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Technology and Culture; Oct 1997; 38, 4; ProQuest Central
pg. 891

Three Approaches to Big Technology: Operations Research, Systems Engineering, and Project Management

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> If the physicists, the chemists, the mathematicians, and the engineers could combine to build an atomic bomb, why could not the same kinds of groups, working in concert, solve other problems, both military and civil? The concept of the multi-disciplinary approach was utilized in World War II, and it was only natural that the techniques thus devised should carry over. [P. STEWART MACAULAY, "The Market Place and the Ivory Tower"][1]

Military "big technology" came into its own in the mid-20th century, driven by the military technological competition of the Second World War and the cold war.[2] Scientists, engineers, and managers from industry and academia developed new weapons for their military patrons, including atomic and hydrogen bombs, jet fighters, ballistic missiles, strategic defense command and control systems, and

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[1] P. Stewart Macaulay, "The Market Place and the Ivory Tower," in *Operations Research and Systems Engineering,* ed. Charles D. Flagle, William H. Huggins, and Robert H. Roy (Baltimore, 1960), p. 6.

[2] Some good introductions to the cold war include Richard Crockatt, *The Fifty Years War: The United States and the Soviet Union in World Politics, 1941–1991* (New York, 1995); Walter Lafeber, *America, Russia, and the Cold War 1945–1992,* 7th ed. (New York, 1993); Paul Dukes, *The Last Great Game* (New York, 1989); and Stephen J. Whitfield, *The Culture of the Cold War* (Baltimore, 1991).

0040-165X/97/3804-0003$02.00

reconnaissance satellites. Faced with extraordinary demands to create and deploy novel, complex systems at a rapid pace, these three groups produced new techniques to manage the diversity and scale of information and technology. Each group developed its own approach: scientists created operations research, engineers created systems engineering, and managers created project management. Each approach reflected the efforts of a specific knowledge community to cope with the complexities of large technological systems.[3]

Operations research, systems engineering, and project management were at home in the world of large research and development organizations, particularly those of the Department of Defense (DOD) and its contractors. All three of these new "disciplines" had distinct roles in the development of procedures for military R&D. They coordinated the activities of other groups through mathematical analysis, engineering coordination, and managerial control, using borrowed mathematical and theoretical methods. Each paid close attention to the *processes* of R&D and developed procedures to control the information and interrelationships among analysis, design, and testing.

Most of the mathematical methods used in these disciplines were applications of existing practice to new problems; probability and statistics, queuing theory, symbolic logic, and matrix techniques existed long before their application to engineering systems. Some, such as game theory, information theory, and feedback control theory, were relatively new but developed prior to and separately from operations research and systems engineering. Some old methods took on new names, such as probability and statistics applied as reliability and quality assurance, and differential equations when ap-

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[3] The concept of "knowledge community" used here is closely related to the ideas of Edward W. Constant II regarding "communities of practitioners," Edith Penrose's theory of firm-specific knowledge, and the more recent work of Ross Thomson on organizational knowledge. See Edward W. Constant II, "The Social Locus of Technological Practice: Community, System, or Organization," in *The Social Construction of Technological Systems*, ed. Wiebe Bijker, Thomas Hughes, and Trevor Pinch (Cambridge, Mass., 1989), and *The Origins of the Turbojet Revolution* (Baltimore, 1980); Edith Penrose, *The Theory of the Growth of the Firm* (New York, 1959); and Ross Thomson, "The Firm and Technological Change: From Managerial Capitalism to Nineteenth-Century Innovation and Back Again," *Business and Economic History* 22 (winter 1993): 99–134. There is a growing literature on large technical systems inspired by Thomas Hughes's classic *Networks of Power* (Baltimore, 1983). It includes Renate Mayntz and Thomas Hughes, eds., *The Development of Large Technical Systems* (Boulder, 1988); Todd R. La Porte, ed., *Social Responses to Large Technical Systems: Control or Anticipation* (Dordrecht, 1991); and Jane Summerton, ed., *Changing Large Technical Systems* (Boulder, 1994). In the terminology of Bernward Joerges, the American ballistic missile programs were "large technical projects," an area not much researched in this literature.

plied to simulation. Network theory applied to scheduling and cost problems became a linchpin of project management.

The essence of these new disciplines lay not in their borrowed mathematical methods but rather in the functions they performed in research and development. Each specialized in the creation and application of what I shall call *procedural knowledge,* academically problematic but practically useful. Project managers imposed new organizational structure and process controls. Systems engineers created a new engineering function devoted to communication processes and documentation across disciplinary boundaries. Some operations researchers transformed their methods into systems analysis, a set of practices for comparing design and operational options for future technologies. Together, these techniques formed "systems management," the military-industrial method for developing new, large-scale technological systems.

Systems management and its component techniques were important elements of the "systems approach," an important intellectual development of the 1950s and 1960s with strong proponents in academia as well as the military-industrial complex.[4] It also had vehe-

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[4] It is important to distinguish between the systems approach as it developed in the 1950s aerospace industry and other "systems thinking." Besides the aerospace systems approach discussed in this article, there are at least five other significant kinds of systems thinking. The first was the combination of information theory, computing systems, and control systems often called "cybernetics," popularized by Norbert Wiener. Although Wiener did not call it a systems approach, others later did. A second systems theory was "general systems theory," initially popularized by Ludwig von Bertalanffy and later extended by Kenneth Boulding into economics. Talcott Parsons popularized a third major systems theory in his 1951 tome *The Social System* (Glencoe, Ill., 1951). Parsons's ideas derived from those of Harvard's L. J. Henderson, who was inspired by the Italian engineer Vilfredo Pareto. Pareto's theory of the "social system" drew from kinetic theory, which makes explicit definitions of the system under investigation, separating it from an external environment. The fourth approach was the system movement in the office supply industry. Office suppliers such as IBM, Burroughs, and Remington Rand sold their goods by improving the office procedures or "systems" of their customers. They developed marketing organizations specializing in understanding those procedures and selling products as part of an improved office system. The fifth approach was Gestalt psychology, which flourished in Weimar Germany. Gestalt ideas moved to the United States with two influential émigrés, Wolfgang Köhler and Kurt Lewin. After World War II these different approaches intermixed. In this article, "systems approach" will refer only to the methods sponsored by the military, operations research, systems engineering, and project (or program) management. Within the historical community, the systems approach has been used as an interpretive framework in a number of technological narratives, most prominently with the development of electrical power systems and railroads. See Hughes, *Networks of Power,* and "Evolution of Large Systems," in Bijker, Hughes, and Pinch; and Bertrand Gille, *Histoire des Techniques l'Encyclopédie de la Pléiade* (Paris, 1978).

ment critics, who thought its application to other disciplines and problems inappropriate, irrelevant, or disastrous. Despite its critics, systems management became the standard method of organizing R&D in the aerospace industry. From aerospace it spread to other industries in the United States and other countries throughout the world. This article describes the techniques that made up systems management and addresses the question of how together they could be so useful to the military and its contractors yet so questionable to their nonmilitary critics.

Technical Complexity and Change

During and after World War II, the pace of technological change in weapons systems quickened noticeably. As each side in the war—and later in the cold war—tried to gain advantage over the other or redress an imbalance of power, each developed ever more powerful, expensive, and complex weapons. Cost was not an issue, because governments were willing to pay, but the forced pace and high complexity of the technologies posed major problems. Complexity grew at an exponential rate. According to Ellis Johnson, the head of the Operations Research Office at Johns Hopkins University[5] and one of the major spokesmen for operations research: "The effect of increasing physical knowledge on the cost of weapons in a weapons system has been very great in terms of money and complexity. . . . It can be seen that this cost has increased ten-fold from 1945 to 1955. . . . In aircraft gas turbines the number of parts has increased from 9,000 in 1946 to 20,000 in 1957. Of precious engineering hours, 17,000 were required to produce a fighter aircraft in 1940, and 1,400,000 in 1955."[6]

Individual engineers no longer had the knowledge necessary to design and build an entire system. Because new technologies such as nuclear weapons, radar, and rocket propulsion had been developed only recently, military and industrial organizations had little choice but to include the physicists and "rocket scientists" (usually

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[5] During World War II and the cold war, Johns Hopkins University, in Baltimore, was the home of the Systems Research Laboratory and the Applied Physics Laboratory (APL) along with the Operations Research Office. All of these were major sites of cold war military technology development. See Paul N. Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America* (Cambridge, Mass., 1996), pp. 218–19, on the Systems Research Laboratory. On the postwar APL, see Michael Aaron Dennis, " 'Our First Line of Defense': Two University Laboratories in the Postwar American State," *Isis* 85 (1994): 427–55.

[6] Ellis Johnson, "Operations Research in the World Crisis in Science and Technology," in Flagle, Huggins, and Roy (n. 1 above), pp. 36–37.

engineers) on their design teams during and immediately following the war. Three significant types of team evolved. One, derived in part from the development of antiaircraft gun directors, was the systems engineering team. Another was an outgrowth of the operational analysis of air and naval forces: the operations research (OR) team. The third was the large "project" team deployed to invent the atomic bomb.

The cold war further stimulated team approaches. Simon Ramo,[7] one of the best-known practitioners of systems engineering, put it this way in his introduction to the First Systems Symposium at the Case Institute of Technology in Cleveland in 1960:

> [W]e are in a new and different kind of a race today—a race that symposiums such as this can make a significant contribution toward winning. We are all familiar with the current contest between the West and Russia, but this is not the race to which I refer, although the contest with Russia has an important bearing on it.
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> The race that I want to discuss here has to do with systems engineering itself. There are two contestants in this race: Systems engineering *versus* the rapidly increasing complexity of our growing technological civilization. The outcome of the race will be determined, in effect, by whether systems engineering as a discipline is able to grow and develop quickly enough to successfully meet the problems of the future.[8]

Operations researchers saw their tasks similarly. As Ellis Johnson wrote: "Operations research involves the study of possible interactions among men and machines within these systems [industry, business, military, government] and is concerned with decision-making, organization (both formal and informal), and internal communications, as well as with 'operating' elements which effect the physical translation of inputs into outputs of the organization."[9]

Neither Johnson nor Ramo believed that system complexity was completely reducible to a technical problem. Because teams of scientists, engineers, managers, and organizations were involved, com-

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[7] Ramo was one of the cofounders of Ramo-Wooldridge, later to become the aerospace giant Thomson-Ramo-Wooldridge (TRW).

[8] Simon Ramo, foreword to *Systems: Research and Design, Proceedings of the First Systems Symposium at Case Institute of Technology,* ed. Donald P. Eckman (New York, 1961), pp. v–vii; italics in original.

[9] Ellis A. Johnson, "The Executive, the Organization, and Operations Research," in *Operations Research for Management,* ed. Joseph F. McCloskey and Florence N. Trefethen (Baltimore, 1954), pp. xvi–xvii.

plexity was also a management problem. Systems engineers and operations researchers believed that management knowledge was not keeping pace with technological change. Their perception of the problem is illustrated in table 1, used by Johnson in a 1960 paper. Because, he believed, the Soviets were able to develop and deploy weapons in half the time it took the United States, management was the most critical factor for American defense.[10] Johnson and Ramo believed that operations research and systems engineering needed to become essential adjuncts to management. Managers agreed but also had their own ideas, which they called project management.

Operations Research

Operations research began in Great Britain at the outbreak of World War II.[11] A small group of scientists in 1939 began to integrate radar into the older system of observers. They were instrumental in improving the efficiency of the air defense system using mathematical methods, a major factor in the Royal Air Force's (RAF) victory over the German Luftwaffe in the Battle of Britain. In August 1940, P. M. S. Blackett of the University of Manchester, a future Nobel laureate, was asked to improve antiaircraft gun ranging and targeting. He assembled a group of scientists, who promptly increased the efficiency of the antiaircraft system using mathematical methods. "Blackett's Circus" next successfully worked on airborne radar ship

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[10] Johnson, "Operations Research in the World Crisis," p. 42.

[11] Historical study of operations research is just beginning. The early history of OR is described in Edwards (n. 5 above); Florence N. Trefethen, "A History of Operations Research," in McCloskey and Trefethen; and in Air Ministry Publication 3368, *The Origins and Development of Operational Research in the Royal Air Force* (London, 1963). Three recent articles are Mike Fortun and S. S. Schweber, "Scientists and the Legacy of World War II: The Case of Operations Research (OR)," *Social Studies of Science* 23 (1993): 595–642; Robin E. Rider, "Operations Research and Game Theory: Early Connections," in *Toward a History of Game Theory*, ed. E. Roy Weintraub (Durham, 1992); and Gene H. Fisher and Warren E. Waler, "Operations Research and the RAND Corporation," in *Encyclopedia of Operations Research & Management Science*, ed. Saul I. Gass and Carl M. Harris (Boston, 1996). Several OR papers were presented at the Symposium on the Spread of the Systems Approach, Dibner Institute, Cambridge, Mass., May 3–5, 1996. They included Erik Rau, "New Times, New Uses: Philip Morse, the Cold War, and the Proliferation of Operations Research"; David Jardini, "Out of the Blue Yonder: The Transfer of Systems Thinking from the Pentagon to the Great Society, 1961–1965"; Arne Kaijser and Joar Tiberg, "The Establishment, Transformation and Diffusion of Operations Research in Sweden, 1945–1980"; and David Hounshell, "The Medium Is the Message, or How Context Matters: The RAND Corporation Builds an Economics of Innovation, 1946–1965." There is also research ongoing at the University of Manchester on the history of OR in Britain.

TABLE 1
DOUBLING PERIODS IN TECHNICAL SYSTEMS

	Years
Scientific knowledge	15
Management knowledge	50
Cost of U.S. R&D	6
Cost of U.S. fighter electronics	3
Man-hours of labor for U.S. fighter aircraft	3
Pounds of aircraft electronics	3
Complexity	10

SOURCE.—Adapted from Ellis Johnson, "Operations Research in the World Crisis in Science and Technology," in *Operations Research and Systems Engineering*, ed. Charles D. Flagle, William H. Huggins, and Robert H. Roy (Baltimore, 1960), p. 39.

and submarine detection. The British Admiralty promoted Blackett to director of naval operation research in December 1941. The army established its own group of operational researchers, thus placing scientists in each of the three major military establishments in Britain. British operational researchers successfully tackled problems such as convoy size and tactics, antisubmarine air tactics and submarine detection, and bomber formation size and tactics. The military gave them substantial latitude to communicate at all levels and to pick their own problems.[12]

The success of British OR groups caught the attention of Dr. James B. Conant, chairman of the American National Defense Research Committee, in 1940. He alerted American scientists, who began to perform similar functions. Ellis Johnson, then head of the countermeasures section at the Naval Ordnance Laboratory (NOL), used war-gaming techniques to analyze the use of mines for offensive operations in 1941. The navy formed its Operational Research Group in March 1942. The United States Army Air Forces modeled its Operations Analysis Division in Britain after the RAF's scientific groups, and the division served as the prototype for other Army Air Forces commands. During the war, American operations researchers contributed to antisubmarine warfare operations, defense against kamikaze attacks, and bombing tactics.[13]

After the war, the American military quickly established permanent operations research organizations. The Office of Naval Research created the Operations Evaluation Group, contracting with the Massachusetts Institute of Technology. The army, a late starter

[12] Trefethen, "A History of Operations Research," pp. 5–12.

[13] Ibid., pp. 12–20.

in operations research, contracted with Johns Hopkins University to create the Operations Research Office under Ellis Johnson. The air force created an operations analysis group in each command after the war and the RAND Corporation to provide long-term research.[14] Secretary of Defense James Forrestal established the Weapons Systems Evaluation Group in 1947 to serve the Joint Chiefs of Staff by providing "rigorous, unprejudiced and independent analysis and evaluations" of present and future weapons. Philip Morse, the foremost American operations researcher, became its first technical director.[15]

Operations research was an influential model for the nonprofit think tank, the RAND Corporation. During the war, operations researchers focused on the tactical operations of existing weapons. RAND researchers extended OR techniques to investigate the potential value of future systems, using many of the techniques developed by operations researchers and extending them with best-guess assumptions regarding the future. They called this future-oriented operations research systems analysis. They started by attempting two comprehensive analyses, an "offensive analysis" and a "defensive analysis" of nuclear war. The air force ridiculed the results, causing RAND to focus on much smaller problems for which systems analysis proved more successful.[16]

The RAND model of systems analysis was propagated into the air force through the efforts of the new Office of Scientific Liaison, headed by Colonel Bernard Schriever. Schriever's close personal friend and RAND promoter General Hap Arnold, commanding general of the Army Air Forces, appointed him to the position.[17] Schriever met influential members of the air force's new Scientific Advisory Board, including Theodore von Kármán and Ivan Getting. While in this position, Schriever created a new process for long-term R&D planning known as the "Development Planning Objectives." The new process required a complete systems analysis reviewing the potential threat, the mission objectives, and the resources needed before undertaking a new weapons project. It then integrated new technology forecasts into the systems analysis to create a long-range plan. This differed significantly from the old system whereby the air force operational commands simply forwarded their needs to the

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[14] Bruce L. R. Smith, *The RAND Corporation: A Case Study of a Nonprofit Advisory Corporation* (Cambridge, Mass., 1966), chaps. 1–2.

[15] Trefethen, pp. 20–24; see also Rau.

[16] Hounshell; Jardini.

[17] The air force became an independent organization in 1947, at the same time that Congress created the Department of Defense.

air staff. Schriever's innovation enshrined systems analysis as the first step in air force technology planning.[18]

Secretary of Defense Robert McNamara made systems analysis one of the Department of Defense's most important (and controversial) tools after 1961, when he brought RAND systems analysts to the Pentagon. Former RAND researchers, including RAND chief economist Charles Hitch, made economic criteria the common denominator for assessments and decisions on future weapons systems.[19]

Like operations research, systems analysis used teams of mathematicians, scientists, engineers, managers, economists, and military officers.[20] Systems analysts borrowed and modified mathematical methods when necessary and applied them to the new problems of complex man-machine systems. Typical methods included game theory, probability, and applications of physical laws such as classical mechanics and electromagnetic theory for radar. Extensive use of computing techniques led operations researchers in the 1950s to develop some new mathematical methods such as linear programming as computational aids.[21]

By the end of the 1950s, operations researchers and systems analysts were firmly established in the U.S. Department of Defense and were making headway in the commercial world. New institutions such as RAND and the Operations Research Office formed a secure base. From there their influence spread, mediated in part through another budding discipline, systems engineering.

Systems Engineering

Before and during World War II, the Army Air Forces procured aircraft from the young aircraft industry and added its own special

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[18] John Lonnquest, "The Face of Atlas: General Bernard Schriever and the Development of the Atlas Intercontinental Ballistic Missile, 1953–1960" (Ph.D. diss., Duke University, 1996), pp. 57–64. Lonnquest's thesis is by far the best analysis of Schriever and the Atlas ICBM program.

[19] See Roland N. McKean, *Efficiency in Government through Systems Analysis* (New York, 1958); Charles J. Hitch, *The Economics of Defense in the Nuclear Age* (Cambridge, Mass., 1960) and *Decision Making for Defense* (Berkeley and Los Angeles, 1967); David Novick, *Program Budgeting* (Cambridge, Mass., 1965).

[20] Fortun and Schweber (n. 11 above), p. 607.

[21] For the practices of operations researchers and systems analysts, see the early OR texts and symposia, particularly Philip M. Morse and George F. Kimball, *Methods of Operations Research* (New York, 1951); C. West Churchman, Russell L. Ackoff, and E. Leonard Arnoff, *Introduction to Operations Research* (New York, 1957); Flagle, Huggins, and Roy (n. 1 above); and Eckman (n. 8 above). They primarily applied existing mathematical techniques and simple theories from classical mechanics and more complex ones from electromagnetic theory.

components and modifications, particularly for the engines and armament.[22] After making these modifications, Air Materiel Command (AMC) shipped the aircraft to the operational commands, which, along with AMC, handled operations and logistics.

As the air force's aircraft and armament became more complex, they also became more tightly coupled to each other. Changes to the aircraft affected the armament, and vice versa. By the late 1940s, Bernard Schriever, among others, contended that the entire "weapon system" had to be designed from the start, including the airframe, electronics, armament, and logistics.[23] One early example of recognition of the "system" problem was in the development of radar-based antiaircraft gun directors.

In the process of developing a mobile ground radar unit, personnel from MIT's Radiation Laboratory created a special committee to act as liaison between the radar and fire control groups. Ivan Getting, an engineer on the liaison committee, soon realized that the radar and fire control components behaved differently when working together than they did individually. The difference was noise. To account for the differences, he noted that "specifications on each unit should be written with full consideration of the features and capabilities of the other."[24]

As the war progressed, Getting sought the authority to ensure that close coordination existed between the radar and gun director groups on new projects. He made himself the liaison between the "Rad Lab" and the Navy's Bureau of Ordnance for the Navy's Mark 56 project. He assigned the Rad Lab the role of "system integrator" for the project, ensuring that it had access to all technical information and meetings, authority to test models and prototypes, and the capability to critique the design during all stages of development.[25]

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[22] There are few references to systems engineering in the historical literature. For a description of the development of systems engineering in the antiaircraft gun director program, see David A. Mindell, "Automation's Finest Hour: Radar and System Integration in World War II" (paper presented to the Symposium on the Spread of the Systems Approach, Dibner Institute, Cambridge, Mass., May 3–5, 1996). Forms of systems engineering also developed in the office machine industry and at American Telephone and Telegraph to maintain the telephone network.

[23] See U.S. Department of the Air Force, Deputy Chief of Staff for Development, Headquarters USAF, *Combat Ready Aircraft: How Better Management Can Improve the Combat Readiness of the Air Force,* special report based on an air force study completed April 1951, Washington, D.C.

[24] Mindell, pp. 8–9.

[25] "Statement of Relationships between the Bureau of Ordnance, U.S. Navy and the National Defense Research Committee, OSRD, on the Development and Production of the Gunfire Control System Mark 56," reprinted in *All in a Lifetime: Science in the Defense of Democracy,* by Ivan Getting (New York, 1989).

After the war, system integration spread to become a new standard for government-industry interaction. Along with the conception that the integrated system was a whole greater than the sum of its parts, it formed a key element in what was to become the new discipline of systems engineering. Getting was a member of the air force's Scientific Advisory Board, and eventually he became the technical director for Air Defense Command. There his systems ideas and civilian status influenced his deputy, the former head of the Office of Scientific Liaison, Colonel Bernard Schriever.[26]

Schriever propagated systems ideas after his promotion to head the air force's intercontinental ballistic missile (ICBM) programs in 1953. After the successful hydrogen bomb test in 1952 convinced physicists that nuclear warheads soon would be small enough and have enough explosive power to be placed on an ICBM, the Eisenhower administration placed the development of ICBMs at the top of the nation's military priorities and gave the task to the air force. The Department of Defense formed a civilian scientific panel to advise the air force on how to proceed and to generate political support.[27]

John von Neumann chaired the panel, which the newly formed Ramo-Wooldridge Corporation administered.[28] The panel recommended that ICBMs be developed "to the maximum extent that technology would allow." In addition, they advised the creation of a multidisciplinary team to lead the effort: "The nature of the task for this new agency requires that over-all technical direction be in the hands of an unusually competent group of scientists and engineers capable of making systems analyses, supervising the research phases, and completely controlling the experimental and hardware phases of the program—the present ones as well as the subsequent ones that will have to be initiated."[29]

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[26] Jacob Neufeld, *Ballistic Missiles in the United States Air Force 1945–1960* (Washington, D.C., 1990), pp. 226–28. See also Lonnquest (n. 18 above), chaps. 2–4. Schriever and Trevor Gardner, special assistant for R&D to the secretary of the air force, were behind the machinations to make ICBMs the top priority.

[27] Donald McKenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance* (Cambridge, Mass., 1990; paperback reprint 1993), pp. 105–13; Edmund Beard, *Developing the ICBM: A Study in Bureaucratic Politics* (New York, 1976), pp. 134–57. McKenzie summarizes three reasons behind the decision to build ICBMs: the hydrogen bomb, the Eisenhower administration's concerns with cost-effectiveness, and the Soviet ballistic missile program. Beard emphasizes the bureaucratic politics within the armed services and their own internal push for the development of ICBMs. The decision to press ahead at top speed with ballistic missile development provided a driving force for technical and managerial innovation.

[28] Neufeld, pp. 98–99; McKenzie, pp. 109–10.

[29] "Recommendations of the Teapot Committee," Feb. 1, 1954, reprinted in Neufeld, pp. 260–61. This is one connection between the Manhattan Project and the

The committee's recommendation led to the establishment of the Western Development Division (WDD), commanded by Bernard Schriever.[30] Following the examples of the Manhattan Project and the Radiation Laboratory, the committee also recommended that a civilian organization coordinate system integration. After its own study in August 1954, the WDD found that the lack of scientific competence among the contractors made it necessary for Ramo-Wooldridge to perform all "systems engineering" and technical direction. The air force managed the program, with Ramo-Wooldridge operating as its technical arm. This arrangement mimicked Getting's position as a civilian technical director working for Air Defense Command. Despite objections from the aircraft industry, this organization became the air force's partner in the development of large "weapons systems."[31]

Schriever explained: "Complex requirements of the ICBM and the predominant role of systems engineering in insuring that the requirements were met, demanded an across-the-board competence in the physical sciences not to be found in existing organizations. Scientists rated the aircraft industry relatively weak in this phase of engineering, which was closely tied to recent advances in physics. The aircraft industry, moreover, was heavily committed on major projects, as shown by existing backlogs. Its ability to hire the necessary scientific and engineering talent at existing pay-scales was doubted, and with the profit motive dominant, scientists would not be particularly attracted to the low-level positions accorded to such personnel in industry."[32]

This is one of the clearest statements of Schriever's belief in the scientific ethos and a clear indication of its postwar influence with the military. Surprisingly, Schriever believed that systems engineering was "closely tied" to advances in physics and that only generically trained physical scientists had the "across-the-board competence" required for systems engineering. The former scientific liaison believed that scientists would be deterred by the profit motive and the "low-level" positions they would have in the aircraft industry. Consequently, Schriever opted for an arrangement to let scien-

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ICBM efforts. On the organization of the Manhattan Project, see Lillian Hoddeson, Paul Henriksen, Roger A. Meade, and Catherine Westfall, *Critical Assembly: A Technical History of Los Alamos during the Oppenheimer Years, 1943–1945* (Cambridge, Mass., 1993).

[30] Neufeld, pp. 102–6.

[31] Neufeld, p. 114. See Lonnquest (n. 18 above), chaps. 6 and 7, for details of the WDD–Ramo-Wooldridge arrangement.

[32] House Committee on Government Operations, *Organization and Management of Missile Programs*, 86th Cong., 1st sess., 1959, H. Rept. No. 1121, p. 75.

tists control the development of ICBMs without becoming low-paid civil servants or being contaminated by capitalist motives.

Ramo-Wooldridge's systems engineers indeed had great authority over the air force's ballistic missile projects. According to Simon Ramo, the systems engineering organization decided "how big the missile should be, what warhead it carries, what accuracy it can be expected to have, how to get that optimum accuracy by the proper interaction between the rocket engines that produce the thrust and the gyros that hold direction." The systems engineer's primary task was "to draw up a set of specifications making the performance of each subsystem and its interactions with every other mutually compatible." After that, the systems engineers controlled "the test and operational environment in which the weapon is put to work." Ramo-Wooldridge's systems engineers performed "laboratory experimental work" when they needed more information, analyzed intelligence data on Soviet tests, and programmed the early missiles. To control the program, systems engineers held "Technical Directive" meetings with contractor specialists, the results of which often became written directives processed by the air force and "released as legal amendments to basic contracts."[33]

Like the operations researchers before them, systems engineers did not generally bring new mathematical or scientific methods to their tasks but rather employed standard engineering and mathematical techniques and added to them commonsense procedures for organizing the activities of the engineering groups. These included the use of matrices for the representation of data for trade studies, the creation of interface specifications, and the development of procedures for progressively "freezing" the design to reduce the "ripple" effect of profligate design changes. Systems engineers thus performed a critical function, standing between the specialized engineers from different disciplines and the military's new "project managers."[34]

Project Management

Before the Second World War, there was nothing in management theory called "project management."[35] In the United States, a solid

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[33] Ibid., pp. 84–85.

[34] For details on the practice of systems engineering, see early texts such as Arthur D. Hall, *A Methodology for Systems Engineering* (Princeton, 1962); Harry H. Goode and Robert E. Machol, *Systems Engineering: An Introduction to the Design of Large-Scale Systems* (New York, 1957); and Robert E. Machol, Wilson P. Tanner Jr., and Samuel N. Alexander, *System Engineering Handbook* (New York, 1965).

[35] The history of project management has yet to be written. Two recent publications dealing with changes in management of weapons in the 1950s are Glenn E. Bugos, "Manufacturing Certainty: Testing and Program Management for the F-4

tradition of management theory and practice in industry and academia stretched back to the genesis of large corporate enterprises, with their cadres of middle- and upper-level managers, and to Frederick W. Taylor's turn-of-the-century theories of scientific management. Business school academicians, industrial engineers, and managers in American industry communicated through their own journals and conferences, which were well established by the 1930s.[36]

Projects were significant for the construction industry, and hence it was not surprising that the government turned to the United States Army Corps of Engineers to manage its largest wartime project, the Manhattan Engineer District. Leslie Groves, appointed to head the effort, brought with him standard Corps of Engineers practices to build the many facilities required. But for the most significant R&D element of the project at Los Alamos Groves's preferences gave way to the team approaches of scientists and engineers, who worked closely together to develop and integrate the technologies of the atomic bomb. This multidisciplinary team approach used by Los Alamos scientists and engineers to develop a specific product was an important example of how to organize the development of complex new technologies.[37]

In the early postwar period, the Department of Defense's missile projects had a single military officer monitoring each project and a small group of engineers in industry performing the work.[38] The Procurement Act of 1947 made government contracting for R&D

Phantom II," *Social Studies of Science* 23 (1993): 265–300, and *Engineering the F-4 Phantom II: Parts into Systems* (Annapolis, 1996). Most other historical work on management has focused on earlier time periods, following the work of Alfred Chandler, *The Visible Hand: The Managerial Revolution in American Business* (Cambridge, Mass., 1977), or tracing the development of Taylorism and scientific management. Good introductions to this literature are Daniel Nelson, ed., *A Mental Revolution: Scientific Management since Taylor* (Columbus, 1992) and Stephen P. Waring, *Taylorism Transformed* (Chapel Hill, 1991).

[36] For an introduction to the history of managerial thought, see Daniel A. Wren, *The Evolution of Management Thought*, 2d ed. (New York, 1979) and Claude S. George, *History of Management Thought* (Englewood Cliffs, N.J., 1972).

[37] See Vincent C. Jones, *Manhattan: The Army and the Atomic Bomb* (Washington, D.C., 1985); Richard Rhodes, *The Making of the Atomic Bomb* (New York, 1986); and Lt. Gen. Leslie R. Groves, "The A-Bomb Program," in *Science, Technology, and Management*, ed. Fremont Kast and James Rosenzweig (New York, 1963), and in Hoddeson et al. (n. 29 above).

[38] For early examples of this structure, see Clayton R. Koppes, *JPL and the American Space Program: A History of the Jet Propulsion Laboratory* (New Haven, 1982); Constance McLaughlin Green, *Vanguard—A History* (Washington, D.C., 1970); Neufeld (n. 26 above); and Charles S. Ames, "The Atlas Program at General Dynamics/Astronautics," in Kast and Rosenzweig, pp. 199–201.

much easier, by allowing noncompetitive, negotiated contracts as a standard practice for the Department of Defense.[39] Nonetheless, missile programs remained a problem, because their novel designs and operational characteristics did not fit the military's normal R&D or logistics structures or procedures. Because the air force's procurement process separated aircraft design from armament and logistics, a primary recommendation of air force reformers was to ensure that new aircraft and weapon designs considered all of these elements.

Based on recommendations formulated by Schriever and others, the air force modified its procurement process in 1953, requiring contractors to consider the entire "weapons system." They also required that the air force's Air Research and Development Command and Air Material Command work together in "special project offices" for each project, consisting of officers from each command. One of the officers in the project office would be named the "project manager" and would have full responsibility for the project. Schriever's Western Development Division followed the new procedure, establishing special project offices for the Atlas and Titan ICBMs.[40]

With the WDD and other military agencies funding a number of large R&D projects, military contractors faced the problem of concurrently developing several new systems. They found that under these circumstances, the old line-and-staff organization typical in American companies no longer sufficed.

Bill Bergen, an engineer at the Martin Company who had worked on the navy's Viking rocket in the late 1940s and early 1950s, created one of the first recognizable project management organizations in industry.[41] His organizational concept, which he called "system management," was a solution to the problem of concurrently managing several large projects. As he described it in a 1954 *Aviation Age* article: "Within the company we have created a number of miniature companies, each concerned with but a single project. The project manager exercises overall product control—in terms of an orga-

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[39] See Allen Kaufman, "In the Procurement Officer We Trust: Constitutional Norms, Air Force Procurement and Industrial Organization, 1938–1948," working paper, MIT Defense and Arms Control Studies Program, Cambridge, Mass., January 1996.

[40] See Robert J. Reed, "New AF Policy Means More Competition—More Selling," *Aviation Age,* August 1953, pp. 20–23; Edward G. Uhl, "Applying the Systems Method to Air Weapons Development," *Aviation Age,* February 1954, pp. 20–23; Neufeld, pp. 109–10.

[41] William B. Harwood, *Raise Heaven and Earth: The Story of Martin Marietta People and Their Pioneering Achievements* (New York, 1993), p. 253.

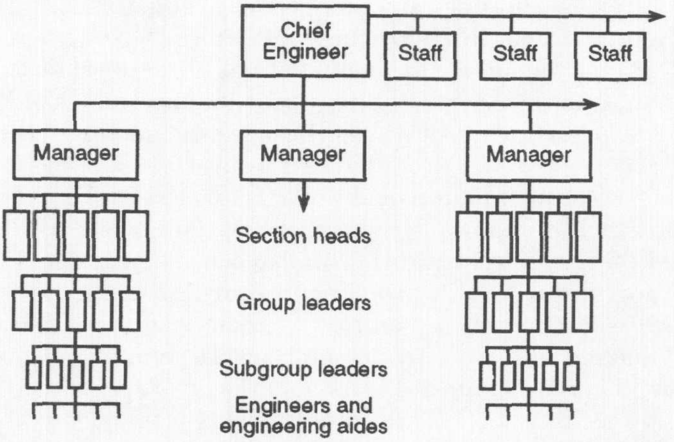

FIG. 1.—Traditional line organization. (Adapted from H. F. Lanier, "Organizing for Large Engineering Projects," *Machine Design*, December 27, 1956, fig. 2.)

nization of all skills."[42] The Martin Company quickly implemented Bergen's concept and expanded it to "cover all functions from design through manufacturing and distribution."[43]

Project management strengthened the communication links necessary to build new large systems. In the old line-and-staff functional organization, communication lines across functional departments became too long for effective coordination. As stated in 1956 by H. F. Lanier, a project engineer for Goodyear Aircraft's aerophysics department: "The problem can perhaps be best illustrated by considering the difficulties of trying to fit a number of creative people into the precise and orderly line organization shown in Fig. 2. [Adapted here as fig. 1.] Under this plan, all work is thoroughly organized and all assignments rigidly controlled. Each individual has a definite area to cover, definite data to work with, and a schedule to meet. He also has a boss who tells him what to do and subordinates whom he tells what to do. This organization once set up is soon limited to the creative output of a few men who lead. Any innovation is difficult to introduce because it requires detailed instruction at all levels."[44]

Lanier concluded that "[t]he major step is somehow to break

[42] William B. Bergen, "New Management Approach at Martin," *Aviation Age*, June 1954, pp. 39–47.

[43] Harwood, p. 278.

[44] H. F. Lanier, "Organizing for Large Engineering Projects," *Machine Design*, December 27, 1956, pp. 54–60. All quotations from Lanier are taken from this article.

down the long lines of communication.'' Aircraft contractors previously used ad hoc means. These were insufficient over the long term and for large projects: ''The usual solution was to allow a great deal of 'co-ordination' and 'liaison' to be handled informally. Effectively, supervisors unleashed their men and gave the program general direction but let detailed instructions be formulated after the fact. The loose method has been reasonably successful. The next obvious step is to attempt to systematize the process. Some new engineering disciplines seem called for and it would be well to train the required people by direct methods to facilitate rapid formation of new teams.''

Often the first attempt at systematization was to form committees of the functional supervisors. This, however, did not work once systems development became large or too frequent. ''Usually the committee members are also line supervisors and hence can meet only for a fraction of the time required for efficient system development. In other words, actual development by a committee is employed most effectively on an occasional relatively huge problem. When large systems problems are the prime business, then a permanent fix must be made.''

For this purpose, Lanier stated that ''the solution seems to be a committee of project or systems engineers—individuals trained to be jacks of all trades, and who are relieved of line responsibility for administering operating sections.'' He believed that the ''project engineer is a feature as old as engineering. Groups of project engineers working in team effort under a project management is a little new.'' The size and frequency of large systems problems made a permanent organizational change necessary, one that involved groups of systems engineers and other project engineers working together in a committee form.

Lanier called this the ''project-line combination organization.'' This, he stated, ''existed in various forms for some time, usually as a special purpose, temporary thing. Now enough work of the large systems nature is under way to warrant the formation of permanent establishments geared to development of large systems.'' The new organizational form was ''two-dimensional.'' He noted that ''several companies are experimenting with the arrangement illustrated in Fig. 3. [Adapted here as fig. 2.] Here specialized creative engineering groups are given a two-dimensional supervision.'' This two-dimensional reporting structure came to be known as ''matrix management.'' In the new organization, the line manager and the task manager both had roles in managing the working group.

General Dynamics Convair division was a good example of the

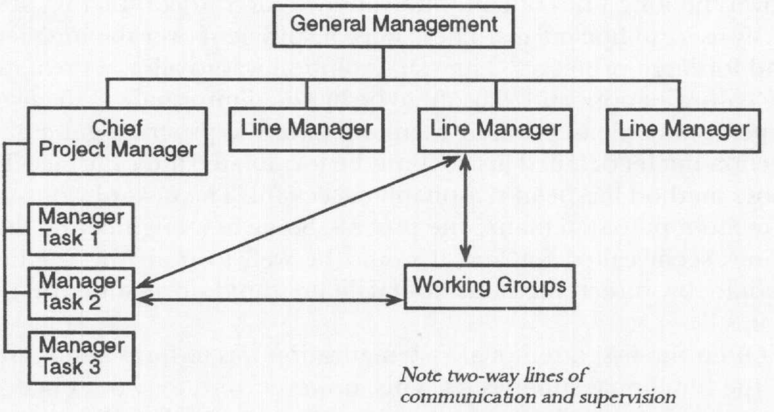

FIG. 2.—"Project line" or "matrix" organization. (Adapted from H. F. Lanier, "Organizing for Large Engineering Projects," *Machine Design*, December 27, 1956, fig. 3.)

evolution of a system to project management and then to matrix management. In 1946, the air force gave Convair a contract to study a rocket-powered missile (Atlas) that could deliver a warhead at a distance of 5,000 miles. Design work began in 1947, but because of funding problems the program remained in the research and testing stage for several years. During this time, the Atlas program was "directed by a project engineer who was assigned a small team of designers and technical specialists plus an experimental shop for fabrication of the hardware." By 1954, one year after the acceleration of Atlas, the project had a force of 300 personnel, mostly engineers. As the project continued to grow, Convair created the Astronautics division in 1955 to carry out the Atlas program. By 1958 the work force had increased to 9,000; by 1962 it was up to 32,500. For most of the 1950s, General Dynamics ran Atlas as a single-project organization. However, with the development of different versions of the Atlas and the development of new projects such as the Centaur upper stage and the Azusa tracking system, "priority problems were created in functional line departments, with resultant conflicts over authority and the jeopardizing of performance, scheduling, and cost."[45]

Astronautics responded to this problem by "utilizing a program control plan called the 'matrix' system which provided a director for each program undertaken by the company." Program directors

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[45] Ames (n. 38 above), pp. 199–203.

and department managers resolved priority issues. By 1963, Astronautics organized every new major program with the project system using a matrix structure. Eventually the production of the Atlas weapons system included "22 industries, 17 major contractors, and 3,500 subcontractors and suppliers." The matrix organization sustained a number of simultaneous development projects.[46]

Similar stories could be told for the navy's Polaris program, Radio Corporation of America's military programs, the System Development Corporation, and others.[47] In each of these cases, as projects grew in size and number the organization shifted first to project and then to matrix structures. In the aerospace industry, these new structures had several names, including project, program, matrix, and systems management. In academia, the new disciplines of project management, operations research, and systems engineering also acquired distinctive names, sometimes including descriptions such as "nebulous" and "nothing but . . . block diagrams."

Academic Recognition and Disputation

Operations researchers, systems engineers, and project managers in the 1950s spent much effort defining and defending their activities, distinguishing themselves from older disciplines and from one another. Universities were primary battlegrounds for establishing academic homes for their emerging disciplines.

By 1961, project management had evolved to the point where academicians such as Keith Davis, the chairman of Arizona State University's Department of Management, discerned new organizational structures, with distinct types of management and functions. Davis completed a survey of manufacturing firms in the West, finding that some of the first companies to use project management were aerospace companies such as Douglas, Lockheed, Martin, and Chance-Vought.[48]

Davis believed that project management administratively tied together projects in the same way that its "end product must be *physi-*

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[46] Ibid., pp. 201–5.

[47] See Harvey M. Sapolsky, *The Polaris System Development, Bureaucratic and Programmatic Success in Government* (Cambridge, Mass., 1972); J. H. Sidebottom, "Management of the Ballistic Missile Early Warning System," in Kast and Rosenzweig (n. 37 above); Claude Baum, *The System Builders: The Story of SDC* (Santa Monica, Calif., 1981); and booklets titled *System Development Corporation,* dated 1957, 1958, and 1959, Charles Babbage Institute 90, Burroughs Collection, System Development Corporation Series, Box 1, "SDC Descriptive Booklets" folder.

[48] Keith Davis, "The Role of Project Management in Scientific Manufacturing," *IRE Transactions on Engineering Management* 9 (September 1962): 109–13.

cally put together before it is a workable whole." "It follows that *the primary reason for project management organization is to achieve some measure of managerial unity*, in the same way that physical unity is achieved with the project." In his survey and investigations, Davis found four distinguishable types of project organization, ranging from having an ill-defined project coordinator to having a full-scale project manager.

Although the term "project management" began to appear in journal literature in the 1950s, project management textbooks did not surface until the mid-1960s.[49] When these did appear, all highlighted the new organizational structures of project and matrix management and the new tools such as systems analysis and the Program Evaluation and Review Technique (PERT). On the whole, business schools incorporated project management smoothly.[50]

The same cannot be said for operations research and systems engineering. Even practitioners found it difficult to agree on methods or definitions. Regarding definitions of operations research, P. Stewart Macaulay of Johns Hopkins wrote that "many have been brought forward and no two seem to be in agreement." One school of thought maintained, he went on, that "practically all problems can be solved by appropriate application of the principles of physics, mathematics, and statistics." Others believed that many problems required "the intervention of such specialists as economists, political scientists, historians, and philosophers."[51]

Detractors could not distinguish between systems engineering and operations research but agreed they both lacked substance. Some alleged that operations research was "nothing but industrial engineering." Systems engineering they derided in similar fashion as "nothing but people who can draw block diagrams" or merely the "engineering process" itself—that is, nothing new at all.[52]

At Johns Hopkins, the home of systems engineers at the Applied Physics Laboratory and operations researchers at the Operations Re-

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[49] Early American texts include Richard A. Johnson, Fremont E. Kast, and James E. Rosenzweig, *The Theory and Management of Systems* (New York, 1963); Rocco Martino, *Project Management* (Wayne, Pa., 1968); David I. Cleland and William R. King, *Systems Analysis and Project Management* (New York, 1968). The first British text appears to be W. J. Taylor and T. F. Watling, *Successful Project Management* (London, 1970). French and German texts also appeared in the late 1960s and early 1970s.

[50] I infer this from the lack of controversy in the management literature.

[51] Macaulay (n. 1 above), pp. 6–7. The first group cited probably alludes to Philip Morse's school at MIT. The second group was the Operations Research Office at Johns Hopkins, headed by Ellis Johnson.

[52] Robert H. Roy, "The Development and Future of Operations Research and Systems Engineering," in Flagle, Huggins, and Roy (n. 1 above), p. 9.

search Office, defining the boundary between the two was important. As Johns Hopkins industrial engineer Robert Roy wrote in 1960: "The operations research team is concerned with operations *per se,* and is more likely to be concerned with operations in being than with operations in prospect. Systems engineers are concerned with operations, too, but are more likely to refer to them as man and machine systems and much more likely to emphasize the machines than the procedures by which the machines are used. Furthermore, systems engineers are more likely to be engaged in the design of systems yet to be, rather than the operation of systems in being."[53]

In sum, systems engineers and operations researchers used similar methods and tasks but for different purposes.[54] Against external detractors, Roy claimed: "The assertion that there is something different and better in the multi-discipline team and the whole system approach is equally important but impossible to prove. . . . Is the assemblage of . . . experts into a *team* for a purpose of making a superior *missile* better than independent work aimed at making superior *component elements?* It is, and those who have worked in such a way will so testify. There is something more in the team idea than merely the 'engineering process.'"[55]

Both operations researchers and systems engineers agreed that they performed services useful to management. In 1954, Ellis Johnson claimed that "with each passing day, it [operations research] is increasing its capability of helping management to solve complex action problems and make major decisions."[56] Arthur Hall, a systems engineer from Bell Laboratories, wrote that systems engineering provided "management with as much information as possible needed to guide and control the over-all development program."[57] Both promoted themselves as the technical arm of the managerial technocrat.

Some of the reasons for their difficulties in defining themselves become apparent when the technical content of these disciplines is compared. Table 2 lists the subjects covered by the early textbooks in operations research and systems engineering. It shows some differences between the subject material of operations research and

[53] Ibid., p. 22.

[54] Note that Roy had not noticed the transformation of OR into systems analysis, with its role in the analysis of future systems.

[55] Ibid., p. 24.

[56] Ellis Johnson, "The Executive, the Organization, and Operations Research" (n. 9 above), p. xi.

[57] Hall (n. 34 above), p. 12.

TABLE 2

SUBJECT MATTER IN OPERATIONS RESEARCH AND SYSTEMS ENGINEERING TEXTBOOKS

	OPERATIONS RESEARCH							SE & OR	SYSTEMS ENGINEERING					
	1951 Morse	1954 McCloskey	1957 Churchman	1963 Goddard	1964 Stoller	1965 Enrick	1967 Ackoff	1960 Flagle	1957 Goode	1962 Hall	1965 Machol	1966 Porter	1967 Shinners	1967 Wymore
Probability and statistics	x	x	x	x	x	x	x	x	x	x	x		x	
Linear programmig		x	x	x	x	x	x	x	x		x		x	
Queuing theory		x	x	x	x	x	x	x	x		x			
Game theory	x	x	x		x		x	x	x		x			
Network analysis						x	x	x		x				
Management		x	x			x	x			x	x		x	
R&D process	x		x				x	x	x	x	x			
Testing			x				x		x	x	x			
Matrix methods				x	x	x					x	x	x	
Information theory		x						x	x	x	x			
Computers		x						x	x		x			
Formal logic		x						x			x	x		x
Reliability and quality assurance								x			x		x	
Control theory								x	x		x	x	x	
Simulation								x	x		x		x	
Human factors								x	x		x		x	
Component hardware									x		x			

systems engineering but also substantial overlap and no universal consensus. The core subjects of operations research in the 1950s and 1960s were probability and statistics, linear programming, queuing theory, and game theory. Other subjects, such as network analysis and matrix methods, were also common, along with "nonscientific" subjects such as management, testing, and the "research and development process," thought to help operations researchers sell their services to management. Systems engineering texts displayed somewhat less consensus. There the most significant topics included probability and statistics and control theory. A number of other technical topics were common, including linear programming, matrix methods, information theory, simulation techniques, and human factors. Management and "R&D process" topics were also popular, along with testing techniques.

Because operations research and systems engineering borrowed their methods from other disciplines, and were commonsense—that is, procedural—disciplines themselves, their claims to academic legitimacy were tenuous. Other budding disciplines such as computer science and applied mathematics also claimed those few areas within operations research and systems engineering that represented something genuinely new, such as linear programming, reliability, human factors, and simulation.

Moreover, neither new discipline could point to a clear theoretical or empirical base. Some traditional disciplines, such as biology, physics, and mechanical engineering, demarcated boundaries by their claim to theory of or application to specific natural or physical phenomena. Others, such as mathematics and control theory, identified with unique theories and mathematical methods, even if broadly applied. Operations research and systems engineering were hampered on both counts. Both could be applied to any phenomena, although in practice they worked with large organizations and technologies. They were inherently cross-disciplinary. Nor could they lay claim to new mathematically oriented techniques, since these were largely borrowed from other disciplines. Both developed "procedural knowledge" that smacked of craft or art as opposed to mathematical "science." One frustrated operations researcher described the problem: "What kind of person is the operations research professional? Many of the descriptions convey a feeling that a 'pro' in this business is a jack of all trades and a master of none. If that were so, this would not, of course, be a profession."[58]

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[58] George S. Pettee, "Operations Research as a Profession," in McCloskey and Trefethen (n. 9 above), pp. 45–46. For other examples of the difficulty in defining operations research and systems engineering in academically acceptable ways, see

Despite the lack of new theoretical content, a few schools with practical interests began to teach operations research and systems engineering. In 1948, MIT in cooperation with the navy established a course in the nonmilitary applications of operations research. The University College, London, gave the first OR course in Britain the next year. Case Institute of Technology established a master of science degree in operations research by 1954. Columbia University set up its first course in 1952, as did Johns Hopkins University.[59] MIT developed a weapons systems engineering course in 1952, associated with Charles Stark Draper's Instrumentation Laboratory, from which 118 air force officers had graduated by 1958.[60] G. W. Gilman of Bell Laboratories began informally teaching systems engineering at MIT in 1950, and in December 1954 he started a systems engineering course as part of Bell's Communications Development Training Program.[61] Recognizing the similarities between operations research and systems engineering, the University of California at Los Angeles, the University of Pennsylvania, the University of Michigan, and Johns Hopkins University all offered graduate courses titled "Operations Research and Systems Engineering" by 1962.[62]

Operations researchers began to publish their own journals and texts. In 1952, American operations researchers established the Operations Research Society of America, which published the journal *Operations Research,* and British "operational" researchers started *Operational Research Quarterly*.[63] Philip Morse and George Kimball published the first book on OR, *Methods of Operations Research,* in 1951.[64] Others soon followed, introducing managers and undergraduates to operations research.[65]

Systems engineering got off to a slower start. Harry Goode and Robert Machol published the first textbook of systems engineering in 1957.[66] With their own operations research and systems engineering groups, academics at Johns Hopkins emphasized the similarities between the two new disciplines in their 1960 text, *Operations*

J. W. Pocock, "Management Consulting and Operations Research," in McCloskey and Trefethen, p. 92; Macaulay (n. 1 above), pp. 6–7.

[59] Trefethen, "History of Operations Research" (n. 11 above), pp. 33–34.

[60] Stuart Leslie, *The Cold War and American Science* (New York, 1993), pp. 94–95.

[61] Hall, pp. vii–viii.

[62] Ibid., p. 20.

[63] Waring (n. 35 above), p. 28; Trefethen, "History of Operations Research," pp. 34–35.

[64] Morse and Kimball (n. 21 above).

[65] See Churchman, Ackoff, and Arnoff (n. 21 above); and McCloskey and Trefethen (n. 9 above).

[66] Goode and Machol (n. 34 above).

Research and Systems Engineering.[67] Other texts followed, emphasizing systems engineering in aerospace and telecommunications.[68] They included most of the topics discussed in operations research texts and in Goode and Machol's earlier book and added descriptions of typical guided-missile and aerospace subsystems.

Despite the proliferation of classes, journals, and texts, neither operations research nor systems engineering contributed much new in terms of mathematical or scientific theories. This made them suspect in the eyes of many academic scientists, mathematicians, and engineers. Their novelty must be sought not in their borrowed academic content but rather in the realm of practice.

Because business schools focused their attention at least in part on procedural knowledge, project management was an acceptable, easily accommodated change to business school teaching and research. By contrast, procedural knowledge was (and is) consistently underrepresented and undervalued in the mathematically oriented curricula of science and engineering departments. There, the procedurally oriented disciplines of operations research and systems engineering struggled for existence among more scientifically oriented departments. To academics who valued mathematical theory, these disciplines were empty of content. To the military and industrial organizations that needed and valued practical ability, they were essential.

Integrating Systems Approaches

The issues and ambiguities surrounding operations research, systems engineering, and project management in academia would continue throughout the 1960s. In the meantime, however, the Department of Defense and the National Aeronautics and Space Administration (NASA) got on with the business of developing new technological systems. They developed standard procedures incorporating these techniques into their programs. Using ideas traceable to the three emerging "systems approaches," both the DOD and NASA used and further developed them into coherent bureaucratic processes.

General Bernard Schriever led the way. In the mid-1950s he managed to keep his organization separate from the rest of the air force, helping to create special procedures that circumvented the air force's regular procedures, funding, and reporting channels.[69] Un-

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[67] Flagle, Huggins, and Roy (n. 1 above).

[68] Machol, Tanner, and Alexander (n. 34 above). See also Hall (n. 34 above).

[69] Neufeld (n. 26 above), pp. 119–47; Lonnquest (n. 18 above), chap. 9.

der these extraordinary circumstances, Schriever and the WDD developed new management procedures that featured systems analysis, systems engineering, and project management. Eventually, Schriever encapsulated the new procedures in the "375-series" of Air Force Regulations (AFRs) for "Systems Management" first published in January 1961.[70]

Systems management spread from Schriever's organization. When the air force consolidated its research and development efforts under a single organization, the Air Force Systems Command, systems management was its primary organizational technique, and thus the air force's standard for large-scale development programs. In the mid-1960s, the McNamara regime made systems management the standard for the entire Defense Department.[71] NASA was quick to adopt and modify systems management. From NASA, the European space programs picked up the techniques. Others also adopted systems management, spreading its ideas and techniques far beyond their military home.

Conclusion

Driven by cold war pressures to develop new military systems rapidly, operations research, systems engineering, and project management resulted from a growing recognition by scientists, engineers, and managers that technological systems had grown too complex for traditional methods of management and development. Existing organizations could not easily assimilate and integrate technologies such as nuclear weapons, radar, and rocket propulsion. Teams of scientists, engineers, and managers worked with old organizations and created new ones, in which they developed new methods to speed technical development.[72]

Scientists used their mathematical prowess to analyze current or future operational systems. They did not build these systems, and

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[70] Department of the Air Force, *Air Force Regulation No. 375-1, Systems Management, Management of Systems Programs*, February 12, 1962. Also Air Force Regulations 375-2, 375-3, and 375-4.

[71] Department of Defense, Directive 3200.9, "Initiation of Engineering and Operational System Development," July 1, 1965.

[72] It is important to note that the military and its industrial allies were primarily concerned with driving technical progress as rapidly as possible. As in World War II, financial matters were a secondary consideration. In the 1960s and 1970s, the Department of Defense, NASA, and the European space organizations modified systems management by tying the technical processes to tighter managerial controls in order to curb costs. See Stephen B. Johnson, "Insuring the Future: The Development and Diffusion of Systems Management in the American and European Space Programs" (Ph.D. diss., University of Minnesota, 1997) chaps. 4–9.

consequently their perception of the problem was *analytical.* By contrast, engineers designed and developed systems to specifications determined by others. Engineers in industry saw systems engineering as a systematic design *process,* consistent with their daily involvement with large projects. Managers made decisions about whether to build systems and controlled their development and use. They organized new communication and control procedures around the technical system. Functional hierarchies gave way to more flexible "team" and "matrix" forms organized around the end product.

Bernard Schriever played a crucial role in the translation of scientific and engineering ideas into forms useful for the military. In his first postwar position as the air force's scientific liaison, he absorbed some of the ideas and values of influential scientists, engineers, and mathematicians, including Theodore von Kármán, John von Neumann, and Ivan Getting. He found RAND's modification of operations research, systems analysis, useful for air force planning and incorporated it into his new Development Planning Objectives. From Ivan Getting, Schriever learned of "systems integration" and the utility of using civilian scientists in engineering leadership roles. This was the basis for his decision to use the Ramo-Wooldridge Corporation for systems engineering and technical direction of the air force's strategic missile programs. Finally, Schriever absorbed the organizational principles of Los Alamos and pressed hard to create "special project offices" to organize air force R&D operations around the products to be built. By the early 1960s, Schriever's organization had combined these techniques into regulations for systems management, a coherent process for air force R&D.

While critics downplayed Schriever's role, portraying him as a "salesman" or a "nontechnical" officer, others believed him to be the critical man in the air force's missile programs.[73] Both the praise and the criticism of Schriever mirrored the praise and the criticism of systems management and its component elements. To supporters, systems management was an essential organizing principle. To critics, it was empty of content, fancy words for something people had always done. Schriever's accomplishment was to develop new procedures using the "system concept." Precisely because he was a master at developing and applying procedural knowledge, theoretically oriented critics believed he lacked depth.

The struggles of operations researchers, systems engineers, and

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[73] Lonnquest, pp. 285–89, describes Schriever's positive public image and the views of his subordinates. I found some of the negative views in informal discussions with some of those who knew him.

project managers for academic legitimacy reflected the same issues. These new disciplines developed as responses to the problem of large-scale, heterogeneous R&D. Each emphasized the procedural knowledge necessary to apply scientific, engineering, and managerial techniques to large systems. Business school academics accepted this kind of knowledge, if only because businesses demanded it. However, in the mathematically oriented disciplines of academic physical science and engineering, procedural knowledge was "unscientific," and indeed operations research and systems engineering were relatively barren of *original* mathematical content. Their specialty lay in the procedural knowledge of how, when, and why to apply mathematical or other methods to the problems of building large-scale systems. Because of the dominance of the mathematical model in science and engineering, operations research and systems engineering found it difficult to maintain themselves in academia. To this day, they exist only in schools with a practical bent. Procedural knowledge remains undervalued.

Why was (and is) procedural knowledge, so essential for large-scale R&D, undervalued in this manner by academics? After all, procedures are perhaps the most explicit kind of knowledge and communication, giving detailed, step-by-step guidance on how to perform a given task.

One example helps clarify the issues. Engineers developing the Apollo spacecraft put together detailed handbooks and drawings describing the functions of each element of the system. However, these alone were insufficient to describe how to operate the vehicles, particularly in time-constrained and emergency situations. NASA astronauts underwent years of training in spacecraft operating procedures. They learned that the equipment could be operated only in particular sequences and that they had to operate in a standard fashion in conjunction with mission controllers from Houston. Otherwise, the equipment would not function correctly or, even worse, could put their lives in danger. Theoretical knowledge was insufficient. Only hands-on, practical knowledge expressed through procedures and practiced so that it became routine would allow the astronauts to function quickly and effectively. In space flight, time constraints and the hazards of the environment left little room for error.[74]

Unlike in academia, where research scientists and engineers had

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[74] See Paul C. Kramer, Apollo Experience Report—Systems and Flight Procedures Development, NASA TN D-7436, September 1973, "Apollo Experience Reports" folder 007822, NASA History Office.

leisure to reason out old and new problems, managers and engineers in industry often had neither the time nor the resources. Every extra hour added significant cost. They had little choice but to work through significant new problems or designs as they encountered them. However, both managers and engineers quickly reduced the solutions to procedural practice to avoid making the same costly mistakes over and over again and to speed development the next time around. Procedures were the means to communicate these lessons across wide audiences. Theodore Porter has argued that quantitative practices standardize scientific communication and that historically they often have been driven by political pressures.[75] In similar fashion, aerospace procedures standardize managerial and engineering communication, and they too have been driven by political pressures to speed development and predict costs. When political or technical constraints make errors unacceptable, then standardized procedures become important. Academics did not usually face these pressures, but managers, engineers, and system operators did.

Despite the relative lack of academic legitimacy accorded operations research and systems engineering, the military and the aerospace industry found them useful. From this solid military-industrial base, they spread to many other countries and commercial industries. Along with project management, they became the most influential and practical applications of "the systems approach," the core of much of the American R&D system of the 1960s and 1970s.

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[75] Theodore M. Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton, 1995).