this letter that it was already too late, because Rex Hopper had sent a personal letter to a colleague in Chile a few days earlier that would literally destroy SORO's programs and seriously undermine the remainder of the DOD effort.

In the course of my work, I had made it a practice to travel to various overseas commands, especially in Southeast Asia, to obtain a first-hand view of the problems of warfare, field operations, hardware needs, and of doing research in the field. In late April, I decided to go to the Panama Canal Zone where the headquarters of the United States Southern Command (Commander in Chief, South, or CINSCO) was based. With the growing attention to Vietnam, Latin America, which seemed vitally important in view of the conflict with Cuba and Castro's promises to stimulate revolutionary movements elsewhere, had received little attention. In the Canal Zone, the Army had a tropical research center where environmental research and equipment testing were performed to obtain data that might be applicable to the jungle environments of Southeast Asia. There were training facilities operated by CINSCO's component commands to train the Latin American military in such diverse skills as military organization and aircraft engine maintenance. ARPA and SORO both had field offices there; and there were other activities relevant to my responsibilities in ODDR&E. I would also obtain my first "official" view of the problems in that part of the world, through the eyes of the U.S. military command, and pave the way for the trips I hoped to make later to countries in Latin America. At the same time, I took the opportunity to tell General Porter, the commander in chief, about our views of the counterinsurgency R&D problems, our growing interest in related social research, our plans to increase Army responsibility in that area, and the possibility of undertaking more research of this kind in Latin America.

This was an interesting time for a visit. The government had just been overthrown in the Dominican Republic, and the situation regarding the possibility of a United States role in that area, if any, was unclear. Although General Porter was obviously busy, we had a lengthy discussion during which he stressed the delicacy of his position as overall commander of all the military advisory groups in the various Latin American countries, and his concern that the DOD not undertake any research activities in Latin America without his knowledge. I raised the problem of freedom of inquiry; would he try to control the subjects or the results of research? He said that, while he had no desire or intention to influence research results or methodology, he could not accept contacts with local government officials and populations by DOD-supported researchers unless he were able to answer any questions that might be raised by their presence, and unless he could intervene to change or delay plans for visits to Latin American countries if particularly sensitive matters or events arose. I assured him that no projects would be undertaken without his first being briefed on them, and that no researchers would be sent to any Latin American countries without a "theater clearance" from his command, as was required by the operating regulations of both the Army and ARPA. I told him the Army would come down to brief him in detail and left after several days in Panama and the Canal Zone, just as the U.S. Marines were landing at Santo Domingo.

On my return to Washington (in early May), I held another meeting with the group of research directors of various government departments. They had already received the Defense Science Board report, and I told them of the March 24 guidance letter and of the developing plan for the Army to establish a coordinating office in Washington in which they would be invited to participate. Bill Nagle of the State Department raised the question of the State's Foreign Area Research Coordinating Group, for which he was responsible. He felt that it should coordinate the planned program rather than the Army, since it was already in existence and the subject was "foreign areas." I pointed out the DOD's responsibility for supervising efforts funded out of its appropriation, and that this responsibility could not be given to another agency of government. Moreover, there were many agencies and departments represented on FAR in addition to the five most immediately concerned with the problem, who were represented at this meeting. FAR was obviously too large a forum to do more than exchange information. I promised, however, to keep FAR informed "officially," and welcomed their advice and suggestions for new research projects. FAR was, in fact, briefed a number of times on the DOD efforts.

(It was also at about this time that I wrote to State suggesting studies that we would be willing to fund under the cognizance, with results described earlier.)

The discussion was inconclusive, and I had the uneasy feeling that FAR was too tempted by the obvious opportunity for extension of its domain. But when the meeting ended, I thought that the position I had stated was understood and accepted. I also believed that our agreement was confirmed in later telephone conversations. I was therefore very much surprised when in late May I received a letter from Nagle that started "I am herewith amplifying the terms of our telephone conversation, in which I was pleased that you so readily accepted my view that any new inter-agency foreign area research mechanism should be under the Foreign Area Research Coordination Group . . . " and then proceeded to describe how FAR, rather than the Army, would establish the coordinating mechanism. But since the new DDR&E memo and a letter inviting State to assign a staff member to the Army's new office were in final preparation and about to go to DDR&E for signature, I judged that this would either set the question to rest or provide a high-level forum for arguing it. The package did go forward about June 10 for DDR&E signature, and I felt quite relaxed about the whole matter.

I was in for another nasty shock when, on June 14, I received a call from Nagle. He informed me that the State had just received a confidential cable from Ambassador Dungan in Chile that said that a Communist newspaper on Saturday morning, June 12, broke a story under the headline "Yankees Study Invasion of Chile," sub headed "Project Camelot Financed by the U.S. Army." The cable then complained that the ambassador had not known of any such research project; he was very disturbed that this activity should have been undertaken in Chile without prior notification and he asked what was going on. He considered the effort to be seriously detrimental to U.S. interests in Chile. With this, Nagle said he would be in touch and hung up. I informed DDR&E and called SORO to find out what had happened.

The next few days were extremely hectic. The story was picked up by Radio Havana and then by Radio Moscow, and then appeared in inner-page dispatches in American papers. We were frantically trying to find out what had happened in Chile, of all places, since there had been no plans for research there; no visits were authorized, per my promise to General Porter; and this was the first I had heard of anything having to do with that country. Operations in the Dominican Republic by American troops were still

very much in the news, and the two stories were being linked together in the dispatches.

The dry and concise language of material that was subsequently prepared for but not presented to the Fascell Committee (we decided to let the Army present its own story) makes interesting counterpoint to the confusion of the events:

> a) The Chilean newspaper that carried the story was *El Siglo*, the Communist Party organ in Chile. The story was subsequently picked up by Radio Havana, Moscow Domestic Service, and London's Reuters.
>
> b) No research related to project CAMELOT had ever been planned or conducted in Chile. However, three contacts by the Special Operations Research Office (SORO) personnel with persons in Chile were identified:
>
> > (1) Dr. Rex Hopper, Director of CAMELOT, had stopped in Chile on personal business for two days in early April. He had attended a social science research meeting in Rio de Janiero in late March—the meeting being totally unrelated to project CAMELOT. While in Chile, he informally discussed CAMELOT with two personal friends.
> >
> > (2) On April 5, 1965, Dr. Hopper wrote a letter to Dr. Galtung, a Norwegian social scientist working with the UN in Chile, inviting Dr. Galtung (who had been at Columbia University with the other members of the CAMELOT staff) to participate in a planning conference to be held in Washington in August. A brief description of the project was given, including the fact that it was sponsored by the U.S. Army. The letter offered a fee of $2,000 for the entire month of August.
> >
> > (3) Dr. Hugo Nutini, an anthropologist from Pittsburgh University, had been a consultant at SORO on Project CAMELOT during February–April 1965. Dr. Nutini, a native Chilean now a naturalized U.S. citizen, was scheduled to go to Chile on other business in April. On the initiative of the project director, he was asked to informally assess the interest of academic and governmental officials with respect to the possibility of doing some related research in Chile (apparently, in terms of Chile as an example of orderly social change) and to determine what indigenous resources existed to do the research. This was to be done in the "natural course

> of events" of Nutini's other business, and he was to be paid as a consultant when he returned. A portion of his fee was advanced to him. Dr. Nutini talked to approximately 150 persons in Chile, including governmental officials and academic personnel.

As I later reconstructed the story from various sources, it seemed that members of the Santiago University community had been upset when Galtung, who was very much opposed to American intervention in Vietnam, had told them of this new evidence of American military perfidy; they had contacted Chile's foreign minister, who had in turn gone to Dungan; while they were trying to sort the matter out, other faculty members had given the story to *El Siglo*.

Two other occurrences from these few days remain vivid in my memory. First, when I learned from SORO that Hopper had sent the formal Camelot task statement to Galtung as an enclosure with his letter, I checked back to it. When I saw the words, "Project Camelot is a study whose objective is to determine the feasibility of developing a general social systems model which would make it possible to predict and *influence* politically significant aspects of social change . . . " (emphasis added), I knew that the whole idea of doing research in Latin America was in trouble, and possibly dead.

Second, Wallace and I had been scheduled to meet with the staff of the Senate Armed Services Committee as part of our beginning effort to explain the expansion of the social science program to Congress. We did have this meeting on June 22 and reviewed with the staff director all the activities that led to the new program plans: the background; why we thought the research was necessary; what the research might accomplish; the DSB report; and our current plans. The staff director had read the Camelot news items and inquired. We explained what the project was, what we thought had happened, and how we hoped to correct the problems. We expressed our hope that this would not hurt the program plans too severely. We noted frankly that we felt it would be hard enough to convince Congress of the value of the work, and that the Camelot news would make it more difficult. We stressed the relatively modest pace of the program expansion and budget, reminding him of the small number of people the budget represented. We discussed the continuing coordination we had maintained with ARPA, the Services, and the State Department. He seemed interested and sympathetic, but obviously made no commitment. So, we could see the be-

ginning of the problems that we had believed we would have to prepare for in any case. Having done our internal work in DOD, we now had to face the outside world, which we presumed would want to be convinced, and it looked none too easy even under the best of circumstances. The current circumstances were far from that.

PART III
DONNYBROOK

Prefatory Note: A Personal Interlude

The next weekend, June 26, began as a peaceful one. We had not heard much more from the outside world that week about Camelot, and our investigation of SORO's jumping of the gun in Chile was proceeding. As the chronicle of their stumbling unfolded, we made plans to assure that they could not do that again; and next week, we would decide what to do about the project. On Saturday, my wife and I drove a 500-mile round trip to take our two girls to a summer camp in Pennsylvania. We returned, dead tired, at about midnight, looking forward to seven weeks of relative quiet. For the first time in 10 years, we were going to be alone together for more than a few hours. At about 8:30 on Sunday morning, we were awakened by a phone call from Colonel Sullivan—had I seen The Washington Star *that morning, and what time could I come to a meeting at his office? He sketched out the problem briefly, we arranged a time, and I rushed out for a paper. On the front page, again in the dry language of the brief later prepared for the congressional committee:*

> *On 27 June 1965, an article appeared in* The Washington Star *alleging that the Department of the Army and the Department of State were feuding over the unauthorized intrusion of DOD into the field of foreign policy matters and that Department of the Army was conducting research in Chile without letting the State Department know about the project.*

The consequences were to be great and to unfold over the next five years or more. Not the least of the causalities, but perhaps not large in the general scale of things, was our peaceful summer.

Chapter 10
The Tip of the Iceberg

The Camelot news was to send a shock through the American social science world, starting a period of self-searching, questioning, and witch hunting that was to disrupt both the established value system and relations between social scientists and the government. DOD research in the social sciences became big news and, for a time, it seemed as though hardly a day could pass without a story about another DOD probe into areas that were coming to be deemed "none of its business." The State Department "moved in" to broaden its scope of control over all such activity and, in particular, to exercise control over at least this part of DOD research; this created a countercurrent of concern in the social science community. While the DOD tried to put its house in order, Congress took a closer look at what it was up to and, as the record will demonstrate, did not like what it saw. In the next fiscal year, the program was cut back essentially to where it had been before the new efforts were started. But, more important, it seemed to me that Senators McCarthy, Fulbright, and Mansfield took DOD social research overseas as a symbol of what they viewed as the DOD's expanding and improper grasp of foreign affairs, and they used this as one of the elements of what were to be years of opposition to the DOD's more general role overseas.

All of this did not, of course, take place solely because of the misguided actions of a few social scientists in a foreign country. The news of the Army's "research project in Chile" broke in a period of heightening concern about the Vietnam War, when the further evidence of American interference in a foreign country's affairs in the Dominican Republic had exacerbated the Vietnam irritation immeasurably and had confirmed the administration's opponents' view of what Senator Fulbright called "the arrogance of power." American bombing of North Vietnam had started in February 1965, just about the time the Defense Science Board's report was published. The public debate, along with the demonstration, teach-ins, and other opposition to American participation in the war, was building up during spring 1965 and was to lead to the first peace march on Washington during the following November. "Dissent" was becoming a fashionable word, and the Camelot news gave many social scientists a cause célèbre within their own house to add to their growing dissatisfaction with American behavior overseas.

The sensitivities of all Latin America had been rubbed raw by the entry of American troops into Santo Domingo, which was viewed as reversion to the policy of the "big stick." It would not have taken much for any Latin American to have believed any story about American intervention anywhere on the continent. The evidence, as far as they were concerned, existed in Chile in the form of the Camelot plan's detailed description of the kinds of information the project's staff and consultants had said they wanted: political party alignments; social conditions; army and police organization and roles in society; economic and social maldistribution and discontent—as the phrase goes, "the whole bit." Many other American scholars were working in Latin America at the same time, under private foundation and university sponsorship, and all came under suspicion. And before too long were to come the revelations, starting with the news that Michigan State University had allowed some of its staff to be employed under contract to the CIA to train police in Vietnam,[1] that the CIA had "penetrated" a number of American universities and the National Student Association. The American community of students of foreign societies could foresee the end of its welcome to perform research overseas—everywhere, perhaps, but in sensitive Latin America especially.

Thus the time for DOD to expand its interest in studying revolutionary war and the structure of foreign cultures turned out to have been inauspicious, and only a slight misstep was required to shake the profession to its roots. SORO's contact with social scientists in Chile was that misstep.

The issues that were raised were relatively few, but they were fundamental. The legitimacy of counterinsurgency as a strategy for the United States, and as a subject for study by the American community of social scientists, was questioned. The word changed from a name for a strategy to a symbol of all that was considered reactionary in American foreign, and even domestic, policy. The propriety of the DOD's supporting research into the function of social systems was challenged; the fact that the DOD was carrying out presidential policy was forgotten. The problem of how the DOD controlled what went on under its research contracts became a crucial one, involving complex issues of centralization of research management, political sensitivity, and freedom of inquiry. It became an article of faith that the State Department should have control of anything having remotely to do with foreign policy or, indeed, with research in or about foreign countries. The fact that DOD could get money to support research in these areas, while State apparently could not, was deplored but not questioned from

the State point of view; many years elapsed before the next step—of trying to get the money for State—was taken. The further question was raised, and became ever more persistent, as to whether the *Defense* Department should display any interest in foreign policy at all. On the one hand, the DOD was condemned for trying to learn something about its task, since if it tried to do so this implied it was seeking control for foreign policy. On the other hand, the military were condemned for being insensitive to the nuances of international affairs and diplomacy. Either way, the DOD was out of line.

State's attitude toward and ability to undertake research were never explored, but underlay the uneasiness of the social scientists about the role that was given to State. This concern led to the further question of whether any part of the government could support such research without having its motives questioned. Next in turn came the problem of personal ethics—whether social scientists could properly lend themselves to purposes of government—and from there, full circle, to the growing schism between the scholarly world and the Defense Department. Issues that were never raised explicitly but were also of fundamental importance (and remained so) appeared in the role of the press, with its devotion to a combination of truth and sensation; in the question of whether valid scientific research could be performed under the conditions of public scrutiny and disorder in the field that obtained; and whether the research results would be of any particular value in the long run, in any case.

All these issues are easily and briefly stated in retrospect. But they developed painfully and over a considerable period of time in a jumble of external events, internal meetings, and bureaucratic infighting that were emotionally charged and kaleidoscopically juxtaposed. The only way to gain perspective and some sense of logical sequence, even now, is to look at each of the many threads in turn, showing as the occasions arise how each interacted with the others. In doing this, it will be convenient (as it has been up to this point) to refer to "the press," "the Congress," "the social science community," and others, as though these were monolithic entities. But surely the reader has observed by now that within each of these entities were diverse individuals and groups, each with unique motivations that sometimes reinforced and sometimes conflicted with the motivations of others. It is, of course, out of the actions of interactions among these various participants that the evolution of events developed, as will become clear in the succeeding chapters.

Chapter 11
The Press Has a Field Day

Suppose we start with the press. Its role was crucial. It brought the Camelot fiasco to the public's attention and stimulated the interest of members of Congress in DOD social research. It fed, if it did not trigger, the bureaucratic conflict between the State and the Defense Departments. When all was said and done, the press could claim much of the credit for having brought the DOD's supposed misbehavior to public account.

But its own behavior was interesting. The press' concern was with the surface phenomena and with their more sensational aspects. Fundamental issues were almost never raised, but the DOD's efforts to undertake social research efforts overseas were never lost as the target. The background and the reasons for the DOD's activity were barely explored; although, as we shall see, that was partly the DOD's fault. Some of the press stories were quite accurate, although these seemed to get less prominence than the others. Most of the stories had a few of the facts and wove them together with half-truths and surmise, so that the output seemed always to be full of distortions or misinformation osculating with the truth. Some of this could be taken as good, clean fun; some as haste or carelessness; and some seemed calculated.

The story that really triggered the furor was not the original dispatch that Reuters had picked up from Radio Havana and the Moscow Domestic Service. It was Walter Pincus' front-page story in *The Washington Star* on June 27, headlined "Army-State Department Feud Bared by Chile Incident." The article told of "a growing conflict between the State Department and the Pentagon"; revealed that the U.S. ambassador to Chile had cabled the Department about the "Army-sponsored study begun there without his knowledge" (obviously information leaked to Pincus, because the cable was classified); talked of State's "open-mouthed amazement" at the DOD's growing interest in foreign policy and social science research; and expanded at length on State's view of the foolishness of trying to do research on social systems, and on how the DOD could get research money, while State could not. Three days later, Secretary McNamara received a letter from Senator McCarthy, asking for details about the project and the State-DOD conflict.

In a continuing series of articles over the next few days, Pincus described Senator McCarthy's preparations for a Senate investigation and,

little by little, described some aspects of the Camelot Project, making it seem, however, as though SORO were already undertaking research without a by-your-leave in Venezuela, in French Canada, and elsewhere. The disparity in funds for research between Defense and State was stressed continuously, at first comparing the $6 million said to have been ultimately envisioned for Camelot* with the annual $140,000 or so for State's external research, then pointing out how difficult it was for State to get its internal $4.2 million INR budget past Congressman Rooney. Pincus mentioned DOD studies in the behavioral and social science field funded at "$20 million, an amount far above anything that could conceivably be requested or received for such studies by any other Federal agency . . ." without inquiring as to what work was covered by that budget, and leaving the implication that this was all for "foreign affairs research"—a misconception that persisted. Whereas Pincus had said only that "research in Chile" had been started without State's knowledge, a *Newsweek* article on July 5, in the breezy style of the weekly news magazines, went a step further, stating that "Dungan may be some time getting a complete answer to his cable. For one thing, the Army's Project Camelot seems never to have been called to the attention to top State Department officials." This idea—that the Army had started a study about foreign policy on its own initiative without informing State—persisted in most later writings, even as recently as a note in the May 1971 *Scientific American*[1] about "a covert research effort in Latin America financed by the Department of Defense."

As a matter of simple fact, aside from the discussion I had with the group of departmental research directors and FAR, the record shows that the Army had briefed diverse groups in State on the project at least a dozen times starting in August 1964. A member of the INR staff had met with the group of consultants at the weekly SORO planning meetings in spring 1965. One of those briefed at State was Walt Rostow, then chairman of the Policy Planning Council, who was reported by the Army attendees to have found the proposed research interesting, to have seen "no objection of bureaucratic nature" (in the words of the Army report), and to have expressed his pleasure that the External Research people had already assured DOD of interest and cooperation. (Despite all the later problems and publicity, he never said this publicly, and Secretary Rusk testified in August that all Camelot contacts with State were minor and that the project was never known at the policy level.) The SORO team had also briefed a behavioral

* But see chapter 9, p. 97.

science advisory committee of the National Academy of Sciences that had been established at Army's request to advise the project, with State Department representatives present. As late as May 25, 1965, Nagle had written to Vallance thanking him for keeping the State Department so closely informed of Project Camelot. Most of this information was made known to the congressional committees having an interest in the problem, and was published in the record of the Fascell hearings later,[2] but little of it appeared in the press for reasons that we shall explore shortly and that were partly, but not wholly, the fault of the press.

The knowledge of SORO's and DOD's efforts to keep State informed made Pincus's claim that there was "open-mouthed amazement," as well as a memorandum in State deploring the project, particularly irritating. It seemed to me to be obvious that Pincus was being fed his information from the State. It also seemed obvious that some people in the State Department, at least, had reservations about the general area of DOD research and about the particular project and were expressing them privately within the department. At no time, although there had obviously been many opportunities, were such reservations raised with anyone in the Army or ODDR&E during all the meetings among Army, DOD, and State Department representatives the previous year. One was let to wonder why, if State were deeply concerned about the potentially adverse impact of such research on American foreign policy (as they later would claim to be), they did not mention those concerns while they had the opportunity to prevent the adverse events from taking place. Since, as I was told, their representative to SORO knew of SORO's plans to contact Galtung, and presumably knew that even some of SORO's consultants had warned against it, a single phone call would have changed the entire outcome. It is not surprising that, at the time, we in DOD attributed a certain bureaucratically oriented malevolence to their actions; this seemed in keeping with what I had interpreted as a power play in the weeks before the Camelot story broke and was reinforced by the subsequent behavior, as will be seen. The Fascell subcommittee was led to remark that, "We cannot condone the type of interdepartmental rivalry which was evidenced in the steady stream of 'leaks' originating in the State Department, undoubtedly intended to preclude any other disposition of this proposed undertaking. This can hardly serve to advance the interests of our foreign policy."[3]

There followed a spate of articles and interest in the DOD's adventures with the social sciences in "trade" magazines, in various newspapers, in *Sci-*

ence magazine (which printed one of the more calm and accurate accounts),[4] and even in *Punch*, which added a welcome touch of friendly humor in the midst of all the sound and fury.[5] After the Camelot Project was cancelled in early July, *The Washington Star* crowed editorially that, "thanks to an exclusive article by the *Star's* Mr. Pincus . . . the strange excursion of the Army into sociology in other lands was brought to light . . . this is a rather strange garden path for the Army to be exploring, and the State Department was properly outraged. . . . After some behind-the-scenes conferences, the Army decided it might better abandon the $700,000 . . ."—again, misinformation that the *Star's* editorial writer, at least, might have checked with its own Mr. Pincus, but his outlook was obvious.

Toward the end of July, I was called by John Goshko of *The Washington Post*, who was covering Latin American affairs and the State Department at the time. He said he had heard about a recent Camelot-type flap in Brazil and asked if I could tell him about it. He said that he knew about a contract ARPA had with a company to do research on counterinsurgency in Brazil and that State had received a message of protest from the ambassador; however, he wanted to know if we had "killed the contract." I explained to him that nothing of the kind existed or had happened and reminded him that, while I would be perfectly willing to talk with him about it in detail, the rules of the DOD news game at the moment required that he first direct his inquiry to Public Affairs Assistant Secretary Arthur Sylvester. He said he would do that, but, as far as I know, he never did. I assumed he had to file his story before some close deadline passed.

The project in question was in the counterinsurgency area, but was far simpler and more direct than Camelot. It was intended to explore, from newspaper data available in any library, whether the reported patterns of violence in a country could be correlated with social change well enough to serve as an "indicator" of the onset of violent revolution. Whereas Camelot wanted to analyze the nature of the illness in detail, this project desired only to determine whether symptoms of the illness could reliably be identified from surface phenomena. Of course, not everyone believed such analysis was necessary or would be successful, since it could be argued that an informed and knowledgeable observer could easily describe the condition of a sick society and predict its convulsions. But the opinions of the "old hands" varied with their antecedents and political coloration and, in many countries, political and social violence waxed and waned without any fundamental social change taking place. It seemed reasonable to try to use more

rigorous analytical methods to determine whether some patterns of events were more likely than others to indicate when such fundamental change was about to take place. As it turned out, the study was only moderately successful—but that was not then, and is not now, the issue. The real issue was whether objective analytical inquiry could shed light on the problem and should be undertaken.

In this case, the study team was originally supposed to visit some countries in Latin America to gather data in local libraries. But when the Camelot news broke, ARPA immediately changed these plans. The ambassador to Brazil had learned of the project through State and, believing the intent was still to visit Brazil, which had been one of the countries on the itinerary, had cabled his concern—and was reassured by cable, ending the "flap." However, the story by Goshko the day after he called me described the "Brazil project, like Camelot," as "designed to study ways of influencing social and political change in developing nations . . . " It then described "the Army's" (*sic*) plans to undertake the project in Brazil and how the ambassador had stopped them, then saying that "since last Friday the matter has been the subject of hurried discussions between State and Defense officials"—an exaggeration bordering on fabrication, to say the least. Subsequent articles told how the project was "suspended," and an editorial in the *Post* said that "the Army, undeterred by Secretary McNamara's death warrant for Camelot, was blithely moving ahead with its Brazilian inquiry . . . " It then castigated the Army for undertaking research that was "not only gratuitous but grossly insulting." An article by Walter Pincus in the *Star* on the same subject carried the misinformation that "sources indicated the impetus to cancel Camelot came from the White House," and this was perpetuated in other, later writings by social scientists. Again, as we shall see, the facts were wrong but the orientation was obvious.

It seems, in retrospect, that at least some reporters and editorial writers feel free to weave a tale creating an impression they want to convey, without necessarily having, or feeling the need to find, all the facts in the case. Perhaps many, or for all I know most, reporters try to find out as many facts as they can consistent with having to meet their deadlines. It does seem difficult for many of them to keep their value judgments from getting in the way of objectivity.

About this time, there had also been a Pincus story about the Navy's "Project Michelson," undertaken at the Naval Ordnance Test Station in California.[6] This story revealed that the Navy was using money (about

$250,000 per year) from its Polaris Program for studies by social scientists of the political aspects of strategic deterrence. The article quoted the chief scientist of the Office of Naval Research as saying that "as a pioneering venture, it would be a good idea to see what the so-called soft sciences . . . could bring to bear on the . . . desirable and undesirable features . . . of sea based deterrents." Titillating report titles, such as "Risk Taking and Risk Avoidance in Soviet Foreign Policy, 1945–62," and "The President's 'Slip-of-the-Tongue' on Cuba, August 1962," were quoted. Congress did not know about the project and again inquired. This added to jangled nerves in the DOD. Once again, and in a completely different area, the DOD's attempts to understand the social implications of military force, in application and as viewed from various aspects of public policy, were made to seem somehow improper.

At this point, it was decided that something concrete had to be done to terminate State's destructive campaign of leaks to the press about DOD's study program. A telephone conversation between Harold Brown and Thomas Mann, then the undersecretary of State for Economic Affairs, resulted in an agreement to limit distribution of messages about these projects within the State Department, while DOD would take steps to change research project titles that could be misleading and damaging when taken out of context. The rationale, on which both the Sate and Defense officials at that level agreed, was that the appearance of the information in the press in distorted form was providing every opportunity for the destruction of a research program still deemed important for national security. This was not the only case where the question would arise of how much information about the government's activities could be made public or withheld in the interest of furthering those activities; *The Pentagon Papers* provided a more celebrated example, and the issue will surely reappear in the future. The more fundamental question, of course, was why the DOD did not make a public defense of its position and in its program, if the latter were so important.

A number of the social scientists who had contributed to the growth of the DOD effort, some of whom were beginning to feel repercussions in their professional lives because they had been associated with Camelot, pleaded with us to release a full and coherent story about the project. The Army did, too, and with good reason—they were essentially taking the public "rap" for having carried out a DOD policy they did not originate. Presumably, if the overall research programs made sense, even though some of it might not have been as well thought out or executed as others, and if the

program were supported by and based upon the best, most knowledgeable scientific opinion that could be marshaled to advise the DOD over a period of years, explaining this to the public would have provided a better basis for judgment and rational argument than the flow of fragmentary, incomplete, and inaccurate stories that actually reached the public.

There were several reasons for the DOD's reticence, not all based on sound logic. First, there was the problem of the acute embarrassment caused by Camelot itself. The first reaction of a bureaucracy—and most of those in it—when it feels itself caught out or under fire is protective. Feelings of guilt follow embarrassment and, closely thereafter, the desire to become as inconspicuous as possible. Then, there was the context. All the stories, at first, were about projects having to do with Latin America. In the prevailing atmosphere in that area about American intervention, following on the heels of the landing in the Dominican Republic and the publication of the Camelot task statement about predicting and *influencing* the course of social change, it seemed impossible to compose a discussion of the rationale for a program involving DOD interest in Latin American social systems that would not exacerbate international sensitivities even further. Our feeling in DOD was that anything that DOD said about such interests would make matters worse and that the United States would be better served if the DOD simply took its lumps, kept quiet, and let it blow over. (Of course, it would not blow over, but at the time the dimension of the problem were not fully appreciated.) Next, there were social research projects of much more immediate importance to the DOD in Vietnam and elsewhere in Southeast Asia that were not getting any publicity, that were classified, and to which it appeared unwise to draw attention under the circumstances. Some of these studies were discussed before the Fascell Subcommittee and a little bit about them became part of the public record. But that record of closed hearings was not published until December, and the press had in the meantime turned its attentions temporarily to other matters.

The initial inclination within the DOD was thus to remain silent. This was reinforced by the general attitude of the DOD's upper hierarchy toward discussions with the news media. It will be remembered that, early in McNamara's tenure, it was easy for any journalist to talk with any official and that many leaks of positions opposed to those of his administration had plagued Kennedy's early attempts to establish civilian control over the services. Tight controls were instituted; the rule became that a reporter who wanted to interview an official must submit his request to the Public Affairs

Office. If an interview were approved by that office (after suitable consultations), a representative from the office would sit in on the interview. This was undoubtedly intended to ensure that officials did not depart very far from "approved" policy, but in my years in the Pentagon I never found it particularly inhibiting. One could always meet the demand for information by holding a "background discussion" not for attribution. The rule could also be viewed as a means to help insure against misquotation or distortion by the journalist that could be attributed to the interviewee to his embarrassment. The other side was that the rule placed the decision about what subject matter could be discussed with the press under strong centralized bureaucratic control. This was part of the pattern that led to the accusation of "management of the news" by the DOD.

This issue is more complex than the simple facts might suggest. While in a democracy the public is entitled to know what is happening inside government, the director of a major department of government needs to assert some policy control over his staff. If each member of the staff is free to express his dissenting or critical opinions on important issues, publicly and at will, such control can become a shambles. The courts have, on occasion, ruled that the risk must be taken; but the bureaucracy has shown no signs that control of public statements on policy is being foregone. The system of "checks and balances" can be seen to have many facets.

Be that as it may, the Public Affairs Office decided that nothing should be publicly said about Camelot by the OSD, and this was not likely to be reversed at higher levels. As had been pointed out in the March 24 guidance memorandum, this area of research had yet to prove its worth. In view of the larger foreign policy and defense issues under debate at the time, such as the increasing virulence of the war in South Vietnam, the bombing of North Vietnam, the intervention in the Dominican Republic, and the ever-simmering military force structure issues, such as that over the F-111 airplane, the subject of social research in foreign areas was not likely to appear important enough for the leaders of DOD to rush to its defense against public opposition; and therein lay its greatest vulnerability. While it was doing no harm, it could be tolerated as an experiment. More, in the internal discussions between DOD and State, the right of the DOD to study the problems that directly affected it and its ability to carry out its mission was supported strongly. But if the work got into enough trouble to warrant a public attack on it, there were limits beyond which even its strongest supporters at high levels did not feel they could go to save it.

Thus, the DOD kept quiet and took its punishment (with the Army bearing the brunt, because it had the largest program and also was the contracting agency through which ARPA usually carried out its own work), while the press helped several more projects come apart at the seams over the next several months. The DOD's silence, it seems in retrospect, probably added to the appearance of guilt and whetted the appetite of the press. This was a result that might have been foreseen; but even if it had been, it probably would have made no difference to the decision not to respond.

One of the first of the additional projects to suffer was the new journal, *Conflict*. This was to be a "quarterly Journal of Revolution and Change," published by SORO in their new role at the center of the DOD's applied research program. Volume I, Number 1 had, over the previous year, been prepared for distribution about July 1965, throughout interested parts of the government and the research community. The first issue would have made a strong beginning. It contained, among other things, an article about Vietnam by Henry Cabot Lodge; an article by an eminent China scholar, William C. Johnstone, about Communist Chinese counterinsurgency in Tibet; a description of the State Department's National Interdepartmental Seminar by its director, Ambassador R. A. Kidder; and a section of articles about the history, geography, and politics of Colombia by a list of distinguished scholars as well as *The New York Times* correspondent in Bogotá, and General Andrew O'Meara, former commander of the U.S. Southern Command. But this section on Colombia, juxtaposed as it was in July against the background of attacks on the Army's research interest in Latin America, caused the Army to have second thoughts. With the agreement of DDR&E and the OSD Public Affairs Office, it was decided to postpone distribution of the first issue of the journal until a more propitious time. This intelligence was somehow picked up by Walter Pincus, who used it as the basis for a story in *The Washington Star* on August 17. It was no longer possible to release the journal quietly, and it quietly died. With it died the opportunity to establish another forum of exchange of information important to government officials and those who assisted them.

The same article by Pincus mentioned two additional SORO projects—Colony and Simpatico. The first, which was being carried out by a SORO anthropologist in Peru with Peruvian government approval and assistance, had been underway when Camelot broke. It involved observation and analysis of Peruvian army efforts to assist the economic development and integration of the Indians into the trans-Andean highlands into the

Peruvian economy and society. The results of the study were also intended to assist the U.S. Army to develop its "civic action" doctrines for military assistance to the armies of the developing nations. It will be remembered that the 1963 Smithsonian report had recommended, as an important area of research, the study of the constructive role that the military in developing countries could play in improving social conditions. "Civic action" was a fundamental part of the American military counterinsurgency doctrine, but it was generally taken on faith that it was a good doctrine. It was recognized that it would be important to obtain some real data to ascertain whether, in fact, assistance by a country's military forces in local economic development, education, and technical training did build social cohesiveness and political awareness in a country's outlying areas.

This, too, is a question of value judgment as well as objective observation. Civic action could be viewed as a means by which a military dictatorship can indoctrinate its population and thereby eliminate opposition to itself. Some of this flavor adhered to the Army's efforts in the press reports. The study was completed prematurely under stringent constraints in Peru, and it was to be continued in Bolivia where a similar Bolivian Army development effort was being planned. But the American ambassador in Bolivia became skittish as a result of the publicity over Project Colony and the Camelot furor and, although the Bolivian government and CINSCO had agreed upon and approved the project, it was cancelled at the ambassador's request.

Simpatico, which had been planned earlier but begun in summer 1965 while the Army was under fire, had a more spectacular demise. It, too, was concerned with military civic action—specifically in the strife-torn country of Colombia. A psychologist and an anthropologist were working in a remote area of the country where the Colombian army was trying to pacify the ubiquitous banditry and terrorism of many years' standing by gaining the support of local villagers. Using structured questionnaires and such psychological "instruments" as the thematic apperception test,[7] the researchers were trying to learn the villagers' attitudes toward the government, the army, and the turbulent events in their society. While questions later arose regarding the scientific validity of their techniques in that setting, this was not the question at the time. The Colombian government was fully aware of the project, having approved and welcomed it, and maintained close contact with the research results in which it obviously had a strong interest. The two American researchers had arranged to hire a Colombian research

firm to carry out most of the fieldwork. Thus, the project was following the two precepts for study in foreign countries suggested in the 1963 Smithsonian report: it had local government approval and interest, and the local research community was involved.

The trouble arose during an election campaign when the Colombian government was attacked by its opponents for permitting, or perhaps even using, the perpetrators of Camelot to undertake similar nefarious "espionage" efforts in Colombia. The story of the blowup appeared in *The New York Times* on February 6, 1966. According to that story, the Colombian researchers, who were politically oriented against the government, had objected to the nature of the information sought in the research and had taken their complaints to the opposition party. The *Time's* news article became the subject of a speech the next day by Senator Fred Harris of Oklahoma on the Senate floor; he questioned the propriety and advisability of DOD undertaking social research at all. Senator Harris was soon to start hearings on the creation of a National Social Science Foundation to do such work. On Sunday, February 13, an article by Dan Kurzman in *The Washington Post* discussed the question of DOD social research overseas based on Simpatico news. In the well-balanced article, Kurzman pointed out the reasons behind the DOD's stated need for the research and the fears of opponents of such DOD research that "the Pentagon hopes to determine the minimum amount of social and economic support that must be given the peasants and the lower class groups in order to avert a revolution." But the more general attitude was reflected by the headline that said "Hey Señor! Do You Beat Your Wives Often?" The study did not survive.

Thus, in the six months or so after Camelot became a public issue, the press had shown again, as it had many times elsewhere, that it could raise an issue that was to change profoundly how the government went about its business. It became the means whereby Congress became aware and involved. As we shall see, it stimulated and reinforced discussion among diverse parts of the interested scientific community who might otherwise have tried to resolve the questions raised in a more leisurely, quiet, and reasoned atmosphere. It helped establish the relationship between the events of the DOD social research program and the broader issues that were concerning the nation. It imposed the reporters' and editors' value judgments on the news, and thereby initiated reconsideration of the value system under which the DOD was involved overseas and was undertaking research to support its involvement. It allowed itself to be used by one part of the fed-

eral bureaucracy to attack another for reasons that appeared not to have been purely those of the high principle. And, of course, it was the rapid drumfire of press reports, almost universally critical in tone and attitude, that caused the DOD itself to take another look at what it was doing, and how, and to try to change its approach.

It is, perhaps needless to add (but nevertheless too tempting to resist) the personal note that this view, from the "inside," of the functioning of the press on an important issue makes one loath to take at face value any other press descriptions of important issues and events where there is no personal knowledge and experience.

Chapter 12
The DOD Puts Its House in Order

The news stories, the editorials, the congressional reaction, and the State Department all seemed to convey the tone that the DOD was a dullard who had somehow stepped out of line and needed to be prodded and watched, lest he do so again. But the DOD was not, before or after the Camelot story, unmindful of the problems and the sensitivities involved in the social research it sponsored. And before it was prodded, it moved to try to limit the damage and prevent a recurrence. The sudden realization that faith in the discretion of the presumed experts on social studies might have been misplaced led to what was consistently fought by the researchers and research managers as overreaction, but what the overseers of the research consistently forced to go further. The first problem was to reduce vulnerabilities elsewhere. In ODDR&E, we started an immediate search to ascertain which researchers were overseas, where, and under what circumstances. Where their presence was known to local American authorities and local governments—largely in Southeast Asia—we could relax somewhat. Where their presence was not known, the attempt was made to have them come home. It had become obvious that ambassadors everywhere had become uneasy and hypersensitive. From our point of view in ODDR&E, as well as that of the ambassadors, it would not do to have DOD-supported researchers discovered by the press to be working in "their" countries. However, the service research managers who had given grants to university scholars to study overseas objected strenuously. The scholars had been working overseas for years without such problems developing. They had intimate friends and contacts within the governments and scholarly communities where they were doing their research. Interference with these scholars' academic freedom was, they said, unwarranted, and the scholars had given sufficient evidence over the years that they could be trusted.

This posed, for the first time in this context, an issue that was not to be resolved until the DOD and the university social science community separated, by more or less mutual agreement, and with some considerable trouble for some of the individuals involved. But this is getting ahead of the story. The result of the reflex was the review, on an individual basis, of each of the few cases where researchers were found to be overseas. A decision was made in each case as to whether the damage—to the project, to the scholar, to the DOD, to foreign relations—would be worse if the research were to

be interrupted or left alone with the risk of surfacing in the press; and a few people were asked quietly to drop their research and come home, at least temporarily.

In the meantime, the spate of news stories about Camelot and the gradual unfolding, in our private councils within the DOD, of the story of SORO's lack of discretion had confirmed our earlier fears about the project and its leadership. It became clear as the days went by that it could not pick up where it left off with any semblance of credibility. What little faith we had in SORO's ability to do the work was destroyed as their lack of sensitivity to the explosive issue of interference in Latin American affairs became apparent. The decision was therefore made to cancel the project, and the discussion turned to how the other pieces of the overall program might be reassembled. The cancellation became a political issue when the DOD was notified on July 5 that Congressman Dante Fascell, chairman of the Subcommittee on International Organizations and Movements of the House Foreign Affairs Committee, was planning hearings on the Camelot affair and inquiring whether DDR&E would be willing to testify. Work on the cancellation memo was rushed, and the memo was sent from Harold Brown to the secretary of the Army on July 7. It stated simply that the recent events surrounding Camelot had shown that the approach to research of that type needed modification. In part, it would have to be handled so as not to be open to distortion in the Communist press abroad and not to embarrass the United States if performed on an unclassified basis. It was clear that, although the type of research represented by Camelot remained important, the usefulness of the particular project was destroyed. Therefore, it was requested that the project be cancelled, and the Army was asked to formulate a plan to carry out the research to provide the military forces with the information they would need if they were called upon to engage in counterinsurgency assistance abroad. It also suggested that the handling of the events leading to the Camelot furor raised questions about the effectiveness of SORO's management. Therefore, the Army was asked to look into that, too, although they were given liberty to use SORO to assist in the preparation of new research plans.

Although a lengthy press release was drafted, the one finally issued said simply:

> The Defense Department announced today that the Army's Project CAMELOT has been reevaluated in the light of preliminary planning conducted to date. It has been concluded that the proj-

> ect as currently designed will not produce the desired information, and the project is therefore being terminated.

While somewhat disingenuous in ignoring the current furor, this was an accurate reflection of the facts. But it was not used by the press—the image of lack of credibility had been created. The newspapers played up the juxtaposition of the cancellation and Vallance's testimony before the Fascell subcommittee, as though it had been spitefully arranged. The cancellation was attributed to McNamara, or the president, or the State Department's influence. The DOD image and the overseas social research program suffered some more.

A few days later, there was an exchange of phone calls and letters between Secretaries McNamara and Rusk, initiated by the latter (or, quite likely, by someone on his staff). An American professor was questioning European government officials as part of a study, under Navy auspices, of the "strategic thinking of European elites," and Rusk was informing McNamara that the governments were protesting. It turned out that the project had begun under private foundation sponsorship, but had been picked up and expanded by Project Michaelson. The professor had not told his interviewees of his government support—raising another issue that was to reach its peak when the CIA scandals broke in 1967. But the professor's connection was previously known to the State Department through routine reporting and information exchange. It can generously be concluded that the heightened sensitivities of the period changed their view of it and made any officials involved less receptive in their attitude. It did not seem to be noticed that the State Department officials who knew of the project earlier, but raised no "red flags," might be equally culpable. Secretary Rusk was promised that, "In the future, Defense or Military Department support for studies involving the use of such [interview] techniques abroad will be individually coordinated with your Department before implementation."

On July 12, a memorandum from the secretary of Defense to all parts of the DOD stated that:

> Hereafter all studies done in or for the Department of Defense, the conduct of which may affect the relations of the United States with foreign governments, are to be cleared with the Office of the Assistant Secretary of Defense (International Security Affairs) before they are initiated.

This was welcomed by us in DDR&E as a useful step to protect our efforts from what had by then begun to appear (to us) as a calculated effort by State to destroy those efforts. The period of negotiation with our counterparts in ISA followed, intermixed with preparations to meet the Fascell subcommittee and intense negotiations with State that will be described later.

The Brown-Mann telephone call that had resulted in agreement about leaks and "titles" had also elicited the intelligence that the DOD's attempt to study problems of revolution and insurgency overseas were still viewed with favor, at that level at least, and that, in view of recent events, cooperation between the departments was all the more to be welcomed. The "package" of memos and letters that had been prepared for signature in June, asking the Army to establish a joint office for social science research and inviting the other agencies to participate, appeared particularly appropriate in view of all the accusations about unilateral and uncoordinated DOD activity in the area, and it was dispatched on July 16. It was a forlorn hope. Friendly and encouraging answers were received from all the other agencies, and the services began to designate people for the office. But the appointment of representatives from outside the DOD was delayed for one reason or another. Later congressional action on the budget rendered the effort pointless anyway, and it all came to naught. Once again, however, the DOD was on record as recognizing the sensitivities and responsibilities associated with what it was trying to do. Once again, the record was private; it never reached public attention and had no impact on later events.

Shortly thereafter, on July 18, another guidance paper was issued by DDR&E regarding the work in question. The paper had, of course, been "coordinated" with ISA and State, as well as with key members of the social science community who were made aware of its contents. It dealt with the problem of sensitivity of studies of foreign social systems related to counterinsurgency and spelled out detailed guidelines for the conduct of such work. It pointed out, first, the danger that could arise from having the complete nature of research projects in counterinsurgency of interest to the DOD widely known. Since such work could be injurious to United States foreign policy and therefore to the national security, it properly came within the definition of research that could be classified. If any of the research were to be unclassified, it would have to be divided into small, individually innocent tasks. The issue of whether the DOD—or other agencies with action responsibilities—could under any circumstances undertake studies of

the kind in question was not yet raised within the DOD; that was to come much later. Guidelines were established for clearance of projects. These were different for contract researchers and university scholars working under grants. Unequivocally, contract research projects were required to be cleared by the military commander responsible for American DOD activities in the county where the research was to be carried out; the latter responsibility included checking with the American ambassador and the local government. No travel was to be undertaken without specific, separate clearance for each trip.

It had been made known to us by several people in the university community, largely those working with ONR and AFOSR grants, that that community did not feel it could tolerate such stringent constraints on its activities. We were still at the time sympathetic and sensitive on the issue of academic freedom. In their case, therefore, although prior coordination of projects was required, and the existence and details of the projects had to be made known to overseas military commanders, ambassadors, and governments, the freedom to travel and undertake the research once a project had been initiated was left to the discretion of the scholar. He was, however, to notify his sponsor when such activities were undertaken.

These instructions were later to form the basis of DOD's continuation of its research in this area, even though there were overlays of instructions from ISA and the State Department. The contract organizations presented no particular problem—they accepted the work with the conditions attached—and there was no attempt to control substance, only procedures [we were to learn later (see chapters 18 and 19) that the separation was not easy or even possible to maintain, in most cases]. But even with our nod to the sensibilities of the academic community and their sponsors in the services, both objected to the lesser degree of control with which they were asked to abide. The academic community made the point that once a study was agreed upon, they were the best judges of how to go about it. Clearance of their projects and control of their movements and contacts would amount to control of their work. Despite the mounting evidence to the contrary, they insisted that their discretion was to be trusted and that any regulation at all amounted to interference with and control of the academic process. We were sympathetic, but it seemed obvious that some controls would have to be instituted if there were to be any research at all. The guidance memorandum tried to reconcile the arguments and establish special conditions for the academic researchers, but the problem did not go away.

At about this same time, there was much discussion of the titles and the public image of the work. The problem seemed to be that in the sciences that were more esoteric in the public view—for example, physics or chemistry—the public accepted that there would be a specialized language and did not question it. "Social science" was a different matter. The public expects that studies involving people will be described in plain English. The fact that psychologists or sociologists or anthropologists may have developed their own specialized languages—jargons—to go with the particular techniques of their research is much less recognized. The problem this poses is that, if their work is described in plain English, the public often does not see what there is to study; examples will emerge later of studies whose results appeared obvious after they were obtained, although they were not always predicted in advance. And if a study is described in technical language, it appears to some that jargon is being used to cover up something that should be obvious. Unfortunately, that is often true. In the words of the House Armed Forces Appropriations Subcommittee report on the DOD R&D budget that year, some "studies appear to be concerned with trivial matters on which intelligent people *should* not require studies in order to be informed."[1] The further problem, however, is that there is often disagreement among reasonable people about what is trivial or obvious. The earlier DSB report had made a fairly solid case that there was very much that was not known. The problem was to convey this message outside DOD. DOD officialdom at higher levels was, of course, sympathetic to the public's problem, because the project descriptions were not couched in their language either.

Steps were initiated to have all descriptive material on social science projects, especially that which might easily become public information, reviewed for language and to have at least the statements of the projects' objectives, if not the descriptions of the research techniques used, couched in plain English. The connection between the research task and the DOD's interest and mission was to be made clear. We even toyed with the idea of making up a list of proscribed words, apparently simple words that seemed always to elicit a negative emotional response in some quarters—examples might be, "cross-cultural," "motivation," "socio-political," "attitudinal stimuli," "cognitive skills"—but common sense prevailed. This was an extraordinarily difficult instruction to put across to those who were speaking in their accustomed, everyday language, and the attempt to shield the social scientist from the outraged senator or amused newsman remained a struggle through the years.

A month after the latest McNamara and DDR&E instructions, on August 10, 1965, ISA issued its more detailed instructions for clearance procedures. In general, the control to be exercised was prudent but permissive. It was pointed out that "it is the manner in which the proposed study is to be conducted, not subject matter *per se*, which is at issue." ISA would not address subject matter unless there were some other legitimate ISA interest or duplication that might not have been apparent to the initiators. The fact that the subject matter could be crucial if the fact of the study were made public had still not penetrated, but it would very shortly. The key point of the instruction was that it continued to leave to the judgment of the sponsoring agency within DOD the decision as to which studies required special attention and clearance.

This was to change again within another month. At that time, Mr. Vance, the deputy secretary of Defense, learned of a study planned by one of the services to explore public and congressional attitudes toward a particular kind of weapon system, one that was at the moment very controversial with respect to Vietnam and otherwise. The exploration was to use interview techniques. It was a small study to be done wholly within the United States and had not even been brought to DDR&E attention (it did not have to be, then). The next day, September 10, a memorandum from Mr. Vance directed that each military department and OSD "designate a single representative who will review and approve all RFP's (requests for proposals) and contracts or grants for research outside the physical sciences . . . " This was broad and restrictive, indeed. The person generally designated in each agency was the general counsel. He had been given no additional staff and was busy enough without this added duty. Strict implementation of this directive could be counted upon to bring all social and behavioral science research to a halt; and clearly, other studies using operations research or systems analysis and having to do with the effectiveness of military operations, weapons systems, and force structure could also be included.

As Rains Wallace pointed out in a memorandum to DDR&E on the subject of controls and clearances, a "bureaucratic hydra" had been born:

> For example, a contract with a university psychology department to examine foreign research publications on programmed learning would, in addition to the regular review procedures required for any contract and some procedure for terminology review, be cleared by a special representative in the particular Military Department, ISA, and possibly the State Department before the

> contract could be released for bid. A contract to evaluate the effectiveness of a computerized system for personnel assignment would receive the regular review, the terminology review, and the political and public relations review. Even assuming that the various reviewing agencies would limit themselves to the specific aspects assigned to them (which seems unlikely), it is apparent that the opportunities for delay and infanticide are tremendous.

He also pointed out that the inclusion of behavioral and social sciences together in all of the review instructions was penalizing the majority of work in these areas (which was internal to the DOD, carried out in the United States, and needed for things like recruitment, human performance assessment, training, and human factors engineering) for the difficulty caused by a small fraction of the work. He proposed separating the categories into psychological sciences and social sciences and subjecting the latter only to the required additional review. This was not accepted.

But after many months of trial, the stringency of the review requirements was relaxed somewhat in implementation. The kinds of studies requiring review were sorted out, and only certain studies required detailed examination. In the ARPA case (the one with which I am most familiar), all such studies went to the OSD general counsel for review when in the planning stage and before money was committed. Delay rarely exceeded two weeks, except in those cases where a real issue existed and warranted the delay. A response could be obtained in a day or two if needed. After this, it did not require further review at this level unless a substantive change was being considered. Thus, while at first the internal controls appeared onerous and unworkable, they were applied sensibly and did help the DOD exercise more positive control over the conduct of work that remained controversial.

At the same time, steps were taken to help the DOD sort out what it could and should try to do in the area of social research and to smooth its interaction with the social science community. Within a few days after the first Pincus story, I was called by George Murdock, an anthropologist from the University of Pittsburgh who also headed the Social Science Division of the National Academy of Sciences/National Research Council (NAS/NRC). I went to see him at the NAS/NRC offices in Washington, and he put the problem straight and frankly: something had to be done about DOD's social science research since it seemed out of control and recent events were endangering all social science research. I accepted this as a legitimate expression of concern. In retrospect, it probably foretold the atti-

tude of most of the anthropologists, who became the first, the loudest, and the harshest against their colleagues undertaking work for government agencies having operational concerns overseas. Out of the discussions with Murdock arose the idea of establishing an NAS/NRC Committee on Social Sciences to examine DOD's need and its role and to see what might be done to improve the situation. The DDR&E accepted the idea and, in fact, felt it would be worthwhile enough that he initiated a requested to the NAS to establish the committee.

The request pointed out the difficulties of work overseas, its sensitivity, and asked the NAS to explore how the DOD could accomplish the needed research without, at the same time, creating antagonisms and suspicion on the part of foreign governments. It asked also for advice on how the problems could be studied in such a way that the Communist press would not pick them up and, by adverse publicity and distortion, create conditions under which the work would be impossible to do. There was still no thought that the DOD should not do such work or that it was improper. In fact, the discussions at high levels within DOD, with the State Department (until Rusk's testimony before Fascell), and with members of the social science community showed that there was still a general feeling that the DOD's research program was necessary; the point was made everywhere (and in Fascell's later report) that the problem with Camelot was not what was intended to be learned but how it was done.

A number of problems had to be resolved. One was the makeup of the committee. Hostility toward the DOD was becoming widespread in the social science community. It was not necessary to establish a committee to say that the DOD should not do social research since there were by then many sources of that advice; one was needed to deal with the problem of how such research should be done. It was obvious from the comments of many social scientists reported in the press that the DOD position would not at this time be generally understood or accepted. Therefore, it was desired that at least some people on the committee be familiar with how the DOD worked and with the problems it faced. This is a problem in all bodies established to review public programs—it appears, for example, when the Atomic Energy Commission establishes a committee to review radiation standards. There is a public presumption of conflict of interest if the committee members have worked with the agency; but not much help can be expected if they do not understand intimately how the agency works, and they are not likely to understand this if they have not worked with the

agency. Therefore, a good deal of thought and discussion went into the makeup of the nominees to the committee, and the balance among their backgrounds. Then, it had to be decided whether the National Academy should deal only with the DOD problem and whether it was proper to fund the committee from the DOD. Obviously, the other agencies were concerned, and the committee should have broader interests. But it was pointed out that traditionally one agency can perceive a general need and take the lead in asking the academy for assistance, with other agencies joining later in the sponsorship.

The letter requesting that the committee be established was sent on July 28, 1965, but it was November before all the problems were resolved and the membership arranged; the first meeting was held on November 9.* State was immediately invited to participate. They were, of course, interested in the problem. But more to the point, they had been assigned review responsibility over all research having to do with foreign areas, and the social science community was deeply concerned about whether the State review procedures and controls might not be so restrictive as to strangle all research overseas. Thus, the DOD had taken another step to try to put its house in order by asking for external review of its efforts by the scientific community and by starting to build a focal point where the DOD, the State Department, and the social science community could converge and seek help in an impartial arena. It was also hoped that this group might help reestablish the atmosphere of acceptance that had been created by the earlier committees of social scientists who had advised the DOD (note that there was some overlap in the membership) at the same time that they helped the DOD fit into the new world that had been created, in part, by its own efforts.

The NAS/NRC Committee later broadened its effort to examine the more general problem of the sponsorship and use of research in the social sciences by government. Reports were issued in 1968[2] and 1971,[3] and Lyons'

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* The members finally appointed to the committee before the first meeting were: Dr. Gabriel A. Almond, Stanford University, political science; Dr. Alex Bavelas, Stanford University, psychology; Dr. Albert H. Garretson, New York University, law; Dr. Allen R. Holmberg, Cornell University, sociology and anthropology; Dr. Morris Janowitz, University of Chicago, sociology; Dr. Lyle H. Lanier, University of Illinois, psychology; Dr. Wilbert E. Moore, Russell Sage Foundation, sociology; Dr. Karl J. Pelzer, Yale University, geography; Dr. Ithiel de Sola Pool, MIT, political science; Dr. Herbert A. Simon, Carnegie Institute of Technology, psychology; Dr. Thomas C. Schelling, Harvard University, economics; Dr. Alexander Spoehr, University of Pittsburgh, anthropology; Dr. George K. Tanham, The RAND Corporation, political science; and Dr. Donald R. Young, Rockefeller University, anthropology (chairman). The membership varied somewhat through the years, of course.

book about social science and government, *The Uneasy Partnership*,[4] grew out of his work with the committee. Although the committee was established initially to give immediate advice to the DOD, its pace, resulting from the time it took to explore the issues in depth, was such that it did not influence the DOD's program. In the next few years, as a sponsor of DOD applied research using social scientists overseas, I was asked to meet with the committee a few times. I was not offered, nor did I ask for, advice or guidance from it. Having been set up for one purpose, the committee then took on a life and direction of its own. We shall return to its reports in due course.

It was now obvious that unrestrained research in the social sciences supported by the government and having to do with problems that had important political implications was not going to work. The mechanism—which included American researchers, overseas military commanders, ambassadors, foreign governments, foreign scholars, parts of the foreign press, the American press, and growing parts of the American scholarly community that were not directly involved—was too delicately balanced and offered too many opportunities for something to go wrong to offer hope that overseas research on matters of substance and sensitivity could be carried out without rather strict controls. Perhaps a hundred researchers could be discreet, establish effective working relationships, and carry out their work with delicacy and tact. If the hundred-and-first did not and this led to a blowup, the work of the other hundred was immediately jeopardized. Moreover, the idea was growing in the U.S. press and in Congress that it was improper of the DOD—and even other operating agencies of government—to carry out studies of social systems. The DOD was therefore pulling in its horns, as well as trying to find alternative means to undertake the work it did wish to accomplish. This would soon have an important effect on its relationships with the university community (see chapter 17).

Chapter 13
State Seeks Control

The State Department raised the issue of who would control research on "foreign affairs." The questions of what research was needed by the country, or how it was to be performed, and if not by DOD then by whom were not raised until much later and in a different forum. By a series of maneuvers that left us in DOD breathless and on the defensive, State gained the sympathy of Congress and the public for its own position. The problem for supporters of the social research program in DOD, then, became that of trying to reassert some reasonable limits on the power State had gained. In this, we were aided by the Budget Bureau and, perhaps surprisingly, by some members of the social science community.

After the June 27 meeting in Colonel Sullivan's office on the Chile blowup, there was virtually no further ODDR&E contact with members of the Sate Department until after the Fascell hearings well into August. The series of stories about DOD research overseas, fed by what seemed to be a carefully orchestrated sequence of leaks from State, led to the high-level telephone call described earlier. By then, other events occurred to keep the program in the news. But the earlier series of stories did manage to create an appropriate climate for State's later coup.

There was in the interim a lengthy private correspondence between Harold Brown and Ambassador Dungan in Chile, who had known each other before. In that correspondence, the positions of the two departments were laid out clearly; it was unfortunate that the correspondence was never made public. Dungan, although he was not a career foreign services officer, reflected perfectly what we in DOD took to be the State Department view of social research (perhaps he, too, had a member of his staff who was a foreign service officer prepare the initial drafts of the letters for him). He expressed anger at what he considered the bumbling and clumsy interference of the military in affairs that were properly his concern; if DOD had money to throw around, he said, he could hire two more good political officers who would be worth more than any number of research projects. He questioned whether the kind of research planned by the DOD was feasible or useful at all, and he questioned the propriety of DOD's funding research in foreign countries, regardless of coordination with foreign governments and collaboration with foreign scholars. The DOD position—that it was deeply involved overseas and needed the information that the research could pro-

vide and that there seemed no other agency with the concern and the budget to obtain it—did not sway him despite apologies over the occasional clumsiness of some of the researchers. The tone of his letter seemed to offer no room for doubt that the diplomatic staff made no mistakes, while the DOD's representatives almost invariably did in dealings with foreign governments. Of course, the conflict between military and civilian was strong in Latin America, and America military assistance with its uses and abuses was always very much an issue, so that one could hardly have expected him to take a calm or dispassionate view.

On Saturday, July 31, I had gone to the office to try to dispose of some of the backlog of other work that had piled up during the initial stages of the Camelot battle. On opening my safe, I found there—apparently delivered to and deposited by my secretary while I was at a meeting the day before—a letter from Secretary Rusk to Secretary McNamara, which had come to me for preparation of the reply.* The reply was due on the following Monday, August 2. The "Dear Bob" letter indicated that he (Rusk) was due to testify before Dante Fascell's subcommittee shortly and, before he did, he would like McNamara's concurrence with a proposed letter, draft attached, for the signature of the president. The proposed letter, at first glance, gave the secretary of State ironclad control over "government-sponsored social science research in the area of foreign policy." These words were to become the centerpiece of later arguments and maneuvers.

Thus I learned of the bureaucratic ploy which, being forewarned and therefore forearmed, I was able to resist in the future. This consisted of joining an issue needing extensive consideration and discussion without leaving time for such consideration and discussion in the hope that the issue would be resolved in haste and in State's favor. Fortunately, on that Saturday, those who had to agree to any proposed reply before it could go to the secretary for signature, especially Harold Brown and John McNaughton, the assistant secretary (ISA), were both in the Pentagon and available.

The reply did not explicitly request that the proposed letter *not* be sent to the president for signature. Instead, it reviewed briefly the problems of and the need for research and pointed out that five key agencies were involved: State, AID, DOD, USIA, and CIA. It proposed that a research

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* It has been said of Washington officialdom, that no one in a responsible position ever writes a letter he signs, or signs a letter he writes. While this may be somewhat apocryphal, the incident to be described would not destroy the legend.

council be established at the level of assistant secretary to review economic, social, and political research needs for all the agencies and decide among themselves what should be done and which departments or agencies should undertake particular tasks. The decisions of this group would be binding or, in the case of argument, sent to the cabinet level for resolution. Thus was escalated an idea that had germinated during the earlier discussions among the research directors of the agencies concerned. It would persist and reappear in various forms for many years.

The letter of reply, addressed "Dear Dean," was signed "Bob" on Monday, and sent, with the anticipation that it would discourage the secretary of State from having his proposed letter signed by the president. Great was our surprise, therefore, when on Wednesday August 4, Secretary Rusk, in his testimony before the Fascell subcommittee, revealed that the president had just sent him the following letter, dated August 2:[1]

> Many agencies of the government are sponsoring social science research which focuses on foreign areas and peoples and thus relates to the foreign policy of the United States. Some of it involves residence and travel in foreign countries and communication with foreign nations. As we have recently learned, it can raise problems affecting the conduct of our foreign policy.
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> For that reason, I am determined that no government sponsorship of foreign area research should be undertaken which in the judgment of the Secretary of State would adversely affect United States foreign relations. Therefore, I am asking you to establish effective procedures which will enable you to assure the propriety of government-sponsored social science research in the area of foreign policy. I suggest that you consult with the director of the Bureau of the Budget to determine the proper procedures for the clearance of foreign affairs research projects on a government-wide basis.

Along with his publication of the president's letter, Secretary Rusk sent a letter to all agency and department heads in the government, informing them of the president's instructions and saying that to implement it he had established a "Foreign Affairs Research Council." This was to be chaired by the director of the Bureau of Intelligence and Research and to include representatives of other offices in State, such as the regional bureaus, the Office of Politico-Military Affairs, and the Policy Planning Council. Supported by a staff (which came to include the very people we had been dealing with

before Camelot who had expressed desire to control DOD's research program), they would "formulate policy for departmental action with respect to Government-sponsored research bearing on foreign affairs . . . determine Department needs for foreign area research . . . (and) . . . also examine Government sponsored research projects in terms of the foreign policy risks . . . and means for reducing such risks."[2] Over the same period of time, during which DOD was trying to exercise control over its own program, and into fall 1965 (and beyond), the discussions and conflicts with State were to revolve around the last of these assigned responsibilities.

The first argument about State's responsibility to review and exercise control over DOD's (and other agencies') research arose within DOD itself. This was over how to deal with State. Those of us in ODDR&E who had been involved from the start were firmly convinced from discussions we had had with State before the Camelot affair, and from what we had observed of and read into their behavior since, that it was pressing hard to gain a stranglehold on the DOD research program. ISA took the position that the president's letter was a reasonable expression of policy and that a means had to be found to implement it. ISA therefore wanted to cooperate from the start to help State organize to review overseas research; whereas in ODDR&E, we wanted to hold back to see precisely what State had in mind before offering anything. The resolution of the argument was brought about primarily by State itself. Before Sate published its proposed review procedures, ISA had sent to it for review a project description for a policy planning study to be performed by Henry Kissinger (then at Harvard), regarding a problem of national security originating overseas. It was a project that John McNaughton personally felt was important and should be undertaken. State reviewed the project and refused to clear it, arguing that it was too sensitive; whereupon the intra-DOD positions converged, focusing on the issue that State was extending itself to judging the *substance* and deciding on *approval* of DOD's work rather than (as Rusk's letter had stated) limiting itself to finding means for, and advising on, reducing the risk that might have been involved.

In this, DOD was supported at the many discussions with State by the Budget Bureau, whose policy remained that one department of government should not exercise ultimate control over work done by another using money appropriated for that work by Congress. Enforcement of this policy position was doubtless aided by the fact that one or two key Budget Bureau positions were occupied by people who had formerly been with the Defense Department.

The issues involved were several and all interacting. First, State was planning to establish a staff to *review* the work that would be larger than the total DOD staff in ISA, ODDR&E, and the services responsible for planning, contracting for, and overseeing it. This appeared to be Parkinsonism with a vengeance; it was obvious to us in DOD that the work of review was less than the work of planning and implementation, but (we assumed) would be extended "to fill the time available," delaying or stopping virtually all work. Secondly, and more fundamentally, the question arose as to whether a group of people who had never performed or managed research could sensibly review it. (We assumed that State would appoint such people to its council.) How, for example, could they understand what research techniques were "sensitive," if they had never employed them? How could they appreciate the interaction among problem definition, methodology, and results, if they had never been through the process themselves?

This issue so troubled a number of the social scientists who were aware of or involved in the DOD programs, and many others who had performed research under other government agency sponsorship (including the National Science Foundation), that they took it up directly with State and also with the newly formed National Academy of Sciences committee. It led directly to an invitation to State to attend the first meeting of that committee. Even those social scientists who were not sympathetic to DOD efforts feared the potentially stultifying effect of review of social science research by those who had never performed any and who had purely bureaucratic motives. Thus, in this time of shifting alliances, DOD found strong support where, in the absence of the threat, none would have existed.

The argument then shifted to definition of the scope of the work that State would have to review. At first, the staff of the Foreign Affairs Research Council proposed that they should review all work with foreign policy implications. They stated that, in their interpretation, any American presence overseas had such implications so this would include virtually all R&D performed overseas. We in DOD pointed out that overseas programs had many facets. The services were supporting work in basic physics and chemistry at foreign universities. There were weapons and equipment being tested in Southeast Asia related to the war there. ARPA and the Army had research programs overseas to measure the parameters of the physical environment affecting, for example, how off-road vehicles could move through jungle and rice paddies or how radio waves propagated through the jungle for use in designing communications equipment. There were also operations re-

search studies in Vietnam, Europe, and elsewhere of military operations and their effectiveness; and there were detailed studies of the motivations, organizations, and operations of guerrillas in Vietnam and Thailand. None of these appeared to us to fall within the definition of "social science research in the area of foreign policy."

With Budget Bureau support, we insisted that the scope of State's review process be limited to a literal interpretation of those words: that is, work performed by social scientists to help determine what foreign policy should be or the implications of alternative foreign policies.* It seemed that very little of the DOD program, or of the research called out in the DSB report, fell in the area of this definition. The State made the point that even hardware research could cause foreign policy problems if done improperly. But we noted that no such work was ever undertaken without the approval and general supervision of U.S. military authorities responsible for DOD presence in a country and of the government of the country itself. We also noted that if State wanted to include *everything*, amounting to hundreds of millions of dollars per year of research and development overseas, their staffing problem (both in numbers and disciplines) was hopeless. Nor did we see how they could undertake such an effort without getting into the substance of the work, which was, except for research that fell within our literal interpretation of the words in the president's letter, far beyond their competence and responsibility.

Another key question was whether classified research performed wholly within the United States should come within the scope of State's review procedures. By definition, analytical and study work was classified if its public disclosure could jeopardize the foreign policy and the national security of the United States. If it was classified, and therefore not disclosed, and not performed overseas, we did not see how it could "adversely affect United States foreign relations," and therefore we did not see why the secretary of State should have to exercise his judgment over it. State's position, as with the ISA study they had blocked, was that "if word of the study gets out . . . " But even at that juncture, DOD's record in such matters was not bad; most

* As with so many documents drafted for interagency or international purposes, the words in the president's letter that caused so much argument probably reflected the originating culture. State's and others' insistence that all work overseas was "foreign policy research" or something akin to it probably arose not from deliberate distortion but from the inability of those without technical training to focus precisely on definitions of technical matters that had no meaning for them. Conversely, DOD was likely to see the technical differentiations in detail, but to miss the nuances of policy implication with which it was unfamiliar.

often, word got out when others in government or outside the DOD chose to publicize a study if they learned of it.*

We pointed out again the impossibility of the task of reviewing for foreign policy implications all classified research performed within the United Sates. For example, the RAND Corporation alone might have some dozens of study projects underway in any one- or two-year period and, since in some way almost all of them dealt with the defense and national security or the United States and therefore with the strategy or tactics of military operations that could take place overseas, would State, in addition to the Air Force and OSD, insist on reviewing all of RAND's programs each year and approving or disapproving all or parts of it? And, suppose that in the free-working atmosphere of that organization one of the researchers decided to undertake a brief study to analyze a recent speech of Ho Chi Minh or De Gaulle (an analysis that no one in State or DOD might know about until the research results were published)—would State insist that their permission would now have to be obtained in advance?

There were also procedural problems. In the draft of proposed clearance procedures that State "floated" for comment on August 27, 1965, it was proposed that State "must be informed of a proposed project before a request for bid is made . . . or a contract is concluded," in addition to "names of researchers and indication of the time of proposed field work . . . " In our interpretation, this meant having to go to State with each project at least three times—before proposals were solicited; before the contract was signed; and before work could begin—and maybe more if contract renewals or repeated trips abroad were involved. In addition, it separated procedures for State, Defense, CIA, AID, USIA, and the Arms Control Agency from those for any other agencies. In the case of the first group, State "approval" would mean, the draft said that "State believes that on balance the value of the project outweighs risks of possible adverse effects on foreign relations." It also stated that "the timing of consultations with or notifications to missions [i.e., U.S. embassies in the countries in question without whose agreement the research could not be undertaken even by DOD rules], will depend on the nature of the project." For the other agencies, a project "will be presumed cleared unless other State action is communicated to the agency within fifteen days . . . " of notification about the project to State. Thus, State was at the same time proposing to exercise judgment over substance by judg-

* There was a certain amount of self-discipline within the DOD community in those years before the celebrated exception of *The Pentagon Papers*.

ing the value of the research to sponsoring agencies whose missions were different from its own and setting the stage for indefinite delay of projects to be undertaken by a few agencies over whose work State would thereby have established domination. But while it was seeking such authority, it was also denying its own responsibility for the work in case something should go wrong again, since they also stated that "clearance is not necessarily an endorsement of the need, method or value of the project."

None of this sat very well with any of the agencies involved, including ISA and the Budget Bureau; and the social science community also raised a storm. Arguments over all of these issues continued until State published its instructions in final form on November 18, 1965. By then, compromises had been worked out that met State's minimum requirements, but still resolved all the key issues in a tolerable way for the DOD. Instructions began with a definition of "Government-sponsored foreign affairs research," as including:

> . . . research programs in the social and behavioral sciences dealing with international relations, or with foreign areas and peoples, whether conducted in the United States or abroad, which are supported by grants or contracts awarded by agencies of the United States. In-house research is not included.

Projects involving foreign travel or contacts with foreign nationals, sponsored by DOD, USIA, ACDA, AID, or CIA, were required to be submitted for clearance (not "approval"). There was a list of expectations wherein the need for notification was left to the discretion of the departments or agencies themselves. The exceptions included projects sponsored by any other agencies; projects involving foreign travel in which contacts abroad were to be made with American officials only; unclassified projects not involving foreign travel or contact with foreign nationals; and projects that initially would not require such travel or contacts, but were exempt until these were required. Ambassadors were to be kept informed of all overseas work and travel by the sponsoring agency or by State, as might be decided for each case. Classified projects planned to be conducted wholly in the United States with no foreign contacts were completely excluded from clearance requirements. A project would be cleared once; routine contract administrative actions, renewals, repeated trips abroad on a cleared project, and the like did not have to be submitted for clearance again. All projects were presumed cleared if State did not respond to a submission in 15 days.

It was stated explicitly that "Research projects will be reviewed only for the purpose of avoiding adverse effects upon United States foreign relations." Possibly as reassurance to the social science community, State added a preamble quoting Secretary Rusk to the effect that

> . . . The Department has reaped some benefits from the research of others and, in general, has welcomed the interest of other departments in social and political research on foreign affairs.

In addition, ARPA made an informal agreement with State regarding the work of its field units in Southeast Asia—Vietnam and Thailand. Since ARPA's program directors were in continual contact with CINCPAC and the local U.S. military commands, embassies, and ships, all work done by them was closely controlled without the need for State's intervention. Therefore, the ambassador's approval was presumed to be substituted for that of the State Department, since they would have to ask the ambassadors in any case. Projects started by ARPA field units did not have to go through State clearance procedures in Washington, but it was agreed that State would be kept informed of such projects. Although ARPA tried several times to have this agreement confirmed in writing, the Bureau of Intelligence and Research refused, in effect, holding over ARPA's head the threat of accusation of lack of coordination with State if something went wrong. But nothing ever did.

By the time State's final instructions were issued, tempers had cooled; many of those deeply involved had departed or were about to depart from the government (including myself, temporarily, and Nagle) and "the system" settled down into a new routine.

ARPA projects, at least (those were the ones with whose fortunes I remained familiar), went through the steps of R&D program review, clearance by the OSD General Counsel, clearance by ISA, and, if necessary State, in addition to the various military, embassy, and other overseas clearances required by DOD and ARPA internal procedures. The entire process took several months—for some projects perhaps a year. But the two steps that had initially been the most feared—review by the general counsel for propriety and germaneness to the DOD mission and review by State—seldom, if ever, added more than a month to the process. In later years, there even came to be some cooperative work between DOD and State.

By the time this routine had been established, however, Congress and the social science community were giving ever-closer critical scrutiny to the social science research program and its ramifications. These are two other threads of the chronicle that must be picked up in July 1965 and followed into succeeding months and years.

Chapter 14
Congress Becomes Interested

As we have seen, the international furor over Camelot in Chile and the news stories about State-DOD infighting over matters that had a strong foreign policy flavor stimulated the interest of several members of the House and Senate. As far as it appeared in public, DOD was inserting itself into matters that were State's responsibility. The events in Chile showed that we [DOD] were not doing a very good job of it. All of the news stories were sympathetic to State. This state of affairs, not unnaturally, bore further investigation.

Senator McCarthy, who was then coming into the lead among those opposing the Vietnam War, had been the first with a query to the secretary of defense after Walter Pincus' first article and had hinted that he would initiate a formal Senate inquiry into the matter. He later deferred to Congressman Fascell, whose committee held hearings over the period from July 8 to August 4, 1965. In the words of the committee's report:[1]

> For the past 3 years, the Subcommittee on International Organizations and Movements has been conducting a continuing investigation of ideological operations in foreign policy. From the beginning of that study, the role of the behavioral sciences—what they tell us about human attitudes and motivations, and how this knowledge can be applied to governmental undertakings designed to carry out the foreign policy of the United States—has been of keen interest to our subcommittee. Reporting on this subject almost 2 years ago . . . the subcommittee acknowledged the contribution that the behavioral sciences can make to the achievement of our national objectives on the international scene, noted that the bulk of foreign affairs research in this field was being performed by or for our Military Establishment, and warned that the heavy concentration of effort in this particular area may lead to over-militarization of our foreign policy.

The DOD was to be treated more gently by this group than by any other in its entire history of dealing with Congress on problems of social research related to its overseas programs. But the key issues, as far as Congress was concerned, were raised here. They were elaborated later by the Senate, and the congressional view of the DOD image and motives then took an even less sympathetic turn. The issues were the propriety of the work DOD undertook; coordination of such work within the executive

branch; and the responsibility of the State Department and other agencies for studies related to foreign policy. Other questions were raised about such research, and it was fascinating to see how the military, in responding to them, could be its own worst enemy in this regard.

In its report, the subcommittee took the position that the knowledge the research sought was of importance to the DOD:[2]

> To sum up, the U.S. Military Establishment, in carrying out its assigned missions, continually comes into contact with individuals and institutions in foreign countries. Our own military personnel abroad—some 1 million today—must draw upon knowledge obtained through behavioral sciences research to avoid situations and activities which can cause friction, antagonize local foreign populations, and create other difficulties. At the same time, "wars of national liberation" with which the free world is confronted are unlike conventional wars and new instruments are needed to fight them. There are no fixated frontlines in those conflicts. The problem here involves the behavioral patterns of the insurgents, as well as of the people of the nation where the war is being fought. To do their job in assisting the nations defending themselves against Communist subversion, U.S. military personnel—and the people who are being aided—must understand the motivations of the enemy, its weak points, and its strengths. Behavioral sciences research helps to provide this basic information. It constitutes one of the vital tools in the arsenal of the free societies.

But then it moved on to the basic problem as it had emerged:

> Nevertheless, as the recent experience with Project Camelot has demonstrated, some U.S. research efforts can provoke extremely unfavorable reactions abroad not only from the Communists and their sympathizers but also from academic and political groups that are generally friendly to the United States. There exists in every country a sensitivity to foreigners probing into delicate social and political matters. Also, the level of sensitivity varies according to who does the research and its subject matter. Careful attention to these factors is certainly indicated in the allocation of responsibilities for research on subjects related to our foreign policy, in the preparation of research designs, and in the selection of foreign areas for on-the-spot field investigations.
>
> Further problems arise when the military become involved in foreign affairs research, and when the scope of such undertakings appears to exceed the bounds of the legitimate interests of a particular

> research project's sponsors. In both instances, the motives of the sponsors often are suspect. . . . It is not entirely surprising, therefore, that the U.S. Army's sponsorship of Project Camelot aroused some concern. What is more to the point, however, is that others who have more central responsibility for the conduct of our foreign affairs and who are directly involved in the task of promoting economic and social progress in the developing countries, had not initiated this type of research themselves.

The issue was posed more graphically by Congressman Donald Fraser of Minnesota during Dr. Vallance's testimony:[3]

> The fact that this kind of basic research is being undertaken on behalf of the military I don't find to be a discredit to the military. I find it to be an indication of a lack on the part of our Government of someone else who should have more central responsibility for this kind of research.
>
> Our military assistance programs are primarily in the hardware field, although we also finance some of these civic action programs. Should this be considered an opening wedge to the study of the processes of development, the cultural changes and breakdowns, and so on? Should this be the entire wedge for this kind of basic research? This suggests to me there is something wrong. Not on your part because you are doing a job, but in terms of the assignment or allocation of responsibilities within our Government.
>
> Basically it seems to me that what we do in this field has to be placed in some context. If it is true that the Army or the military or Defense requires certain intelligence information so that they can better predict and project their own planning, that is one thing. But ultimately our goal for these nations is the development of mature economic systems predicated on their own sovereignty. When we are working with these nations to help them, it seems to me it ought not to be the military that is providing the main thrust for this, and the research that is involved ought not to be flowing from the military.
>
> I make the statement so that you can comment on it, but it should be taken in no way as a criticism of your work because I think your work is important and valuable.
>
> I only wish there was an agency of our Government that was not military which was sponsoring this research because I think the

> problem of development of these countries is very crucial, that Defense ought to be playing a very secondary role in this concern.

At this point, then, the DOD was not being blamed for what it had undertaken. Rather, its involvement was considered unfortunate and inappropriate, and the State Department was being questioned for not carrying out the logical responsibilities it should have assumed in the area.

Along with this fundamental question went the subsidiary one of how such work was coordinated across the government. Again, the facts of the case had been brought out but did not appear to the subcommittee to provide enough substance. Its report stated:[4]

> Second, there is no single focal point within this growing Government-wide effort for a sustained and fruitful collaboration with private scholars and the academic community. The Department of Defense, it is true, receives counsel from the Defense Science Board which, at least on occasion, included representation of the behavioral sciences. The relationship here, however, is limited by the Military Establishment's primary concern with military matters. The Arms Control and Disarmament Agency has its own Social Sciences Advisory Board, which focuses upon subjects of interest to that Agency. The Department of State, through the External Research Staff and other offices in the Bureau of Intelligence and Research, compiles information about private research pertaining to foreign affairs and, as necessary, seeks the advice of individual specialists on particular problems that the Department may encounter in the field of behavioral sciences research. These and related arrangements are in themselves specialized and fragmentary.
>
> To help correct these deficiencies, the subcommittee recommends that there be established an Office of the Behavioral Sciences Adviser to the President. Such office could provide the direction essential to an effective Government-wide effort in the field of behavioral sciences, develop mutually beneficial long-term relationships between the government and the academic community, strengthen both the formulation and implementation of foreign policy, and assure orderly development of the Government's programs in this field.

This recommendation was not different in spirit from the recommendation that had been made in the McNamara-Rusk correspondence. Another variant with significantly different implications was to be made later

by Senator Harris of Oklahoma, when he initiated hearings on the subject of a National Social Science Foundation. But the issue being raised was, in a sense, a response from a different quarter to the question asked in the 1963 Smithsonian Report: "Is it polite to study friends?" The response here was, first, that it depended on who did the studying and, second, that such study was so politically sensitive that a responsible focus in government at the highest level must be provided to pass on it. It appeared that the subcommittee, whose report was published well after the president's letter to the secretary of State was written and signed, did not believe that State would or could carry out this responsibility successfully and felt it should be centered in the White House. That was unlikely to happen, of course, because the prevailing view did not rate its importance as high as the committee did. And, as always, it would be easier for the president to pass it on to the Secretary of State than to try to grapple with it when the value of such work did not appear obvious to him or his subordinates in the White House.

The question of the value of this social research and whether it should be done at all ran as an undercurrent through the hearings alongside the issue of its role in affairs of state. Questions in this area were raised explicitly by Congressman H. R. Gross of Iowa. He made no secret of his feelings about researchers hired by the government who, he said, purported to tell the military how to do their business:[5]

> Mr. Gross: Who does let those negotiated contracts with the universities to provide the brainpower to run the military departments?
>
> Mr. Deitchman: Contracts with universities are let by the military departments and ARPA.
>
> Mr. Gross: Are you mixed up in this business of war gaming in Vietnam?
>
> Mr. Deitchman: No, sir.
>
> Mr. Gross: Do you have anything to do with that?
>
> Mr. Deitchman: No, sir.
>
> Mr. Gross: Do you know about it?
>
> Mr. Deitchman: Yes, sir.
>
> Mr. Gross: Tell the committee a little bit about it. I don't think they know about this war gaming in Vietnam. . . . All right. I

guess we all know what war gaming is, what fighting a war is, but what I am talking about is why the Pentagon hired a private contractor with some so-called civilian smart boys to go over and tell the military how to fight the war in Vietnam. This is what I am talking about, the war gaming I am talking about. If you know anything about this, tell the committee, please.

Mr. Deitchman: When civilians are hired to help with a war game, they know about data gathering and analysis of data. War games are done with military people. The military people work with them. They use the civilian firm to assist with the mathematical aspects—

Mr. Gross: I am glad to know the military people work with them. Apparently they don't work with the military.

Mr. Deitchman: They work together. When the military wishes to study a war gaming situation, it obtains the assistance of a firm that can help with the statistical analysis of what has happened in the war game, and they study any military situation this way as a matter of learning about how such situations may work.

Mr. Gross: Tactics, strategy, it is all wrapped up in the same bundle, isn't it? That is what we were told yesterday.

Mr. Deitchman: The only war games I know about that are done with contractors are games that look to tactics. Games that look to strategy are done by the military, by the Joint Chiefs of Staff.

Mr. Gross: What qualifies this civilian contracting firm to have an understanding of military tactics and strategy in Vietnam?

Mr. Deitchman: Their long experience in working with military people on such problems.

Mr. Gross: How long have they worked with them?

Mr. Deitchman: I am not sure what firm you are talking about. Among the firms that I know the military use, there has been experience since World War II and even prior days in working with the military.

Mr. Gross: You mean they have military personnel in these contracting firms?

Mr. Deitchman: Yes, sir.

Mr. Gross: Why doesn't the Pentagon avail itself on an inhouse operation of this military personnel?

Mr. Deitchman: Many times it does. There is a division of labor.

Mr. Gross: I have taken more than my share of time.

Mr. Fascell: Mr. Rosenthal?

Mr. Gross: And gotten nowhere, incidentally.

Congressman Gross opened an area of questioning that led others on the subcommittee to come to share his view. The issue was not Camelot but the study of the Viet Cong based on prisoner-of-war interrogations that had been undertaken by the RAND Corporation. It had been mentioned in our prepared testimony and, after the above exchange with Gross, Congressman Rosenthal of New York asked about it. The study was described by Major General John W. Vogt, then chief of ISA's Policy Planning Staff. The transformation from what the scientists who had initiated the project understood it to be, to what the committee was told, was appalling to me, and the researchers shared the responsibility for the transformation.

The study was called "VC Motivation and Morale." The original intent was to learn something about the nature of the Viet Cong revolutionary movement, including answers to such questions as what strata of society its adherents came from; why they were adherents; how group cohesiveness was built into their ranks; and how they interacted with the populace. While much is now known on the subject, almost nothing was known when the work was conceived and begun. This was to be a study of social systems, and the people who did it had to know, a priori, something about the particular social system—about Vietnamese society, culture, history, and about events there since World War II. Preferably, at least some of them would know the language and would be known to some important Vietnamese officials, easing the problems of access to and interaction with prisoners. There were not many people with such qualifications in the entire United States. If there were a few in the military or intelligence communities, they were likely to be heavily occupied with immediate operational problems associated with the war and would not have had the time to spend several months on these detailed questions—important as they were. The military and the DOD could be faulted if they ignored the questions (which, in my view, they did for much too long), but they did not ignore them indefinitely. They found the appropriate experts, and as a matter of convenience they gave a contract to the RAND Corporation, an instrument of the military system,

to perform the study. This all appears, even now, eminently reasonable, and it might even have struck most of the members of the subcommittee that way. But this was not the story they heard.

After the first part of the study, dealing with the questions outlined above had been completed and the researchers had come home to prepare their report, while another member of the RAND organization had gone to Vietnam to continue the study. With the concurrence of the U.S. military command in Vietnam, he had begun to probe into current VC operations—how they organized; how they operated in the field; their methods of recruiting and military training; and their reactions to the increasing American air and ground combat operations. This approached much more closely to straight military intelligence. Nevertheless, it was welcomed by the U.S. military community because, in the hurried buildup of the MACV organization, the military intelligence part of that organization had remained rudimentary (although ultimately it carried on this aspect of such work itself).

In late 1964 and early 1965, the first translated interviews from this new direction of the RAND study were just coming in, and they were very exciting to the military in Washington, whose contact with events in Vietnam, however frequent, must perforce be limited and fragmentary. It was these results, not the earlier ones, that the committee heard about. This happened largely as a bureaucratic accident, since it had been agreed in advance that General Vogt, who was responsible for overseeing this study rather than the OSD civilians, would tell the committee about it if the questions were asked.

The reaction was predictable:[6]

> Mr. Frelinghuysen: I don't mean to sound skeptical of what I have heard of this study of the Viet Cong. However, it should be a natural responsibility of the military to interrogate prisoners and come up with some intelligent conclusions about the nature of the opposition, and the fact that the opposition is changing as they draft people and so on.
>
> I wouldn't think it would take a wizard to know, whether you are talking about aborigines or sophisticates in Washington, that people have meals in the evening; anyone could draw whatever military conclusions he wants from the fact that they have evening meals. There may be something of more substance that couldn't have been obtained through conventional channels. On the face

of it, it would seem to me that you might come up with the idea that such a survey wasn't getting us anywhere except to increase our understanding that it was not easy to beat the Viet Cong.

I can't help feeling some sympathy with Mr. Rosenthal's position. We haven't spelled out exactly what it is that all this massive effort is accomplishing. It really is a massive effort, as there are not so many behavioral scientists around and we are trying to concentrate a good many of them in particular areas. What is this contributing to our understanding and the effective use of our force, whether it is military or otherwise? . . .

General Vogt: Let me give you an example. The military man is interested, of course, in finding out what is difficult for the other fellow in fighting his war, what is it that really makes it difficult, what kind of an operation on our part would impose extra-heavy burdens on this fellow.

We have discovered through this study that I have described here that one of the things that they find most difficult is the business of having to move a camp from point A to B repeatedly. We have discovered quite a few things about it.

When we talk to the fellows who are actually involved in this fighting, we find that they have been fighting all day and we have forced them out of their encampment area, they have to find another place, locate sources of water, provide means of getting that water up to the main part of the camp, dig trenches, put up a warning post.

They have to go through a very elaborate business of re-establishing a new location and encampment. This wears them out when they have to do it periodically. Just physical digging of the trench and working all night so their position is secure before they can go to sleep is a back-breaking proposition to them. This changed our feeling about how they lived.

Mr. Gross: Did you have to hire a consulting firm to tell you about this? The more you gentlemen from the Pentagon talk, uniformed and civilian, the more you indict your own establishment for lack of inhouse capability to do the things for which you are supposedly trained and for which we are spending one hellavu lot of money.

That is implicit in what you are saying. You go out and hire people to tell you how to run the establishment known as the Penta-

> gon and pay a fee to these so-called nonprofit institutions to tell you how—excellent salaries to those boys at the top levels in these outfits, I may say parenthetically. The more I hear you talk, the more I am impressed with your admission that you just don't have the capability to run the establishment over there, that you have to go out and hire these people to do it.
>
> How did the consultants that have been sent over to Vietnam find out what time the Viet Cong eat supper? Are they up at the front where the mortar shells are falling and machineguns are going, to find out when they eat supper, the Viet Cong? Or did the first increment of American soldiers sent in there, or Marines or whatever they were, did they find out when they eat and provide this information at some rear area?
>
> You know the answer to this. You know that we already knew all about what time the Viet Cong usually eat supper. We didn't have to send consultants over there to find that out.
>
> Some years ago, Walter Reed Hospital put out handbooks, if you may call them that, on the habits of the Viet Cong and South Vietnamese. They are available and have been for several years. Go get them. They are enlightening. You didn't have to send any consultants over there, I hope, to tell the military how to fight the war . . .
>
> Mr. Frelinghuyen: Your illustration disturbs me. The fact that the research has uncovered the fact that it is an effort in a jungle to move from one place to another seems to me a conclusion that could have been drawn by a child. Surely it could be drawn by anyone who has had the experience of being in a jungle. Anyone having to move from one place to another, as the Viet Cong makes our own side move. As an illustration that is a good illustration of where we should not have to depend on something outside the establishment.
>
> I don't mean to sound critical when I say this. What disturbs me is the fact that we are going to spend 3 man-years to come up with a conclusion that in a jungle it is hard for people to move. It is hard for people to move in a military sense under any condition, and of course in a jungle it is harder.

Here, then, was another illustration of how the subtle understandings of the social scientists could be distorted when they were accepted and retranslated by those with operational responsibility and without the train-

ing of the social scientists to look at problems their way. The study, which had been intended to elicit the basic workings of a revolutionary movement and the motivation of its adherents, had been made to seem like a routine examination of minor enemy tactical operations. While it might be possible to make a case for civilian assistance in the former area, the latter should have been derived from straight military intelligence. But, of course, social scientists had abetted these oversimplifications and distortions by the way they themselves changed the direction of the study. Discussions such as this must have led to the remark of the Appropriations Committee regarding some of the work not needing study by intelligent people.

There were other bases for the congressional attitude, as well. During summer 1965, after the Camelot fiasco and while the committees were still deliberating about the DOD budget, Rains Wallace and I went to visit one of the committee staffs to discuss the social science program. The members of the staff were interested in the reasons for DOD's efforts. There was a lengthy discussion of the background to the DOD's social science research efforts and the need that had led to its expansion in the current directions. The conversation turned to research in the villages in Vietnam—of how anthropologists and other social scientists go about gathering and interpreting language and culture; of the substance of what had been learned about the intricate paths of the Vietnamese political factionalism and the complex roles of their diverse and strange (to us) ethnic and religious groups, such as the Hoa Hao and the Cao Dai; and of the importance of learning about and understanding these things if American military personnel were to work successfully with their Vietnamese counterparts.

One of the committee staff remarked after this discussion was over that, all this notwithstanding, his congressman believed that if he wanted to know what a Vietnamese villager thinks, he would go and talk with one for a while, and he would know. He saw no need for expensive research projects to learn what any politician can find out, and does find out, in two or three days. It was a cold comfort to reflect afterward that some congressmen evidently do not find out because they fail to get elected, even where they speak the language and know, and are part of, the local culture. Nor, at that time, did many politicians, even Vietnamese, talk to the Vietnamese peasants even for a few hours. In any case, interviewing was not enough. Knowing what to ask for (in the Vietnamese language), arranging not to appear as an important politician (so that people would talk frankly), and making sure to talk to a representative sample are all required to find out

what is really going on. Nevertheless, the differences in outlook between research-oriented R&D managers and politically oriented congressional staff were highlighted, unmistakably.

The Appropriations Subcommittee, later supported the full House and the Senate, reduced DOD social science budgets by almost precisely the planned increase for FY 1966. Ironically, quoting Dr. Robert Sproull, a physicist and director of ARPA, who said during his testimony:[7]

> I might add that this is a very difficult field in which to be sure one is sponsoring only high quality work. . . . It is a field in which it is very difficult indeed to tell the articulate linguist from the really promising scientist.*

The committee reduced the ARPA, Air Force, and Navy budgets for behavioral sciences by $500,000 each and took two bites out of the Army budget—$500,000 from the "behavioral and social sciences" part of the "military sciences" "line item" and $1 million from the "human factors research" "line item" from which SORO was funded. The program was thus set back to where it had been when the DSB panel undertook its study. Some parts of the DOD had been stimulated to look into a new class of problems, and this was to continue later. But the entire concept of research contained in the DDR&E directives and the DSB report had been nullified.

The congressional actions thus far had been rather benign in comparison with what was to follow. On September 15, 1965, Senator Fulbright made a speech on the floor of the Senate[8] in which he reviewed the recent events in the Dominican Republic in great detail. His main point was that the United States had to face and support the prospect of social revolution in Latin America, but that the opportunity to side with the forces of freedom and social advancement had been missed in the Dominican Republic when we acted to support a military regime he viewed as corrupt out of what he considered to be unreasoning fear of communist elements that had sided with rebels. Almost in passing, he remarked:

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* Some social scientists of my acquaintance were later to express their indignation about this statement, pointing out that the difficulty existed primarily in Dr. Sproull's mind and that the distinctions could be made by trained social scientists whom Dr. Sproull had only to consult to be instructed on the subject. Of course, in view of the performance Dr. Sproull had just observed on the part of some trained social scientists who had participated in Camelot, he might be forgiven for being uncertain about whom to consult.

> . . . one notes a general tendency on the part of our policymakers not to look beyond Latin American politicians' anti-Communism. One also notes in certain Government agencies, particularly in the Department of Defense, a preoccupation with counterinsurgency, which is to say, with the prospect of revolutions and means of suppressing them. This preoccupation is manifested in dubious and costly research projects, such as the recently discredited Camelot; these studies claim to be scientific but beneath their almost unbelievably opaque language lies an unmistakable military and reactionary bias.

So much for Dr. Vallance's "stable society with domestic tranquility and peace and justice for all."

In a now virtually ancient work entitled, *How To Think Straight*, Robert H. Thouless remarks that "words which carry more or less strong suggestions of emotional attitudes are very common and are ordinarily used in the discussion of such controversial questions as those of politics, morals, and religion."[9] In the brief passage of his speech, Senator Fulbright expressed the new attitude that had grown toward the American military and the DOD as a result of the Vietnam War and the attitudes that were being espoused by diverse members of the American public and the majority of the social science community in the universities about America's participation (the more emotional words in the universities being "intervention" or even "imperialism") overseas. The death of DOD's Camelot bespoke the death of Kennedy's Camelot that had helped spawn it.

This was the beginning of Senator Fulbright's effort, which was later joined by Senator Mansfield, to reduce the DOD's influence on foreign policy. In part, he attacked DOD research on problems overseas as a manifestation of that influence. He made periodic speeches about DOD's "research on foreign policy," often inserting lists of projects into the record whose titles illustrated the whole range of DOD's interest in foreign countries and overseas conflicts.[10] We shall see some of their impact shortly.

The articles of Walter Pincus had told of DOD's "$20 million for social science research." Our testimony before the Fascell subcommittee had broken this down[11] (it was actually $27 million in FY 1965), showing that about $5 million of this had to do with "studies of foreign countries, counterinsurgency, and unconventional warfare," and military assistance, with about an additional million for "foreign areas information"; and the point was made that of this sum only about 1.5 million was actually spent in over-

seas research and the related work that had been described in our formal statement to the Fascell subcommittee. But in his later testimony before that subcommittee, Secretary Rusk said that "*the foreign affairs research community* spends at least $30 million per year in support of contract studies that *relate to foreign policy* [emphasis added]. . . . $30 million spent in the behavioral and social sciences can have a far-reaching impact upon foreign affairs. These studies were contracted out by . . . the Defense Department, by AID and ACDA . . . less than 1 percent of this amount is spent by the State Department." Later, in his June 1966 hearings on a National Social Science Foundation bill, Senator Harris noted that "The Federal Government spent approximately $35 million last year for social and behavioral science research *in* foreign countries" [emphasis added].[12] Thomas Hughes, head of State's INR, later guessed that the DOD share of this might be "roughly 12.5 million" of the social science contract research relating to foreign areas and foreign affairs.[13] This was later contrasted by Senator Harris with State's "measly $200,000."[14] From all this, the impression conveyed was that DOD's "social science research on foreign policy" was large—much larger than it actually was in terms of people working on the problems. This, of course, made it look much more threatening than it actually might have been. State's approximately $4.5 million and 300 people devoted to the same problems inhouse[15] were disregarded in the public debate, apparently (and perhaps in fact) being considered irrelevant.

Senator Fulbright, of course, was concerned about the fact of the work and its orientation. Much other work, unrelated to foreign policy, was swept up in his net as the lists of projects in the record show. He (or, more likely, a designated staff member) followed the subject closely; whenever a news article appeared that referred to it or a request for bid or pertinent contract information was announced in the forum of the *Commerce Business Daily*, whether the subject was a contract or a DSB study or a column about a research project in Thailand, a letter from Senator Fulbright to the secretary of Defense followed, asking for particulars. Most of the time, the carefully written responses to his letters were followed by silence, leading us to hope that the particular question had been satisfactorily answered. But of course, that was a vain hope.

In 1968, amid the tensions of the peak of the American buildup and employment of combat forces in Vietnam, Senators Fulbright and Mansfield made a major attack on Defense Department "social science research."[16] Among the study titles that were cited as examples of the military

establishment's interest in matters far beyond their sphere were a study of the economy of India and a report entitled "Witchcraft in the Congo." The first study had been justified after the Chinese attack on India in 1965 as necessary to plan the military assistance program then being undertaken to assist in the strengthening of India's armed forces. It was designed to help formulate a policy of loans, grants, and assistance in building an arms manufacturing industry; the justification being that for the last item, at least, something had to be known about the Indian economy's flexibility and capacity for expansion.

The Congo "study" was not a study at all, but represented a summary paper written in answer to an Army question put to the Counterinsurgency Information and Analysis Center at SORO (see p. 100); the latter was now reorganized and renamed the Center for Research in Social Systems (CRESS) and separated from its contractual relationship with American University. This was the time when the United States was providing a small amount of assistance to the government of the Republic of Congo against a group of rebellions that threatened to tear it apart. American policy was that stability in central Africa depended on the Congo remaining a viable nation. While there was no vocal quarrel with this policy on the part of Congress, the sensitivity to the possibility of American involvement in wars in developing nations was such that a storm was raised over four transport aircraft sent to transport Congo government troops from one part of that huge, underdeveloped country to another.[17] The Congo rebels called themselves "simbas" (lions) and were reported to have convinced themselves by diverse magical ceremonies that they were invulnerable to bullets. Mindful that the British had some success in Kenya with a "de-oathing" ceremony to undermine the psychological effect of the Mau-Mau blood oaths, the Army asked CINFAC what qualities there might be in the beliefs of the Congo rebels that would offer similar opportunities. The paper on "Witchcraft in the Congo," the product of a few days' work by the center, was the response to the question. While this was explained, such studies and reports remained symbols of the DOD's interest in studying foreign peoples, which Senator Fulbright believed was an improper activity for the DOD to undertake.

It appeared obviously necessary to the DOD to try to counter the effects of this furor. Dr. Donald MacArthur, Deputy DDR&E for Science and Technology (i.e., responsible for overseeing all of DOD's scientific research program), his assistant Rodney Nichols, and I visited the staffs of

the House and Senate Armed Services committees and Armed Services Appropriations subcommittees. We explained the nature of the projects in question, showed the context of the large scope of the work in which they were embedded, and tried at length to justify the studies as were done. We also explicitly made clear DOD's attempts to ensure that all such research was relevant to the DOD mission. We agreed that some fraction of the work might be subject to argument in this respect and that the DOD was willing to make adjustments to meet the will of Congress in these areas, but we expressed the feeling that it seemed highly unfair to have one or two studies—and minor ones, at that—singled out as the basis for an attack on the entire DOD research program.

The conversations ranged over the philosophical problem of performing specifically oriented basic research. Basic research could always be shown to have *ultimate* relevance to DOD problems. For example, research in the sciences of materials had led to improved engines and armor; research in basic physics led to new forms of detection systems (some of which would also be applied later to prevent airplane highjacking); research in lasers led to communications and range finding systems. But the research itself did not always have the application in view, and one could not always expect a researcher working on the frontiers of knowledge to understand immediately what the ultimate application would be. (This, in fact, became the key argument of those in the universities who wanted to reject all DOD-supported research as evil.)[18] In the social and analytical sciences, the DOD (since Camelot) was making much more of an effort to ensure the direct and obvious relationship between research subjects and applications that appeared necessary to justify such work.

One of the Senate staff members at this point asked about RAND's "VC Motivation and Morale" studies. I pointed out that these had developed most of the information and deep understanding (beyond the "order-of-battle" analyses of primary concern to the military intelligence community) that had become available in the past three years about the National Liberation Front, its adherents, its ways of relating to the population, and its means for keeping its own members under tight organizational control. Much of this knowledge was reflected in Douglas Pike's book on the Viet Cong.[19] Moreover, the methodology for such a study, primarily the means for gathering and analyzing relevant data in an alien culture, had been integrated into the work of the intelligence community. The work was obviously directly relevant to the military's problems.

None of this persuaded any of the staff members of the four committees. While as individuals they might accept and understand our explanation of the studies and analyses, they pointed out that Congress was becoming extraordinarily sensitive about DOD research. Ordinarily, DOD research in the physical sciences was viewed quite permissively; but with the rising antiwar sentiment and with the university community disengaging from and making attacks on classified DOD-supported research on campus, and in some cases on all DOD-supported research on campus, all DOD-supported basic research was coming into question. The controversial studies and analyses DOD insisted on supporting, representing less than 1 percent of all the DOD's $300-plus million worth of basic research, was jeopardizing all the rest, and they did not think they could defend any of it unless DOD backed down on the 1 percent.

If we needed confirmation that the staff were accurately reflecting the attitudes of their principals, we could pay heed to Senator Stennis's remarks on the floor during one of the discussions on the subject initiated by Senator Fulbright:[20]

> I consider that [social and behavioral sciences] to be the softest spot in all the [DOD] research and development program, although I did not have intimate, personal knowledge about it.

Thus was another nail driven into the coffin.

Senators Fulbright and Mansfield delivered the coup de grace the following year. But we have already moved far ahead of other parts of the story. It is time again to backtrack and see what was happening elsewhere. Before we do, however, let us pause for a moment's reflection.

We spent many hours in DOD trying to devise ways to placate Senator Fulbright with respect to this particular problem. It seemed to us that he was, at the same time, castigating the DOD for being a blundering, ignorant, trigger-happy giant and, yet, objecting when the DOD tried to learn enough to act in a more educated enlightened way. Surely, some of us thought, if we could meet and reason with him, we could convince him of the logic and the need for most, if not all, of the research and studies DOD felt it had to undertake to play its role properly. But we finally concluded that this would serve no useful purpose. We assumed ultimately that, from his point of view, if the DOD were studying foreign areas, this must inevitably appear to be undertaken against the contingency that the DOD would have to operate in those areas. And if DOD expected to do that, it

had enough power and influence in the government to make the planning a self-fulfilling prophecy. Senator Fulbright made clear his belief in the September 15, 1965, speech that the DOD exerted too strong an influence on foreign policy in any case, and that this influence was, in his own word, "reactionary." Whether this was so or not, it appeared logical to us as a next step that if the DOD could be prevented from studying foreign areas, this would at least symbolically reduce its appearance of influence; if this acted over the long run to reduce its influence in fact, all to the good.

This interpretation of the senator's views did seem to fit with what he said on the Senate floor. For example, in 1968:[21]

> . . . I would like to see if we could not begin to get this matter under control. . . . Not only does it not produce anything of value, but it is also affirmatively bad to go around messing in a foreign university and studying the civilian life of foreigners and what motivates them and so on, and especially under the auspices of the Defense Department.
>
> It gives the impression that our country has become 100 percent militaristic, which I know is not so. *I do not want it to become so* [emphasis added], and nobody else here wants that to happen, either . . .

And, again in 1969:[22]

> I suppose that in some areas psychological understanding is necessary and helpful. If we had had any understanding of the psychology of the Vietnamese, I do not think we would have gotten into that war. There was a complete lack of understanding of what the Vietnamese people were like. I cannot say it would not have been helpful.
>
> *I do not think that particular one has any relation to the military responsibility* [emphasis added]. I doubt that the military is the proper one to handle it simply because they are not familiar with that type of activity.

In addition, the senator said often that in many ways the Camelot Project was misguided and stupid; and each time he allied with it the thought of DOD's improper role. If his outlook was as we surmised, it became clear to us then that we were involved, purely and simply, in a clash of convictions and beliefs in the political arena. No amount of explanation would help. So we kept answering the senator's letters as reasonably as we could, hoping to assuage his wrath each time, but inexorably losing in the broader conflict.

Chapter 15
Social Science Has Second Thoughts

The reaction to Camelot by the news media caused annoyance and some resentment in the DOD, but the media had reacted to DOD doings of greater magnitude in the past and this new reaction was no surprise. That many members of Congress should see impropriety in DOD's social science research was also readily comprehensible, if regrettable. State had telegraphed its attitudes and moves well in advance. The clamping down of controls by the chiefs of the DOD was taken as a natural consequence for a program that had caused embarrassment while it lacked intrinsic support. But it is also fair to say that the depth of the anti-DOD reaction, the emotional soul-searching, and the virulent condemnation of their esteemed colleagues by members of the social science community was very much unanticipated by all of those social scientists and others who had a hand in trying to build the program of social research related to the problem of revolutionary warfare.

The fundamental issue was presented as an ethical one, although, as we shall see later, it carried overtones of political conflicts among social scientists. It was voiced in many forms, but basically it came to the question of whether, by working on applied problems to help government achieve its purposes, social scientists were not violating their academic freedom and professional ethics. Implicit was the view that study of social processes from the viewpoint of *avoiding* violent rebellion was reactionary, and that revolution, violent or not, should be studied and supported as a beneficial and necessary instrument of social change. It was not expressed explicitly, but in a large segment of the social science community that had been deeply disturbed by the Vietnam War, the value system had changed.

The violence of the reaction was a shock, in part, because the documents supporting and encouraging the DOD's efforts to enlist social scientists in these endeavors were in the public record and presumably known, at least within the social science community. And, as the Harris hearings later showed, it was also well known that ONR and AFOSR supported research by social scientists in foreign areas and about problems of foreign policy; some of the academic community had found this support welcome and respectable. Through all the years past, there had been no complaints of the sort that were now being raised. Whence the change? It was com-

plex and multifaceted, and, one must add in fairness, it was far from universal in the community.

The case against the DOD and its adherents was staked out, as it were, in two articles published soon after Camelot "broke." Most later writings and discussion represented elaborations on the theme. The articles were by Irving Louis Horowitz, professor of sociology at Washington University,[1] who later testified before the Harris committee[2] and edited a book, *The Rise and Fall of Project Camelot*,[3] and by Kalman H. Silvert,[4] a political scientist specializing in Latin America, who was with the American Universities Field Staff and also testified.[5] Silvert's revised article also appeared in the Horowitz book. The Silvert article appeared in July and that of Horowitz in November. They punctuated a cacophony of letters and articles in diverse journals, especially from Latin American scholars; and they elevated to "scholarly" debate the barrage of news articles, congressional statements, and investigations already described.

It had already become clear that the Camelot news had reinforced an atmosphere of suspicion in Latin America that was affecting all American researchers there, including those supported by university or private foundation funds. Silvert pointed out that American research on Latin America was not well supported, had few adherents, and few students who understood the sensitivity of Latin American scholars to domination from the north. He remarked that "The extremely noisy debacle . . . cannot be explained in the narrow terms of the few bumbling individuals or even of misguided policy; the ground for today's disgrace was well prepared by the ethical incomprehension, cavalier attitudes, and tolerance of ignorance manifested by American universities and scholars for many years." He questioned whether scholars could preserve their "academic" freedom and still work for the government on policy problems, specifically ". . . how does a scholar under contract know that he is adopting one hypothesis instead of another for truly scientific reasons, rather than because of a particular applied interest or even political prejudice? . . . do these scholars think themselves beyond the lures of money, prestige, and personal political passion?" The statement, one might add, would seem to apply to many other scholarly activities as well.

These relatively gentle statements of parts of the problems were elaborated further and from a different point of view at a meeting of the Social Science Research Council held in September 1965, in which I was invited to participate. The council is a nonprofit organization formed in 1923, rep-

resenting the various social science disciplines. It is privately supported to "articulate and advance the research interest of imaginative and highly competent social scientists wherever they may be." Its board is designated by the professional associations in the individual disciplines;[6] thus, in some sense, it represents the entire professional community. The subject of this particular meeting was the problem of "access" in foreign area research. I was asked to join a panel discussing the problems of government-supported research overseas; and at the heart of discussions was a search for the lessons of Camelot.

Two points that were made in the course of the panel discussions, and in later conversations I had with members of the council, stood out. The first, which led me to realize that Camelot was not so much the cause of the furor as it was the trigger for a bomb that had long been ticking, had to do with the problems created in Latin America by privately supported American researchers. This was the question, as alluded to by Silvert, of their attitude toward local scholars—a certain arrogance—a feeling that the Americans were coming to put the local people under the microscope rather than to work with them on their problems. This had been manifested by such incidents as American professors taking their data "home" for use in their own publications rather than analyzing them abroad with their foreign colleagues. And there were a large enough number of researchers in the various countries of Latin America to irritate the people they interviewed—sometimes the same people were interviewed on similar problems by two or more completely independent researchers. Of course, all research in Latin America was not like this, but there was enough like this to be remembered and to create the near-hostile climate into which Camelot intruded. These sensitivities, and sensitivity to official American activity, were obviously mutually reinforcing.

The second major point was whether, if such research results as DOD desired were really needed, they could not be obtained by some independent, "objective" agency, such as the National Science Foundation. This was a complex issue indeed, even more so when viewed in retrospect. The immediately obvious problem was whether an agency needing *applied* research results—specific answers to such specific questions as those raised in the Defense Science Board report—could seek the necessary research and information through an agency designed for basic research, one that depended on scholars to originate proposals on subjects that interested them, for work to be performed according to their own schedules. It did not seem

likely, and the Fascell subcommittee was to recognize this, saying in its report, "Research activities should always be related to each department's or agency's specific operational responsibilities."[7] In any case, the basic research questions and the *applied* questions were likely to be only remotely related to each other, necessitating some work by the operational agency to translate one into the other. Therefore, the problems of getting the applied work done could not entirely be evaded by fobbing them off on a foundation organized for basic research.

A more fundamental difficulty was the shifting into a new context of the ethical problem facing individuals, or hiding their sources of support if these happened to be the DOD or the CIA. The implication of the suggestion to shift support of the work to a government foundation was that, if the operating agencies could not undertake to support overseas research directly, perhaps another innocuous agency could do so by providing a "cover" when the scholar himself was not supposed to. By then, the innocuous agency would have to take the money covertly from the one desiring the work, a process later forbidden to the CIA and the DOD; or it would have to ask Congress for the money publicly and that would "blow the cover." It was evidently not yet recognized that it was the *combination* of the nature of the question and the fact that the U.S. government was asking it that raised hackles overseas, not the subsidiary issue of what instrument that government selected to obtain the answers. The increased frustration attending this realization was to embitter many in the social science community even more against the U.S. government, and came later to be reflected in the harshness of the "New Left's" attacks on those who worked with government support.

Earlier, Horowitz expanded on this problem and on the related ones. Regarding the support of the research by the Army and its meaning, he said:

> In deference to intelligent researchers, in recognition of them as scholars, they should have been invited by Camelot to air their misgivings and qualms about government (and especially Army sponsored) research—to declare their moral conscience. Instead, they were mistakenly approached as skilful, useful potential employees of a higher body, subject to an authority higher than their scientific calling.

What is central is not the political motives of the sponsor. For social scientists were not being enlisted in an intelligence system for "spying" pur-

poses. But given their professional standing, their great sense of intellectual honor and pride, they could not be "employed" without proper deference for their stature. Professional authority should have prevailed from beginning to end with complete command of the right to thrash out the moral and political dilemmas as researchers saw them. The Army, however respectful and protective of free expression, was "hiring help" and not openly and honestly submitting a problem to the higher professional and scientific authority of social science.

But, of course, that last had been done in 1958–63, and the Army was following the advice proffered at the time.

After attacking the project as scientifically unworkable (because it was not designed to take a morally "objective" view), Horowitz went on to say:

> In one Camelot document, there is a general critique of social science for failing to deal with social conflict and social control. While this in itself is admirable, the tenor and context of Camelot's documents make it plain that a "stable society" is considered the norm no less than the desired outcome. The "breakdown of social order" is spoken of accusatively. Stabilizing agencies in developing areas are presumed to be absent. There is no critique of U.S. Army policy in developing areas because the Army is presumed to be a stabilizing agency. The research formulations always assume the legitimacy of Army tasks—if the U.S. Army is to perform effectively its parts in the U.S. mission of counterinsurgency it must recognize that insurgency represents a "breakdown of social order . . ." But such a proposition has never been doubted—by Army officials or anyone else. The issue is whether such breakdowns are in the nature of the existing system or a product of conspiratorial movements . . .
>
> It never seemed to occur to its personnel to inquire into the desirability for successful revolution. This is just as solid a line of inquiry as the one stressed—the conditions under which revolutionary movements will be able to overthrow a government. Furthermore, they seem not to have thought about inquiring into the role of the United States in these countries. This points up the lack of symmetry. The problem should have been phrased to include the study of "us" as well as "them." It is not possible to make a decent analysis of a situation unless one takes into account the role of all the different people and groups involved in it; and there was no room in the design for such contingency analysis.

> In discussing the policy impact on a social science research project, we should not overlook the difference between "contract" work and "grants." Project Camelot commenced with the U.S. Army; that is to say, it was initiated for a practical purpose determined by the client. This differs markedly from the typical academic grant in that its sponsorship had "built-in" ends. The scholar usually seeks a grant; in this case the donor, the Army, promoted its own aims. In some measure, the hostility for Project Camelot may be an unconscious reflection of this distinction—a dim feeling that there was something "non-academic," and certainly not disinterested, about Project Camelot, irrespective of the quality of the scholars associated with it.

He also indirectly accused the social science community of remaining silent about the project because it did not want to jeopardize its sources of support in the DOD.[8] Thus was the motif of corruption of the scientists joined with that of imperialism of the sponsor.

At the same time, Horowitz castigated the State Department for taking part in "a supreme act of censorship"[9] and for missing the moral issues—all the executive branch (at least that part having to do with foreign affairs) became part of the immoral system. R. A. Nisbet was to write later, in apparent contradiction,[10] that whether behavioral scientists make the contribution of research results to the military directly or through foundations or universities is a matter of operational significance but not an ethical matter. But then he rejoined Horowitz, pointing out that the unethical aspect of their behavior was the failure of the social scientists to tell the Army that the project was professionally and scientifically unwise. To others' accusation of moral failure on the part of the social scientists, he added the failure of purely professional ethics. He went on to stress Congressman Royball's question at the Fascell hearings: "Wouldn't the fact that the Army was heading the project itself create a problem in many countries?" This, Nisbet felt, the scientists should have pointed out. That is, instead of taking the Army's money to do the job the Army wanted done, they should have demurred and pointed out to the Army that it was improper for it to spend its money that way.

Thus far had we come from the judgments, values, attitudes, and recommendations of the 1963 Smithsonian report. And, as Silvert, after asking "How many scholars who knew of this widely publicized project actually wrote to SORO questioning the wisdom and ethics of the matter?" pointed out in a footnote, ". . . The writer was requested to join in Project Camelot

last year. He declined, but raised no troubling questions." In other words, the realization of the problems now being identified came *after*, not *before*, the hue and cry about Camelot was raised.

It appears that what was at least partly involved can only be labeled as professional rivalry—a fear on the part of some members of the community that their sources of overseas research data (their professional life's blood, one might say)—was being jeopardized by other parts of the community. Gabriel Almond, who was at the SSRC meeting described previously and was, at the time, the incoming president of the American Political Science Association, was reported in a newspaper article by Walter Pincus[11] to have said at a luncheon session of the association's convention where Camelot's consequences were being discussed that the great expansion of social science activity by American scholars abroad had, in addition to resulting in some cases of poor preparation and understanding, made "legitimate" foreign research difficult for all U.S. scholars. That is, he seemed to be suggesting that the field was getting too crowded and this made things difficult for those already there. Others at the meeting were reported to have spoken against support of research by the DOD, describing such events as an academic (not DOD-supported) survey of Latin American diplomats (presumably "legitimate") running into trouble when the diplomats feared the researcher represented the CIA or another U.S. government agency. Thus, it was implied, the field was not only becoming overcrowded, but that the new entries were sowing distrust of everyone.

One is also tempted to speculate about whether the emotionalism of the reaction to Camelot did not bespeak a certain feeling of guilt on the part of some of the social scientists who decried Camelot the loudest. All the elements were there. Many of them had been taking DOD money to support their work or their colleagues were taking DOD money. This had been combined with other money for general support, a procedure that had not been thought unethical at the time. Many of them knew of Camelot—they did not speak out against it until their Latin American colleagues pointed their accusing finger. The outcry reminded them that their own behavior had not been impeccable; some of them, too, had displayed signs of the arrogance of their particular power—in this case, ample money to go abroad and undertake research of more interest to themselves than to their foreign colleagues. And, underlying it all, a deep and growing dissatisfaction with their country and its part in what was becoming a dirty and unwinnable war. There was much sensitivity abroad on this subject, as I was told at the time

by a number of scientists who were in contact with their foreign counterparts, especially in Latin America. All of this, it is now clear, was simmering in the background. Camelot acted like a mirror in a room when the light was turned on suddenly; in an instant, many social scientists saw themselves as they believed they were viewed from the outside. They were unhappy with the image and they turned violently on their colleagues and their colleagues' mentor, who were responsible for turning on the light and who they could accuse of having caused the trouble. And they undertook their own share of soul-searching and self-accusation.

Thus, the history of the war and the DOD's experiment in social research remained intertwined. From the time the United States entered the war in Vietnam in an expanding role and the research experiment was conceived, until the time the experiment saw the light of day in a glare of accusatory publicity while the United States found itself deep in the war with no resolution in sight, the value system had changed in an important part of the American intellectual community—including key members of Congress, many social scientists, and important representatives of the press. Almost every one of the original premises, given in chapter 3, was overturned for this group:

- Communism in Vietnam, or anywhere, was not a threat to the United States, nor was Communism in small countries necessarily in sympathy with the sources of Communism—the USSR and China—who were no longer monolith.
- America's relations with developing nations should recognize the urges of their people toward greater freedom; if those urges led to violent revolution, the United States should support it instead of supporting reactionary and suppressive "status quo" regimes.
- "Counterinsurgency" was the means by which these legitimate urges toward freedom, possibly expressed in revolutionary action, were suppressed; it opposed revolution and represented intervention by the United States in the affairs of developing nations to aid and abet such suppression by their dictatorial governments. It was not a morally acceptable strategy for the United States.
- The DOD (along with the CIA) was the American instrument of counterinsurgency; its support for studies of foreign peoples was therefore unacceptable because its motives were sinister.

- Social scientists who worked for the DOD and other "mission-oriented" agencies were giving up their academic and intellectual freedom.

One does not have to agree with or accept all or part of either set of premises to recognize that a *change* had taken place. Perhaps it was not a change in values; perhaps, one could speculate, the two sets of values existed side by side in American life. One was in the ascendancy for a time, until events let to the resurgence of the other. Sometimes the shift took place within individuals; at other times, different individuals gained the public attention. Whatever the explanation, the new premises gained even wider acceptance. They evolved over time into a new sort of catechism, which is illustrated in the extreme by the following passages from Michael Klare's article in *The Nation*:[12]

> Every imperium has been faced with the task of finding enough troops to maintain hegemony over colonial territories without straining the financial and manpower resources of the mother country. The occupation army of an imperial power is always outnumbered by the indigenous population of a colony; when a liberation movement has secured the active support of sufficient numbers of people in a country to offset the technological advantage of the occupier, the colonial reign is doomed.
>
> Like all imperial powers of the past, the United States has been obliged to employ mercenaries in order to maintain a favorable balance of power in its colonial territories. In fact, a primary objective of our Asian policy is to install client regimes in each country that can be compelled to supply native troops for America's counterinsurgency. . . . Since even when in control of the governmental apparatus the ruling junta is dependent upon U.S. aid to finance development projects and meet military payrolls, Washington can insist that such regimes provided troops for combat against insurgents in their own or neighboring countries.
>
> In Southeast Asia, the Department of Defense (DOD) found that the use of indigenous troops in counterinsurgent operations creates problems that do not arise in actions against an external enemy. Since such troops are sent against their own countrymen—and often against members of their own village, or even their own family—serious questions of motivation and morale arise. . . . To develop a research and development (R&D) program for our mercenary armies, similar to the extensive R&D program for the American Army, Defense Secretary McNamara

> in 1961 established an on-going program of counterinsurgency research known as Project AGILE.
>
> . . . In the area of behavioral research, AGILE has sponsored research on ethnic minorities thought to be potential sources of mercenaries (e.g., the Montagnards of South Vietnam), and on the development of strategic doctrine for the armies of client governments.

The anthropologists, in particular, felt that they had been "used"—a feeling reinforced by the revelations of CIA support for scholarly and university research and institutions. Shortly after the Camelot news broke, the American Anthropological Association met (November 18–21, 1965) and a violent argument ensued over whether the members of the association should be permitted to perform research for action-oriented agencies of the government. A resolution to explore the ethics of sponsored research was introduced[13] and referred to the Committee on Research Problems and Ethics headed by Professor Ralph Beals of UCLA, then the president of the association. At a meeting a year later (November 19, 1966), the association argued about the report of its committee and referred it back to them for further work. A "Statement on . . . Research and Ethics" was finally approved by the membership in April 1967.[14] It was lengthy and established many guidelines for anthropologists' professional behavior. Among its statements were the following:

> I. Freedom of Research
>
> 1. The Fellows of the American Anthropological Association reaffirm their resolution of 1948 on freedom of publication and protection of the interests of the persons and groups studied:
>
> Be it resolved:
>
> (1) that the American Anthropological Association strongly urge all sponsoring institutions to guarantee their research scientists complete freedom to interpret and publish their findings without censorship or interference; provided that
>
> (2) the interests of the persons and communities or other groups studied are protected; and that
>
> (3) in the event that the sponsoring institution does not wish to publish the results nor be identified with the publication, it permit publication of the results, without use of its name

and sponsoring agency, through other channels—*American Anthropologists* 51:370 (1949).

To extend and strengthen this resolution, the Fellows of the American Anthropological Association endorse the following:

2. Except in the event of a declaration of war by Congress, academic institutions should not undertake activities or accept contracts in anthropology that are not related to their normal functions of teaching, research, and public service. They should not lend themselves to clandestine activities. We deplore unnecessary restrictive classifications of research reports prepared under contract for the government, and excessive security regulations imposed on participating academic personnel.

3. The best interests of scientific research are not served by the imposition of external restrictions. The review procedures instituted for foreign area research contracts by the Foreign Affairs Research Council of the Department of State (following a presidential directive of July 1965) offer a dangerous potential for censorship of research. Additional demands by some United States agencies for clearance, and for excessively detailed itineraries and field plans from responsible scholars whose research has been approved by their professional peers or academic institutions, are contrary to assurances given by Mr. Thomas L. Hughes, director of the Bureau of Intelligence and Research, Department of State, to the president of the American Anthropological Association on November 9, 1965, and are incompatible with effective anthropological research.

4. Anthropologists employed or supported by the government should be given the greatest possible opportunities to participate in planning research projects, to carry them out, and to publish their findings.

The resolution, which was approved by a 12-to-1 mail vote,[15] effectively prevented most anthropologists from participating in work for the DOD since they could not easily suffer the antagonism of their peers and continue as part of their professional community. A minority opposed the constraints implied by the resolution, but the general orientation of the group included attitudes toward applied research completely antithetical to any usage of the results of their work by government for its purposes, except, as noted, in case "Congress declares war." The definition of "public service" in the sense of the resolution is subtle, indeed, under these circumstances. What

was really meant, one presumes, was that they could engage in the form of public service that met with general favor in the professional community at any particular time. Moreover, some of the anthropologists later showed that they could be rather hard on their fellows who did not wish to march to the same drummer.

With the condemnation of research on problems applied to ends these social scientists disapproved of came a new view of their role. In addition to avoiding what they viewed as unethical involvement with government, they came to view it as their responsibility to change the policies with which they disagreed. Showing some cleavage even in these ranks (since the view did not square with the lofty detachment advocated by Horowitz), it was determined that research was no longer to be the sole objective. Political activism was to follow the knowledge gained through the research. This reflected in the various resolutions against the Vietnam War, or against specific operations in Vietnam, such as defoliation or the use of napalm, passed by many professional organizations. While this might appear as a new attitude, it is clear that the social scientists were acting in the "tradition" of the physical scientists who had developed nuclear weapons during and after World War II, when the latter resolved that scientists must assume a share of social and political responsibility for the uses to which the results of their work are put. Camelot appeared to be the social "atomic bomb" that led the social scientists to share the physicists' view. Typifying the attitude was a "draft declaration of Latin American specialists on professional responsibility," circulated for signature in October 1965. It said, in part:

> We considered it our individual and collective personal and professional duty to promote improved conditions of human life, political and economic independence, political and social democratization, and economic development in all countries of the hemisphere. We pledge ourselves in our work and in our private activity always to promote and never to hinder the achievement of those goals. *Whenever U.S. government policies conflict with these goals, we consider it our duty to promote alternative policies* [emphasis added].

While these words are high-minded, idealistic, and in keeping with American values, they appear subtly different from the general concern for the survival of mankind expressed years earlier by the physical scientists. There are narrower, more culturally oriented implicit value judgments here, which suggest that the framers and signers of the resolution would decide

(presumably agreeing among themselves on what policies would and would not meet the goals) what policies were suitable and desirable for "all countries of the hemisphere." But the pattern was set, even if the community bent over so far backward that they tainted themselves with the sins for which they castigated the government. The politicization of the sciences had begun and was to continue apace as a subject for debate and conflict in the scientific community.[16] Of course, it could be argued that all client-oriented research is political if the client is political. The scientist can then choose, if he wishes, the political orientation of the client he works with. We will examine this question in detail in the last chapter of this book.

In all that has preceded in this chapter, we have referred to the "social science community." It must be clear, however, that this "community" was not monolithic. I have already alluded to a certain amount of what appeared to be professional rivalry underlying the strength of the reaction to the Camelot revelations. There had been, in fact, what might be termed a "DOD social science establishment"—the group, and many others like it, that had participated in the Smithsonian studies and whose members, with many colleagues, worked with the government and with the DOD. These men were some of the key people in the development of a strong association of social scientists to which the government could turn. Their role and position are ably suggested by Thackray:[17]

> In 1962, W. H. Auden could say, "When I find myself in the company of scientists, I feel like a shabby curate who has strayed by mistake into a drawing room full of dukes."
>
> . . . The demand for apparatus beyond the pure of any one society or institution and the continually increasing need for federal support have also contributed to changes in the social system of pure science. Perhaps the most obvious of those changes is the emergence of the new breed of dominant dukes. Unlike either the elegant amateurs or poor professionals, these men draw their fame, their monetary, intellectual, and social rewards, and their power in society directly from their enormous scientific ability.

There were, it might be noted, a number of such "establishments." Another included many leaders of the professional associations (e.g., some of those serving on the NAS/NRC committee, or as leaders of the SSRC), who had become vocal against DOD-supported work but who, nevertheless, were not willing to go as far as the more radical members of their groups in condemnation of the government and any relationship with the government

by social scientists. Both these "elite" groups were represented on the newly formed National Academy of Science Committee, and in that forum their differences were muted. But the group that had been working closely with the DOD was attacked by the other indirectly in the press and in the professional societies; the attackers came to assemble many more supporters than the group sympathetic to the DOD. The latter tried in a number of ways to defend themselves, but the DOD, as with most bureaucracies, had become a rather impersonal and ungrateful master, made the job harder by refusing to enter the fray in the media, where much of the battle was being fought; and by refusing to agree that those who had been associated with Camelot could enter the fray officially on the DOD's behalf.

These social scientists could, of course, have discussed their work without violating any trust. The problem was that the government did not wish to speak up regarding its need for Camelot, or something like it, or about its need for the support of the social scientists who had been helping it. Without this kind of support, the latter could only appear to be a small, beleaguered group who had been caught out and were now trying to explain their malfeasance. Many of these social scientists simply "dropped out." They went back to their own work quietly and with great fortitude in view of the damage that was being done to them professionally. They did not switch sides or join the attacks on the government or on their colleagues. A few maintained their ties with the government and undertook a more active defense.

In an editorial in the *American Behavioral Scientist*, "A. de G." asked:[18]

> 1. Is it not true that since 1940, the Army, Navy, and Air Force have contributed incomparably more to the development of the pure and applied human sciences than the Department of State?
>
> 2. Is it not true that the State Department might on dozens of occasions have sought much more extensive research and intelligence facilities than it has actually sought or employed?
>
> 3. Is it not reasonable that the Armed Forces' mission in respect to insurgency should include research on areas where revolution might occur?
>
> 4. Are Cuba and Santo Domingo, Lebanon and Vietnam, and other cases, too, going to stand as historical proof that the Army can send men in to be killed but cannot help anyone go in to forestall by preventative understandings the occasions of killing?
>
> 5. Is "clearance" so vital to an ambassador that a large, important activity should be destroyed for want of it?

> 6. Is it wise for any agency to seek to get a few more research funds by invidious comparisons with the worthy research efforts of another department of government?
>
> 7. Are leaks, false assertions, quotations from anti-American sources, and other tactics to be condoned in treating problems of scientific research?
>
> 8. Should the Social Science Research Council, the American Political Science Association, the American Psychological Association, the American Association of University Professors, The American Sociological Association, the American Historical Association, and the American Anthropological Association, in conjunction with various international counterparts, have acted promptly to investigate the situation, inquiring, among other matters, whether issues of freedom of inquiry were not present? And, while they are at it, might they not investigate the ugly and distorted articles carried in the Washington press, particularly *The Washington Star*, against Project Camelot and social science research?
>
> 9. Should Senator McCarthy and Ambassador Dungan be reproved by agencies of opinion for acting hastily, crudely, and quite possibly wrongly in the Camelot incident?

He then commented:

> There is absolutely nothing an American can do in any country of the world to avoid all criticism from all quarters of the country. Should American companies surrender a billion dollars of French investments because General de Gaulle makes menacing noises toward them? Why then should American professors surrender? The task of the American ambassador is to defend American rights, not to surrender them, and certainly not to surrender them out of pique.
>
> Project Camelot was an open project, conducted by the American University, with Army funds, to solve problems of pressing and universal interest in the present day. It was skillfully manned, well planned, and supported by some of the best foreign scholars in Latin America. Certain State Department officials have little to be proud of in the incident. They may have harmed the national defense effort and impeded social science.*

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* This and the preceding quotes are from Alfred de Grazia, "Government and Science: An Editorial," *American Behavioral Scientist* Vol. IX, No.1 (September 1965), p. 40. Reprinted by permission of the publisher, Sage Publications, Inc.

But most of the defenses were private. This was unfortunate, because it allowed continuation of the one-sided public view of the controversy (although it is doubtful, even now, whether the press would in general have presented the other side fairly if it had been argued in public). Dr. Joseph E. Barmack, chairman of the psychology department at the City University of New York, who had been a consultant to the DOD, wrote to the dean of his college with respect to the Silvert article:

> Silvert's article on first reading appears to be a dispassionate, objective assessment of the state of American social science research in Latin America. However, on closer examination, he had seriously distorted the analysis and I am afraid his article may do more harm than good. He has transformed the problem of getting more social science information about Latin America and other areas into a problem of ethics, a problem of academic status, and unwittingly, into an overestimation of academic versus real world values. The available facts do not support his analyses.
>
> He goes on to question . . . whether social scientists who are consultants to the Department of Defense should have a haven in a university. He believes that any such consultantship compromises objectivity. This is a gratuitous interference. Problems of involvement with a client are common to all applied fields; in medicine, in industrial psychology, in clinical psychology among many others. However, ways have been developed for dealing with clients objectively and ethically. Political science is not a science but a technology. It is an applied field, too.
>
> There is a serious risk in his proposal of discouraging academically based people working on problems of the government. The risk is that the academician will isolate himself from the problems of the real world and preoccupy himself with trivia or pseudo-problems. There is also a risk in restricting research on government problems to people in government. They are far more vulnerable to the biases of the power structure than are the university people.

Ithiel de Sola Pool, who had participated in the DOD's efforts from the start and was to remain active in them afterward, wrote privately to Senator McCarthy:

> The press reporting of the relationship between the Defense Department and the State Department in regard to social science research on foreign areas has been misleading. There has been no

lack of effort on the part of the Defense Department to keep the State Department continuously informed about its social science research activities. The problem is indeed the contrary one. The research people in the Defense Department recognize that the kind of information they need about places like Viet Nam, the Dominican Republic, or other countries in which they have future responsibilities are often also the kinds of information that the State Department should want. They have urged the State Department to conduct serious social science research on foreign areas, too, but to no avail. The research that the Defense Department has supported in the social sciences is in general absolutely appropriate to its mission but that does not exclude it being also useful to the State Department. Ideally, one would expect close liaison and cooperation in securing studies of such topics as the organization of guerrilla forces and the conditions under which the populace will support them or resist them. In fact, the support of serious research has been left by default to the Defense Department, which has taken its responsibility seriously, while continuously informing the State Department and involving it as much as possible.

The absence of an effective State Department role in research may be attributed to two factors: expectation of congressional non-support and prejudice on the part of some persons within the State Department against social science research as such.

The social sciences are but one way of viewing what goes on in the world. The national policy maker should have available to him several sorts of information coming through different channels and collected by different methods. He has the information collected by foreign correspondents and reported in the press, he has the reports of experienced foreign service officers, he has intelligence reports. In addition to these three channels, he should also have studies by social science foreign area specialists. Each of these channels has its advantages and its defects. The press, the F.S.O.'s reports, and intelligence reports are far more topical than anything an anthropologist or political scientist or area specialist might provide. On the other hand, recent events in the Dominican Republic have demonstrated once more how fallible the ordinary channels can be. The social scientist tends to spend months or years in one place interviewing systematically chosen samples of hundreds of respondents and uses a variety of other devices for providing extreme depth to his observations. Had there been some good anthropological or sociological studies of

> the Dominican Republic going forward, American and OAS policy makers would be in a much better position now to interpret intelligently the realities and needs of the situation.
>
> . . . The question has been raised, however, whether the ambassador should not have a veto on governmentally supported research activities. In many cases, the answer is, of course, "yes." The answer, however, turns out to be fairly complex. Clearly the kind of thing that it was erroneously alleged Project Camelot was doing, namely conducting a Defense Department sponsored survey in a country, should only be done with the knowledge and approval of the ambassador. [What I understand happened in that case was that a university researcher used the Camelot name without authorization.] On the other hand, a great deal of scientific research these days is partly or indirectly government supported.
>
> Clearly a university scholar carrying on his work overseas should not feel that because some government research money has gone into his funding, he should either be authorized to say his is a government activity or be required to check out his research plans with the local mission. The major universities in this country that are engaged in archeological, anthropological, and sociological studies abroad will not accept the constraint that their studies will have to be politically cleared just because they have received some NSF or NIH or ARPA or ONR money. The scientist continues to think of himself as a private university researcher and should be encouraged to do so. That is why the issue is complex.

But such representations made little difference in the general drift of events. For one thing, the drift of opinion in the entire intellectual community favored those who were attacking the existing order. The other "establishment"—the one including the leaders of opinion in social science professional associations—was being given their day in court. Almost exclusively, they were the ones called upon to testify before the Harris committee. The testimony was diverse, subtle, and sensitive. These men were not radicals by any means, and criticism of their colleagues was muted. They were trying to learn the lessons of the immediate past and to see ahead to what should be done about them. But almost universally, the testimony ended to reinforce the new value system that was emerging. The idea of a separate National Social Science Foundation received strong support, and it was reflected in a bill Senator Harris introduced in 1969 to establish such a foundation (a bill that was not enacted by Congress).[19]

One of the few premises of the old system that was not overturned or attacked in the testimony before Senator Harris, or in any of the other forums where the issues were being argued, was the idea that socially oriented research was "good"; was important; could help solve social problems; and should be undertaken. Not surprisingly, all the social scientists who attacked the DOD, and those who testified before Senator Harris, continued to support this view. And the DOD itself—or those in it who still cared, and there were many—continued to hold this premise and to act upon it. The subsequent events and consequences were many, important, and instructive. They are the subject of the next chapters of this book. But first, two more observations are in order. These are personal observations, but they do bear on the later evaluation of the meaning of these events.

It was interesting to note, first, that the social science community—that part of it that attacked the DOD and the researchers who had been working with the government—became very unscientific even as it made its attacks on the supposed distortion of science that Camelot represented. It must be granted that many of the arguments against DOD research on foreign social systems, and those pointing out implicit biases of the military sponsors of such research and the risk that participating social scientists would share those biases, have some validity. But, as in any deep and serious public controversy, the arguments that had been initially made in favor of the work did not immediately lose their validity. There remained two sides to the question, especially since the social scientists involved, and many of the military and civilian officials at the upper levels of DOD who supported them, believed that the country was on a path essential to the maintenance of its position on the international scene and should be helped to follow that path as wisely as possible—not to establish and maintain an overseas "empire," but to maintain a position protecting itself against many forms of indirect attack by others avowedly trying to destroy it. The sometimes conflicting desires of ensuring assistance to social development overseas in consonance with basic American democratic values and preventing deterioration in American foreign relations while carrying out a foreign policy in the American interest motivated all of them.

One would expect the news media and the general public to move from one view of foreign policy to another through advocacy of a new set of arguments replacing an older set. This is in the tradition of American politics. But one would not expect this from those who called themselves "scientists," and for whom "objectivity" was supposed to be the most fundamental pre-

cept of their intellectual lives. Those who attacked their colleagues for not having paid attention to both sides of the social political issues then proceeded to be just as biased on the other side. None of the writings, testimony, speeches, or resolutions by social scientists examined both sides of the issues fairly, trying to draw a balanced assessment that could enlighten and uplift the public debate. For example, Horowitz, in the preface to his book, states that he did his best to interview all parties involved in Camelot—critics as well as supporters of the project. But to my knowledge, he did not interview any of those in OSD (civilian or military) who had created the conditions that made Camelot possible about their motives for doing so. While many of those people were not officially available to the press, they would have been available to Horowitz. One must conclude that the exclusion reflects an unconscious bias for the positions he was already taking. Nor did Horowitz (or any other of this group), after accusing the DOD of ignoring the possibility of beneficent revolution, explore concurrently the possibility that all revolutions might *not* be in the best interest of the societies where they took place.

In Horowitz' writings, and also in virtually all that others in the social science community wrote about Camelot, the many errors of fact that had originally been perpetrated in the press stories were picked up and repeated uncritically. None of the social scientists who entered the fray as the issue became more intense took the trouble to investigate sufficiently to get the facts straight, much less to understand and describe the background of events that had led to Camelot in the first place. If the earlier group who had worked with the encouragement of the DOD could be accused of looking at only one side of the questions they were dealing with, those who accused them were doing the same. Both groups based their writings on the values and premises they held and did not look at the implications of the possibility that opposite values and premises might exist concurrently.

Another aspect of this question involves the very question of morality and ethics that the DOD's opponents raised. The "anti" chorus was very strong, and the condemnation of their colleagues was loud and frequent. In the morality that was growing in opposition to the Vietnam War, "dissent" became the watchword. If you were in dissent, you were "concerned"; if you felt "concern," you had to take an activist's role and become "involved"—*against*. "Constancy"—a fundamental property of what earlier generations knew as "character"—did not receive attention or was rejected. If you felt the government was wrong, you should not try to help change it; you must

oppose it. If a commitment had been made to help it, and government did not instantly follow your advice, the commitment should instantly be rejected.

One wonders, when thinking about the social scientists who desired to continue to help and who suffered ever more virulent attacks for their trouble, what is more virtuous: to remain steadfast in one's convictions against all opposition, or to sense the winds of change and allow one's self to be wafted whichever way they blow? Truly, Camelot and the reaction to it both raised and reflected one of the great moral issues of our time. The analogy has been drawn, by some on one side of this issue, between the United States at this time in its history and Hitler's Germany. One who had adult awareness at that earlier period instinctively rejects the comparison. But it will be left to the more dispassionate historians of a later generation—those who were not "involved"—to judge, if ever a final judgment can be made.

PART IV
DOD KEEPS TRYING

Prefatory Note: A Personal Interlude

At the end of January 1966, as the immediate furor over Camelot came to an end and many of the other research and development efforts I had tried to stimulate seemed (unlike the social research program) successfully launched, I left the Pentagon and returned to the Institute for Defense Analyses. In the relative peace and quiet of an atmosphere removed from the daily crises of the government bureaucracy, I had a chance to sort out my thoughts about the stimulating two years just past. There was enough to do of immediate interest, and I lost track of the DOD's efforts to apply the social sciences to counterinsurgency problems. I knew that ARPA was continuing its attempts, and I was aware that SORO was sinking back into its original pattern of library studies after changing its name and many of its research personnel. I had no contact at all with the Air Force and the Navy programs, but assumed they had returned to the earlier pattern as well. If I thought about it, I was certain that they would try to carry on but would have a harder time of it; and this was confirmed by the few items of news that came my way.

The return to IDA turned out to be just an interlude. In August 1966, Dr. Charles M. Herzfeld, who had been Sproull's deputy in ARPA and had then replaced him as director, asked me to join ARPA as director of Project Agile. During my earlier tour, Herzfeld and I had worked together closely. We shared many accomplishments in stimulating research and development efforts in the physical sciences and engineering related to the ongoing conflicts in the world. In addition, he strongly believed in the importance of applying the social sciences to the DOD's problems overseas; he often risked the displeasure of his superiors in the DOD to make the point and support this part of his program. As director of Agile, I would be in a position to initiate and supervise directly some of the important efforts that, from ODDR&E, I could only try to persuade the services or ARPA to undertake. And so, in November 1966, I once again entered the bureaucratic maze.

Although the social research effort was but a small part of the Agile program—running at a total of about $2.5 million per year, which it never exceeded—I still believed in its importance. I felt that I had learned a few things about how to undertake such a program. I was well aware of its sensitivity and the many obstacles to its success; and I had no illusions about the permissiveness of Congress or the upper levels of the DOD. Forewarned by earlier events and presumably forearmed, I set about building a program in the social sciences that I hoped would be useful and important, and trying to establish it in such a way that it could be protected from the worst ravages of the outside world until it had had a chance to prove its value. The demand and impact of that world were, nevertheless, to pose continuing problems and crises; constant adjustments in subject matter, choices of talent, and ways of doing business were necessary.

Chapter 16
Return with a Low Profile

The first steps in reshaping ARPA's program of social research on insurgency problems were designed to give that program as broad and solid a base of executive branch support as possible and to reduce its vulnerability to attack. In taking these steps, we adhered to the principle that the work must be useful to the DOD, and therefore that it must be clearly and demonstrably related to the DOD mission and operations. In this, we did not go as far as Senator Fulbright would have wanted, since we still focused attention on the overseas operations of the DOD and the allies of the United States. But each project was defined in such a way that it dealt with a problem that could be described in terms of international military and military-related operations then taking place—for example, the Military Assistance Program, if not actual warfare as in Vietnam—that rendered the subject of the research of direct concern to the DOD.

Every effort was made to select problems for research support about whose importance, both in fact and in appearance, there would be little doubt. This was sometimes difficult when we had to respond to specific requests of foreign governments to the U.S. government or to requests overseas commands or embassies made of the DOD. But in such cases, there was, at least, the fact of the request to help establish the legitimacy of the work. We were most careful never to undertake work on our own initiative—that is, we never initiated a project simply because ARPA or the researcher happened to feel it was interesting. Wherever a project proposal started—and it could begin with a researcher or with ARPA (presumably, as a research organization that had been delving in depth into these problems for some years, ARPA should have some institutional knowledge of what the important long-term problems were), or with an American military command or embassy (which had short-term problems and often sought help in solving them), or a foreign government connected with the United States through a military assistance agreement—we insisted that the project could not start until it went through the entire approval chain within the DOD and outside it.

It was recognized that this might often preclude what might appear to be interesting and useful work; but that which was undertaken would, we believed, be far better protected by several branches and layers of the government bureaucracy in case questions were raised about why it was being

done. Moreover, it would also be firmly connected with the "user" community so that the results would have a "home" for implementation. In effect, this approach was intended to ensure that the research and the DOD origination that undertook it were *instruments* of policy and not *makers* of policy. If the work were attacked, all those who had requested and approved it would have to be attacked as well. This approach had its obverse aspect also. We found that *most* of the time, if a project were worthwhile, the case for it could be made, while on occasion, if a member of outside officialdom pressed us too hard to undertake a task that seemed too risky, or too sensitive, or useless, we could maintain friendly relationships while relying on other parts of "the system" to help exert better sense and judgment.

Steps were taken to reduce the visibility of the work. This may seem inconsistent with the efforts to ensure a broad base of support. But it is all in the point of view. The innovator—and there have been a number—who creates a new element of technology or makes a new scientific discovery against all opposition, often "bootlegging" funds, is admired and frequently rewarded. But if he fails, he is prepared to receive, and often does receive, the censure of his peers and other elements of society. This seemed a risk that would have to be taken. If the research provided obviously useful results that affected national policy in ways that were viewed as beneficial, it would be accepted. If it did not, it would soon wither in any case. But it needed some protection while undergoing the test, or there would never be a chance to find out. This, in itself, turned out to be an interesting experimental research question of which there was sufficient awareness even at the time.

At any rate, there was not very much that could be done to reduce visibility. The program was kept relatively small. It supported at various times between 15 and 25 professional researchers and a number of foreign research assistants overseas in all the activities. An identifying label for the research program—"social science research"—was eliminated. All the Agile programs were problem-oriented and identified by the subject of the research. We were studying real problems, and some of them required social scientists, just as others required physicists or engineers. Therefore, the program was identified and projects were distributed according to the problem areas with which they were concerned—village defense, or troop training, or civic action, and the like. And finally, since in most cases the projects dealt with ongoing operations of both the American and foreign governments, much of it was classified. The intent was both to minimize the pos-

sibility of embarrassment to the United States and host governments where the work was underway and, since the results were generally expected to bear on policy and operations, to prevent knowledge of such results from telegraphing the moves of the United States and its allies to others who might use the knowledge for unfriendly purposes. The classification also had the effect of making it harder for—but certainly not stopping—the press from continuing its barrage of hostile or amusedly tolerant articles regarding "DOD social science research." All this may seem now to have been rendered somehow sinister by the affair of *The Pentagon Papers*; again, it is all in the point of view, and there appears no need at this point to do other than relate the facts as they existed.

I had also determined early during my ARPA tour to try to orient this part of the Agile program as much toward quantitative measurement and analysis as possible. There were several reasons for this. One was a continuing interest, which I shared and believed in, in furthering the intent of the Defense Science Board's recommendations about "hardening the soft sciences." It seemed, on reflection and after extensive discussions I had with Rains Wallace during the period we worked together, that if social science were to be dignified by the name "science," it ought to be able to do well the first step of science, or that of measuring the phenomena with which it tried to deal. In the words of Lord Kelvin:

> I often say that when you can measure what you are speaking about, and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meager and unsatisfactory kind; it may be the beginning of knowledge, but you have scarcely, in your thoughts, advanced to the stage of science, whatever the matter may be.

This was, in fact, the approach that helped make the work of the behavioral sciences for the DOD in the selection, training, troop performance, and human factors engineering areas so successful.

But there were many reasons why this aim was especially difficult to attain in the counterinsurgency area, which we shall discuss later. Nevertheless, in addition to its desirability from a purely scientific point of view, the approach would help focus the research on matters where there was some chance of obtaining some data to support what would otherwise be arguable opinions, helping thereby to reduce the speculative elements of any study where so much more political sensitivity and controversy were

likely to exist. It would, in particular, help to avoid pitting the judgment of the researcher against the judgment of the political operator in an area where the latter ruled. In this program orientation, the research would have to be based on attempts to gather and analyze data, thereby dealing with real events, places, and people and only with those phenomena where data could be expected to exist. It would, by this means, have to deal with the evaluation of situations, actions, and programs, rather than with *predictions* about them. It would thereby concentrate on providing verifiable information for policy makers and operational program directors to use in the own planning decision making, rather than trying to forecast in competition with the judgment of these responsible officials what would be likely to happen. This seemed a proper role for science.

Thus, the new pattern was set. It was restrictive and not exactly the type of program that the social scientists who had authored the Smithsonian reports had had in mind. But it seemed nevertheless that if the social sciences had anything to contribute, this should give them a reasonable chance of doing so. Now let us see what happened.

Although the issue that emerged and conditioned the results were many and complex, they can be grouped into four problem areas within which there were many subtle variations: who could undertake the work and how they could interact with the bureaucratic system; the problems of planning and performing research under the diverse constraints that were imposed; technical problems of the research and of performing it in the field; and, how the results were used, if they were used at all. We will explore each of those in turn, and then see how the outside world was responding while all this was going on.

Chapter 17
Estrangement from the University Community

It was clear from the start that the field of available people for the research would be restricted. Many social scientists had made apparent their antipathy to the DOD and what it was trying to do, and the university community would obviously have to be approached very gingerly. On the other hand, there was some strong feeling, not without justification, that at least some of the researchers in the non-university community lacked the expertise, the political awareness and sensitivity, and, shall we say, the wisdom to undertake the research that was required. Thus, throughout the entire process, there was a search for appropriate and willing sources of expertise and, as expertise grew, the field was narrowed to the point where very few such sources remained.

The problem with the more hostile university scholars was brought home rapidly—again, by the anthropologists. Within a few days of my entering ARPA, the entire social science program that ARPA had contained since Camelot threatened to explode the way Camelot did. The first event had to do with a proposal that had been submitted to ARPA in preliminary form by a university-connected research group in the Washington area, regarding the possibility of studying some tribal societies in the Congo. The leader of this group, without having had any indication about whether the research proposal would be accepted, had set about arranging his team for listing in the proposal. In the process, we later gathered, he approached an anthropologist experienced in that area, giving him to understand that he—the research director—firmly anticipated receiving an ARPA contract for tribal research in the Congo. The anthropologist immediately raised a storm, writing to the American Anthropological Association and the press, that an attempt was being made to enlist him in intelligence activities for the suppression of Congo tribes in the conflict that was then in its final stages there. Only a firm denial by ARPA that a contract existed or was contemplated allowed the matter to come to rest.

About the same time, I received a letter signed by Charles Keyes of the University of Washington from a group that also included the anthropologists Lauristan Sharpe, Michael Moerman, and Herbert Phillips—all experts on Southeast Asia and Thailand—saying that they had learned that ARPA was about to undertake a massive social research program in Thailand that sounded much like Camelot. They wanted an explanation, and if

they did not get one or were not satisfied with the one they did get, they would go to Congress and the press. The program they were referring to, which came to be known as the Rural Security Systems Program, was just getting underway. In the words used to describe it initially to Congress, the purpose of the program was:[1]

> [to] gather and collate critical information on the local geography, the way of life of the local people, and on their attitudes toward the Government; . . . set up and help maintain current files on insurgent incidents and operations, and on the many Government programs and activities undertaken for counterinsurgency purposes in the northeast; and . . . provide assistance in analyzing the effectiveness of various counterinsurgency programs, as well as helping, through analytical techniques to plan further CI programs.

As part of the initial planning, ARPA (before my arrival) had let a small contract with a social scientist, who had just left his association with an industrial organization to establish his own company, to conduct a survey of the possibility for and interest in social science research in Thailand. He had written a form letter to some 300 diverse social scientists (I seem to recall that high number, but could be wrong at this late date), describing the nature of the research contemplated and implicitly conveying the impression that many social scientists with their families would converge on Northeast Thailand to perform research in connection with the gradually appearing insurgency there. It was through this means that the anthropologists who wrote to me had acquired the information that led them to write; and I came face to face with the problem virtually on my first day back in the government.

After some internal efforts to ensure that no such plan would be seriously considered by ARPA, I invited Keyes and the others who had written to visit me and talk about the program. They were reassured that no serious thought was being given to an effort of the kind described. But it was also pointed out that a program *was* being initiated to help the Royal Thai government with full approval and agreement by that government, the American ambassador (Graham Martin), and the State Department to resolve the problems of insurgency in its northeast provinces and to help it ascertain in the process how it might go about developing this Appalachia of its country to make insurgency there less likely. This was the very problem that troubled the anthropologists, since they felt that by providing this

assistance the United States was increasing the pressure for rapid change and modernization beyond the capability of the Thai civilization to accept it. This professed uneasiness manifested itself in a number of ways. One was the suggestion that rapid social change was destroying a culture that had yet to be observed and understood.

This argument appeared in a more explicit form shortly after this meeting, when we were informed that Peter Kunstadter, an anthropologist working with tribal groups in Thailand, had expressed his concern to a fellow anthropologist who was visiting him that if the DOD saturated the area with social scientists studying the local people for "applied" reasons, he would not be able to continue his research on the culture in its existing state. Our interpretation was that, in effect, he was concerned that we would be spoiling his museum.

After the discussion with Keyes and the others, they agreed that modernization was rapidly coming to all the peoples of Thailand, both through the offices of their own government and from the inexorable drift arising from prolonged contact with the West, and that if the DOD were supporting research on how the changes affected people and on how to ease the inevitable burdens of their cultural evolution, this was an objective which they would not condemn. Having reached this happy conclusion to a delicate confrontation, I then asked whether, since they were among the recognized American experts on Thai culture and history, they would be willing to help us do a better job by helping in the research. The responses varied. One said that if the work were later to be criticized, he would not want to be associated with it but would rather be free to join the critics (although he later sent us a copy, which was very helpful, of his yet-to-be-published PhD thesis on life in Thai village society). Others promised benevolent neutrality.

But one of the group decided that it was time to "put my money where my mouth is" and to help us if he agreed with our objectives. This was Dr. Herbert Phillips of the University of California-Berkeley, who became an ARPA consultant and who, in the course of the next two years, was to provide much useful understanding of the background to the problems with which ARPA was involved in Thailand. He was to be attacked later for this particular interpretation of ethical behavior; and Michael Moerman was also to suffer, although his connection with the DOD was much more tenuous. Other anthropologists' view of their efforts differed from theirs; an article appeared later in the *New York Review of Books*, describing the

government's association with social scientists, suggesting that behind a request for pure research, a research grant, or a consultant's fee would be a government that could use the knowledge gained to *damage* the subjects of the very same research.[2]

The occasion for assessing this judgment was a "summer study" held by the Institute for Defense Analyses' Jason group,* in 1967. One of the Jason group members, Murray Gell-Mann (who was later to win a Nobel Prize in physics), was interested in Thailand; was concerned over U.S. policy there; and convened a group of government officials and social scientists knowledgeable about Thailand to explore the policy questions. The group included Phillips and Moerman. The objective was to learn what was known to Americans about Thai society and to assess the impact of American aid and policy on that society. If useful information could be synthesized, it could become a report to the government on the subject.

For three weeks, the visiting officials and social scientists spoke freely, frankly, and not always complimentarily about Thai society, Thai government, and American activities there. As it turned out, the picture that emerged was mixed; the group found some things it liked and some it disliked about both the Thai government's actions and American policy in supporting them. There was no special report, but detailed minutes of the discussions were kept by the participants. Through what was later reported to me privately as an act of theft by a student activist working in his office, the copy of the minutes held by one of the attending social scientists was made public in 1969. Not unexpectedly, the intent of the meeting was lost, and an outcry ensued about the anthropologists who were helping the U.S. government's "counterinsurgency." Phillips and Moerman were attacked by members of the American Anthropological Association.[3] The view now strongly held (in 1969, when this news emerged) was that anthropologists must keep themselves "pure" and must be willing to champion the oppressed peoples of the world, including those whom anthropologists would define as "primitives" or "peasants."[4]

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* Jason is a group of eminent university scientists, primarily in the field of physics, who devote a significant portion of their time to organized study of defense problems. Meeting at IDA periodically to learn about those problems during the academic year, they then meet together in another location for several weeks during each summer to perform substantive analyses of a number of those problems. Their reports have been valued at and have been advisory to the highest levels of the DOD. In 1973, their affiliation was transferred for administrative reasons from IDA to the Stanford Research Institute.

The arrogation to self of superior attitudes toward other peoples by "defining" them was apparently lost on the authors of such words and the community that agreed with them. But it was clear that any research to gain knowledge that could be used, for any purpose, had become anathema to them. It was not clear, however, whether it would be acceptable to use the knowledge for purposes these anthropologists could agree with—for example, revolutionary protests—and was unacceptable only if they did not agree. Or, were they honestly interested in science that could be called "pure," if such were definable? If the latter, would they, as their colleague in Thailand appeared to, require the peoples they wished to define and study not to change while the "pure" scientists studied them? The positions of various social scientists on these points are not clear even yet.*

In this case, the American Anthropological Association set up a board of inquiry under the highly respected Margaret Mead to ascertain whether there had been any wrongdoing. In its report, while the kind of research at issue was condemned and some rules of ethics were recommended for anthropologists to protect the subjects of their research from government interest, the specific individuals were absolved of sinister motives and malfeasance. But the association refused to accept this verdict and, in effect, sent the committee back to find against the accused.[5]

Phillips and Moerman were not the only university scholars attacked for association with the DOD. A scientist at a midwestern university who was an expert on Thai institutions and governmental systems undertook an ARPA contract to explore the institutional problems of change in Thailand and, incidentally, how these affected and were affected by the various conflicts in different areas of the country. He finished the preliminary, exploratory part of his work, and we were negotiating with him for a contract to continue while clearance for the project was being sought. He was well known to Thai social scientists and to Thai government officials; the prospects were hopeful. He planned several years of his future career on the basis of the anticipated ARPA support, and he counted on his relationships with Thai government officials to assist in obtaining the data needed for his research. When his past and planned work became known, I was told, he was attacked by fellow faculty members and student activists, who demanded that he give up the work or resign his position. Sadly, the contract was not

* This lack of clarity was pointed out, as the original version of this book was written in the early 1970s. However, judging from the reaction of the anthropological community to the recent Minerva Initiative (noted at the end of chapter 3), the comment likely still applies to many anthropologists in 2012.

worked out after all for a variety of reasons having to do primarily with the American embassy's view of the research; and so he suffered for naught.

Still another case involved Gerald Hickey, the anthropologist who had devoted 10 years to study of the mountain tribes of Vietnam. His colleagues granted that he was sympathetic to the Montagnard, had preserved his intellectual freedom, and had probably saved the Montagnard from extinction during the ravages of the war by interceding on their behalf with the American and Vietnamese governments. Nevertheless, he was denied even an office to work on a book at his alma mater, the University of Chicago—the issue was not whether an office was due to him or available, but that he had committed the unpardonable sin of accepting DOD money through the RAND Corporation to support his work in Vietnam.[6]

These incidents make clear that many of those who expressed their concern for academic freedom meant only a very special kind, and they were not ready to grant true academic freedom to their own colleagues.

The impact of the general outlook of the activists on university facilities and in student bodies was felt keenly in indirect ways as well. At least three not-for-profit research organizations were working for ARPA in Thailand on a variety of problems in counterinsurgency, only some of which involved the social sciences. These were the Stanford Research Institute (now SRI International), the Cornell Aeronautical Laboratory (now Calspan Corporation), and the University of Michigan's Institute of Science and Technology. All three organizations were independent, but connected with their parent universities in various ways—through boards of trustees, sharing of research funds in some cases, use of faculty as consultants, and use of full-time institute staff as part-time faculty.

In 1967 and 1968, as the Vietnam War was building to the peak of American involvement, these organizations came under attack by members of the associated university faculties and activist students for contributing to "war research" and for being part of the military-industrial complex. The work they were doing in Thailand for ARPA figured strongly in these attacks for many reasons. In Students for a Democratic Society (SDS) publications, Project Agile assumed an image much like that of the CIA as an object of condemnation for events in Southeast Asia. The faculties felt that the government was corrupting their universities with classified research and especially the counterinsurgency research. In the case of the Cornell Aeronautical Laboratory (CAL), scholars of Latin American affairs at Cor-

nell University played a strong role in the attack because they reasoned that, if CAL (which was actually a separate organization in another city) were performing counterinsurgency research using the name of Cornell, this would jeopardize their own ability to do research in Latin America, a la Camelot. Thus, lengthy conflicts began that, in the case of SRI and CAL, led to breaking the organizations' connections with the associated universities. This was particularly painful in the CAL case because the separation was accompanied by a long court fight over whether the university could sell the not-for-profit laboratory to a profit-making financial combine, leading to serious erosion of the organizational integrity and key staff of the laboratory. At the University of Michigan, the Institute of Science and Technology did not separate until many years later, but it gave up much of the work that it ordinarily performed under DOD sponsorship.

All these events, most of which occurred over a two-year period in 1967–68, led to a gradual separation between the universities and ARPA in this area of research. This was reinforced by problems within ARPA's basic behavioral research program, which was distinct from Agile's program. Those problems were typified by a study program in which ARPA had funded a number of independent scholars to perform research in the Himalayan region. There was no immediate, applied objective; as part of the intention to build capability for overseas social research, ARPA had undertaken to support a number of scholars through university grants in studies of their own choice regarding the peoples of that region. Of course, it could be argued, and would undoubtedly have been true, that the DOD had ulterior motives; that, if at some time, for example, the United States became involved in a further India-China conflict, the knowledge would have applied value. But anyone's published work could be put to such use.

The fact that the program existed came to public attention when one of the scholars learned that his support came to his university from the DOD and spurned the support. Senator Fulbright noted the incident on the Senate floor.[7] The Indian government disavowed the program, and ARPA felt impelled to do so also.

The ultimate consequence of all these problems, however, was the growth of the view in ARPA as articulated by Dr. Eberhardt Rechtin, who succeeded Herzfeld as director in 1967, that research at universities on applied matters, which was classified and supported by the DOD, simply represented an incompatible set. There were, in addition, problems of another kind arising largely from the university researchers' bent for seeking DOD

funds to perform research of interest to themselves, but only incidentally of interest to the government. For example, as late as 1968, one anthropologist, who had earlier been concerned that he not come under attack by his colleagues for undertaking counterinsurgency research, indicated his willingness to accept ARPA support for an overseas linguistic study he had in mind. Therefore, the decision was made in mid-1968 that no further work of this kind would be supported by ARPA at universities.

All of this took some time, however, and while it was happening other experiments with institutions for research were also under way. One, inparticular, showed early promise of becoming a model mechanism for university scholars to undertake research of this kind if they wished to independently of the institutional conflicts that were emerging in DOD-university contractual relationships. Its genesis was some years before all these events. An independent research organization—the Simulmatics Corporation—had been formed that served as an "applied research outlet" for a number of university professors and students. It had been active in social research of the kind (but not in the places) sought by the DOD. As far as we in ARPA were concerned, a group like this had impeccable credentials and helped avoid many problems. Those who were members of the organization, such as Dr. Ithiel de Sola Pool, who had long been associated with the DOD's social research efforts, could easily attract other well-known scholars; among them were many who were experts on Vietnam, had been there before, spoke the language, and knew many of the key Vietnamese figures who could grant "access" for research. The contracting mechanism would be with a private organization, not a university; and the researchers would join it as individuals to study a particular problem.

In 1967, it was not difficult to see the trend of the university-DOD relationship, and the Simulmatics alternative appeared to be a mechanism through which such problems could be avoided while we could still have the benefit of university scholars' expertise. Therefore a substantial contract covering several research tasks in Vietnam was let, and we hoped that by this means we could build an institution that could carry on the DOD's work in the social sciences overseas. It did not work out this way, but for a set of reasons wholly different from those that might have been anticipated. These originated in the institutional constraints that will be examined in detail in the next chapter.

The first problem arose from the administration of the company and its general management of the work. Its expenditures overseas were, in the

DOD's view, loosely controlled in a pattern that was completely at odds with the stringent reporting requirements of Defense contracting agencies. Many expenditures were questioned by those agencies, such as the house or "villa" Simulmatics rented in Saigon for living and office space. (The villa itself was not an unusual thing—researchers had to have someplace to work and live. In this case, it was the "style" and the cost that upset the government auditors.) Then, after agreements to spend certain amounts of money on each task in the contract, the expenditures varied widely, apparently in keeping with the researchers' varying research priorities and opportunities, so that it was hard to keep up with what was being done on which topic, and it was clear that large "overruns" would be in the offing on at least some of the tasks. None of this is to say that the company's management was dishonest or excessively careless. It was, rather, a result of an ethical system and outlook that differed markedly from that of its sponsors. The differences cropped up in many ways, in addition to the one associated with financial management. One of the others occurred in matters of family.

In general, researchers in Vietnam agreed to go under the same conditions as those obtained for the U.S. military, which, after 1963, meant that no families were sent. All systems of ethics have their contradictions, and this one was no exception. While the military could look with equanimity on wives coming over on Vietnamese government visitor's permits and working for civilian contractors in Saigon (for example, Standard Oil or BRJ-RMK, the large construction contractor), they felt strongly that it was wrong for the wives to be employed by the company that their husbands worked for. Yet a number of attempts were made by younger members of this company to make such an arrangement. In another case, a university professor, who joined Simulmatics to perform what became an excellent study on an important subject, made his wife a member of his research team and wanted his son to join them as a research assistant. His wife was, in fact, a qualified psychologist, and the son was a social science student in college who was well qualified to perform the research assistant's tasks. The professor saw nothing wrong with this, since both could contribute to the research and, in his son's case, he would do as well as any other research assistant while the experience would be useful for his career. But, again, the DOD auditors presumed it to be a "boondoggle," and the matter had to be sent all the way to Dr. Foster, and the DDR&E, for approval. The wife was approved but the son was not; and it was clear that we would not routinely go through "one more like that."

Other problems related to those of ethics, and with other overtones of substance, arose from the very eminence of the scholars who agreed to do the research. They were very busy people accustomed to taking on several jobs at once and using graduate students and research assistants to gather data and perform analyses under supervision. This worked well in the United States where they could keep close watch on what was happening; but some of the procedure carried over into work in Vietnam, causing a stir on several levels. First, it was not easy, administratively, for them to travel between the United States and Vietnam, since a "theater clearance" was needed for each trip, and sometimes such events as the Tet Offensive in 1968 could stop civilian travel to South Vietnam for some time. Second, the DOD believed it was contracting for some months of the professors' time, thinking the time would be consecutive, and was distressed to learn that in many cases the principal investigator had planned several short trips to oversee much less senior, less expert researchers who would do most of the fieldwork. Among other things, this meant that the data gathering—the contact with foreign officials and population—would not be done primarily by the one who had been asked to do it because of his expertise, sensitivity, and "connections," but by some junior researcher unknown to us and in whom there was much less confidence.

The most serious of these problems arose in the area of "academic freedom" from a wholly unexpected (to us) direction. The scholars felt they had undertaken a contract in which they were the experts who would specify what should be done, when, where, and how. At least in part, any task was not simply a service the experts had agreed to perform, but was of professional interest that coincided with their own research and career plans. (For this reason, I believed, the researchers felt free to rearrange the funding associated with specific tasks.) When they arrived in Vietnam, they wanted to get on with the job with complete freedom of movement and freedom to see whomever it was necessary to see to get the work done. All they wanted from the government was logistic support—assistance in arranging meetings if necessary, communications, and transportation.

The view held by the military members of MACV and the military representatives of ARPA in Saigon was different. They felt, first, that they were responsible for the safety of the visitors. "Getting around" in a war-torn country involved the provision of military aircraft and ground transportation at some cost to the government and adherence to some rough schedule that depended on aircraft availability. To ensure the researcher's

safety it might be necessary in some cases to provide a military escort. Therefore, the military wanted some advanced planning about who was to be visited by whom, when, and where. This led to a demand for a certain rigidity in the research schedule that many of the social scientists from universities were not willing to countenance, since some research contact could lead in a direction that might not have been anticipated in advance.

In addition, visits with Vietnamese officials could be sensitive politically (or, at least, the official American community felt that they were), and individuals among the American military and civil authorities were not always willing to give of their time freely to the many researchers in the area unless they felt they had some stake in the study, which was rare for most of them. Thus, the military people responsible for monitoring the work wanted advance notice of such visits for the additional reason that they could help with the arrangements, clearing administrative roadblocks, and reassuring themselves and those to be visited that the visits were necessary to the studies. The social scientists viewed this as an infringement on their freedom to perform the research for which they had been contracted. While the military sponsors denied any intention to control the direction of the study, it could not be denied that they exercised a measure of judgment with respect to the answers to the question, "Why do you have to see so-and-so?" "He might be a useful source of information," which would be justification enough for the researcher, was not always so for the military contract monitor. Not only did this inevitably mean that the military were in some degree imposing their views on how the research was to be done, it reflected also a measure of concern about the view of the research that their superiors in MACV would express. Like all good bureaucrats, the military man viewed a study that had been "approved" as an obligation that he had to protect, even against the danger he perceived that the one who may originally have proposed it might destroy it by his own lack of circumspection. And so the judgments about who to see included bureaucratic as well as scientific influences that the social scientists interpreted with some justification as interference with their work.

There was no clear-cut right or wrong in all these differences; they were, rather, the clashes that could be expected between two cultures who saw the world through different eyes. All these differences led, through several research tasks, to an increasingly strained atmosphere between the Simulmatics Corporation and ARPA. This was exacerbated by the freedom with which some of the researchers talked to reporters (who pursued stories ag-

gressively in Saigon), leading to periodic press stories; in one of them, a social scientist working for one organization expressed doubt about the validity of the work of the others (neither were identified in the story, but we had a good idea who they were from the context). These articles heightened the feeling of sensitivity about the work at the upper levels of the DOD and stimulated interest in the General Accounting Office, which was starting, on congressional request, to investigate some of Agile's overseas work.

Finally, it became necessary to make a judgment as to whether the problems being created by this contract and the risks it increasingly posed to other parts of the ARPA program were worth any results that might emerge. The tensions obviously reduced the potential of the results to be useful. There had been one or two reports of unquestioned substance and value, while the remainder were controversial in terms of their scientific validity or adherence to the desired work program; they were as yet incomplete while the allotted funds had been spent; or were being challenged by officials who disclaimed their utility. Reluctantly, we decided to terminate the contract. Thus, this experimental approach to enlisting university social scientists in the research for the DOD failed, largely for bureaucratic reasons, despite all the promise it had shown at the beginning.

There is a certain irony in this situation. When this particular program was beginning, I had met with many of the group social scientists who were going to Vietnam in anticipation of some of the very problems of working in the wartime military environment that they later encountered. I had tried to impress upon them that, as social scientists, they would have to learn about and how to work with a strange (to them) social system of which they might not be fully conscious—the U.S. military in Vietnam—in order to perform research with respect to the social system in which they were most directly interested—the Vietnamese, on both sides of war. Clearly, this lesson had not been learned by any but one or two from this group of social scientists.

Thus it came about that the DOD was inexorably separated from the university social science community—even from those whose commitment to the U.S. government's purposes had not been changed by the Camelot events—and often through no obvious fault on either side. This left private research institutions as almost the only source of talent for the research the DOD desired. This was not an unmitigated good source either. Private industry on occasion hired psychologists or sociologists to work with the hardware teams; but basically people from these disciplines were alien to the

culture of this part of the "military-industrial complex," their ideas never took on the force that would have come with "critical mass," and their contributions were minimal. Most of the not-for-profit firms had trouble attracting the high-quality social science staff needed for the research in a time of problems, such as those described earlier, that some of them were having with the universities. We were left, therefore, with very few sources of first-rate talent—the RAND Corporation, which had a social science department of long standing and excellent reputation, and one or two private firms, such as the American Institutes for Research—specializing in social science research of the quantitative character we were looking for.

RAND's social science group at that time was composed largely of political scientists with a sprinkling of other disciplines, including anthropology. Many of the men in this group were "lone wolves" who preferred to pursue their own interests and ends, and it was a constant struggle to ensure that the different parts of their work remained related to each other and relevant to our needs. But some interesting and useful research, which will be examined in another context, was performed when the circumstances could be arranged that their interests coincided with those of the DOD; this occurred mostly during 1964–67, when the group that had undertaken to study the Viet Cong through prisoner and defector interviews was at its full swing. Given the disciplinary orientation of the researchers, however, much of their work had a particular methodological orientation whose validity was later criticized by others in the social science community for reasons that will be discussed in chapter 19. This lent an uncertainty to some of their results that was most troublesome in a number of contexts.

The second kind of organization generally undertook measurement-oriented research as visualized in the DSB report's recommendations and in the planning for the ARPA program. This, then, seemed the last, best hope—and it was steadily eroded by the problems of field constraints on subject matter and methodology that we shall examine in the following chapters. We also tried to hire social scientists as part of the ARPA staff for overseas research. Qualified people who were willing to join the government for a period of time could be found for this purpose. But the bureaucratic system raised a host of problems connected with salary, "job description," personnel ceilings, and the like, and it was virtually impossible to arrange for an available social scientist, who probably would have to arrange leave from a university, and a desired task to meet at the same time. Therefore, we gave up that approach as well.

It is appropriate to mention, in the context of the narrowing field of talent available to the DOD, the continual appearance of serious people who made themselves heard, but whom I came in my impatience to view as a sort of "lunatic fringe" who made the job harder by interrupting the always difficult task of trying to make sense of a research area that appeared to defy logical management endeavors. In one case, a proposal was made to establish a computerized system of extensive personal data on all of the population of Vietnam. It was thus hoped that the government would be better able to do such things as collect taxes and, in particular, to keep more effective information on the movements of the Vietnamese population, thereby helping to identify and subsequently isolate the Viet Cong from the neutral or friendly population. We had to point out, first, that this would put the United States in the position of imposing a system, a la George Orwell's *1984*, on the Vietnamese that we were unwilling on moral and ethical grounds to see imposed on our own population. In addition to this moral question were a number of particular ones. One was the anticipated difficulty the Vietnamese might have in mastering the highly sophisticated technology that would be required. Another was the difficulty of access to all the population in a war-torn country, and the subsequent injustice to many who might miss being entered into the system. Finally there were the many opportunities for graft and corruption, which the South Vietnamese were displaying enough of at the time, that would be opened by the creation of a data system subject to manipulation by those with appropriate motives and access.

In a related context, we were approached by a psychologist who expounded the theory that one of the reasons the United States was having such trouble in the war in Vietnam was that we could not get the Vietnamese government and population to behave as we wanted them to. She proposed that the methods of operant conditioning, including punishments and rewards for particular behavioral manifestations or desiderata, be applied so that eventually we could make the entire population of Vietnam and its government behave in a way that the United States decided it should. She professed to be able to induce the famous behavioral psychologist, B. F. Skinner, to go to Vietnam to direct such an effort. Again, the question was raised as to the morality of such an approach, which we certainly would not tolerate in our own country. I also pointed out that there would be far from unanimity within the United States as to what behaviors were desired. Moreover, it was not clear that if pigeons, or even individual humans, could be thus conditioned in the laboratory, this technique could be extended to

an enormously diverse population of some 14 million people with a culture alien to ours and under wartime pressures and field conditions.

The trouble with these and other spuriously clever ideas from this group was that they were also occasionally visible to others at the upper levels of the DOD. This added to the appearance of disorder and illogic in the view they held of social research, and it made the program we were interested in that much harder to justify.

Chapter 18
Working within Constraints

There was, of course, no choice but to work within the rigid system of approvals that had been established for all work overseas, especially including research on social problems. Otherwise, nothing would have been done. But acceptance of these procedures narrowed the scope and content of work that could be undertaken very considerably and had a profound adverse impact on its real value. In effect, by allotting to various external power centers the authority to say what work could be done—or, more particularly, what work could *not* be done—the DOD gave those centers rather than its central R&D planning offices or its experts on research and social systems (inhouse or contracted for) the final word in shaping the program and trying to make it useful.

The constraints on the work were never applied directly to achieve desired answers. As in the interaction between the university scholars and the military described previously, there was to all outward appearances no deliberate and conscious attempt to restrict intellectual freedom or to mold research results to preconceived notions. Rather, the influences derived from much more subtle interactions between the interests and policy premises of those who passed on the work, on the one hand, and their view of the propriety and sensitivity of the work, on the other.

As soon as an ambassador or a commanding general was given the opportunity and responsibility to judge a research proposal, he must necessarily ask himself a number of practical questions. It was well known by this time that some of the subjects of research could cause a stir. It would first have to be asked, how great was the risk of this happening? It would then have to be asked, was the subject important enough to take the risk—whether the potential utility of the results that could be obtained justified the trouble that might be caused? Inevitably, and because we are all human, a subject that fell within the ambassador's or general's or admiral's interest, with hypotheses that fit his conceptions of the problem and the likely solutions, was likely to be weighed as more important and worth more risk than a research problem that threatened to challenge those hypotheses, even though in the long run the latter might prove much more important to him. The definition of "sensitive" was thus elastic.

In addition, as far as these responsible leaders were concerned, a study was a study—ideas of scientific methodology in study of the social systems they dealt with on a practical basis every day were not within their training. If a sensitive question could be approached through an internal "staff study," this was equivalent to a scientific research project as far as they were concerned, and often preferable. The difference could be explained, but the going was uphill.

The same forces acted on the side of the host country, whose officials had their own ideas of problems and hypotheses that were interesting and important—often different from those of the Americans, and sometimes more liberal or permissive in outlook, but always oriented to their own preconceptions. And, of course, the American ambassadors and military commanders did not pass on these matters entirely by themselves, although they frequently gave them their personal attention. They were at least in part responsive to the opinions and judgments of directors of the subordinate agencies of the American mission or subordinate military staffs and commands. Therefore, the agency heads and staffs imposed their own ideas as well—sometimes advocating a piece of work and sometimes opposing it—all on different subjects at different times and for different reasons.

Any research proposal, therefore, had to fit a series of differently shaped templates, and only a few came through the tests successfully and with a significant problem left to study. Thus, the "approval" process acted also as a selection process that molded the subjects of study in safe and sympathetic directions, as far as those in the hierarchy of power and operational responsibility were concerned.

A number of interesting results emerged from this process. The "system" would obviously be more permissive about research subjects farther from its nerve centers. Approvals, therefore, were more forthcoming for work of lesser significance or importance—with the proviso that there was a lower bound, where if the work were insufficiently important it would be ruled out as a waste of resources. This forced the research community, which was always pitting its own knowledge and expertise on the country and subject against that gained by different means of the operating community, to tread a fine line between narrow bounds in defining and proposing its research subjects. They could be neither very sensitive and, therefore, probably of vital importance, nor of purely academic interest. Further, since all those involved in the approval process had different ideas and desires, the researchers would approach one and then another, seeking paths of support

and indifference; trying to avoid or soothe opposition; and trying to gain advocates who could influence the ultimate decision. Thus, the definition of research programs departed from objectivity, science, and analysis and became in large measure a political negotiating process whose outcome was as uncertain as the outcomes of all other such activities. It was impossible under these circumstances to define and carry through a coherent and logical research program where all of those parts were interconnected and mutually supporting. The program was somewhat more orderly than random, but it was far from the logically planned sequence of studies in specific, interrelated subject areas that would satisfy the scientific mind.

Another consequence of giving the power of life and death over all research to operational authorities resulted from the fact that individual research projects often did produce interesting results. The research resources overseas were in effect the resources of the authorities to manipulate, and they knew it. Therefore, particular research projects in areas of interest to ambassadors, agency heads, or military staffs and commanders would be requested by them. Knowing where the real power was, we usually agreed to undertake projects requested this way. The subjects of the requests were likely to be reasonably important in most cases. An applied social research program in an area of conflict *should* be useful; and it would always be helpful to be able to tell Congress or the GAO that a project was undertaken at an ambassador's or CINC's request rather than on the researchers' initiative.* But this put more pressure on the fragile and relatively scarce resources, fragmented the program still further, stimulated a certain process of "horse-trading" which politicized the research planning even more, and consequently created underlying tensions that arose to haunt us at inconvenient times.

This continual negotiation for research approvals and resources forced the individuals in the research community to involve themselves in numerous alliances with American and local government officials. As a result, projects came to depend on the interests of those officials. Given the frequent turnover in overseas posts—changes of ambassadors, changes of military commanders, the annual rotation of officers in Vietnam—many a project found itself in midstream, partly completed with the new officials wondering why that particular effort had been undertaken in the first place. Some significant fraction of projects was terminated prematurely and in-

* We used to keep a "Gabriel" file of such requests and of post-research thanks and commendations to flash at appropriate times.

complete, and the utility of those that were completed was left very much in question. And all of the negotiations took time. As the situation became more complex and more and more actors came on the scene, the time between initial proposal and final approval to proceed stretched out until it was a year and sometimes more. In the rapidly moving events of Southeast Asia, the answer to a question that might take six months or a year to obtain through research was often desired and needed when the question was put. By the time the resulting research project was approved (except for those explicitly requested by the ambassador or commander himself), the question might well be passé—and even more so by the time the research was finished. No wonder the new men on the scene questioned its existence and had low tolerance for its continuation.

These, then, were the research planning and administration problems that accompanied the determination and necessity to "go by the book." Let me illustrate a few cases.

Two areas of effort that had been singled out for attention in the earlier advisory reports encompassed the problems of military advisors—in effect, the implementers of policies determined elsewhere and military civic action—the attempt to use a nation's armed forces in nation building rather than war. Two years after Camelot, these still seemed to be key problems, and ARPA decided to try to undertake coherent, long-term research programs in each area through a cooperative effort with the Army.

On the advisor question, it seemed impossible to hope that thousands of men could be found each year who would perform like Lawrence of Arabia. The evidence from many overseas areas was that only relatively few men really succeeded in establishing the relationship that allowed them to be effective in "advising," training, and furthering American policy. A few earlier studies had hinted at the circumstances of success; but it seemed still to be a matter of accident that the right man was found in the right place to the right time. To a great extent, it seemed to be more a matter of personality than training. What kind of person, we asked ourselves, was able to fit into a strange environment and establish an effective relationship with a counterpart from another culture? What were the criteria according to which a man could be predicted to achieve success in the job; and once he was selected, how could he best be trained for the job? And by what criteria would it be known that he succeeded? Because he was well regarded by his counterparts? But suppose they did not learn well? What if he made enemies, but his counterparts learned well what he had been assigned to teach?

A proposed research program was built around such questions as these, and we set about obtaining the approval of the authorities. Part of the data obtained would have to include job performance ratings of many advisors to be correlated with personality profiles, background, and training data. At this point, the military commanders overseas balked. They would not allow probing into efficiency reports or interviews with their advisory staffs despite any amount of explanation about the research design or preservation of anonymity in the results. One MAAG chief expressed the opinion that he could pick a potentially successful advisor in one interview. Maybe he could, but we knew some of his staff. However, the attitude was typical. The program was never undertaken.

One small project in this area was initiated, however, in Thailand, where the chief of the Military Assistance Advisory Group was interested in improving relations between his men and their counterparts. The research design required comparative interviews with the American advisors and with the Thai military whom they were advising. Therefore, the Thai military establishment was brought into the discussions. As the result was expressed by one of their generals, "We think it's fine if you study what's wrong with your people, but we know what we think of our relations with you so there's no need to talk to our people." And permission for that part of the project was denied. The project was carried out by interviewing Americans only, and it was changed to a one-sided effort in support of American training for community relations. The original objective had been lost.

The civic action effort failed for a different reason. The philosophy of civic action was that if the military forces of a government have contributed to local development, this will create a benevolent image which will, in turn, be transferred to the government and established authority as a whole. The act of development assistance would, further, help make the government more benevolent, and the local rural people would be less inclined to succumb to the blandishments of insurgents and revolutionaries. Finally, the civic action would help develop the backward areas of the country, a good result per se. While all this was "doctrine" to the American military assistance advisory corps, it seemed to the research community to be more in the nature of hypotheses to be tested and proven or disproven by research. Some previous, isolated projects (Project Colony, described in chapter 11, was one) and informal discussions with many advisors overseas had suggested that the desired transfer of affections only sometimes took place—that it often depended on particular circumstances, culture, history, and on how the de-

velopment projects were carried out; whereas to the American military, the important matter was *what was done*—build a road, or a well, or a school—not who wanted it and who used it. There were many anecdotes of civic action "demonstration" projects that led to raised, and later disappointed, expectations; of projects undertaken because they made sense in one culture but were ignored or counterproductive in another; and of claims for success that went far beyond what a searching study of the facts would warrant.

A program of research was planned to find out whether the whole idea of civic action made sense and, if so, under what circumstances it would and when it would not. To do this, it was necessary to gather data systematically on many projects and to ask some searching questions. But then the premises and "doctrines" of the U.S. military advisory staffs would be open to question. We learned early that statistics on civic action projects that were much touted were rudimentary or nonexistent. I was once told by a MAAG officer in Iran about an "enormously successful" road-building program by the local army unit. How many miles of road had been built, I wanted to know, and where, and what were the roads used for? There were no answers. Moreover, we found that local governments were not always anxious to learn that populations who were recipients of what the government believed to be largesse might, in fact, be more dissatisfied than they had been before the largesse was distributed. So this research, too, was not undertaken. An understanding of one of the fundamental premises of American counterinsurgency theory, and of the conditions causing many apparent successes and failures of military civic action in developing countries, had to be left to a few isolated, tantalizing studies and to many anecdotal, incidental, and unverifiable reports.

Another instance of the effect of sensitivity to the subject and possible findings of social research occurred in an attempt to learn something about the impact of the presence of American bases on local attitudes and cultural change in Thailand. With the lesson of Vietnam that such a presence could have devastating effects, including inflation, destruction of traditional cultural life, and disruption of local government authority over its citizens, the problem appeared a serious one. It was discussed with Ambassador Graham Martin, with the idea that a serious study using social research techniques might elicit enough about the nature of the dynamic interactions between and within the respective communities—American and the local population—to point the way to solutions that would minimize the deleterious effects of the interactions and the inter- as well as intracultural tensions.

Ambassador Martin's response was that he knew perfectly well what would have to be done to minimize the effects of the American presence. Any such study might simply serve to critique or interfere with his approach and possibly heighten Thai sensitivity to the problem, none of which he was interested in having happen. Ambassador Martin then went to a new assignment and his successor, Ambassador Leonard Unger, agreed that this was one of the most important problems he faced. But he, too, pointed out its sensitivity. He would agree to a study if one could be defined that would not stir up local government resentments and would not obviously interfere with the outward tranquility of either community.

While we set about eliciting a research plan and proposal from a contract research organization, an unsolicited proposal in the same area for research to be undertaken in the same country came to us by coincidence from a university source. This resulted from the initiation of Project Themis—a program to improve the research capability of the smaller universities—for which President Johnson, with congressional urging, had instructed several departments of government, including the DOD, to allocate funds. A system of reviewing Themis proposals had been established by DDR&E, and one dealing with the impact of a large American presence on a traditional, developing society, submitted by Felix Moos, a well-known social scientist knowledgeable about both the country and the appropriate research techniques, had come to our attention. It appeared interesting, and some tentative inquiries were made of the ambassador. It became immediately clear that the subject was considered too sensitive to risk the possible consequences of the kind of free access and movement that university researchers would demand, and the idea was dropped.

In the meantime, the contract research organization had taken some time to develop a plan for the research. When this was finally discussed with the ambassador, we learned that after our initial conversations he had asked one of his political officers to study the question and make recommendations; the subject was now closed to inquiry by the research community. Doubtless the political officer's analysis (which I did not see) was anecdotal and terribly unscientific. But it may well have been perceptive and perspicacious. Certainly it satisfied the ambassador's need for discretion and unobtrusive inquiry. In the long run, the impact of the American presence was far less disturbing in this country than in Vietnam, even though it was not totally unobtrusive. Clearly, this time, the tradeoff between the desire to minimize risk and the need or desire for detailed information was

resolved in favor of the former and, from the evidence, with reasonable success anyway.

It had been found that many of the studies undertaken in Vietnam had produced highly interesting results. Studies of Viet Cong origins, organizations, and patterns of behavior in the villages; of the origin and problems of refugees from the war; of the effectiveness of the paramilitary regional and popular forces: and of problems faced by American advisors had given the American military and civil authorities in Saigon information and understanding they did not have and were glad to obtain. For a time, in late 1966 and early to mid-1967, such studies reached a peak of acceptance and popularity with those authorities. Two consequences of this success were that the military command more frequently requested studies of particular interest to them, and that the research community felt emboldened to propose studies that, it turned out, transcended the bounds of political acceptability. The effect in both cases was disastrous, and social research in Vietnam rapidly passed the heyday of its support by the operational community.*

In one case, we were asked to undertake a study of psychological warfare in Vietnam with specific attention to three questions: the potential vulnerabilities of the Viet Cong to psychological warfare; what programs to undertake to exploit these vulnerabilities; and how it would be known that they were effective. The query came from General Westmoreland directly, and a rapid but careful response was in order. Rather than go through lengthy contracting procedures, we assembled a group of the country's best-known experts on psychological warfare and counterinsurgency, added the support of military "psywar" specialists, and asked the group to accomplish the study in a short time.

This group (including the military members who were easily convinced) felt that if the questions were interpreted narrowly they would be too restrictive and that the study would have little value. To provide satisfactory answers to specific questions about psychological warfare, they felt they had to explore the entire area of the impact of military operations by both sides on each other and on the Vietnamese population. The group pointed out, for example, that it did little good to paint a stark picture on leaflets of the Viet Cong as vicious monsters (a common practice) when the recipients of

* Economic research is not included here; such research was initiated during this period and continued to be useful and important to the American and Vietnamese planning efforts until the collapse.

the leaflets knew many of the Viet Cong as their relatives and friends and knew that the issues were not clearly black or white, but gray and complex. And they indicated that any attempts of the Saigon government to say that it acted in the best interests of the population would be negated if the conduct of the war, with such activities as bombing of villages in response to Viet Cong ground fire (even though there might be a deliberate effort to draw fire), were destructive of the population and its possessions. Therefore, the specific questions were answered within this broader context, and a number of suggestions, both specific and general, were made for improving psychological operations and for affecting the enemy's and the neutral population's attitudes by changing the conduct of the war.

The report ran into a multiple buzz saw. The broad approach had been approved by General Westmoreland when it was discussed with him but general supervision of the study had been delegated to lower staff levels. Many of those who had stimulated the question and encouraged the approach that was taken, such as Barry Zorthian, the head of the Joint U.S. Public Affairs Office (JUSPAO), were gone, and there were new men in their places. Not only did the report deal with some subjects on which the collective military command did not want the opinions of civilian experts, but its specific recommendations, which included proposals for organizational changes that appeared essential if the psychological warfare effort were to be made more effective, cut across existing rivalries between the military (MACV) and the civilian (JUSPAO) psychological warfare practitioners. The analyses and recommendations highlighted shortcomings in the operations of both and in their interaction and coordination. Neither group would consider them seriously. In fact, the new arrivals on the scene, who had already started to make changes according to their own ideas, viewed these new inputs that reached them, apparently "from the blue," as gratuitous and inappropriate. It was said in the letter General Westmoreland signed on the subject, as it often is in such situations, that the report was not really responsive to the request; but it was overtaken by events and "we're already doing what it recommends." And that was the end of it, except for a marked increase in resistance to the idea of further research.

But the subject of the use of artillery and aerial bombing within Vietnam remained a troublesome one. Virtually all of the social scientists who were in contact with the population had come to the conclusion, not based on study but rather on scattered conversations with villagers and with some American advisors, that more was being lost in terms of loyalty and respect

of the population for the Saigon government and the Americans than was being gained in hurting the Viet Cong by bombing and shelling of villages—even where it was known that they were Viet Cong strongholds or where Viet Cong attacks against allied forces were deliberately based in and mounted from the villages. Ithiel de Sola Pool, who had long worked with and had excellent standing with the military community both in Washington and Saigon, broached the subject of performing an extensive field study, using interview techniques, of the "rules of engagement." This would try to determine, first, the impact of operations—air and ground—on the general population and on the Viet Cong. Then it would try to make an objective assessment of what was being accomplished militarily against the Viet Cong under the rules as they were. Finally, changes would be recommended for consideration that, based on the data, would be expected to reduce the negative impact on the general population but still maintain, or perhaps increase, the effectiveness of operations against the military forces of the Viet Cong.

This was a technically ambitious scheme and, perhaps, impossible to implement. But it dealt with one of the key issues in the prosecution of the war and, for that reason alone, it might have been worth serious attention and possibly a pilot study to test feasibility if only to make certain that the question was explicitly considered. General Westmoreland, however, made it clear when the proposed project was discussed with him that the subject was not for study, and the matter was dropped.

The kinds of studies that were undertaken successfully in Vietnam generally fit the pattern of operations and philosophy for prosecution of the war that were common among the military and civilian authorities in both Washington and Saigon at the time. Some of these, such as the "VC Motivation and Morale" studies, have already been mentioned. Others included support of the anthropologist Gerald Hickey in gathering information about the tribal groups of the Central Highlands, which was used to assist in trying to effect a reconciliation of the ancient enmity between those groups and the ethnic Vietnamese; assistance in designing and implementing the Hamlet Evaluation System; establishment of computerized systems for storing and comparing troop performance data taken at different times; logistic analyses; and the like. All this work presented problems in scientific research methodology that those who reflected on it came to feel was generally inadequate and was leading to uncertain and possibly misleading results. These problems will be examined in more detail in the

next chapter. The point to be made here is that a theater of war offered little opportunity for careful adaptation and testing of research methods suited to the situation and the local cultures. Such things take time, and results were wanted quickly.

Those who approved the research programs assumed that methods existed to carry them out. Satisfactory outcomes could not be promised from new and experimental methodological departures; and the researchers, until they gained some experience, tended to assume that the methodology they knew would be readily applicable. But, in fact, each study was in effect an experiment with methodology. While some few social scientists appreciated this early and sometimes adapted successfully, we began to realize that a more orderly and deliberate attempt to improve the scientific basis of the research was in order. The war environment was a poor place or such an effort—in terms of access to subjects, control of comparative situations, stability of situations, or any other desiderata for careful scientific work. Therefore, at the same time that work directly relevant to the war was undertaken in Vietnam, an attempt was made to pursue the longer term and "iffy" experimentation with methodology elsewhere.

But the "official" climate "elsewhere" was scarcely more conducive to such undertakings. In one case, we attempted to apply a new method of eliciting the basic values and socio-cultural attitudes of a population in a situation of low-level insurgency in Thailand, where it appeared that if village culture and the changes affecting it were better understood the government could be more effective in improving the lot of its people. It would seem on the face of it that such values and attitudes would be obvious, at least to the countrymen of the villagers. But deeper reflection shows that most groups do not easily articulate their values and cultural orientations, and that their attitudes often appear only in actions bespeaking tensions between groups and antagonism toward those in authority. One of the causes of violence in social change appears to arise precisely from such divergences between those in authority and the groups they are supposed to govern, since the two are likely to have divergent cultural and social backgrounds. There could, for example, be as much cultural distance between a wealthy, Western-educated Vietnamese, or Thai, or Indian, and the peasant villager in his country as there is between that member of the elite and his Western counterpart.

A standard approach to eliciting social patterns, norms, and values is through the work of anthropologists, who live with the respective groups

and come to understand them intimately over periods of years. Obviously, if information were desired much sooner, other methods would have to be tried. One could, of course, ask for the information from anthropologists who had been working in the area for a long time previously, and this was done. But in periods of rapid change of culture, attitude, values, and in multiple interacting communities, even an anthropologist might not have been at all the places where such changes had taken place, and where it was important to know about them and about their subtle variations.

Thus, it would be valuable to have a method for assessing values and attitudes in a new area without reliance on the happy coincidence of prior anthropological study or the need to wait years for new study. A novel method for studying the values and attitudes of groups or subpopulations had been devised in another context by the experimental social psychologist, Alex Bavelas.* At ARPA's urging, a contract research firm proposed to adapt the method to basic attitudinal studies in Thailand. Known as Echo, the technique called for obtaining a sample from the population of interest of answers to such questions as "List ten good things that can happen to you"; or ". . . .that you could do"; and "list ten bad things"; etc. A group from the same culture then placed the answers in like categories (e.g., "getting an education is a good thing" might appear in several forms easily recognized by one who shared the view) and ranked them according to frequency of appearance. From these data, it would be possible to construct a picture of the values of the population, uncontaminated by the values of the researcher—what, in general, they felt was good, bad, or important. Comparative tests of the method with groups of a given population, asking them to express preference between the set of values obtained from other members of their population by the Echo technique and another set of values obtained from another population, confirmed that the set elicited from the test population was more satisfying to the population it described, permitting the inference that these were the values of that population.

Much work was necessary if the techniques were to be applied across language and cultural barriers, and if it were to be extended to cover a broader range of attitudinal data; it was not even certain that the "target"

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* At the time of these events, Dr. Bavelas, who was a professor of psychology at Stanford University's Graduate School of Business, had specialized in group psychology. During his career, he explored, among other things, patterns of communication in task-oriented groups and had devised a number of extraordinarily ingenious experiments in group psychology to trace paths and patterns of communication under various conditions.

population could accept and respond to Western conceptions of answering such questions and grouping the answers in categories. Much more difficult would be the problem of extending the technique from simple elicitation of values to such detailed and subtle questions as attitudes toward certain authorities, or responses toward specific actions of particular social groups or programs. If it were successful, however, the method might offer a rapid sampling technique more subtle, discriminating, and accurate than the conventional opinion surveys, which could well be biased or misleading, but that the operational community was always ready to accept.

Some preliminary work in developing and testing of the technique was done in the United States, and a proposal was made to try to develop it for practical application in Thailand. The proposal was greeted with official skepticism, but the ambassador reluctantly agreed that preliminary tests could be made with a sample population of Thai nationals who worked for the American community. This was undertaken, and the initial results were encouraging. However, several untoward circumstances began to converge. More money would have had to be added to continue the project at the anticipated scale. But U.S. stakeholders clearly did not view it in a favorable light, and there was serious question as to whether it would be allowed to go further. The principal investigator, violating the rules he had agreed to abide by, undertook exploratory discussions with members of the Thai university community in Bangkok; while they were interested, their official community—the Ministry of Defense, under whose cognizance all ARPA work in the country fell—neither understood nor shared that interest. Rather, it questioned whether a research project was being undertaken without prior approval. And the growing congressional scrutiny was reaching the point where the preliminary methodological work in the United States, essentially basic research using university students, would defy acceptable explanation. Therefore, we decided not to pursue the effort further, just at the point where some payoff might have been in sight.

One of the key factors in this decision was our interest in another project, which to us appeared much more important and for which it was decided that it would be preferable to risk what little credit remained for social research. We have already noted, in many different contexts, the view that the social sciences had not yet learned to measure accurately the social behavior of groups of people. Most of the techniques available require interaction between the researcher and the subjects of the research. The fact of the study, its subject, and the presence of the researchers all affect the so-

cial system studied in ways that are uncertain and difficult to assess. We had already become concerned about the validity of some of the prisoner interview results in Vietnam where, in at least some of the cases, there was reason to suspect that the outcomes of interviews were in the direction the interviewee believed the interviewer desired. Thus, it seemed that the Heisenberg principle of physics—the instrument of measurement affects what is being measured—applied in social research, perhaps even more markedly.

We concluded that it would be highly desirable to develop, for the situations of concern, what Webb had termed "unobtrusive measures."[1] We wished to see whether, instead of having the researcher interact directly with the social system under study, it would be possible to discern attitudes and responses to events, government officialdom, and government programs from behavior that could be observed indirectly or at a distance. Such an approach is not new, but had not been applied in a non-Western village culture that was in the process of modernization and in the beginning throes of revolutionary political change through guerrilla warfare.

The task would require successive steps of research beginning with the known interactive methods and gradually changing them, comparing the results from each step with those from the previous one, until analysis based on such data as routinely gathered village statistics or provincial archives could be tested for interpretation of attitudinal and culture trends in the village society. We wished to test whether it would be possible, for example, to find from such data whether young men were leaving the village and where they were reappearing, since this would say something about the drift of traditional family patterns; it might be possible to do this from routine census data. Or, the record of interaction with officialdom for routine business purposes might be correlated with particular economic changes described in other statistics. Would any of these economic and demographic changes correlate with other social change, such as variations in religious practice? And, if so, what could all of it tell about the day-to-day life of a society in transition? In particular, would it be possible to show that routinely gathered statistics and other "neutral" observations were correlated with particular attitudes toward local government, institutions, and behavior that bespoke instabilities of fundamental character in society?

It was conceded that this would be an extremely difficult task. But the methodological problem converged with a practical one—the budding insurgency in Northeast Thailand. This was replete with the usual factors of

injustice within the society and stressful interactions among the rich and the poor, the young and the old, in a situation of rapid social change. And there was apparent political stimulation, training, and material support for the insurgents from Laos and North Vietnam, across Thailand's northeastern borders. There were the usual village propaganda meetings, raids on police posts, and assassinations of government officials. The Royal Thai Government, with diverse political, economic, and social advice, with American financial help, and with American military assistance, had undertaken a large number of programs to provide physical security in the villages, local economic development, community development, modification of the political system, and paramilitary training for local defense forces. The approach was a combination of trying to improve economic conditions, trying to provide the villages with some means of defending themselves against guerrilla raids, changing police and military procedures to make them at once more effective against the insurgents and less irritating to the general populace, and amnesty for guerrillas who returned voluntarily to the fold—all in an attempt to convince both the general population and the insurgents to give their loyalty to king and country.

But the problem was, precisely, that no one knew whether the multiplicity of programs undertaken by the Thai government was satisfying the population or not, whether it was meeting their aspirations or frustrating them, whether it was gaining their loyalty or driving them deeper into revolution. As is usually the case, these activities were measured by physical inputs and outputs—how many roads were built, how many wells were dug, how much the crop harvest increased, how many village police were trained, and so on. The crucial questions of attitude and aspiration, which would determine the outcome in the long run, were anybody's guess.

In one Northeast Thai province, for example, the governor became famous for a vigorous program of development associated with amnesty for terrorists, and it appeared that the number of insurgent incidents was declining markedly. He was the hero of the American community. But a native Thai political scientist, who undertook a study of village attitudes with ARPA support, said he found evidence that the villagers were unhappy because they felt that in order to get any attention from the governor one had to become a Communist terrorist first. Was this "normal" grousing, or did it bespeak a volcano rumbling under the surface? There were as many judgments on this as there were people who considered it. This was just one of many indicators that better means were needed for such assessments.

We decided to combine the practical and methodological problems to see whether it would be possible to learn more about which programs were successful and which were not, so that the government could plan its moves based on more accurate feedback from past efforts than it had been able to obtain thus far. At the same time, the attempt would be made to develop newer, unobtrusive, and easily applied techniques for making such assessments. This was to be a major undertaking planned as part of the Rural Security Systems Program (see pg. 202) that would last five years, which we called Program Impact Assessment. While Americans would initiate the task, it was planned so that the entire responsibility for assessment of government programs would become that of the Thai government and would be supported by a trained staff of Thai researchers who would be distributed as appropriate among the government agencies involved. This means that the staff would have to be educated and trained in methods of social research. It was planned that a cadre of American- and European-trained Thai social scientists would be involved from the beginning. Hopefully, they would be responsible for all the planning and the work by the end of the third year, and the Americans associated with the project would become advisory and supportive.*

This ambitious effort faced three major hurdles beyond those of undertaking and developing the technical aspects of the project. First, Ambassador Unger was very cautious. While he appreciated the methodological problem, this was of relatively minor concern to him. What concerned him most was the difficulty and risk of having American researchers who were

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* This project was one of the prime targets of the agitation and adverse publicity regarding participants of university social scientists that was described earlier. Its morality was questioned; the project staff were accused of working with American imperialism and reactionary local government to find ways to suppress the population. The accusation was made that the sophisticated approach would help the government repress an already underdeveloped and underprivileged peasantry. There is, of course, always the question of how the results of research will be used. In this case, the members of the research community who were involved were well aware that providing data on popular attitudes and responses to programs could have a negative result. However, detailed knowledge of Thai society indicated that, in many respects, the government, while it was an oligarchy, was rather more flexible than most such governments in responding to the demands of external events as they were expressed through popular behavior. The *hope* was that, in examining very closely how the population was responding to particular programs and in trying to devise programs that would cause responses of satisfaction and popular support for what the government was trying to do, the government itself would become more closely involved with its population, starting a cycle from which there would be no return and in which the relationship between government and governed might evolve in mutually beneficial directions. Obviously, there was no certainty that this would happen. It is clearly a matter of individual judgment and decisions as to whether one believes it is worth making the attempt or that the attempt, once made, will have the desired outcome.

outside the official American community in a position to probe into the details of local government programs that, unavoidably, would have to involve some analysis of local politics and the American mission's interaction with those politics.

Second, it was not all certain that local Thai politicians would be in a position or have the desire, against their vested interests, to change programs if they found them to be ineffective or counterproductive. This would also be true of the leaders of the American mission agencies, such as AID's U.S. Operating Mission (USOM) in Bangkok, which was responsible for supporting most of the Thai security-related development programs, since they often had some stake in any local program, having urged its implementation, having helped to plan and fund it, and in some cases having conceived it and then negotiated long and hard to have it accepted. It was quite possible that in these circumstances both local and American officials would have preferred not to know of the faults and failures of the programs if these were identified by the research studies, and would ignore them or find ways to attack the validity of the research if results of this character emerged. On the other side, we saw evidence that the local Thai social scientists were likely to be young and idealistic, so that they might prove fertile ground for revolutionary ideas and actions if indeed the above outcome should come to pass.

The third major problem occurred within the DOD itself. In keeping the character of insurgency and counterinsurgency, which represented a type of warfare departing far from and encompassing much more than purely military affairs, most of the specific research in the Program Impact Assessment effort was to deal with the economic and social development aspects of government actions rather than with the military aspects. This raised the question of whether the work was appropriate for the DOD to undertake and whether it could be defended before Congress, since we were making every effort to assure direct and obvious relevance to DOD's mission and responsibility.

To resolve the last problem, all the written material on the program was given a military conflict-oriented cast, which distorted it from the beginning. We tried to minimize the distortions, but did not feel able to describe freely what was intended in all its subtle ramifications; and this had adverse consequences later. It was the language of defense-oriented counterinsurgency, stressing internal security problems and inhibition or modification of armed rebellion, that the university community, when attacking

this program, picked up and perhaps justifiably gave the negative interpretation that they did.

The first two problems were closely interlinked. From the research point of view, the question about the potential response to negative results could only be answered in the doing—by obtaining specific results and seeing what effect they had. But this approach did not suit the ambassador all at once. He would, I am sure, have preferred that the proposal not be made at all. He agreed to have one American social scientist, Dr. Paul Schwartz of the nonprofit American Institutes for Research, who was to be the leader of the research group, come to the country and work in the embassy, more completely defining the program and working through the official American community to probe delicately toward the local government agencies and obtain their reaction, based on specifics rather than on generalities they were not likely to understand. If successful, then more American personnel could appear, and the program could be advanced slowly.

It is a tribute to this principal investigator's ability to grapple successfully with real-world problems that, after three months, the ambassador was urged by key members of his staff and agreed to give the go-ahead for a broader and more intensive effort. The program moved within six to eight months from dealing with innocuous subjects far removed from the substance of the counterinsurgency problem to evaluating some of the action programs that were at the heart of the local government's social development and counterinsurgency effort, such as community development, police training, and road building, using indigenous Thai researchers and with the full support of the Thai government agencies responsible for those programs.

This sounds like, and it was, one of the few success stories in this rather gloomy narrative. However, its path was not easy. Despite the great attention to "doing it right," the program suffered from the problems outlined earlier and from others as well. One of the first and most difficult tasks was to help the Thai government officials understand the nature and potential value of the work. The first year of effort devoted much attention to this with the consequence that methodological soundness, and its attending time and complexity, was sacrificed in favor of early results that would demonstrate the principle of evaluative research. Essentially, then, the first year and a half was devoted to the opening phase. It would be another two years (1969–71) before it would be possible to tell whether the work could be undertaken with the necessary sophistication and would have the desired impact.

In the meantime, the usual turnover of official personnel continued (including my own final departure from the Pentagon in September 1969). That was also the time when Senator Mansfield persuaded Congress to write into law the conception of "relevance" for DOD research that had been one of the principles of operation for ARPA's social research program. And, of course, the principle then took on the rigidity of enforcement that distinguishes legal requirements from the flexibility of voluntary action. As a result of this pressure, and with no strong advocate for social research remaining, the ARPA program succumbed, consciously or not, to the general trends affecting all research having to do with counterinsurgency and social systems. Thus, the planned five-year program, after two years and just at the point where payoff was imminent—or at least at the point when one might determine whether any payoff was in the offing and how it might be brought about—appeared as though it, too, would die. A successful effort was made to have the AID mission in Thailand assume support of the project. Since the programs that were being evaluated under the Program Impact Assessment effort were largely under AID cognizance, this appeared appropriate. Shortly after this was agreed—over a year after it was decided that ARPA could no longer support the work—Congress reduced the AID program. In addition, the report of a presidential commission recommended disbanding AID and the distribution of its functions to other agencies.[2] The research continued into 1973, however, gradually moving along the path initially charted for it. At any stage of its existence, it was not certain that it would enter the next stage. The methodological developments were advanced, and the Thai government has gradually been assuming responsibility for the evaluative activities, so that the project itself succeeded. However, the ultimate question—whether the work has had the desired impact—still cannot be answered.*[3]

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* This was written in the initial draft of this book, and it remains true today. An informal history of the program that I wrote in 1970 after leaving the Pentagon concluded that only two aspects of the program could definitely be termed successful to some degree: (a) the creation of a border control mechanism that prevented infiltration across the Mekong River in the northeast—a relatively simple task because boats on the river could be detected by radar and then inspected by river patrols to try to ascertain whether their occupants were farmers crossing to market their products or infiltrators in disguise; and (b) the development of a progress measurement system based largely on population activity statistics that the Thai government was willing and able to implement. I am unaware of any indicators as to whether these results persisted beyond the end of the large American presence during the Vietnam War or whether, as stated by General MacArthur about old soldiers, they simply faded away as the American military effort in Southeast Asia came to an end. One major conclusion drawn privately by the ARPA participants in the program (myself included) was that trying to undertake a rigorous "systems approach" to a politically charged societal problem in a wartime environment had proven impractical. It remains for a future evaluation of the results of the Minerva Initiative to learn whether that conclusion remains true.

Thus, again, an attempt to undertake research work requiring long-term program development and stability was distorted and interrupted by short-term and extraneous considerations having nothing to do with the merits or requirements of the work. The pace of events in the real world and the time required for exacting and careful scientific work appear incompatible to say the least.

Chapter 19
"Technical" Problems

All of the issues dealt with thus far have had to do with defining the problems on which research studies could be performed and with obtaining the agreement of the entire "system" that these problems should be studied. The next important step was to gather data in the field based on sound research designs and to extract useful and reliable results from those data by means of sound scientific analysis. The research designs and the analyses had to be planned together, of course, because no amount of analysis could yield valid and reliable results if the research design did not provide for the necessary and appropriate data to be taken. The constraints of field operations profoundly influenced the research community's ability to fulfill this, its raison d'être and supposed stock-in-trade.

A number of adaptations of American social science methodology had to be made to work across cultures. To obtain the information on attitudes toward events and on aspirations that was desired from local subjects of research, it was first necessary to determine whether the "instruments" of measurement were valid in other cultures. A number of techniques for cross-cultural research—for example, Hadley Cantril's self-rating scales[1] and Bevelas' Echo technique, already described—existed that would have to be redeveloped for and tested in the specific cultures where they were to be used.

These were used occasionally; and Phillip Worchel of the University of Texas was also able to adapt such methods as the thematic apperception tests for use in Vietnamese culture. Primarily, however, straightforward interviews were used, and the value of having social scientists undertake the interviews appeared to lie mainly in the fact that some of them—those with appropriate training in psychology—could structure the interviews in a neutral way so that more information *could* be obtained than would be obtained by a layman, and it would be obtained in a form that could be checked for internal consistency. But even this was often not done well, especially when the researchers were of disciplines, such as traditional political science, that had not especially trained them in the necessary approaches.

One innovation, a matter of necessity, made most of the studies possible in the first place. However knowledgeable the research professionals might be about the countries, their histories, their people, and contempo-

rary events, most of those who were anxious to undertake this work (the anthropologists were the primary exceptions) had not been there and could not speak the language nor did they have the deep and intuitive feeling for the culture that would accompany real fluency in the language. Some attempted to learn the language but, in the few months or year available to them for language training prior to undertaking research, they could only learn enough language for normal pleasantries and to request necessities of life, at best.[2] Almost all the researchers, therefore, turned as a matter of necessity to the most ready expedient—the use of intermediaries who were bilingual local nationals—for administering questionnaires and assistance in reduction of the raw data represented by the responses.

This approach had two main advantages in addition to expediting the performance of the studies. American researchers who were new to the local scene had much to learn "on the job" about the local culture and background, and their interaction with their indigenous research assistants helped to accelerate the process markedly. In general, the assistants were university students, or even graduates often in the social sciences, and they were quick to understand what was wanted and needed. Further, when the questionnaires were taken to the field to be administered, the foreign presence of an American researcher, which could be expected to influence the respondent's attitudes and answers, could be minimized or eliminated.

There was, however, another side to this. First, the bilingual capability of the research assistants was highly varied, so that it was not clear what effects their translation and retranslation of questions and responses between two languages would have or what variability would be introduced into the results thereby. These biases were almost never themselves the subject of investigation (e.g., by having other individuals translate back to English a question that had been rendered from English to Vietnamese, for comparison with the original). The expedient was essential if any work were to be done at all in the time available, and other pressures on the research designs inhibited attention to what appeared at the time to be a fine detail.

Then, it would nevertheless be known to the respondent that his questioner was somehow connected with the government (because only the government in most places would undertake such studies),* and the indirect connection with the Americans could be inferred. In one case, a local social

* In some cases, where local universities' scholars undertook social studies on their own, the local press accused the government of having inspired them clandestinely.

scientist with excellent credentials criticized an entire area of research because, he said, the respondents were slanting their answers to questions according to what they felt the government wanted to hear. He offered to undertake a replication of some of the research, since he was not connected with the government, to illustrate what the differences in the responses would be. Some further confirmation that he might be right was offered by comments of an American anthropologist in the area who lived among the people and spoke the language fluently and understood the culture intimately.

The experiment appeared worthwhile. But it was never undertaken for reasons that were largely political, and that illustrated well the further problems of working even with the most highly qualified indigenous social scientists. For it was found with experience that, while the idea of separating politics from scientific objectivity is deeply ingrained in Western philosophy, it was much less firmly held in the non-Western cultures with which our program interacted.* In almost every case, it was found that the local social scientists wanted to use their research either to provide data that could be used for political purposes (which therefore made their "objective" analysis of the data suspect), or to use the fact of their working on a subject related to affairs of state as a stepping tone to political power. Thus, the desirable trend, which appeared a priori "good," toward involving qualified local nationals in social research that was intended to be objective had its own built-in limitations.

The combination of the press of time, sensitivity of research subjects, and wartime constraints and dislocations all interfered with the planning and implementation of scientifically sound research designs. In a detailed analysis of most of the research studies performed in Vietnam, Webb[3] showed that in all but one or two studies there was little or no attention to the basic qualities of research design that would have given scientific validity to the studies. For one thing, the studies were begun at one point in time in a dynamic situation, after the forces driving the changes had had a profound impact, since few had thought before the conflict started that it might be of interest and importance to explore the initial conditions. Thus, there were virtually no baseline data comparable with that now being taken against which to compare and to assess the effects of change. There had been academic peacetime research in Vietnam by university scholars and by the

* As we have seen, the attempt at politicization of social research in the United States was one of the consequences of Camelot and the Vietnam War and was (and still is) a radical idea in the American scientific context.

French, and these results served as broad and unstructured guides for those who wanted to search them out; but it was obviously difficult to compare the situations of refugees or Viet Cong defectors before and after they became refugees or defectors. In the first case, it was not known who would become a refugee, and villages varied enough so that study in one did not necessarily apply in another, although broad and generalized (and therefore not very informative) inferences might be risked; and in the case of the VC, there was obviously no opportunity for conscious and structured access to the people before they became defectors. In addition, the society had been in rapid flux and turmoil since the Japanese invasion in 1942, so a "baseline" in, say, 1958 would be of uncertain value for comparison with 1964 and beyond.

There were, however, opportunities to compare "before and after" on a local scale and in particular subject areas, but these were not often seized. Even when such a plan could be made deliberately, events were more likely than not to interfere. One study in Vietnam, for example, planned to determine the effects of a new government-sponsored "village TV" program. With advanced knowledge of the program, surveys of attitudes on particular subjects were undertaken in villages where the government planned to place television sets before the villagers were aware of the plans. There were then to be periodic surveys after TV entered these villages; and it was not planned to have control villages that would not have TV to pinpoint any effects of TV more exactly in terms of attitude changes over time. The trouble was, when the TVs were distributed, the researchers had no influence on the operational plans, and the sets did not go to the originally planned places, so the careful research design was destroyed.

Since most studies were performed by means of straightforward interviews, they included all of the weaknesses and biases that this implied. Only a few studies used more than one method to gather attitudinal and historical data, so results based on data from different methods could not be compared; the variability due to the methodology itself could not be ascertained. Before-and-after comparisons and control groups could rarely be built into the designs. Therefore, the studies represented, primarily, the taking of a single data point in time relative to any events, and such observations could be made by almost any intelligent observer not necessarily a social scientist trained in sophisticated methodology.

Even these crude observations often lacked statistical validity within their limited scope. In those cases where it was possible to compile data from a large sample of respondents, it would be found that more often than

not it had been impossible to randomize the choices of a subject with respect to the uncontrolled variables and still obtain a large enough sample of each population to achieve statistical validity. A sample of prisoners, for example, would cover a span of several years in the time when they joined the National Liberation Front, how long they had been members, and when they became prisoners. Similarly, the experiences of individuals in the Viet Cong of the sample, and where these occurred, varied widely (even a hundred-mile distance in Vietnam could mean vastly different conditions of combat and interactions with the population). And access to prisoners anywhere was determined far more by the political desires and whims of the Vietnamese government officials who provided that access than by the desires of researchers for the requirements of their research designs.

By the time a sample of as many as 150 prisoners was sorted into classes, where even a few of these variables could be considered fairly constant for a study of why they joined and why they defected, there might be only one or a few individuals in any group described by particular values of the variables: and there were rarely enough individuals in a single group (e.g., people in one age group who came from one village at about the same time) to allow statistically valid comparison of the effects of one variable (e.g., the difference between relatively well off and very poor villagers). The next sample of 150 prisoners was likely not to have individuals from the same population as the previous group at all. Therefore, even though in time a very large group including well over 2,000 prisoners was interviewed, *scientifically* significant results could not be derived from the interviews.

Similarly, although attitudinal research was performed in villages, this was usually done on a one-village-at-a-time, one-time-each basis, so that comparisons between villages in different circumstances with statistically significant numbers of villages, to explore in detail the effects of specific events was rarely possible. Even worse, studies on related subjects were never started with similar hypotheses to be tested (if, indeed, they started with any). And even when the samples might be similar, the parameters describing them were not standardized—for example, one study might divide the male population into age groups of "zero to 15, 16–25, 25–40, and over 40," while another could consider groups of "under 15, 16–45, over 45." How, then, is it possible to draw conclusions about a particular age group based on *all* the available data? Thus, although some numbers of studies might be undertaken on particular questions, the data usually could not be combined or the results compared very precisely.

This is a very sketchy outline of some very complex problems in research design. A detailed review would bring to light many more subtle problems. But the problems mentioned are not subtle, nor were the researchers, by and large, poorly prepared for the technical aspects of their tasks, inept, or intellectually incapable of understanding research design problems. Why did such crudities creep into the work? The answer lays in the exigencies of field research in a country at war and also in the misconceptions many of those who supported the *idea* of such research held about what was involved in actually doing it.

The most critical problem was that of "sensitivity." As we have noted in several contexts, the readiness of high-level, operational directors or commanders to permit entry of researchers into particular geographical areas, or to talk with particular people, varied inversely with the interest and importance they attached to the subject under examination. There were many reasons for this: fear that a delicate situation would be disturbed; fear that unfavorable information might be made public; or a desire, perhaps, not to obtain critical results that would lead "Washington" to want to meddle. But it became almost axiomatic that the more substantive and important the problem requiring the research, the less access would be permitted to the sources of necessary data.

These inhibitions were unevenly distributed and took many forms for there were subjects of importance on which those in control genuinely desired information. But even then, they kept a firm and stultifying hand on the reigns. Permission might be given to enter one village in a "hot" area, while a sound research design might require interviews in eight. Access might be given to one group of defectors who had not held very important positions in the party apparatus, while permission would be denied to see a few "high-level" defectors whose motivations for joining the party, if understood, might be indicative of the requirement for a whole new outlook on the nature of the opposition. The variation of the methodology might be restricted by constraints on time and access and by explicit direction, so that certain techniques could not be used; or the number of researchers who could go into the field was limited, so all the data could not be gathered in the requisite form in the available time. Of course, *all* studies on important subjects did not suffer from such constraints, but enough of them did to establish a tendency on the part of the researchers—who were vitally interested in and captivated by their work—to gather whatever meager data could be obtained under the constraints that were imposed and to try to

make the best of whatever data they could gather in later analyses. This invariably made for single or small samples of observations across broad ranges of variables without careful sampling or control of important parameters and consistently worked against good research design.

The time pressure was one of the most important factors. We have already seen in another context that results on most subjects were needed "yesterday," often for understandable reasons. Since carrying out the full effort on a complex research design could take a great deal of time, the temptation, when the researcher felt himself in a position to influence policy, was to cut corners. The time pressure and the need to "produce" worked in more subtle ways, even on those projects where the overt need to hurry did not exist. In the Program Impact Assessment project, for example, while one of the main objectives was development of methodology, this had to be disguised by promising early assessment of real programs (this, despite the reluctance on the part of officialdom to hurry into such assessments). The methodological problem was two-fold, however: development of indirect measurement techniques to observe and interpret social attitudes and behavior; and development of approaches whereby the responsible local government officials would desire, permit, and participate in the application of those techniques. While the first could be disguised in the act of obtaining substantive results on program evaluation, the second could not be evaded; it was necessary if any research were to be done at all. Therefore, the first program assessment efforts had to be highly simplified in keeping with the level of sophistication of the officials involved, and they amounted to little more than a demonstration of what evaluative research was like. By the time this succeeded and interest was shown in proceeding to more elaborate and firmly based methods, the existence of the project was in doubt because of its DOD sponsorship, so that it appeared the real substance would never be reached. This pressure was later alleviated with the transfer of responsibility to AID, but the budgetary vicissitudes of that agency in the years around 1970 could offer no assurances of orderly process and completion either. This sort of problem, which in one form or another was endemic in almost all the work, made it difficult to keep *science* in the forefront.

It was also found—to the surprise of many, including the social scientists themselves—that properly designed social research would be expensive; this interacted with the generally somewhat negative external view of social research to constrain the limits to which rigorous design could be

pushed. For example, in Thailand during 1968, the government was toying with the idea of extending its capital-based TV network into the farthest, least accessible reaches of the country as a means of enhancing social cohesion and reducing existing separatist tendencies. It requested help for a pilot test to assess whether the impact would be worth the effort and expense. Here was a chance to undertake a study at a relatively leisurely pace, obtain baseline data, establish controls, and give attention to all the details that would make for a scientifically sound effort. We, therefore, worked with the government to determine a group of villages where TV would go, then identified villages where it would not initially but where the people were likely to know about it, and identified others where there would be no TV and it would probably be some time before people would know about it—eight villages in all in a complex design with multiple controls and baseline data established for all. Before and after surveys would be made using a number of different kinds of measurement "instruments," and a fairly large crew of Thai field assistants had to be trained and dispersed to the villages periodically. The villages were in isolated locations and had to be reached by helicopter if months were not to be spent on each data-gathering sortie. The project would have continued for two years, with periodic surveys to ascertain the impact of TV (if any) over time.

When the projected cost of the study was determined, we were shocked to learn that, what was in effect a relatively small project to determine the reaction of the population of a few villages to government TV, would entail a cost approaching a half-million dollars. It did not appear that, if Congress asked the questions, this could be justified—intuition said it might "buy" $50,000 but not $500,000. So the project was scaled down with the carefully worked out research design and, therefore, statistically valid, internally consistent, reliable results became the primary casualties. The Program Impact Assessment effort was also expensive; the upshot was that, while it was underway, we put all our efforts and energies for political defense into it, so that all other such work suffered as we tried to keep the total of social research within a tolerable budget figure.

It was easier to spend more money per project in Vietnam under the pressure of wartime needs and MACV's insistent requests for projects. But whereas elsewhere there might be reasonable time to undertake carefully designed research, even if we had wanted to in Vietnam we could not. The combination of pressure for instant results and rapidly changing people, situations, and conditions meant that nothing would hold still for carefully

planned research no matter how hard we tried. We have already seen what happened when we tried to undertake "before and after" surveys of village TV in Vietnam. In the case of the prisoner and defector interviews, the combination of access problems and movement of the war conspired to prevent any thorough searching out of groups with even moderately uniform backgrounds. Similarly, the attempts to study the problems of refugees in a province could be interrupted by major military operations of either side that would change the situation, mix, and outlook of the population being studied. The problem of measuring specific population parameters under such circumstances was much like that of trying to measure the length of a car from the side of the road while it moves past the measurer at 60 mph. In the case of the car, instruments could be devised for such measurements. In the case of the social systems, this was much more difficult because we had to observe the system "on the fly" even to determine what to measure.

Most of the research community was well aware of the risk of biased effort in their work for all the reasons given. This had to be balanced against the continuous pressure to undertake studies of this or that subject. In addition, the opportunity to study, learn about, and try to influence the management and course of an event as important as the Vietnam War in contemporary U.S. history was one that no one in the research community—workers and management alike—was willing to pass up lightly. Therefore, the perishability of the situation and the data were always balanced against the purity and rigor of scientific research requirements. The urge to push on at all costs inevitably relegated careful science to the back seat.

The impact of these imperfections varied with the task at hand. In the case of motivational and attitudinal research, they could be profound, although the researchers always managed to convince themselves that there were some deep insights and common threads that pointed toward "truth." In the case of studies performed to elucidate the patterns of more objective events, such as a study that was undertaken to observe the interaction between Viet Cong and government operations in a village over a period of time, or another in a Mekong Delta province to piece together the interlocking patterns of Viet Cong political and military activity in an area straddling one of their main supply routes, the effects of the biases were easier to fathom and rectify. For in these cases, there were other data in the records of the war and the military operational system and the memories of some of that system's members against which to check.

A different pattern of biases appeared in the tasks that might be called technical assistance—those in which the research community was asked to help set up a data system (e.g., the Hamlet Evaluation System) and analyze the results. Although all the researchers involved felt intuitively that the kind of data that could be obtained and the circumstances under which they were obtained by the operational commands in the field would inevitably bias the results, the operational community's "headquarters," driven by ever more insistent demands from Washington for better "indicators" of how the war was going, insisted on using the latest approach to analyzing numerous statistics no matter how imperfect the data or how biased the consequent results might be. There was an interesting evolution here from a relatively fruitless search for "indicators" of success through attempts to solve analytical problems that dragged on and on without useful results because the basic data were simply not good enough to simply setting up a computerized data system for the operators to use and then working with that system to modify and improve it to remove biases which everyone sensed intuitively were there but which no one could define rigorously.

At the same time, there gradually emerged two more aspects of research in the field situation that had not been reckoned on when we started. The first was what appeared to be the impossibility of integrating the quantitative and nonquantitative disciplines in social research in the field. Much of this went back to the data problems that always existed. The social scientists strove mightily to add quantitative statistical aspects to their work. Some succeeded in some circumstances; but most of the time it was clear that, although they peppered their reports with tabulations and statistics, the chief value of their work derived from the insights they reached intuitively as the work progressed. These insights were usually fascinating and instructive, although it might be hard to prove by rigorous analysis of the data that they were generally valid or "true." For example, while virtually all Viet Cong prisoners claimed to have joined for ideological reasons—driving out the American imperialists and their lackeys and variations thereof—their actual reasons for joining seemed to be mostly very personal. These could vary from inability, for bureaucratic reasons, to attend a desired secondary school, to love of adventure, to fear of the press-gang, to the fact that the prisoner had lived all his life in a Viet Cong area and could conceive of no other way to go. Many were motivated by real injustice that they had experienced or witnessed. But with the poor statistical samples that characterized these interviews, it would be difficult to know just how the different reasons for joining were distributed across the prisoner population and, by inference, the pop-

ulation of Communist troops. Any program to entice defectors for amnesty, or to induce surrender of Viet Cong units, or to inhibit Communist recruiting would, ideally, be based on knowledge of why most of those troops were there. Ultimately, therefore, the insights about why men joined the Viet Cong were interesting, but not of much practical value, nor could the "truth" be "proved," even though it might be sensed intuitively.

At the opposite pole were the operations research people, who manipulated any data they could get—because the military statistical system was never, however much we tried to influence it, set up to permit detailed analysis of events and their causes—attempting to find patterns of enemy activity that would show the directions for counter operations best to succeed. Most of the time, these results were dry and told nothing the military could not sense intuitively. For example, one lengthy report analyzing dozens of operations proved only that the Viet Cong operated most of the time at night and in a certain relationship to the waxing and waning of the moon. Strangely enough, no one had previously demonstrated these facts with assembled and integrated data; but once demonstrated, it was so obvious that the analysis did not appear significant.

These differences between the disciplines seemed to result more from profound differences in the mental "sets" of the quantitative and non-quantitative groups—subcultures within Snow's two cultures,[4] if one will—than from simple matters, such as training. This was indelibly impressed on my mind by a conversation between Murray Gell-Mann and Michael Moerman that took place at the Jason Summer Study on Thailand mentioned earlier. The conversation opened on the question of how many people might be listed as authors of a seminal paper in Gell-Mann's and Moerman's respective fields. In physics, the number could be many, accounting for each of those who contributed importantly to the theory or experimental verification of discovery. In anthropology, the author had "pride of ownership" over "his" village, where his research would make his reputation, and he must do this alone. He could not, and in his professional circles was expected not to, share the credit with anybody. Further, while the anthropologist would certainly try to adhere to rigorous practices of observation and analysis, he would much rather be famous for a well-turned, insightful phrase than for the extent and precision of his observations.

We never succeeded in bringing the two outlooks together by joining the different disciplines in a single successful study. Occasionally, a social scientist—almost invariably a psychologist—was employed who understood

some of the mathematical approaches of statistics and operations research, or an operations analyst was found who had an understanding of the patterns of motivation of behavior. The results were then impressive. (But that did not mean that they were used, as we shall see.) Moreover, the appearance of the combination could not be predicted in advance because the man who did the work not only had to be of the right discipline and background, he had to be willing and able to go overseas to do it, sometimes without his family. The selection process, though it was intended to be purposeful, was, in effect, almost random.

The second aspect of the field research that gradually emerged was that the field situation posed unanticipated but poignant ethical problems of a kind different from the relatively broad questions of purpose and morality that were appearing in the criticisms of the social scientists who worked for the government. But the net effect on the researcher's ability to serve his conscience, his science, and his government simultaneously was the same.

The most obvious problem came about when the researcher, who spent a great deal of time with the local population, gained their confidence and started to learn about the inner events and motivations of the community he joined. He might, for example, learn that certain unsuspected members of the community were insurgents, or even that they planned some action inimical to the local government or the remainder of the community. He was at the spot to gain data of assistance to the local government, and supported by U.S. government funds allocated to help the local government. Thus far, his problem was more or less standard, and in our experience most researchers who faced it would favor their local informants and their information sources and keep quiet. But his problem usually went deeper. The researcher had many acquaintances among the local population, and his personal sympathies might go to the people in *both* rival groups. It was thus not a question of having sold one's soul to the devil, but rather of keenly felt personal sympathies that might go both ways—more akin to the doctor-patient problem in a divided family. In all the years of this research program, the problem arose only a few times. In Vietnam, General Hickey had managed to gain the respect of both sides and to maintain his detachment, nevertheless, to the point where he could remain aloof at appropriate times and yet not jeopardize their liking and respect for him. Many of the other researchers were troubled by anticipation of the question, and some of us in ARPA even considered the advisability of devising a policy to guide the researchers we supported. But in the end, it remained a matter of individual judgment, and it must have been painful for many.

Such intelligence-related aspects of the work affected the program in other ways simply because of the uncertain boundary between field studies to gain information for research and those by other agencies to gain intelligence. The confusion the question engendered on the part of some of the congressmen who participated in the Camelot hearings has already been noted. In another more complex case, a study was undertaken to explore the impact of various psychological warfare gambits initiated by the government on changing attitudes in the villages of a Mekong Delta province where the Viet Cong were very active in 1967 and 1968. The research leader was a Vietnamese social scientist familiar with and widely acquainted in the geographical area. The research technique to be used was that of arranging a number of "participant observers" in several villages to report what was happening on the spot as the "psywar" schemes were implemented. The latter might be as simple as passing out some small but valuable tool having a pro-government slogan on it or as complex as starting a series of conflicting but adverse rumors about the Viet Cong. It became apparent as the initial phase of the study was coming to a close that the participate observer arrangements had led to the establishment of what could easily be interpreted as, and in fact could easily become, a private intelligence net. Our Vietnamese scientist, we suspected, might not be above using it for political purposes in the ever-present Saigon intrigues in which he had displayed some extensive interest during several conversations. This approached too close to the boundary for comfort, and it was decided not to continue the effort. We were aided in this decision by the fact that the research scheme was found to be simply not very useful; the data obtained by this technique were of an uncertain quality and not easily subject to checking by other means.

Other problems of an ethical character have been mentioned earlier in connection with the differing views of the university and military groups regarding project management, accounting for expenditures, employment of families, and in the general antipathy between the two groups with respect to the bureaucratic constraints and, in some cases, about how widely the results of research could be published. The trend toward conservative interpretation of the issues by the government officials involved, including myself, was continually reinforced by apprehension about the potential impact of adverse, critical, or satirical press reports and by the presence of the General Accounting Office (GAO), which for a substantial part of the period under discussion was investigating the operations of Project Agile at congressional request. One was not inclined to take risks with the GAO

looking over one's shoulder—or at least risks were taken primarily when the stakes and the solidity of the work appeared high enough to warrant and permit a strong defense. Many projects that might otherwise have appeared "worth a try" were ruled out for this reason.

The net effect of these problems—the difficulty of merging disciplines and ethical problems attending extensive field work—was that methodological innovation and careful research design that would run costs up tended to be avoided in favor of getting the job done with least risk and in minimum possible time. In retrospect, it seems that the appreciation of what scientific research had to contribute in contradistinction to simply gathering information about interesting subjects was largely lost. Studies were undertaken for the sake of doing the studies and, in all but a very few, considerations of careful and *scientific* research design to be certain of the validity of the results drifted invariably to last place in the order of priorities.

Chapter 20
Who Will Buy My Wares?

Despite all the problems, the overseas social research produced new knowledge and understanding, fresh and sometimes startling insights, wherever it probed. If the research designs did not ordinarily lend scientific validity and confidence to what emerged from most *individual* studies, the subjects overlapped enough with each other, with the knowledge of the few anthropologists who had spent years in the areas being studied, and with the efforts of operating people to make sense of related events, that it seemed possible intuitively to separate that which "rang true" from that which should be viewed with suspicion and distrust. Whether we had scientific proof or not, we felt we were learning much that was new and valuable.

In particular, a picture of the diverse revolutionary movements of Southeast Asia began to emerge with details of what motivated their members, explication of the points of conflict within the existing societies, and with understanding of how these movements made their way through the peasant populations, gaining or forcing adherents and destroying the writ of the existing order and governments. The behavioral patterns of those governments became clear also. There emerged a detailed view of the traditional, cultural, and institutional constraints that put the governments at odds with increasingly large segments of their populations. One began to perceive and understand the emergence and expression in modern idiom of age-old conflicts between diverse groups and subcultures and to trace the twin impacts of change from a colonial to a nationalistic world and from a traditional to an increasingly modern society.

The actions of governments could be put in perspective. It became possible to predict what actions might be effective and which would simply exacerbate, or put a new twist on, existing conflict. One could usually foretell when a course leading to the latter result would be taken because to do otherwise would run against deeply ingrained behavior patterns and might threaten the continued existence of governing elites. It became obvious, for example, that the Thai government would use military force to try to put down a rebellion among the Meo tribes in their northern provinces that they half feared and half viewed as subhuman, rather than try to rectify the effects of longstanding attitudes and injustices inflicted on the tribes by the dominant Thai population. Such a course seemed inevitable, just as did the resistance of the white population of Mississippi to school integration in

1954. As another example, we found how and why in such areas as the Mekong Delta region of South Vietnam accommodations could be made, and were made, among local populations, governments, and insurgents when this suited the best interests of all, with the result that conflict was contained at a certain level and never got worse—although by mutual agreement it never got better either. These were but two examples among a constant stream of them that emerged from the deepening insights that the research results stimulated.

But, just as in Mississippi in 1954 and in following years there were some members of the governing elite who recognized the inevitable and were willing to accept change and seek reconciliation, there were significant parts of the governments of these countries who were willing to try a departure from the past. In Thailand, for example, there was one general in the Communist Suppression Operations Command who tried to take an approach that combined efforts to lure adherents away from the terrorists and attacks on their bases at the same time that Thai army commanders were undertaking harsh measures against villages suspected of harboring terrorists. And the point was, the United States was dealing with all such groups and exerting the influence deriving from its presence, the distribution of its military and economic aid, and the advice that went with them. Thus the knowledge gained from the social research could have been used, with perhaps some significant effect, to guide the advice and the disbursements. This may have worked if the American community had approached the problem with some coherence of viewpoint. But the American government and its representatives overseas brought their own sets of complexes and contradictions to the situation.

The United States was always ambivalent about whether it was simply giving aid to be used freely by the recipients according to their own conceptions of need and utility, or whether it would use its aid to exert "leverage"—to induce the recipients to do certain things and act in certain ways. It was obvious that aid could not long be given for activities that defied the directions that a majority of Congress would approve; there was also the problem that the various members of the American community in a country had their own ideas about how economic or military aid (for whose oversight they were responsible) should be used, and there was no unanimity on that score. There was a diversity of ideas and beliefs as great as, or greater than, that within the host country and various American officials aligned themselves with various sets of ideas or factions within the host government's councils.

On the one hand, there was also tension between the idea of assistance willing and freely given with the desire not to earn the constant accusation that the recipients were "puppets"; and on the other, the need to lookout for Uncle Sam's interests and dollars, according to one's best understanding of the situation, the conditions behind the appropriation of those dollars, and the needs of the local government and population. And some solutions, such as the idea of accommodation in the Delta provinces, were simply outside the range of acceptance by the collective value system that urged the use of American as well as Vietnamese troops to try to suppress the Viet Cong there. Thus, research might indicate the real possibility of reducing conflict through accommodation among government officials, populace, and insurgents, and some U.S. officials might want to use our aid to encourage that view. But to most of them, it would defy the idea of "winning," which was the reason we were there; and it would look much like a form of corruption; and our resources would, some could say, be used to support "the enemy"; and such a solution was simply beyond the pale.

There is nothing new here; the description fits the pattern of real-world political milieux everywhere. The point was that it was precisely to help all groups involved to guide themselves by something better than "seat-of-the-pants navigation" that the social research had been undertaken. Other products of the research had indicated how the American presence and aid fitted into and affected the local picture, and the small amount of research on the advisory function had shown ways in which advice at various levels might be effectively given and sometimes willingly received. Thus, imperfect as it was for all the reasons given and more, the research had developed results that could be useful and that would meet the needs originally perceived. All of the trouble and the problems would have been well worth wrestling with if the research had fulfilled these ultimate ends. But the crazy-quilt pattern of power politics and value conflicts made the use of the results an "iffy" proposition—sometimes apparent and beneficial, sometimes hard to trace, and many times patently nonexistent. On the whole, for all that it taught some of those involved, the impact of the research on the most important affairs of state was, with few exceptions, nil.

A key problem of implementation arose from the difference between the "language" of the research community and that of the operating community. Often, the social scientists wrote beautiful reports of their work. They presented the background to the study they were describing in all its intricacy and subtlety. The conditions of the study would be described, and the

methodology, and the data sources, and how the results were analyzed. More often than not, conclusions were presented, each in a paragraph or a page with the qualifying conditions and subsidiary speculations. Recommendations were direct, but not necessarily any more brief. Buried somewhere, usually toward the end of the background discussion, would be a statement of the problem being studied. This was the language of social research.

And the busy executive would not wade through it. He knew the background (he thought) and was indifferent to methodology. He wanted a concise statement of the problem studied and the results, and he would leave the remainder to the appendices for others to judge. If the study could be reduced to a one-page "fact sheet," so much the better; but he would not read a hundred or more pages of eloquent and often irrelevant verbiage to get the answers to the questions he felt he already understood pretty well (chances were, he did not understand them that well, but he was not about to take the trouble to find that out). If the results challenged his intuition, he was more likely to dismiss the report as poorly done than to inquire searchingly into the basis for his own beliefs or ask for a critical review of the report to see whether it might possibly be correct.

Now, there is an element of parody in these descriptions. There were social scientists who wrote concise and lucid reports, and there were officials who would read lengthy reports and learn from them. But the trend was the other way for valid "cultural" reasons. Much as he wished to convey his results to the "user," the researcher wrote also to satisfy his own need for thorough presentation of that which had occupied his mind for months or years, and to meet what he felt would be the judgments of his colleagues about his scientific ability and integrity an the soundness of his work. The "user" was usually on a 20-hour day and days or weeks behind in all his work. Although he wanted to be enlightened and educated, he was willing to take scientific rigor on faith and wanted the answers to his questions swiftly and without the need to work too hard to get them. Along with his "fact sheet," he might have some brief judgments by his staff as to the validity of the work and how seriously the results should be taken. Needless to say, the staff might have their own fish to fry, and their comments might not always do justice to the research results. Thus, the study report and the official whose personalities matched, as it were, were a rare combination; but the combination was essential for the work to be taken seriously.

There were other impediments to acceptance of research results. We have already seen the reasons for frequent lack of coincidence between the

appearance of a study report and its teaching the official who originally asked the question. In these circumstances, a report was not often able to find its way into the hands and mind of someone who really wanted the results and was prepared to act on them. Or, as an alternative, a report on some critical subject could reach the desk of the one who could act, but the arrival was in a sense random; it could come at the wrong time and be ignored, or at the right time and influence events profoundly. In either case, there was a large element of chance involved. Two examples are illustrative.

At about the time the United States entered into the Paris peace negotiations in 1968, some of us, including Chester Cooper, who was then working with Governor Harriman on the search for a settlement of the Vietnam War, became concerned that, whereas the North Vietnamese negotiating team and positions had both the venerability and rigidity of 20 years of Viet Minh doctrine, it was not at all clear that the United States had either a unified position or an understanding of the implications of some of the proposals that were being made. Since Cooper was in a position to influence the negotiating positions on our side, or at least to obtain a hearing for new ideas and insights, it was agreed that State should ask DOD to undertake some appropriate studies to illuminate some of the issues better. In due course, the requests arrived through appropriate upper-level channels (from William Bundy to Dr. Foster), and the studies were initiated. In some months, the first results began to appear. They included, among other things, a perceptive analysis from available prisoner interview and Hanoi radio data of how the Viet Cong might view the advantages and disadvantages to them and to the Vietnamese government of a cease fire, which was being discussed at the time, and an historical analysis of how the Viet Minh had used the issue of French prisoners of war to further their cause in negotiations with France at Geneva in 1954. But by the time the reports appeared, the personnel composition at State had changed. Cooper had left, and there was no one we could identify as really desiring to have these reports. They were distributed, but there was no feedback.

At about that time, however, I learned that a general with whom I had worked closely during the Camelot crisis had become the military member of the U.S. negotiating team in Paris, and I sent copies of the reports to him. While the course of negotiations might be too slow to see whether they had an impact, at least we got some feedback, and the U.S. negotiating team seemed to have enough time for some reading. As soon as this general was replaced by another, the feedback stopped, although we still

sent reports when they were completed. Ironically, by the time the prisoner issue came to the fore, some three years later, and the report on the related subject of the French prisoners could have warned of some of the booby traps, that report must have been buried deep in the previous administration's files. At least, it seems that it must have happened that way, from the way the clear lessons about North Vietnamese use of the prisoner issue appeared to have been ignored.

In the second case, two reports, separated by about three years in time on the same subject, met vastly different fates. The subject was the effectiveness of the regional and popular forces (RF/PF) in Vietnam—those semicivilian quasisoldiers who had borne the brunt of the war in the villages for all the years it had continued. It was obvious in 1964 and 1965 that their ability as soldiers, their training, and their morale, left something to be desired. When the opportunity offered, I had asked Jeanne Mintz—a SORO social scientist who was interested in the problem—to write a report using the results of a small research project then underway. Based on some 70 or 80 interviews with American soldiers and civilians who had returned from Vietnam and who had served there at all levels and all kinds of jobs, the report—anecdotal rather than scientific—brought to light the various problems the RF/PF faced: low pay; lack of training and leadership; neglect by the Vietnamese government; failure of survivors' benefits—all the things that would make for a poorly organized and motivated fighting force.

The report was delivered and duly sent to all the places where it could be of interest. But from the response, or rather the lack of it, and the lack of action afterward, it seemed to have dropped into nothingness. Three years later, another report on the subject was written. This was the result of one of the few studies in Vietnam performed under the aegis of the Simulmatics Corporation by the psychologist Phillip Worchel of the University of Texas after a request from the military command that was able to follow a somewhat rigorous research design and careful scientific methodology. Interestingly, the results—the reasons making for good or poor RF/PF units—were much the same as those that had emerged from the earlier, cruder effort, which had been based on the intuition and experience of American participants rather than on study of psychological motivations of the RF/PF themselves. The second report was prepared, as well, in a style designed to catch the official eye. But from all the response, it seemed that it, too, would be consigned to limbo. Then, on a visit to Vietnam in Octo-

ber 1967, I chanced to have a conversation abut the RF/PF with General Westmoreland. When I asked, it turned out that he had not seen the report but was interested; I had a copy in his hands the next day.

In the ensuing months, it was clear that this time a research product had struck home. Reforms of the entire RF/PF system were being instituted, and combat reports indicated that they were beginning to give a much better account of themselves, often defeating North Vietnamese army units. Of course, the time was right for such a change; it was the height of the war and much increased emphasis was then being given to the pacification program in general. So the report on the subject finally entered the system at the right time and at the right level and, one likes to think, it helped rectify a problem when the awareness was there. On such chancy stuff was the impact of the entire research effort based.

Part of this "impact" problem, of course, was the exact manner in which often unexpected results struck the preconceptions of the recipient who was responsible for the action. We have already seen how this interaction helped shape the research program through selection of subjects for research. With respect to the research product as well, if the subject were neutral or if the results fell in with the preconceived needs and desires of the officials who would have to act on them, the research reports were well received and well used. For example, the continuing work to establish a reasonably sound statistical basis for interpreting the hundred or more detailed "rating" questions that contributed to the Hamlet Evaluation System "scores" for thousands of hamlets and villages was always viewed as necessary and helpful, and it helped shape that system into as reasonably rigorous an accounting of intangibles as the data would allow. Similarly, an effort by an expert in psychological testing techniques to prepare culture-free tests, independent of literacy, for assessing military aptitudes and guiding the military assignments of conscripts led to the institution of an entire new system for drafting and training the armed forces of a country (the country was not in Southeast Asia). This happened because the country's leaders knew there as a problem and wanted to remedy it; they asked for, and were receptive to, suggestions for change.

But on matters that challenged the old ways of doing things, or that indicated failure in a government program to which there was strong commitment by important officials, there was never such easy acceptance. One of the major questions raised at the initiation of the Program Impact Assessment effort (described earlier) was whether anything would happen if

programs were assessed as nonproductive or counterproductive, and ways emerged to change them that ran against the cultural grain of interested officialdom. Was it worthwhile to spend considerable sums of money, displace people overseas, and undertake an arduous effort if the final outcome would be to have the results of all this ignored? Obviously, it was decided to take this risk. But in part, the awareness of the risk led to the very cautious approach, starting with relatively inconsequential programs, that both the research and the official communities adopted.

After his Vietnam research, Worchel, who became the director of ARPA's large research unit in Thailand (1968–70), suggested an indirect approach to the acceptance problem. He postulated that the problem potentially facing officialdom was that of recognizing publicly that programs and actions for which they were responsible were having deleterious or counterproductive effects. When faced with stated conclusions and recommendations to do something differently, the admission of failure must be explicit if the recommendations were accepted. The natural reaction was defensive and resistive. The proposal, then, was that the research community should not draw firm conclusions or make explicit recommendations. It should simply design its research to elicit the facts, making causes, effects, and consequences very clear. These would be presented to the officials, who could draw their own conclusions and devise their own solutions.

In theory, this seemed a reasonable approach and entirely consistent with the role of a social research investigator. The problem that could be immediately anticipated, however, was that "none are so blind as those who will not see"; confronting an official with incontrovertible evidence of the consequences of his actions would not assure that he would act differently. And, of course, it would be an unhappy lot for the social scientist to have to ferret out data and interpret events based on them, and then have to refrain from drawing the inescapable conclusions and making the burning recommendations, while watching the one who needed the assistance refrain deliberately from drawing those conclusions or acting inappropriately (in the scientist's view) on them. But one form of being ignored is in many senses much like another, and it was decided to test this approach as part of the Program Impact Assessment effort. Unfortunately, in the event the delays attending the interagency transfer of the program were lengthy, and the DOD overseas social research program began to wind down rapidly shortly after this decision was taken. Thus, we in DOD had no opportunity to test the viability of the approach. Even in 1973, after the program per se

had been well accepted by the Thai government, it was not clear that it would have the desired effect on their social programs.

In another case, there was, at long last, the *appearance* of success in this area without any certainty of the existence of its substance. We had given a relatively modest amount of support to a Thai social scientist to enable him to carry out some studies of social developments in farming villages. The objective was to ascertain whether some of the stresses of modernization were creating restiveness among the younger inhabitants and causing them to join the insurgents. While some of the American social scientists criticized the technical quality of his work, he managed to keep its political implications submerged and to make a reasonably complete report on the attitudes toward the government and its works of key social groups in the villages. These results were sufficiently impressive to Thai officialdom that, some years after the work was initiated, the social scientist and his Thai coworkers were given an official position as a supporting research arm of one of the agencies concerned with controlling insurgency in the country. This result alone was highly gratifying since it represented the hoped for outcome of the financial support given to the individual under circumstances when the entire idea of supporting indigenous researchers overseas was being challenged in Congress. However, the agency to which this research group was attached was not the one exercising the ultimate power of decision in governance of the countryside. At the time of writing, therefore, it is not yet clear whether the data developed and presented by this group will be taken seriously enough at the high levels of Thai government to affect the established patterns of official behavior.*

In matters where research results might have had a profound impact on large-scale events and where they dealt with the fundamental premises, understandings, and approaches to matters of war, peace, and interactions with foreign populations and governments, it proved impossible in the long run for those results to have any effect at all. Secretary McNamara, for example, had established a standing rule that all "software" reports—operations research and social research—emerging from DOD studies in Vietnam were to be sent to him with a covering summary sheet, outlining the results and their value. We knew that he read many of the reports that were sent—especially those dealing with the impact and operation of the Viet Cong and the Government of Vietnam (GVN) in the villages. A picture of the

* Nor, viewed from a longer distance, was there the corporate memory to try to find out long after the U.S. withdrawal of its massive Southeast Asia effort in 1975 and later.

events gradually emerged from those reports, showing how during that period (1964–67) the Viet Cong were gradually extending their writ among the population; how the GVN through sheer ineptitude played into their hands; and how American policy or local advice would often exacerbate the situation or simply fail to have the desired effect. This is not the place to dwell on the history of the Vietnam War. But it can be observed that the knowledge transmitted by the reports could have had no effect because, by then, the inexorable course of the war, with its attendant policies and behavioral patterns, had been set. The changes that might have been required would run counter to the direction in which "the system" was already fixed, and it is clear in retrospect that far greater forces than the observations of a small number of researchers were not able to change it until the Viet Cong themselves did so in a far harsher way, with their 1968 Tet Offensive.

But even when the system was more fluid and more open to suggestion, those with responsibility for the grand sweep of events did not seek answers from those with research expertise. In one case, early in 1964, the late George Carroll, a thoughtful official who was responsible for coordinating Vietnam affairs in ISA and who had "connections" into the White House, asked me whether our research had turned up any traditional Vietnamese institutions on which a democratic government credible to Vietnamese could be built. It did not take long to have a report prepared for him (through the agency of SORO's CINFAC) on the Vietnamese village councils. These comprised a venerable form of local government, predating the French in Vietnam, to which respected village elders were "elected" by approbation. Although the village-council system had been corrupted and emasculated by the French and by Ngo Dinh Diem, who appointed his loyal followers to the councils, it was still more or less respected by the village populace and was familiar to them as the local government that took over when "the Emperor's authority stops at the bamboo fence." A system of representation in which village councils were elected, and they in turn elected representatives to higher bodies through district and province until representatives were sent to the national government, would have been congenial to Vietnamese from ancient times. But on the day the memorandum containing this proposal and the descriptive report on the councils reached Carroll, we were both astonished to hear the White House call for national elections in the American style in Vietnam. To our knowledge, no one familiar with Vietnamese history had been consulted before that proposal was made. Village elections were held many years later, but were never related to national representation; and the sorry history of South Viet-

namese national elections in the American pattern, and their outcomes, from that first suggestion until fall 1971 is well known.

Many experts on Vietnam pointed out to American officials from the earliest days that the very choice of "hamlet" rather than "village" for significant attention bespoke American ignorance and was unfamiliar to Vietnamese. The hamlet was the smallest group of dwellings in heavily populated rice-growing parts of South Vietnam and clustered where convenient to the rice fields and to the roads or canals to market. But several hamlets made a village, and the village was the smallest administrative unit. Thus, the creation of a system of "strategic hamlets" served to confirm the isolation of the population away from their familiar informal groupings and displaced from their land, their access to markets, and the graves of their ancestors. This added to the already difficult burden of trying to make that alien system succeed; and of course, it did not. Similarly, the later use of a *Hamlet* Evaluation System in relation to pacification could show half the hamlets in the country free of Viet Cong guerrillas, but would mask the fact that guerrillas might command all or parts of the adjacent hamlets so that few *villages* might be truly "pacified." It was not until 1970 that this distortion of view in the American influence on the administration of Vietnamese society was rectified in the official recordkeeping on the war.

None of this is to say, of course, that these considerations, if reflected earlier in official policy, would necessarily have changed the course of the war very much. But it would seem that a necessary, if not a sufficient, condition for rendering assistance to one side in a society undergoing revolutionary conflict would be to understand thoroughly the forms and traditions of that society. Then, at least, reforms could have a starting point rooted in the culture rather than imposed and unfamiliar from the outside. In this, the research community could have helped; but it was rarely if ever asked, nor was much heed paid if it spoke. (This then begs the question as to why the official *Vietnamese* did not point these things out. It is sad to have to note that the urban elite, who were influenced by their French-oriented upbringing, that ruled a peasant multitude in turmoil were separated from that multitude almost as much as were the Americans, and they could have benefited equally well from the advice.)

Even on a more mundane level, policy and long-standing patterns of operation transmitted from one soldier to the next could not be modified easily. Some of the same reports that were sent to the secretary of defense, including an excellent analysis of the American advisor and the reasons for

his ability or inability to relate to his Vietnamese counterpart, were made required reading for the MACV staff by General Westmoreland. They may have proven useful to some individuals, but as a general matter, the beneficial changes that could have been made in the selection, training, and assignment patterns of advisors or in some specific operations that affected and interacted with the village population were rarely discernible. And we have already seen that some vital subjects, such as the impact of the war on the population, were not even open for study, even though the researchers had already observed enough to suggest that then-current directions for waging the war might be counterproductive. Much of the knowledge and understanding of the motivations, organization, and operations of the Viet Cong had their roots in the thousands of interviews the social scientists conducted with prisoners, defectors, and villagers. The results may not have affected policy, but they did eventually permit the assembly of a "Viet Cong Manual" in English and Vietnamese that set forth what was known about the Viet Cong and the NLF organizations and methods. This became a reference book and training aid and doubtless served in the education of thousands of officers and men concerned with trying to fathom the working of that organization. This was a clear demonstration, again, that where research results could fit into the pattern desired by "the system" they could be accepted. Even so, there was no way to ascertain whether the availability of this book made much difference in the struggle against the Viet Cong in the villages after all.

This pattern of acceptance was not limited to Vietnam, which might be said to be a special though all-pervading case. In another instance in 1969, we had decided that several years of research in Thailand—a country faced by lesser, but nonetheless threatening guerrilla warfare—had amassed a wealth of information that would be of vital interest and importance to the officials responsible for determining American policy with respect to that country. A volume was prepared that integrated the results of three years and several million dollars worth of work into a fairly coherent picture of conflict within Thailand and its culture. The volume had the requisite "executive summary." It was distributed to all those in the administration who it was believed had an interest in and responsibility for policy toward the country in question. A number of kind and complimentary letters were received in return.

But it was discouraging to learn that the staff of Henry Kissinger (who had just become the president's national security advisor at the time), whose

understanding of the country and its problem would at the time be the most critical for policy about Thailand, had been instructed to prepare a "fact sheet" on the report for his use. He had not time to read it all; all his time was being taken up by the president. Of course, any fact sheet simply represented a brief, one-sentence-each summary of the volume's main points. These could not help but be assembled selectively and constitute a very subtle form of recommendation for action. True, the recommendations would, as sifted by the staff, be influenced by the knowledge developed in the research. But the problem of educating a key official to the subtleties of the policy choices on which he would be likely to have the final say would not be met. Perhaps at that time, Thailand was not important enough in the larger scheme of things to warrant more attention, since peace in Vietnam was obviously first on the list. One never knew whether to be grateful that the results of millions of dollars' and many years' worth of effort could, by whatever indirect means, be given any hearing at all or to decry the missed opportunities when they were not given the hearing for which they designed.*

Thus far, we have been exploring whether and how the research results could have any influence on events at all. To place the whole exercise in somewhat better perspective, it must be noted that there were times when we were fearful of that influence and wished fervently that it did not exist. One such case occurred at about the time the prisoner of war interviews from Vietnam were becoming available. Some of the DOD officials who had initiated that work felt it most important that Secretary McNamara be kept up to date on the results as they became known, and they arranged to have him briefed periodically by the project director—a dangerous procedure with unevaluated research results. The project director played it straight and pointed out that the results he was briefing were preliminary, or simply "impressions." But it seemed to some of us that the nature of those impressions might possibly be highly misleading. In the particular matter of the impact of the war on the population, the "impressions" were that the vil-

* And indeed, at a later time (in 1974), reports were sent forward to the White House describing the antagonistic relationship between the ethnic Vietnamese and the Montagnard in Vietnam's Central Highlands. At that time, however, we were in the midst of the "Vietnamization" program and of handing the warfighting to the Vietnamese Army preparatory to our withdrawal from the war and the country. I was appalled to learn that the Vietnamese Army had been given responsibility for command of the Montagnard units, and indeed, those units decided they did not want to get involved in a fight between Vietnamese factions. So, when they were attacked at the Central Highlands town of Ban Me Thuot, they melted into the forest, and the North Vietnamese Army exploited the opening to start the drive south that brought them to the gates of Saigon not long afterward.

lagers were blaming the Viet Cong, not the Americans, for the death and destruction—because the Viet Cong were there, that is why the bombs fell.

Some of us felt that even if this knowledge emerged from talks with a number of villagers, it paid to be suspicious when the results of research were so close to what one might want to hear. We did not know how seriously Secretary McNamara was taking these results, or whether he, too, might share this suspicion. We felt that such stuff should not even be taken to him until the "preliminary" data could be much better wrung out by detailed analysis. And so, we undertook to get the periodic briefings turned off—itself a delicate procedure. About the time we succeeded, more extensive, detailed, and fully analyzed results began to be available; and they showed that indeed the "preliminary impressions" had been warped, and the attitudes described were not generally descriptive at all. The analysis and later research studies of refugees and why they had left their homes showed that the Vietnamese peasants blamed the *war* for their problems: the Viet Cong, the government, the bombing and fighting—they simply wanted to be left in peace. If someone on the outside wanted to blame the Viet Cong for the population's problems, or blame the Americans, he could find support for either view in the interviews.

All of which illustrates that, much as we wanted the results of this social research program to influence events and policies, we were always treading a fine line between trying to get the work used and trying to inhibit excessive and premature enthusiasm. The controls that were exerted had to be based largely on the judgments of the research managers and a host of other officials above, below, and to the side of them. In the long run, the performance in terms of the main purposes of the research was spotty, to say the least. Some few studies had a profound effect; some were helpful to executive officialdom but most did not and were not. The chances of any one study having major influence were small. The closer a study came to dealing with problems in which important policy issues were at stake, the less likely that it would matter. Many factors influenced the reception or impact of a particular report, the greatest number of them accidental and unpredictable.

One conclusion that can be dawn is that to achieve a few relevant, high-quality outputs that might have a predictably great effect, very many studies in related and overlapping areas would be needed. But the constraints on social research in this context—internal and external—were such that the necessary large volume of work could not be undertaken. When all was said and done, therefore, an enormous amount of effort and adrenalin were

expended to accomplish the relatively small amount of work that was completed with little certainty that any of it would be useful and great certainty a posteriori that most of it would not be.

Now, this is nothing to cry over because such is the fate of much research, including that performed in the life sciences and that which leads to hardware. But there are some important differences. Everyone knew, for example, that hundreds of approaches had been or would be necessary to find a vaccine for polio or to find cures for various forms of cancer and that hundreds of millions can be spent following false leads before the few correct (because they succeed) directions are found. It is recognized and accepted, also, that in this work many individual research efforts will produce nothing useful; many will produce interesting and useful knowledge or methodology, even if they did not succeed in their original objectives. Similar occurrences are routine in attempts to develop new electronic devices or new machines to perform old jobs more efficiently or to perform new jobs. Sometimes, the projects that did not succeed could have been predicted in advance by specialists, but the projects go ahead anyway.

There are many cases, in fact many cases never recognized, where Congress and the public have accepted with relative equanimity the expenditure of $5 million and sometimes $50 million or more worth of bad physics or bad engineering. The Mohole Project—to drill through the outer crust of the earth for geological research—caused hardly a stir outside the scientific circles directly involved when it was cancelled. No one outside ARPA noticed when I cancelled a clearly unworkable radar map-matching project I found when I entered there, even though the single project had already spent more money than a year's entire social research budget. But the threshold of forgiveness is low with respect to social research, and the expenditure of even $5,000 of public funds on something that appears unproductive can, as we have seen, call down the wrath of the gods on the spender and place an entire departmental research budget in jeopardy. There are many reasons for this. Two important ones are the sensitivity of the subject matter, which is almost always higher in significant social research than in the life and physical sciences and engineering; and the need, which is generally not recognized with regard to social research as it is relative to the other subject areas, for specialized expertise to judge the technical quality, feasibility, and value of the work. At least, in social research, the willingness on the part of the lay public to recognize when and why some expertise may be needed to make a judgment is not very great.

The point is that the recognition of the speculative nature of all research is not readily granted to social research, so that in the latter, *where it is paid for by the public*, every project must count and, precisely because it does not and cannot, there will never be enough projects for the few high payoff, high impact efforts to have much probability of being undertaken (note that the few highly successful projects described earlier accounted for about 10 percent of the total expended on this program).

Chapter 21
Of Time and Tide

While at one level of awareness we were learning about and trying to grapple with the diverse problems that may be called internal to the research and its application, at another level we continued to feel the pressures of the outside world. The interest of the press and Congress in DOD's social research, especially that undertaken in Vietnam, did not abate. Although our redefinition of the program made it less visible for a time, this could not and did not last very long. If we were performing problem-oriented studies, some of which were worked on by local social scientists, then we began after a year or so to be questioned about the projects, whatever they were, that were performed "mostly by social scientists."

By describing them in "plain English," assuring in the descriptions their relevance to the DOD's mission and their heavy orientation to the war in Southeast Asia, we managed to stave off disaster. Not one Agile social science project was noted by Congress or cancelled by congressional instruction during 1966–69. But by adhering to the self-imposed rules outlined previously, we had essentially put the handwriting on the wall ourselves. It was obvious that any social research program tied to both the military mission and the war would not long survive the growing public disaffection with both. In effect, then, we were simply buying time until the inevitable happened.

Other than the congressional committees' continuing questions about diverse projects, the one positive action they took, ironically, was on the question of "coordination." However much we felt we took pains to ensure that all interested branches of the administration were informed of and were able to comment on our work and help shape our program, the congressional committees still believed we were going ahead with insufficient intergovernmental coordination. In its report on the FY 1968 budget, the House Defense Appropriations Subcommittee said of Project Agile:[1]

> . . . this year brought out the fact that the program is being extended to other geographical regions [than Southeast Asia]. The committee believes that much better coordination among the various departments and agencies of the government is required before a useful and meaningful program in many of these areas can be undertaken.

This was to be a recurring theme. The Senate Foreign Relations Committee had continued to express its unhappiness with Defense "foreign policy research." GAO (we supposed, at the committee's behest, but we never learned for certain) continued its probes and made its report. The House Defense Appropriations Subcommittee's report in 1969 (FY 1970 budget) contained these words:[2]

> Some studies in foreign countries raise the question as to whether or not they should be funded under the foreign assistance program rather than in the Defense budget. Additionally, ARPA has made no attempt to have participating countries share in the cost of such projects.

Of course, the program was planned that way, since the rationale was that these programs were in American self-interest to improve the effectiveness of our planning and implementation of military assistance programs. In retrospect, it is clear that the constant calls for more coordination as they shifted ground were really reflections of a deeper dissatisfaction. The above words showed that we were being hoisted on one of our many self-made petards, and that Senator Fulbright was finally making his point.

In the summer of 1967, the Defense Science Board held another brief study session, chaired by Rains Wallace, in which he and a number of the social scientists who had participated in or managed the DOD's behavioral and social research programs tried to chart a new course in the after light of the Camelot events and the effect they had had on the outside world's interest in DOD social research. Among other things, the report ranged over the DOD's entire social science program and selected "for increased effort and funding" in the area of "increasing understanding of operational problems in foreign areas." It said specifically:

> Despite the difficulties attendant upon research in foreign areas, it must be explicitly recognized that the missions of the DOD cannot be successfully performed in the absence of information on (a) socio-cultural patterns in various areas including beliefs, values, motivations, etc.; (b) the social organization of troops including political, religious, and economic; (c) the effect of change and innovation upon socio-cultural patterns and socio-cultural organization of groups; and (d) study and evaluation of action programs initiated by U.S. or foreign agencies in underdeveloped countries. Solid, precise, comparative, and current empirical data developed in a programmatic rather than diffuse and opportunistic fashion are urgently needed for many areas of the world.

> This goal should be pursued by: (a) multidisciplinary research teams; (b) series of field studies in relevant countries; (c) strong representation of quantitative and analytic skills; and (d) a broad empirical database.

And it was also observed, with regard to the problems of involving first-rate professionals, that:

> More high quality scientists could probably be interested in DOD problems if DOD would more frequently state its research needs in terms which are meaningful to the investigator rather than to the military. . . .

While the full dimensions of the DOD's problems were only slowly becoming apparent at that time, it is easy to see now that the report was bucking the long-term trend, and some in the DOD noted at the time that such recommendations had passed beyond the realm of political feasibility.

The report reached the news,[3] and its recommendations were quoted extensively. Senator Fulbright immediately questioned whether the DOD really meant to implement such recommendations. He was reassured that the DOD would view them with caution and, in fact, they quietly died (at least those recommendations having to do with research on and in foreign areas).

Meantime, the National Academy of Sciences/National Research Council committee that had been established after Camelot had kept at its work. Its base and interests were broadened, as illustrated by the title of the report it issued in 1968: "The Behavioral Sciences and the Federal Government."[4] After cautioning that "There is no assumption . . . that knowledge is a substitute for wisdom or common sense for decision making," it went on to say that "The behavioral sciences [anthropology, economics, history, political science, psychology, and sociology] are, nonetheless, an important source of information, analysis, and explanation about group and individual behavior, and thus an essential and increasingly relevant instrument of modern government. . . . There is need to be concerned as much with the development of the behavioral sciences as with their use. . . ."

A number of recommendations were made: to strengthen the behavioral sciences and their role in federal policy planning; to use the National Science Foundation as a source of support for basic research to build the necessary "technical" base and underlying subject knowledge; and to achieve better social science representation in the councils of science—the Office of

Science and Technology and the President's Science Advisory Committee—closest to the head of government. These will be of interest in a later context (see chapter 22).

With respect to the kind of work we were trying to have done through ARPA, the committee report recommended:

> That each major department and agency, with the assistance of an advisory panel of behavioral scientists, develop a strategy for the use and support of the behavioral sciences and maintain under continual review a long-range research program that includes:
>
> a. A broad spectrum of research activities from applied research to investigations of fundamental behavioral and social processes relevant to department or agency missions;
>
> b. Opportunities through internal staffs and contract and grant arrangements to utilize research resources both inside and outside the government;
>
> c. Continuing programs for the systematic maintenance of historical and operating records as essential sources of research data; and
>
> d. Application of behavioral science knowledge and methods to program evaluation and analysis with provision for experimental projects designed to provide relevant information for future planning.

This was almost identical with the Smithsonian report's recommendations, and represented advice that the DOD had diligently been trying to follow since 1963. The report also stated that:

> 3. The major mechanism for relating research programs in international affairs on an interagency basis is the Foreign Area Research Coordination Group (FAR). FAR, however, is a voluntary group of some 20 participating agencies with no binding authority over its members and no firm lines to the policy planning process. The Foreign Affairs Research Council in the Department of State serves as another clearinghouse through its function of reviewing research projects for their sensitivity to foreign policy issues. Neither mechanism provides a basis for defining government-wide objectives for research in international affairs. There are no organized means of assuring that areas of research essential to policy planning are supported and that cumulative bodies of knowledge on international problems are developed.

. . . the Committee recommends:

> 4. That, in the field of foreign affairs, long-range behavioral science research objectives be drawn up by an interagency planning group headed by the Department of State, with the support of the Office of Science and Technology, and that the research programs of all departments and agencies that operate overseas, including the United States Information Agency, Agency for International Development, Department of Defense, and the Peace Corps, be continually related to these long-term objectives through the Foreign Area Research Coordination Group and foreign affairs planning mechanism like the Senior Interdepartmental Group

Thus, a number of continuing threads—positive and negative—were entangled. The idea of an interagency coordinating group once again reared its head with (it turned out) about as much chance of being adopted this time as any of the other times. And it appeared (to one not familiar with the internal deliberations of the group) that State had finally found a friendly ear for its contention that FAR should be in the driver's seat for research having to do with foreign areas. In the last analysis, a recommendation to this effect by the NAS/NRC committee was mischievous because the committee could ignore the key problem that had plagued the idea of FAR control from the start: giving one department of government authority over programs in another department. But, worse still, FAR was, by the report's own findings, not organized for or adequate to do the job. While the NAS/NRC committee hoped they could rise to it or be made to do so, the negative advantages of the management practice of giving responsibility and authority for a job to a manager one knows in advance is inadequate in the hope that the capability will grow should by this time have been obvious, at least in this context. This was the trap we fell into before Camelot, in assigning work to the Army and its instruments, and there was no reason to believe in this case that the outcome would be different.

At any rate, it turned out to be irrelevant, for Senators Fulbright and Mansfield were preparing moves that would continue the trends against social research regarding foreign areas, regardless of what the scientific community desired or recommended; and that they were to have much more profound implications for basic science in general. In 1969, the dissatisfaction in Congress with the DOD, its wars, and its works was increasing. There was a new administration making a point of trying to bring the war to a conclusion and at least somewhat agreeing that "priorities" had to be

"reordered." In August of that same year, Senator Fulbright introduced an amendment to the FY 1970 Defense authorization bill to reduce the DOD research budget by $9.5 million—$3.5 million to be taken from Federal Contract Research Centers, $1 million to be taken from "foreign research" and "social and behavioral sciences," and $5 million from Project Agile.[5] The amendment was incorporated in the bill, which passed in the Senate and was ultimately sustained in the House and in conference.

Shortly afterward, Senator Mansfield introduced another amendment, which became Section 203 of the bill, that said:[6]

> None of the funds authorized to be appropriated by this act may be used to carry out any research project or study unless such a project or study has a direct or apparent relationship to a specific military function or operation.

In approving its version of the bill in September, the House Armed Services Committee included Section 203, saying also:[7]

> We interpret "military function or operation" in its narrowest sense.

The final act included Section 203, of course. In introducing his amendment, Senator Mansfield stressed basic university research sponsored by the DOD as the culprit, saying later[8] that the DOD was supporting $400 million of "non-mission oriented research and development projects," with basic research "of the kind traditionally carried out in the universities at a level of $311 million in comparison with $277 million for the National Science Foundation." Congress, he said, "by writing Section 203 is giving clear notice (among other things):

> 5. That primary responsibility for government support for behavioral science research and training conducted in foreign countries by universities in the United States be placed in agencies and programs committed to basic research and research training, particularly the National Science Foundation, the National Institutes of Health, and the proposed Center for Educational Cooperation under the International Education Act.

But though he stressed basic and university research, he devoted the details of his reasoning to DOD research in the social sciences, giving a long list of DOD-supported social science projects (ironically, not including any from Agile's program) that he said were not relevant to the DOD mission.

The impact of Section 203 was not limited to social science projects, however, nor was it intended to be, for Senator Mansfield indicated his *intent* to attain the "reasonable goal" of reducing the $311 million of DOD funding academic research to no more than 25 percent of that funded by NSF by the end of FY 1971. Yet it was only by FY 1973 that the NSF budget was increased by more than $100 million above its previous level, so that if this had been followed faithfully universities would have lost over $200 million per year in support for scientific research.[9] Senator Mansfield thought that the president, "as a matter of national policy, might decide to reduce the overall level of support for academic research. That latter, I would add, would not be a national calamity."

Only time will tell whether it was. But since DOD support for basic science was one of the pillars upholding it, the Mansfield Amendment (Section 203) can be said to have marked a turning point in national support for scientific research and the beginning of a leveling off and downturn in federal budgeting for such research from which the universities and the scientific community in general have not yet recovered.[10] The immediate impact of Section 203 was to eliminate about $10 million worth of DOD research projects. However, there were now more stringent limitations on the work DOD could support, and it appears that most other federal agencies are taking heed and are wary of supporting research that cannot be shown to meet the same criteria of mission specifically in their own areas. As we have seen in connection with the constraints on DOD work overseas, one of the best ways to stultify free inquiry into important questions, "applied" or not, is to subject them to the test of being necessary to support an immediate operational mission. And as Nichols has pointed out,[11] the Mansfield Amendment threw into focus the problem of science policy and became a rallying point for those who did not believe the "tired rhetoric" supporting R&D and felt there were better things to do with the public's money. If Section 203 was not the entire moving force behind the changed national attitude to and support for science, it added a strong force to the downward trend, and thus it seemed that the waves made by Camelot were now, four and five years later, eroding much wider and farther shores than just "DOD social science research."*

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* Basic and applied research in the American economy has had a rollercoaster kind of history since the time of the Mansfield Amendment. On average, however, total government-supported research and development (R&D) increased by more than a factor of two from the time this was written to a peak in FY 2008 (from roughly $60 billion to $110 billion in constant FY 2000 dollars, according to U.S. government data at www.whitehouse.gov/files/documents/ostp/pdf/federal_rd_spendingchart.pdf).

Senators Fulbright and Mansfield worked closely together to ensure that the provisions of Section 203 were carried out. The occasion for a sharp exchange with the DOD arose in October 1969, when Senator Fulbright sent Dr. Foster, the director of Defense Research and Engineering, a clipping about a Themis contract that had been awarded to the University of Mississippi and asked whether the project in question did not stretch the constraints of Section 203.[12] Ironically, this was not a social science project in the usual sense of those the senator objected to; it had to do with birds and illustrated well the problem of what I have described as the "lunatic fringe" among research scientists. For while, this project may not have done as much violence to scientific, ethical, and humanitarian consideration as the others from the periphery that had been seeking my own support; it was "far out" enough to tickle a newshawk's curiosity. The contract—a serious one—was to investigate the possibility of training birds to perform various military tasks. There had been some recent experience in this area. It has been shown, for example, that pigeons could be trained to recognize certain kinds of routine military targets in an aerial photograph more reliably than human photo-interpreters could, and that they could be trained to recognize people in ambush in real life, stop in front of them, and peck a radio transmitter to give an alert. The contract apparently (I have no first-hand knowledge of it, but am surmising from what I have seen written) intended to see whether such skills and related ones could be extended in scope and to other species of birds. It may well have been trying to stretch an originally interesting idea too far.

A group working with birds for such purposes had tried to obtain Project Agile support for their work for some years. I had declined on the basis of my past view of some of the early results. These demonstrated that, even if an important range of skills could be trained into birds, the military system as a whole was unlikely to take the accomplishment seriously, and therefore the effort would be a waste of money. But the bird's trainers in that case persisted and eventually found a source of support in the DOD. Perhaps this same group had turned up at the University of Mississippi—I do not know—and perhaps the University of Mississippi group found support elsewhere when their Themis contract with the Army was eventually terminated. We had found that a determined and persistent group could eas-

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Currently, as this is written, there is concern that the federal R&D budget will be cut drastically in the drive to reduce the national debt without significant tax increases (*Science* Vol. 338, #6108, November 9, 2012, editorial, 722).

ily keep itself alive by following the money and adapting to the current "buzz-words." The news report described it thus:[13]

> Flying Off to Combat?—Birds Alerted for War—Would you believe that war is for the birds?

At any rate, the response to Senator Fulbright's letter on November 3 after explaining the bird contract, said that:[14]

> The research programs of the military departments and Defense agencies are under continual review by elements of DOD and receive, in addition, the critical scrutiny of my office. It has long been DOD policy to support only research which is relevant to military functions and operations. Most of our projects in the research and exploratory development budget categories (from which comes most of our university funding) are, in fact, relevant to many military operations. From time to time, however, we eliminate support for research fields which are no longer relevant to DOD needs; high energy physics is a recent example. I do not expect, therefore, that implementation of these sections will entail any new type of review or selection. Nevertheless, Secretary Laird, Secretary Packard, and I have been instituting a number of new management approaches which will provide a basis for more coherent and explicit presentations to the Congress about the basis for our budget requests.

This triggered a storm. Senator Mansfield said on the Senate floor[15] that "Congress, when it enacts its laws, does not attempt to waste time on futile gestures. . . . It is very upsetting to see any executive agency disabusing the clear expression of congressional intent. . . . ," and Senator Proxmire added that "There is no question that Federal research has been overwhelmingly sponsored by the Department of Defense in the last few years. . . . If we provide funds for the Department of Defense, this is one area where we do not adequately scrutinize them . . ." (This depended, of course, on how much of the DOD's $7-8 billion of RDT&E money is considered "research." Nichols has pointed out[16] that the DOD share of federal support for university research went from 47 percent in 1955 to 14 percent in 1971, while HEW's share grew from 19 percent to 45 percent, and NSF's from 5 percent to 18 percent.

So violent was the reaction that Deputy Secretary of Defense Packard had to repudiate his DDR&E, writing to Senator Mansfield:[17]

> There is absolutely no question that the Department will comply fully with the law. I have directed all components to review critically all current and proposed research and development projects and studies to ensure that they have a direct, apparent, and clearly documented relationship to one or more specifically identified military functions or operations. Any project or study which does not fulfill the criterion of Section 203 will be terminated.

Congressman Daddario, head of the House Subcommittee on Space and Astronautics, in his last term prior to resigning to run for Governor of Connecticut, pointed out the dangers of this course of action:[18]

> I fear that there exists today a very really danger that research in the universities and elsewhere, now funded for defense appropriations and which should be continued in the national interest, will be fatally disrupted by a mechanistic and legalistic application of the strictures of section 203. . . .
>
> Congress must give urgent and immediate thought to arrangements that will identify and provide for the orderly, uninterrupted transfer and continuation of research adversely affected by section 203, which should still be carried on in the national interest.

But the problem was not treated with urgency.

Senator Fulbright returned to the attack the following year.[19] Noting that he had been refused a copy of a study, he said was prepared for the DOD by the Institute for Defense Analyses on the Tonkin Gulf incident, as well as his continued irritation that the DOD was still letting contracts on such subjects as "Soviet military policy" or "European security issues," he introduced a two-part amendment that, in his words, would:

> First. Limit the Defense Department's spending for research by outside organizations on foreign affairs matters to not more than the amount appropriated, or transferred by other agencies, to the Department of State in the preceding fiscal year for such research; and
>
> Second. Insure that congressional committees are given access to Government-financed research studies carried out by private individuals or organizations unless "executive privilege" is invoked.

Senator Fulbright pointed out that, despite substantial budget cuts, "The military is spending nearly 20 times as much on foreign affairs re-

search as the agency assigned the primary responsibility for conduct of the Nation's foreign policy.

However, now that the primary objectives of all the years of pressure on DOD's "foreign affairs research" had been achieved, this represented but the winding up of a campaign, as indicated by the following exchange between Senator McIntyre of New Hampshire, who had chaired a subcommittee on R&D of the Armed Services Committee, and Senator Fulbright:[20]

> **Mr. McIntyre**
>
> . . . The Defense Department's foreign area research program has always represented only a small part of its work on the behavioral and social sciences. The great bulk of its effort has been expanded in the areas of: first, human performance—studies of the performance of men under stress; second, manpower selection and training—studies of the best methods for training men for various positions in the Armed Forces; and third, human factors engineering—studies to insure that military hardware is designed for safe, efficient, and effective use under battlefield conditions. The foreign area research budget of the department itself divided into two components—foreign military security environments and policy planning studies.
>
> . . . The fiscal 1971 Defense Department budget included a request of $9.9 million for foreign area research. In light of the department's reluctance to approve outright transfers of funds to the State Department and in light of the policy expressed in section 203 of last year's bill, the committee subjected this request to a thorough, almost painstaking examination. As a result of this examination, it recommended a reduction of $3.1 million—over 30 percent—from the department's proposed budget, brining it to a level of $6.8 million. This reduction was directed primarily to work in counterinsurgency operations and work proposed by the military services but deemed more appropriate either to the State Department or the International Security Affairs Office of the Department of Defense. The remaining funds are earmarked for projects which, while of interest to the State Department, bear a clear relationship to the Defense Department's own mission.
>
> . . . As for the Defense Department itself, its foreign area research budget now declined from $16.1 million in fiscal 1968 to a committee-recommended fiscal 1971 budget of $6.8 million, an overall reduction of 58 percent.

I would like to ask my colleague, as chairman of the Foreign Relations Committee, what actions he has taken to increase State's own foreign area research budget in recent years, and just what he feels has to be done by the Congress to get State moving in this area?

Mr. Fulbright

. . . I do not know how to inspire the State Department to assert its responsibility in this area. The State Department has not in recent years had very much influence in the budget process. Matters that are clearly within the State Department, such as the exchange program, have been restrained very severely through the budget and by action of the Appropriations Committee.

As a matter of fact, the Senator knows that all agencies other than the Department of Defense have great difficulty when it comes to getting money. . . . The State Department has had other pressing budget problems and they have not tried very hard, apparently, to get more money for research. I have counseled that they do so but there have been no effective results.

Mr. McIntyre

. . . The Senator just supported the amendment I offered, which is an outgrowth of section 203, which is the application of the relevancy test, which has given us quite a few problems in connection with the defense budget. This amendment is an attempt to bring the National Science Foundation into this picture more clearly as an institution solely devoted to research. When there is budget stringency and a need to cut, the cut is too often at the research end. This amendment points one direction in which we have to go. But there have to be increases, too, in the research budgets of other mission agencies. The State Department is one of these.

It is said the Department of Defense is doing too much in research. The Department of Defense will meet that argument by saying, "Those areas where we are carrying on research may well go to the National Science Foundation and to the State Department, but we think these areas of research are important and should be done, and no one else is now doing them." So we will need the Senator's cooperation, as chairman of the Committee on Foreign Relations, to bring about this reordering of research within the Government today.

Senator Fulbright then withdrew the first part of his amendment.

This appears as good a place as any to bring to a close this chronicle of the DOD's efforts to enlist social research in support of its overseas operational assignments. It is now time to look back and see what the broader meaning of these events might be. But the stage might well be set by a somewhat wry epilogue.

This is provided by still another report of the NAS/NRC committee that, having published the report about social science research for the federal government in general and having experienced some turnover of its membership, turned its attention back to the problem that originally led to its creation: the DOD's social science research program. After two more years of deliberation and some drafting, it published another report, "Behavioral and Social Science Research in the Department of Defense: A Framework for Management," in February 1971. It is not necessary to dwell in detail on its findings and its reasoning; suffice it to say that the nature of the subtleties and sensitivities in social research overseas was recognized. One area was that of the clash of values between the researcher and the supporting agency, about which the report had this to say:

> The most significant distinction among the categories of research is that foreign military security—environments and policy—planning research is inherently politically sensitive, while manpower research is not, although it, too, has had its controversial projects. Indeed, research on man-machine relationships and "human engineering" has an Orwellian tone, but by and large a psychologist could work on problems of improving selection and training choices through psychological testing without having to confront possible conflicts between his own value system and the value system implicit in the area of research. This is less true for categories of security-environments and policy-planning studies. Regardless of questions of political sensitivity, much policy-planning research has to be done by and for the Department of Defense because it deals with issues of strategy, force structure, and budgets. *The Department of Defense should not be foreclosed from undertaking such research* [emphasis added].

Another problem area was that of having applied research done in such a way that basic knowledge could be applied to and influence real problems. The report's words have a familiar ring:

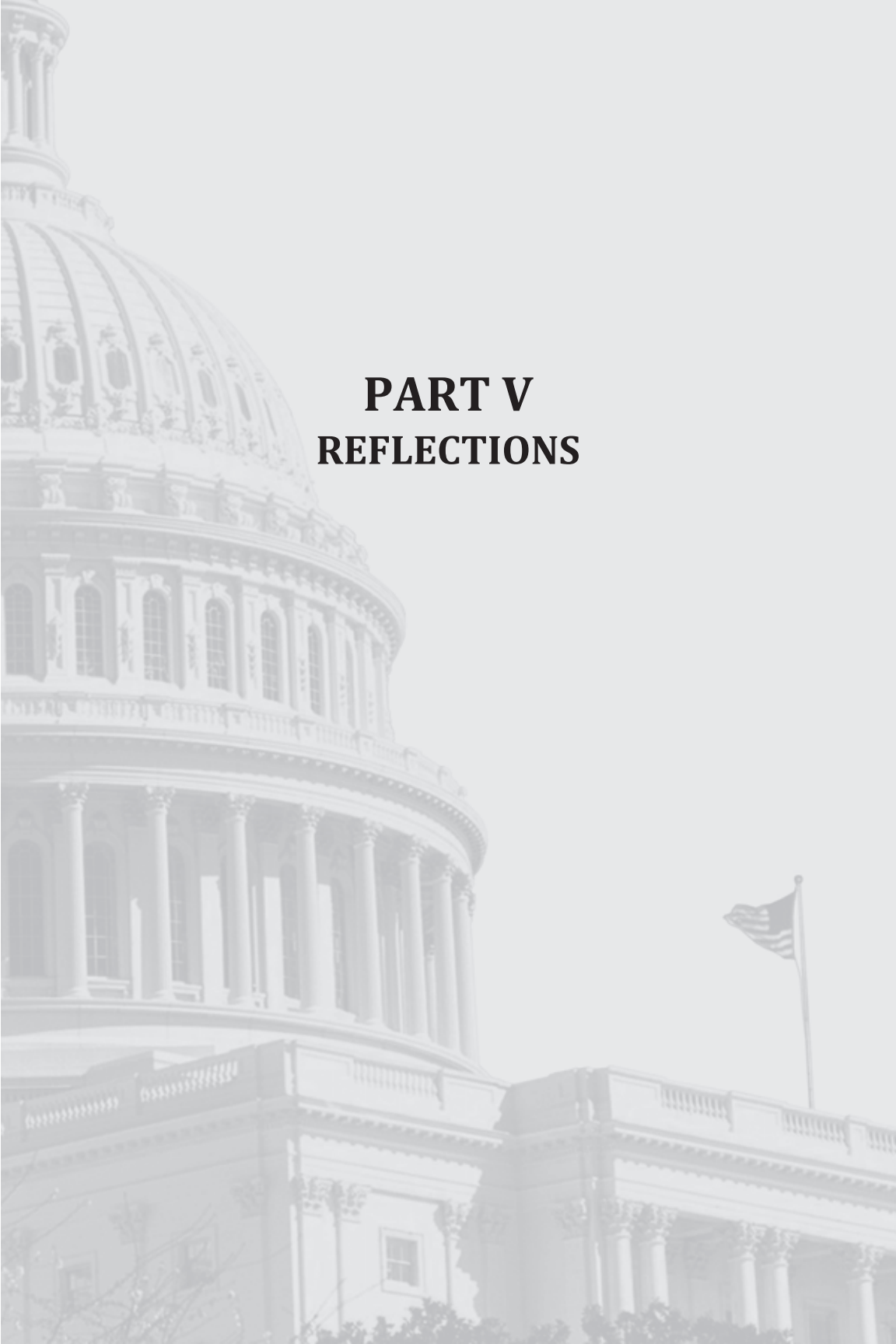
> In the national security area, the methodology of social science research now permits and warrants substantial funding for such efforts as simulations of the operational environments that policy makers may posit as constituting the range of possibilities for which the nation must be prepared. Computer-based simulation studies on a large scale are likely to be expensive. They may be used either for fundamental research or for engineering development, and it is particularly difficult in the social sciences to draw a dividing line between different stages of research and development. But, under whatever label, they constitute a qualitatively different mode of behavioral research than is encompassed by the traditional expectations of many foreign-affairs practitioners, who believe that the social sciences can provide little or nothing beyond humanistic, individual, historical research. The limits of utility on "engineering development studies" in the behavioral and social science areas of Department of Defense research are not yet known with any precision. However, it is clear that those limits have not been approached and that an adequate effort to develop the engineering side of the behavioral and social science research has not yet been undertaken. The potential for such work is perhaps especially great in the area of manpower research (psychological testing mechanisms, for example), but it also exists in the policy-planning and foreign-area spheres.

Among the recommendations were the following:

> . . . The Department of Defense should actively seek the transfer of responsibility for the support and management of foreign area research, and it should strongly endorse the creation of a government-wide institutional structure—to which it would have access and in which it would have a chance to voice its informational needs—in which this responsibility should be logged.
>
> . . . The national security agencies jointly establish a task force on social and behavioral science research priorities in the area of national security policy.
>
> . . . The Department of Defense, in order to bring about a more effective managerial relationship between the producers and consumers of research:
>
> . . . provide funds for retrospective studies in the social and behavioral sciences designed to establish the relationship, if any, between basic research and programmatically useful results.

> . . . allocate funds for evaluative studies of on-going programs that allow for the questioning of policy assumptions and the proposal of programs alternative to those under analysis, in order to suggest how programs might be modified in the future.

And so, despite the subtle differences in context and appreciation of the problems, it would seem that *plus ça change, plus c'est la même chose*. The lessons gained through eight years of trying to do what was recommended anew had, apparently, not yet sunk in.



PART V
REFLECTIONS

Chapter 22
"What Have You Learned"

During the Camelot hearings of the Fascell subcommittee, Congressman Frelinghuysen asked:[1] "What have you learned as a result of your experience which has resulted in the termination of the project?" The only answer I could give him at the time was the immediately obvious and superficial one: we had confirmed our suspicion that the Army's approach to social research on the problem of revolutionary change would not work and, since the DOD still felt it needed the knowledge and data that the research sought, we would have to find another, more subtle way to get the job done. This was, of course, not a satisfactory answer for Congress as they conveyed by their subsequent action in cutting the budget for this kind of work.

The Camelot experience represented but the beginning of learning. It led to experimentation with various approaches to the research and with diverse institutional arrangements for its support. With all that under our belts, it is possible to answer the congressman's question with more insight. To state the conclusion first, it is roughly as follows: in our culture, government support of social research to help government's own ends must be approached with circumspection, great selectivity, limited purposes, and careful attention to the potential effects of the very performance of the research, as well as the results, on the researchers, the objects of the research, and the general public.

It can be argued that Congress had known this all along, and that the proposition was stated in different words at the time of Camelot. But the expression then was essentially a reaction to particular events. It referred explicitly to the DOD, and it was stated in terms of propriety. It is my contention at this juncture that the lesson has wider application. If the reasons for the problems and ultimate failure of the DOD program can be explained by factors more universal than those particular to the setting of an unpopular war in unfamiliar lands then those factors should be identifiable in other situations. If they are, then social research in those situations may fare about as well (or as poorly) as did the DOD's social research efforts in its particular problem area.

It appears to me now that the key question is not the propriety of work undertaken by a particular agency, but rather the feasibility of this kind of work in the social and cultural setting of government trying to get its job done in the public eye. It is a matter of how the parties to the research con-

tract interact and of the effect of their interaction on the subjects of research and on outside observers. Let us explore their motivations from the vantage point of all that has passed.

To perform effective research, the social scientist would like to keep the situation he is studying fluid but under careful control. He wants, ideally, to be able to examine all the important variables of the problem, keeping most of them fairly constant while others are varied systematically, and data on their effects and interactions are carefully recorded. That is, he would like to change some variables (here, conditions of a social group), while others remain about the same, so that the effect of such changes can be observed and analyzed in a systematic way. From the scientist's viewpoint, this means that a social program should not "gel" very early. In fact, the initial purpose of his research is also to ascertain the direction in which a social program should be molded and guided before it is allowed to assume immutable characteristics.

The bureaucrat, on the other hand, has a job to do with operational objectives that, however defined, are normally viewed by him as clear and straightforward. The task may be educating people, or raising them from poverty, or helping them form economic institutions to create jobs, or helping them form quasi-political institutions to deal with the more formal elements of government. The achievements are likely to be measured as concrete outputs; so many children in school; so many curriculum changes; so many jobs; or so many neighborhood associations. Whether the children become educated or the jobs are "satisfying" takes too long to determine and is left for the next generation to judge.

While the program director wants to keep his "options" open at the beginning, this is not so much for the purpose of understanding the subjects' responses thoroughly and adapting the program to them, as it is to help ascertain the overt signs of potential success—public acceptance and the accommodation of all the various political, economic, and bureaucratic interests involved. Once the bureaucrat sees his path through this essentially political jungle, he wants to fix the program and change it as little as possible. Each undertaking within the program represents a commitment of prestige and money on the part of the bureaucracy in a visible arena. Changing the program can easily be read by the bureaucrat's political superiors and by the public as a sign that the bureaucrat does not know what he is doing. In many cases, there will have been, and may continue to be, vocal opposition to the program, the effect of which is to cause the bureaucrat responsible for its

implementation to fix its dimensions and parameters even more rigidly than he might otherwise have been moved to do.

Thus, the ultimate desiderata of the researcher and the bureaucrat diverge from the beginning. The researcher wants to be able to design and mold a social experiment so that its results can help design a better social action program. The bureaucrat wants to get a job done, where that job is likely to have been defined rather vaguely by the instruments of society at large, and more precisely by himself and his colleagues. These divergent motivations lead to clashes over matters revolving around timing and publicity. The bureaucrat has only so much time before he must undergo the usual tests of bureaucratic success. The researcher, on the other hand, wants his research and data gathering to be paced by the changes in behavior and attitudes of the subject population. Thus, each of the participants has his critical milestone. Events in the bureaucrat's life are tied to budget cycles and electoral cycles, whereas events in the researchers life are tied to the evolutionary periods of attitudes and social institutions. The lengths of the respective periods can differ by years or decades.

Among the main personal satisfactions for the researcher, other than that in a job well done, are the fame and fortune that arise from making his work known and having it recognized by his colleagues. It is important for knowledge and for future programs that the current program's vicissitudes, the false starts and their effects, and the fortunate or unfortunate chance variables be made a part of this published record. Since he is dealing with a society and all its interacting segments, some of these variables involve the people guiding the program as well as those affected by it. To the bureaucrat, publication of such information is anathema. His budget and his success lie not with his mistakes or with his learning processes, but with his ultimate achievements. Exposure of his intermediate operations, especially before his results are made apparent, exposes him to scandal in the press, questioning by Congress, the displeasure of his superiors, and the machinations of his rivals. He is therefore motivated to suppress the material that is the scientist's nourishment.

The disparate motivations interact strongly with the events under study. While the researcher's ultimate objective is to affect them, the best usages of science require him to observe them first. The operator, on the other hand, wants to affect events immediately, and once he has found the means to do so he wants to sustain their momentum. In some situations, such as those that have been the subject of this book, the events, once initiated, may be beyond anyone's control, and they may move too fast for careful obser-

vation. Moreover, as they become more important and sensitive, observation becomes more difficult because access to the population and the key players is reduced. This happens partly for lack of time, but primarily because the stress of the events themselves makes both population and bureaucracy less willing to subject themselves to examination and less able to perceive what is happening and to articulate their reactions clearly. The bureaucracy's need for secrecy intensifies while it evaluates the effects of the events, both on its own position and efforts and on the population that simultaneously serves and tries to lead.

If the bureaucracy controls access to the so-called target population by virtue of its sponsorship of the research, it is able to enforce its need for privacy or secrecy easily by denial of that access. In such circumstances, systematic collection of data becomes difficult, if not impossible, and the research is forced into an increasingly unscientific mode. The less rigorous and carefully controlled the research becomes, and the less coherent and definitive the data, the more the researcher is forced to make intuitive judgments to fill the gaps if he is not to give up altogether (and the impulse to keep going, when the affairs under study are of vital concern, is difficult to resist). But when the researcher, who is really an observer on the outside looking in, makes intuitive judgments instead of reaching conclusions based on data, these judgments place him in conflict with the bureaucrat, who is really in control of the relationship and whose own judgments are the ones that lead to action and (he believes) control his future. The researcher thus finds himself increasingly less able to influence events and to achieve his own professional objectives. If he becomes sufficiently frustrated, he will turn on the bureaucrat in a mood of disaffection and criticism. And, of course, the bureaucrat will fight back, using the ultimate sanction—withdrawal of sponsorship and money.

All these problems are, however, merely reflections of something more basic—they are rooted in fundamental clashes of values among the participants in the drama of social research. Myrdal has pointed out that social research is not value-free.[2] In the case of government supporting social research to help achieve its ends, each of the participants has his own value system. The researchers adhere to a set of premises about the world. (These were illustrated for the beginning of the events described in chapter 3). The government must share these premises if it wants to employ the researchers and if the latter are to be willing to work for that government. In finer detail, the members of the government bureaucracy must share values with its leaders—the president and his appointees—if the president expects the bu-

reaucracy to carry out his instructions effectively. The frustration of President Kennedy with the State Department was an illustration of the consequences when such a coincidence of values fails to exist within the executive branch.[3]

The population being studied must also share premises and values with researchers (or at the very least they must feel neutrally about any differences) or they will not respond to the researchers as the latter go about their business of gathering information about the population. Suspicion on this point led South Vietnamese officialdom to control carefully the American researchers' access to particular prisoners; and there was, in turn, suspicion on the part of at least some American researchers that respondents were telling them only what the respondents thought the researchers wanted to hear. This suspicion was a tacit recognition that ideas of "objective" inquiry play a different role in Vietnamese and the other Eastern cultures than they do in our own.

A high-level Thai government official once asked me why American anthropologists were so interested in the hill tribes on the fringes of his country. If they came to Thailand for research, why did they not study the *Thai* (i.e., the dominant ethnic group)? No amount of explanation of the inherent interest these anthropologists had in learning about primitive and disappearing cultures would quell his suspicion that the tribesman were being examined to satisfy ulterior purposes in the control of Thailand's border areas. It also emerged in this conversation, interestingly enough, that study of the Thai culture might lead to data proving the Thai ancestory of peoples in neighboring countries; those countries had been formed, it was told, in part by the accretion of provinces striped from Thailand by the French and the British. This proof of their Thai origins could support hopes of reconquest, just as study of the hill tribes could fuel suspicion of further colonialist adventures. The basic idea of performing research primarily to *know*, taken for granted by the Americans, was alien to local officialdom. Of course, that officialdom had occasions to learn of policy recommendations made by the "objective" researchers that were inimical to its own interest—for example, that rebellious hill tribes could be "pacified" by changing Thai law to accommodate the swidden agricultural and other customs of the hill tribes, instead of forcing the tribes to accommodate to laws not written with them in mind. Even the people being studied assumed that the researchers were there with some ulterior purpose, and indeed the researchers often had to earn their welcome by demonstrating support for the people they worked with if they became involved, by their very presence, in issues affecting those people. The Americans' desire to be wanted and helpful against injustice fell right in with these local purposes.

These examples illustrate the need for consonance or reconciliation of values among the major participants in social research. Of course, the government bureaucracy, the researchers, and the population being studied are the ones directly involved in the issues. But the majority of the public also has values, premises, and points of view. Although this public is composed of many diverse elements, it is probably a safe generalization that is tolerant of "long-hair" stuff, such as social research, if that research does not challenge its ideas of propriety, thrift, and suspicion of the obscure or dishonest in government and its activities. Should such a challenge arise, the press is ever ready to expose it and Congress to act by cutting off funds and otherwise prohibiting the activity. There will be no outcry of opposition to reduction of research, especially social research, by the general public.

This is not to imply that any of these responses are "wrong" or unjustified. Again, Myrdal[4] has made the point that values operate on two levels. One is a "higher" level that in our society specifies what is "right" and "wrong" in the view of the vast majority of the people—for example, in our society, adherence to the Ten Commandments can be considered "right" and coercion is "wrong." Most people adhere to values at this level more when acting or reacting as part of a group than as individuals. Then there is a lower, or "practical" level, where individuals reconcile their sometimes-unhappy daily interactions with their fellows for personal interest and gain with the higher level of social objectives that continues to hold the lure of ultimate attainment. When events are important enough to shake people out of their daily interests and to make them feel that some group is violating the higher values, the response is natural to want to call those violators to account; there is likely even to be a certain unconscious assuagement of conscience in this, relating to their earlier support or tolerance of the undesirable behavior. As we have seen in connection with the Vietnam War, when members of the press and the university community of social scientists began to turn against the war, those performing social research for the government in connection with the war came, to them, to acquire the same sinister qualities and motives that were ascribed to the government, especially to the U.S. military that was actively prosecuting the war.

Further, I have noted earlier that in social research on substantive questions a phenomenon akin to the Heisenberg Uncertainty Principle appears to operate. The fact and means of measurement and observation affect and change the phenomena being observed and all the participants. Even if they begin supporting the government view, the researchers will elicit facts and insights and will form new viewpoints that lead them to understand the

government approach and programs differently and, quite likely, to want to change them. The government bureaucracy will, in the fashion of all bureaucracies, recoil defensively. The population affected by the government programs—the subjects of the research—will become sensitized to the issues by the very fact of the research. That population's outlook and its responses will change; and these changes will become more widely known and will affect the more general public, whose views and tolerance will change as well. The more fundamental the issue, the more likely that these changes will take place. The closer to the problems of human survival and dignity the issues of concern, the more profound and emotionally charged the changes are likely to be. The longer the research takes, the more likely that these changes will occur before it is completed. For reasons such as these, even if government initiates the alliance with science, that alliance is unlikely to continue amicably until the task is done.

Seen in this light, the events, conflicts, and outcome of the Defense Department's attempt to use social research to help it grapple with the problems of revolutionary warfare on a world-wide scale are easy to understand. Viewed in retrospect, Camelot and the subsequent storms were simply the shocks of adjustment as stresses were relieved and the entire intricate, delicate structure of relationships took on a new form.*

All of this is, of course, speculative theorizing that cannot be "proved" with the evidence I have available. But it seems to me to meet the test of reasonableness, and it appears to be supported by extensive and detailed observations made in the course of the program we have been examining here. If there is more general value to those observations, the conclusions derived from them should apply to other social research programs that have been of interest to and supported by the government. While social research is not my field and I claim no special expertise in its history or applications, my meager readings suggest that this wider relevance does indeed exist. It seems appropriate to close with a few observations as to why I believe this

* It is worth interjecting at this point that this kind of conflict between the value systems of "players" and observers still takes place (in 2013) during this period of conflict in the world of Islam that spills over to affect the more politically settled and democratic parts of North America and Europe and parts of the Asian Far East. In the current case, there has been little publicity about the results or impact of the Minerva Initiative mentioned earlier, but the same clash of values between "thinkers" and "doers" can be seen between those in government who are, for example, trying to deal with the string of wars in the Middle East and the punditry that plays the role of the outside observers—the "researchers" of Camelot days. An obvious area where this operates, for example, is in connection with the role of women in parts of Islamic society that are trying to recapture the ancient glory of Islam by rebelling against Western notions of the "equal" role women should play in society, and many other such areas can be identified as the "jihadist" conflicts spill over into Western societies.

is so. Others may find that it is a worthy subject of research to test more rigorously the hypothesis I have tried to convey.

It is readily observed, first, that the kinds of phenomena encountered during the DOD's overseas research efforts in Southeast Asia were not unique to that program or the situation. Sommer[5] has written of the difficulties of performing field research in such areas as the black ghetto. They include the inability of the researcher, especially if he is white, to overcome his obvious "separateness" from the group being studied; the aversion of both observer and observed to the "prying and spying" that attends the gathering of social data; fear of observing illicit behavior; lack of patience; and personal danger and discomfort—in short, all the problems encountered by the DOD's researchers in Vietnam and elsewhere, even apart from the added difficulties imposed by interactions with officialdom. As to the latter, Campbell[6] has observed in connection with such events as the Connecticut clampdown on speeding in 1955 to reduce highway deaths, that:

> . . . . given the discrepancy between promise and possibility, most administrators wisely prefer to limit evaluations to those the outcomes of which they can control, particularly insofar as published outcomes or press releases are concerned.

Moynihan confesses that "I have been guilty of optimism about the use of knowledge gained through social science in the management of public affairs."[7] He points out that, in the first half of the twentieth century:

> Social science was asked. . . . to attest to the equality of the races; to legitimize the demand of wage workers under capitalism to organize and bargain collectively; to provide measures of intellectual worth so that applications for college admissions and such might be judged by objective criteria; to prescribe measures for a high-level functioning of the economy. All these it did.

But, he asserts, the "old symbiotic relations" between social scientists and social activists are breaking down over the problems of implementation. He also notes that:

> The methodology [of social science] is now quite beyond the comprehension of non-social scientists. In particular, it is beyond the ken of the lawyer class that tends to wield the levers of power in American government.

This all sounds familiar. It is reinforced by all the signs of malaise in the partnership between social science and government on the domestic scene

brought out in a lengthy 1967 staff study, "The Use of Social Research in Federal Domestic Programs," for the House Committee on Government Operations.[8]

The more recent signs of disaster or indifference emerging from value clashes in government-supported social research and experimentation are legion. The affair of the Blackstone Rangers in Chicago,[9] the failure of the Clark Plan[10] for restructuring the District of Columbia school system, and the repudiation by the president of the recommendations of his commissions on pornography[11] and population[12] provide examples of the heat that can be generated when the social scientist espouses a view in his work that is not shared by the public or the government that supported the work. The failure of powerful research results to influence institutions when the findings challenge established values is illustrated by two court decisions (in California and the District of Columbia) requiring equal taxation for and distribution of educational funds among jurisdictions[13, 14] in the face of the Coleman findings[15] that educational success and funding are not correlated; and by the failure of the results of the New Jersey income tax experiment[16] to change national and state policies opposed to financial support for the working poor.

These are all questions that go to the very heart of the deep social ills that plague modern American society.* If they cannot be studied with government support or, if when they are studied the results are ignored or rejected, how can the social scientists who have tried to rationalize the issue be more effective in helping the government and the public to recognize the true nature of the phenomena and seek solutions that appear to be more in keeping with the facts that might emerge from study? Or, stated otherwise, are there ways in which social scientists and government can undertake investigations to shed light on these complex questions so that society can gain the resulting knowledge, even though the findings may challenge the prevailing values? Experience has shown that there may be some ways.

First, the results of the DOD's efforts, and the burgeoning of evaluational research in connection with government programs that are avowedly experiments, show that where clash of values is not a severe problem social scientists can be employed to design experiments and evaluate the results. A measure of social experimentation is coming to be accepted. Such things as the New Jersey income tax experiment, experiments with educational

* And the reader will be able to judge how those social ills have changed through the years even as the general statement continues to hold true.

contractors who are willing to accept the "payment for performance" principle for teaching children to read, and health insurance experiments all show that in areas where society is willing to countenance some slight departure from the norms—where no one's ox is particularly being gored—social scientists can help design the experiments and evaluate the results.

But even here, caution is necessary. We know that members of the public cannot be manipulated just to collect objective scientific information even if the results are obviously important to the public at large. It is now recognized as an ethical principle that some type of informed consent is necessary before people should be involved in an experimental manipulation. Moreover, the results may or may not be used, depending on the circumstances and motivations of those involved, as we in the DOD learned during the Program Impact Assessment effort. A 1970 report on evaluation in the federal domestic programs[17] points out that "The recent literature is unanimous in announcing the general failure of evaluation to affect decision making in a significant way."

Submerged value clashes can rise to the surface, or early results can stimulate them. A good recent example came to light when a group of Stanford Research Institute (SRI) contracts to evaluate preschool education programs were suspended by HEW because SRI was accused by two young lawyers of wasting the $12 million involved.[18] One defense that was given against this charge, to the effect that since "The government was consciously investing in building up a capability in this field. . . . there might well have been some waste . . . as there was in the early days of defense contracting," can be viewed in juxtaposition with the remark made by the lawyers that the $12 million "could have financed a whole class of students through Stanford." Large expenditures for research, with a substantial fraction "wasted" in learning a new area, are still more likely to be overlooked in the physical than in the social science arenas.

Second, the social scientist can contribute as a consultant; he can give his knowledge to government administrators and planners directly or through advisory committees. Many have been effective in such roles. But they must recognize that situations may arise in which their greatest contribution to society may be the publicity attending their resignation from such positions. The conflicts of values and objectives can obviously work in the person-to-person consulting relationship as well, and the one who could continue to be heard (and paid) must have a high frustration tolerance insofar as the following of his advice is concerned.

The social scientist can, as an alternative, join the government as an administrator at least for a time. He can then use his knowledge gained from his past research to try to "move the system" and have some of his ideas implemented. This has the greatest advantage that he need not depend on a government-to-scientist relationship or on the vicissitudes of sponsored research to make the attempt. But he will have to recognize at the outset that, even as a member of the bureaucracy—indeed, *particularly* as a member of the bureaucracy—he will face the same constraints that he would have faced as a researcher trying to work under the sponsorship of that bureaucracy or the constraints faced by any other government official trying to get a job done. He will, again, have to have a very high tolerance for frustration (or, if one will, patience), and temper his commitment to the ideal with satisfaction about what he finds possible, which is likely to be very much less than he had initially hoped for. He will have to give up being a scientist and behave like an administrator.[19] Eventually, the constraints and the environment will impose on him the values and outlook of the bureaucracy. In addition, the intellectual capital he brought with him to the government will become stale, outdated, and obsolescent; he will find himself routinely administering the few good program ideas he was able to have accepted without disasters imposed by internal bureaucratic conflicts or the curiosity and sensation seeking of the press. And then it will be time for him to leave the government.

If the social scientist explicitly wants to undertake extensive research on the key social issues of the day, he had best do so with private sponsorship. This is not to say that he would always be successful, even then. But the constraints and conflicts of sponsorship will be smaller; he will not have to face accountability for using public funds for purposes the public does not approve. He will run less risk (but, nevertheless, *some* risk) of being accused by the subjects of his research of being the representative of their oppressors. And he will be in a better position to be responsive to and concerned only about the demands of his interests, his profession, and his peers. Of course, his impact on society may still be small, distant, or irrelevant. But to the extent that the flow and evolution of the ideas of the age can have an impact on what people believe and on what government does, he will, by having eschewed government sponsorship, be in a position to press the logic of his findings at least as effectively as he could from the base of government sponsorship and probably more effectively.

And so, I am led to the final thought deriving from the experiences described in this book. As they did in the context of those experiences, the

community of social science is likely to urge and has urged that increased government support of research on the great social problems of the day. With due recognition for the government's need to collect data to help it plan and evaluate the social programs it is expected to undertake, I have reached the conclusion, nevertheless, that the opposite of the social scientists' recommendation is in order. The research is needed, without question. Some of it, especially in the evaluation area, is necessary and feasible for government to sponsor. Beyond this, its support should be subject to the economic and political laws of the intellectual marketplace. And the government should do less, not more, to influence the workings of that marketplace. It should support less, not more, research into the workings of society. It should select that which it does support carefully, attending only to those ideas and objectives finding ready acceptance elsewhere. It will find soon enough that it is, itself, subject to the effects and influences of research on social problems performed outside its purview. In the area of learning about societies, their values, and their behavior, I now believe that government can be most effective if it follows, rather than leads.*

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* And, at this distance from the events, some additional thoughts come to mind.

The first is, that since much of what we had hoped to learn in the program that spawned Project Camelot could fall under the aegis of the intelligence agencies, perhaps it would be useful to task those agencies with eliciting those data and judgments. Likely this is done—I have been far enough removed from such activities that I have no way to confirm or deny this, but a Google search for social data rarely comes up dry, and those data must come from somewhere, both inside and outside government.

A second thought is that, since the time of Camelot, the use of polling to anticipate how populations will respond to government actions has become routine and extensive. One hopes that social scientists somewhere are examining how poll results correlate with and emerge from a population's value system and its culture; how that relationship interacts with those of the leaders of government and what they hope to accomplish by the actions they take; and how they evaluate their success or failure.

And finally, the reader should not think that bureaucratic infighting of the kind described here is a thing of the past. We can expect such infighting to go on forever since it represents a quest for commanding organizational positions and control of budgets, people, and actions. Recent examples—ironically, still involving the State and Defense Departments—emerge from the wars in Iraq and Afghanistan. In his book, *Imperial Life in the Emerald City,* (New York: Knopf, 2006) *The Washington Post* reporter Rajiv Chandrasekaran describes in detail the arguments between Ambassador Bremer and the U.S. military commanders about "deBaathification"—eliminating from the Iraqi government all elements of the Saddam Hussein regime. This process included disbanding the Iraqi army, which threw many thousands of trained, armed men into the civilian economy with no sources of income and no work for them to do. The results of this action are well known as the history of what amounted to a brutal civil war in the country and an extension of the need to extend the deployment of American troops far longer than had initially been anticipated. Another argument of this kind involved the use of private security contractors to protect American civilians in Iraq and Afghanistan (see http://www.washingtonpost.com/wp-dyn/content/article/2007/10/05/AR2007100500865.html). This activity led to an international furor over what appeared to be random killings of civilians by members of the Blackwater USA firm (who, ironically, included many former U.S. servicemen, especially from the Special Forces). As noted earlier, the context may change, but bureaucratic behavior, being an extension of the culture that spawns it, changes only in detail not in character.

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