Post-Industrial Engineering: Computer Science and the Organization of White-Collar Work, 1945-1975

by

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Committee in charge:

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Abstract

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The development of computing after the Second World War involved a fundamental reassessment of information, communication, knowledge — and work. No merely technical project, it was prompted in part by the challenges of industrial automation and the shift toward white-collar work in mid-century America. This dissertation therefore seeks out the connections between technical research projects and organization-theory analyses of industrial management in the Cold War years. Rather than positing either a model of technological determinism or one of social construction, it gives a more nuanced description by treating the dynamics as one of constant social and technological co-evolution.

This dissertation charts the historical development of what it has meant to work with computers by examining the deep connections between technologists and mid-century organization theorists from the height of managerialism in the 1940s through the decline of the "liberal consensus" in the 1970s. Computing was enmeshed in ongoing debates concerning automation and the relationship between human labor and that of machines. The work that would become known as "artificial intelligence" grew out of studies of mental work in an attempt to automate the process of making routine decisions within large organizations. Likewise, the technical content of operating systems and programs reinforced ideas about what constituted meaningful labor, even as they created a new basis for assessing the value of mental work. The development of these technologies occurred in a direct relationship with ongoing conversations about American economic development in the 1950s and 1960s. By the mid-1960s, large computer systems were viewed through the prism of the Great Society, while smaller minicomputers were associated with a libertarian backlash. The direct experiences of working with different machines provided a foundation for rethinking the organization of the American office and the place of mental work within an "Information Age."

Contents

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Introduction

We have many histories of computers but we have no histories of the information age. A history of the information age should be one in which information technologies matter in terms of how they are embedded within larger social dynamics. While our many histories of computing are increasingly sophisticated—examining both hardware and software, following users and designers, exploring gender, race, and class, and so on—these histories still, curiously, almost all treat computers as discrete objects, with easily identifiable users, solving certain well-defined problems. While this may have been true in the age of the personal computer, it has not always been true nor will it always necessarily be so.[1] It is as though our histories of technology dealt solely with particular artifacts without engaging with the larger culture of engineering practice or with ideas concerning science, technology, and society. This dissertation starts from a different premise, that the culture of computing as developed in the middle of the twentieth century was primarily concerned with the problems of administration.[2] Instead of positing a particular, limited form for computers, this dissertation looks at computing wherever and whenever it occurs, and pays particular attention to the forms of computing that are in flux. Physical instruments, while certainly important, were, in a sense, only *secondarily* so. That is, computers as technological artifacts take on a new significance when viewed from the perspective of administrative theory. This does not deny that the power of the computer has strongly influenced the development of administrative principles, for it clearly has. But in its early days it did so from within and not as a radical exogenous factor.

A common organizing principle in conversations about the history of computers concerns the allegedly increasing freedom of information and of computing power. This historical theory insists that information wants to be free. In this narrative computing has gone from an older regime of top-down bureaucratic control to one of freedom and grassroots innovation. The outlines of this story are both simple and compelling. As Manuel Castells put it, "in spite of the decisive role of military funding and markets in fostering early stages of the electronics industry during the 1940s-1960s, the technological blossoming that took place in the early 1970s can be somehow related to the culture of freedom, individual innovation, and entrepreneurialism that grew out of the 1960s' culture of American campuses."[3] Early machines, including the famous

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[1] Consider, for instance, the use of smart phones and PDAs as computing platforms, or the recent development of "cloud computing."

[2] Readers familiar with the literature may suspect from the preceding sentences that the most influential works in the background of this dissertation are Jon Agar, *The Government Machine: A Revolutionary History of the Computer* (Cambridge, Mass.: MIT Press, 2003) and Paul N. Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America* (Cambridge, Mass.: MIT Press, 1996). Rather than keeping the reader in suspense throughout this introduction, the author acknowledges their spectral presence up front.

[3] Manuel Castells, *The Rise of the Network Society, 2nd ed.* (Malden, Mass.: Blackwell, 2000), 5. A more detailed examination of this transition is Fred Turner, *From Counterculture to Cyberculture: Stewart Brand, the Whole Earth Network, and the Rise of Digital Utopianism* (Chicago: University of Chicago Press, 2006). The two most important surveys of the field are Paul Ceruzzi, *A History of Modern Computing* (Cambridge, Mass.: MIT Press, 2003); and

ENIAC and the Mark I, were built with the material support of the military for the stated purpose of performing military calculations. Government patronage of computing in the cold war similarly focused on calculations for weapons development or for government-funded scientific labs (themselves part of cold war state competition), or for the administrative management of the state, including analyzing census data and processing social security payments. Commercial data processing was hardly any better, dominated by IBM and employed by insurance companies and banks. Only the very largest organizations could afford to use these machines, and these organizations used them to centralize and expand records on individuals and to engage in the arms races and expensive Big Science projects of the era.[4] This was the age of HAL, the murderous intelligence from the film *2001* that (or should we say "who?") could not let its ("his?") human captain jeopardize the mission.

Later machines, beginning with minicomputers and the creation of hacker culture and hobbyist groups, allowed ordinary individuals (or at least ordinary middle-class male engineers) to operate machines directly, leading to an explosion of innovation as hackers found new uses for their machines, new types of products built out of code rather than hardware, and new markets for these goods. Computers extended their reach into small firms, schools, and even homes. Eventually the repurposing of a scientific/military communication system for commercial and domestic audiences opened up unprecedented opportunities for free communication and the minting of ever-younger billionaires. HAL (the letters of IBM, each dialed back one position) was a nightmare of the past; the embodiment of our future mechanical overlords has become IBM's sleek Watson (named after the company's founding father, Thomas Watson), competing on a trivia show. HAL, serious to the end, must be rolling in his grave.

That this linear development obscures more than it reveals should hardly be surprising. By now it is understood that early computing offered opportunities for mathematicians on the margins—particularly women—that vanished within a generation and have not returned.[5] The contemporary regime of computing on the Internet is increasingly structured by both commercial and state power, offering unprecedented access to both the collective wisdom and the collected pornography of the wired world.[6] The commercialization of a space created for sharing scientific information has had consequences that are far more complex than early Internet utopians could have anticipated.[7]

Martin Campbell-Kelly and William Aspray, *Computer: A History of the Information Machine* (Boulder, Colo.: Westview Press, 2004).

[4] Peter Galison and Bruce Hevly, eds. *Big Science: The Growth of Large-Scale Research* (Stanford: Stanford University Press, 1992).

[5] See Thomas J. Misa, ed., *Gender Codes: Why Women are Leaving Computing* (Hoboken, NJ: Wiley, 2010).

[6] At the time of this writing, with revolutions occurring throughout North Africa and the Middle East, the role of communications technologies in fomenting revolution is a major question. For a critical take, see Evgeny Morozov, *The Net Delusion: The Dark Side of Internet Freedom* (New York: PublicAffairs, 2011).

[7] The classic example of this utopianism remains John Perry Barlow, "The Declaration of the Independence of Cyberspace," 1996, available at https://projects.eff.org/~barlow/Declaration-Final.html (retrieved 3/17/2011).

Although this complexity is recognized within the literature, it is not always welcome. The oppositions invoked by conventional studies of computing—of control versus freedom, of top-down versus bottom-up, of rationalistic versus humanistic—remain deeply embedded in the language used to describe the technological and cultural milieu of computing centers in the second half of the twentieth century. By now these are comfortable analytical categories with long and distinguished histories. As terms to describe the development of science and technology in the contemporary world, they draw upon strands of social thought from throughout the twentieth century. Yet the ease with which they fit the history of computing should give us pause.

Historiography I: From Computers as Between Science and Society…

Lurking in the midst of the historiography of computing is a preoccupation with the idea of rationality. This concern takes many different forms, including the degree to which computers contribute to a "rationalizing" of society or of the market, whether or not developments in computing fit together in some coherent way, and whether developments in computing centralize or decentralize authority. These issues concerning the rationalizing tendencies of bureaucracies suggest that the ghost of Weber (and other scholars of administration and bureaucratization) continues to haunt the discipline.

There are, of course, important distinctions to be made within the literature that point to different ways of conceptualizing the computer. The first important distinction is the basic one between internal, technical histories of computing and those that focus on external groups, such as research patrons and corporate user groups. The basic question motivating this distinction concerns the significance of the machine—whether technological developments have a logic of their own and drive the selection of problems appropriate to these machines, or whether patrons from the military or from large corporations have stamped computers in their own images.[8]

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[8] For technical histories of computers, see William Aspray, ed., *Computing Before Computers* (Ames, Iowa: Iowa State University Press, 1990); Paul E. Ceruzzi, *Reckoners: The Prehistory of the Digital Computer, from Relays to the Stored Program Concept, 1935-1945* (Westport, Conn.: Greenwood Press, 1983); N. Metropolis, J. Howlett, and Gian-Carlo Rota, eds., *A History of Computing in the Twentieth Century* (New York: Academic Press, 1980); Raúl Rojas and Ulf Hashagen, eds., *The First Computers—History and Architectures* (Cambridge, Mass.: MIT Press, 2000); Nancy Stern, *From ENIAC to UNIVAC: An Appraisal of the Eckert-Mauchly Computers* (Bedford, Mass.: Digital Press, 1981); for a more mathematically-oriented history, see Herman H. Goldstine, *The Computer from Pascal to von Neumann* (Princeton: Princeton University Press, 1972); and Donald MacKenzie, *Mechanizing Proof: Computing, Risk, and Trust* (Cambridge, Mass.: MIT Press, 2001). For histories focusing on patrons and users, see Jon Agar, *The Government Machine*; Atsushi Akera, *Calculating a Natural World: Scientists, Engineers, and Computers During the Rise of U.S. Cold War Research* (Cambridge, Mass.: MIT Press, 2007); Paul N. Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America* (Cambridge, Mass.: MIT Press, 1996); Kenneth Flamm, *Creating the Computer: Government, Industry, and High Technology* (Washington, DC: Brookings Institution, 1986); Kenneth Flamm, *Targeting the Computer: Government Support and International Competition* (Washington, DC: Brookings Institution, 1987); Thomas David Haigh, "Technology, Information, and Power: Managerial Technicians in Corporate America, 1917-2000" (Ph.D. dissertation, University of Pennsylvania, 2003). Also see the many business histories, including James W. Cortada, *Before the Computer: IBM, NCR, Burroughs, Remington Rand, and the Industry they Created, 1865-1956* (Princeton: Princeton University Press, 1993); Arthur L. Norberg, *Computers and Commerce: A Study of Technology and Management at Eckert-Mauchly Computer Company, Engineering Research Associates, and Remington Rand, 1946-1957* (Cambridge, Mass.: MIT Press, 2005). Many histories synthesize the technological developments with the priorities of patrons. See Janet

Maintaining a firm commitment to one side or the other of the internalist/externalist division is no more productive here than it is elsewhere within the literature in the history of science. The computer as a technological artifact must remain important, and its technical characteristics really do matter. At the same time, computers matter because of how they are used—which suggests the necessity of fully understanding the many communities of users. The prominent position of biographies within the computing historiography has been a response to the need to integrate the technical and the social on a manageable scale.[9]

More specific to the case of the computer is the distinction between hardware and software, which concerns the question of whether the physical computer is what is significant or whether the particular programs that run on it are.[10] A quick survey of the literature shows that the relative importance of the two has not remained constant. This suggests that if we wish to maintain a working definition of "computing," it will need to be a protean one.

But there remains a third distinction, less often acknowledged in the literature, between histories that take the dynamic of growing computer power for granted as an engine of economic and social change and those that take a critical stance toward the changing nature of computing. The first camp includes detailed studies of computing in various sectors of the economy, often

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Abbate, *Inventing the Internet* (Cambridge, Mass.: MIT Press, 2000); Arthur L. Norberg and Judy E. O'Neill, *Transforming Computer Technology: Information Processing for the Pentagon, 1962-1986* (Baltimore: Johns Hopkins University Press, 1996); and JoAnne Yates, *Structuring the Information Age: Life Insurance and Technology in the Twentieth Century* (Baltimore: Johns Hopkins University Press, 2005). On the issue of technological determinism, see Merritt Roe Smith and Leo Marx, eds., *Does Technology Drive History? The Dilemma of Technological Determinism* (Cambridge, Mass.: MIT Press, 1994). Two articles on related subjects that take substantially different approaches with regards to determinism are Paul E. Ceruzzi, "Moore's Law and Technological Determinism: Reflections on the History of Technology," *Technology and Culture* 46 (2005): 584-593, and Paul Forman, "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960," *Historical Studies in the Physical and Biological Sciences* 18 (1987): 149-229.

[9] Many such biographies shade into biographies of the computers themselves. See William Aspray, *John von Neumann and the Origins of Modern Computing* (Cambridge, Mass.: MIT Press, 1990); Thierry Bardini, *Bootstrapping: Douglas Engelbart, Coevolution, and the Origins of Personal Computing* (Stanford, Calif.: Stanford University Press, 2000); Kurt Beyer, *Grace Hopper and the Invention of the Information Age* (Cambridge, Mass.: MIT Press 2009); I. Bernard Cohen, *Howard Aiken: Portrait of a Computer Pioneer* (Cambridge, Mass.: MIT Press, 1999); Hunter Crowther-Heyck, *Herbert A. Simon: The Bounds of Reason in Modern America* (Baltimore: Johns Hopkins University Press, 2005); Steve J. Heims, *John von Neumann and Norbert Wiener: From Mathematics to the Technologies of Life and Death* (Cambridge, Mass.: MIT Press, 1980); J. A. N. Lee, *Computer Pioneers* (Los Alamitos, Calif.: IEEE Computer Society Press, 1995); M. Mitchell Waldrop, *The Dream Machine: J. C. Licklider and the Revolution that Made Computing Personal* (New York: Viking, 2001). Also relevant is the "founding father" character of Babbage. See I. Grattan-Guiness, "Charles Babbage as an Algorithmic Thinker," *IEEE Annals in the History of Computing* 14 (1992): 34-48; Anthony Hyman, *Charles Babbage: Pioneer of the Computer* (Princeton, NJ: Princeton University Press, 1982); and Simon Schaffer, "Babbage's Intelligence: Calculating Machines and the Factory System," *Critical Inquiry* 21 (1994): 203-227.

[10] Martin Campbell-Kelly, *From Airline Reservations to Sonic the Hedgehog: A History of the Software Industry* (Cambridge, Mass.: MIT Press, 2003); Nathan Ensmenger, *The Computer Boys Take Over: Computers, Programmers, and the Politics of Technical Expertise* (Cambridge, Mass.: MIT Press, 2010); and Ulf Hashagen, ed., *History of Computing: Software Issues* (New York: Springer, 2002).

employing the type of business history pioneered by Alfred Chandler.[11] Within this genre of business history, the association between the use of computers and the growing rationalization of the economy and society is almost taken for granted. Perhaps the most significant effort in this vein is James Cortada's *Digital Hand* trilogy, which explicitly presents information technology as reshaping the way that economies are organized, supplementing both the market mechanisms of Adam Smith's invisible hand and the corporate coordination of Chandler's visible hand.[12] Cortada makes an important point in situating computers (and other related technologies) as cutting across industries in such a way as to organize broad swaths of economic activity. The ghost of Weber hovers over these histories with their fixation on computers as tools to improve the processes of administration. But the great breadth of Cortada's work comes at the cost of missing the broader cultural shifts and the redeployment of power and resistance within this digitized economy.

These narratives, however useful, fail to address the basic questions of how the meaning of computing has changed historically and how such changes have reflected fundamental shifts in the organization of power within society. A critical intervention is essential to get at the larger significance of computerization in the second half of the twentieth century.[13] As noted above, such analyses have drawn upon the basic critiques of science and technology from the middle of the century. Many of these came from European scholars, including but not limited to those from the Frankfurt School—though an independent group of disillusioned American progressives in the postwar years contributed to these analyses.[14] These works have a special resonance for the study of computing because they were frequently composed in opposition to the technocratic arguments that surrounded the development of early computers and the creation of social management systems that invoked bureaucratic rationality.[15] When critical studies of science

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[11] See Alfred D. Chandler, Jr., *The Visible Hand: The Managerial Revolution in American Business* (Cambridge, Mass.: Belknap Press, 1977); idem., *Scale and Scope: The Dynamics of Industrial Capitalism* (Cambridge, Mass.: Belknap Press, 1994); idem., *Inventing the Electronic Century: The Epic Story of the Consumer Electronics and Computer Industries* (New York: Free Press, 2001); and Alfred D. Chandler, Jr. and James W. Cortada, eds., *A Nation Transformed by Information: How Information has Shaped the United States from Colonial Times to the Present* (New York: Oxford University Press, 2000).

[12] The trilogy includes James W. Cortada, *The Digital Hand: How Computers Changed the Work of American Manufacturing, Transportation, and Retail Industries* (New York: Oxford University Press, 2004); idem., *The Digital Hand: How Computers Changed the Work of American Financial, Telecommunications, Media, and Entertainment Industries* (New York: Oxford University Press, 2006); idem., *The Digital Hand: How Computers Changed the Work of American Public Sector Industries* (New York: Oxford University Press, 2008).

[13] See Agar, *The Government Machine*; Edwards, *The Closed World*; Donna Haraway, "A Cyborg Manifesto: Science, Technology, and Socialist-Feminism in the Late Twentieth Century," in *Simians, Cyborgs, and Women: The Reinvention of Nature* (New York: Routledge, 1991), 149-181. A useful background for critiquing the foundations of such formal systems is Theodore Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton, NJ: Princeton University Press, 1994).

[14] A leading example is Lewis Mumford. See Lewis Mumford, *The Myth of the Machine: Technics and Human Development* (New York: Harcourt, Brace & World, 1967); idem., *The Myth of the Machine: The Pentagon of Power* (New York: Harcourt, Brace & World, 1970).

[15] In addition to the focus on computers, the nuclear establishment drew its share of criticism, as did space exploration. Contemporary equivalents may be found in certain areas of biotech research.

identified the elements of instrumental rationality that were central to mid-century fascism and militarism and tied those elements to the self-described goals of the scientific project of modernity (control over nature, the projective utilization of resources, the desire for universality at the expense of particular differences, etc.—the litany should be familiar by now), these were bracing thoughts.[16]

And yet these critics of the technostructure bought into the same central dynamic of ever-increasing rationalization. However, instead of celebrating the efficiencies of rational administration and coordination, they pointed to the great costs of this rationalization for individual freedom. While the celebratory histories of computing (such as Cortada's) praised the expansion of the sphere of rational control over chaos and conflict, critical histories (such as Edwards's) decried the totalizing power of administration. Both styles recognized that computers somehow brought more of the world into the "rational" side, against which resistance was the primary contributor to human freedom. Both brought a strong sense of fatalism to the development of computers, though they differed in their sympathies.

Yet amidst the threat of control are the seeds of liberation. The power of computers and information technology has promised opportunities for self-creation as well as for top-down control. This possibility, eloquently described by Donna Haraway in her "Cyborg Manifesto," cuts against the traditional storyline of computers as leading inexorably to efficiency and technocracy.[17] Her analysis—and those of scholars working in this tradition, such as Paul Edwards—challenges us to better understand the fork in the road ahead. How exactly does the development of information technology contribute to rationalizing the world? Within an increasingly technologically determined environment, what does freedom mean? How are we to achieve it? What would a society of cyborg subjects look like?

Historiography II: … to Reconstructing Science and Society from the Work of Computing

To explore these topics we must put aside that strand of science and technology studies that focus on the large-scale dynamics and instead turn to the detailed study of particular

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[16] This is by no means intended to be an endorsement of Edwin Black's claim to have uncovered an explicit connection between IBM and the Nazis, as the many shortcomings of his work have been extensively noted. His work is Edwin Black, *IBM and the Holocaust: The Strategic Alliance Between Nazi Germany and America's Most Powerful Corporation* (New York: Crown Publishers, 2001) and reviews include Michael Allen, "Stranger than Science Fiction: Edwin Black, *IBM and the Holocaust*," *Technology and Culture* 43 (2002): 150-154. I refer instead to the more general claims that modernity contains an impulse within itself that led to domination and totalitarianism. See Max Horkheimer and Theodor W. Adorno, *The Dialectic of Enlightenment* (Stanford, Calif.: Stanford University Press, 2002) and Herbert Marcuse, *One-Dimensional Man* (Boston: Beacon Press, 1964). A more recent take on the self-defeating limits of the modernizing impulse is James C. Scott, *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed* (New Haven, Conn.: Yale University Press, 1998).

[17] The role of information technology in this narrative fits the lines of Hölderlin quoted by Heidegger: "Where the danger is grows the saving power." The saving potential for the technology exists alongside its potential to exacerbate the rationalization of the world. But where Heidegger later lamented that in a cybernetic age "only a god can save us," Haraway argues that the cyborg's ability to transcend tired essentialisms provides an opportunity to escape from this rationalization. She stands Heidegger on his head and declares that she would "rather be a cyborg than a goddess." See Haraway, "Cyborg Manifesto," 181.

computing work. The more empirical studies of science have shown how the practice of science differed in profound ways from the scientific community's descriptions of what it was doing.[18] Far from being paragons of rationality, scientists exhibited the same personalities as the rest of humanity. The same psychological tics influenced how scientists interpreted data and questioned theories. The same clannish loyalties and political maneuverings influenced the direction of their work.[19] These studies were no less subversive than their "grand theory" kin in puncturing the pretensions of the scientific community and demonstrating how science remained a fully human activity.

The result of these studies has been a two-pronged reassessment of scientific research. On the one hand, the pure ideal of science as transmitted to countless students and readers of the popular press bore only the faintest relationship to the work done in the laboratory and in the boardrooms of scientific patronage agencies. On the other hand, the very goals of science were being implicated in the most destructive aspects of modernity.[20] The study of computing has tended to draw more inspiration from this latter component through its engagement with rationalization. Where conflict exists within computer narratives, it tends to be between the high priests of institutional computer centers, funded by the military or the largest corporations, and the hackers futilely fighting for digital freedom but always being co-opted by either The State or The Market. But when we zoom in we see computers being built for purposes that only partly follow the expected script. Our analytical atom is the man-machine hybrid of a worker using a tool to manipulate information. Our strategy is to study these atoms and observe how they create new configurations of both "the rational" and "the social." This reverses the traditional approach, for which centralized and de-centralized organizational forms exist as readymade analytical tools and for which the rational and the social exist as preexisting spheres of historical development.

Bruno Latour has contributed the most to this style of science studies. He has described the fundamental constitution of modernity as requiring both a strict separation between "the

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[18] Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts* (Princeton, NJ: Princeton University Press, 1986) and Bruno Latour. *Science in Action: How to Follow Scientists and Engineers Through Society* (Cambridge, Mass.: Harvard University Press, 1987).

[19] For some historical studies, see Paul Forman, "Weimar Culture, Causality, and Quantum Theory, 1918-1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment," *Historical Studies in the Physical Sciences* 3 (1971): 1-115; Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (Cambridge, Mass.: Harvard University Press, 1995); Philip J. Pauly, *Biologists and the Promise of American Life: From Meriwether Lewis to Alfred Kinsey* (Princeton, NJ: Princeton University Press, 2000).

[20] While the field of science studies has been accused of undermining trust in science, the confluence of these two analytical trends actually provides a substantial defense of science as an important activity. Acknowledging that the practice of science is far more complex than suggested by its ideological defenders (such as Robert Merton and Karl Popper) means acknowledging that it is also not actually complicit in the reductiveness and anti-humanism that certain critics claim. By describing science as it is actually practiced, the pragmatism of the enterprise takes center stage. The need to set it apart through some essential characteristics vanishes. A defense of realistic science would take the wind out of those who disingenuously use the scientific rhetoric of skepticism, disinterestedness, and caution to undermine the scientific consensus on evolution, global warming, and secondhand smoke. See Naomi Oreskes and Erik M. Conway, *Merchants of Doubt* (New York: Bloomsbury, 2010). On Merton and Popper see David Hollinger, "Science as a Weapon in *Kulturkämpfe* in the United States during and after World War II," in *Science, Jews, and Secular Culture* (Princeton, NJ: Princeton University Press, 1996), 155-174.

scientific" and "the social" and the continued proliferation of "hybrid objects," even as their existence as hybrids must be denied to maintain the purity of the scientific and the social as separate spheres. The scientific sphere encompasses all that is transcendent and necessary while the social sphere encompasses all that is immanent and subject to human will. The work of modernity has involved creating hybrid objects that refuse to be neatly categorized while declaring that they must be so categorized in order to maintain the validity of this separation. The increasingly sophisticated production of hybrids has made the work of explaining them away increasingly difficult. However, in Latour's understanding, the difficulties of maintaining this arrangement allow us now to grasp the futility of the modernist arrangement. By understanding its impossibility we can see that we have, in fact, "never been modern."[21]

Placing the history of computers within this framework draws attention to the computer as a hybrid object that has raised extraordinary difficulty for those scholars who have insisted on placing it on one side or the other of the science/society binary. Yet computers existing totally outside that schematic simply could not be tolerated; the work of maintaining the social and the scientific was too important to abandon, and the power of the computer was too great for it to be easily ignored (hence the eternal questions: is the study of computers to be considered "science" or "engineering?" What is at stake in this distinction?). The job of categorizing the computer therefore operated along two dimensions: one of locating computers as variously technological or social artifacts (or, more fruitfully, situating components of computers and emphasizing the hardware/software distinction), and one of explaining away the difficulties of pinning it down—a process, I argue, that contributed to the very developments in STS that made Latour's insights possible. The study of communications (as communications relates to both social processes and the inner workings of machines) brought scientific analysis into the social world, while simultaneously bringing social forces to bear upon the construction of scientific truth.

The worker at the machine constitutes a particularly problematic hybrid. The computer itself, as Michael Mahoney observed, is a blank object and multi-functional.[22] It is not even necessarily mechanical—the hardware matters, but code reigns supreme in terms of marking the boundaries between spheres of freedom and of necessity. The human worker, meanwhile, only sometimes has the technical knowledge to understand the workings and the failures of his or her electronic partner. As a historical development, the increasing power of machines has tended to correspond with an increasing complexity that renders operators more reliant upon the machines.[23] We might agree with the pessimists that something essentially human is lost to the computer operator in this relationship, even as we agree with the optimists that these workers gain an unprecedented power over the world. Defining the parameters of necessity and freedom is a task that can only be done by following the man-machine relationship as it evolves over

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[21] See Bruno Latour, *We Have Never Been Modern* (Cambridge, Mass.: Harvard University Press, 1991).

[22] Michael S. Mahoney, "The Structures of Computation," in *The First Computers—History and Architectures*, ed. Raúl Rojas and Ulf Hashagen (Cambridge, Mass.: MIT Press, 2000), 17-32.

[23] See Thomas K. Landauer, *The Trouble with Computers: Usefulness, Usability, and Productivity* (Cambridge, Mass.: MIT Press, 1995); Donald MacKenzie, *Knowing Machines: Essays on Technical Change* (Cambridge, Mass.: MIT Press, 1996); and Lucy Suchman, *Human-Machine Reconfigurations: Plans and Situated Actions* (New York: Cambridge University Press, 2007).

time. This hybridity required new ways of thinking about science and society. As mentioned above, one way in which it was explained was through the articulation of critical theories of technology.

Clarifying the position of human workers vis-à-vis computers also raised questions of a more traditionally political nature. The man-machine hybrids at the center of this dissertation do, after all, include humans whose lives involved more than carving out autonomous spaces within the rationality of organized systems. Watching this relationship evolve through time shows how the work of humans on the margins has been displaced into the operations of machines, with consequences for the exercise of power within computerized organizations.[24] The larger historical dynamic of access to computers being democratized through the 1960s and 1970s also raises questions of what it meant for control over this power to be distributed after having been limited to large public and private organizations.

Questions of gender also cannot be ignored. Gender is, in fact, a recurring undercurrent throughout the dissertation. This is not only because of the notable shift away from female computer operators and programmers to male ones, but also for more abstract connections between the development of computer systems and ideas of gender through that concept's own instability with regards to the nature/culture distinction.[25] If computers matter as particularly troubling hybrid objects, the ur-hybrid remains the female body, defined in terms of a biological capability for childbirth and embedded within complex social codes.[26] The challenges of Latour's "purification" have been intrinsic to the major themes of feminism. These theoretical resonances, coupled with the importance of women in the history, mean that an analysis of gender must occupy a position of some importance in any interpretation of computing.

These various developments have much to do with the politics of the second half of the twentieth century. The widespread use of information technology in the heart of state administration and in structuring relationships between individuals suggests that theories of contemporary political behavior neglect technology at their peril. There are, of course, many ways of taking technology into account. Few of these are sufficiently attuned to taking seriously how technology works. We can speak intelligently about the competitive advantages that accrue to the users of this or that technology, of the significance of policies that promote innovation, and of the thorny problem of understanding how access to technology is stratified by class and other social distinctions. We are even becoming aware of the many unintended consequences of

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[24] See David Alan Grier, *When Computers Were Human* (Princeton, NJ: Princeton University Press, 2005).

[25] Haraway, "A Cyborg Manifesto"; Sandra G. Harding, *The Science Question in Feminism* (Ithaca, NY: Cornell University Press, 1986); Marie Hicks, "Compiling Inequalities: Computerization in the British Civil Service and Nationalized Industries, 1940-1979" (Ph.D. dissertation, Duke University, 2009); Jennifer S. Light, "When Computers were Women," *Technology and Culture* 40 (1999): 455-483; Misa, *Gender Codes*.

[26] In chapter three I suggest that the jobs that are considered to be replaceable by computers are those done by individuals on the margins; clerical work was ripe for automation for the same reason that it had been deemed appropriate for women.

technologies that complicate their common portrayal as pure instrumentalities.[27] We understand that technologies matter on the brute level of distributing political power.

While important contributions to thinking politically about technology, these analyses do not go far enough. Explaining how technology acts within a world structured by politics requires thinking about technologies as inherently political artifacts. This does not mean that technologies directly lead to certain political outcomes—that the Internet is a vehicle for freedom and democratization, for example. It instead means thinking about technologies, in all of their multifaceted complexity, as interacting directly with humans, animals, and other members of our world. This means recognizing that computers, even as mute artifacts, nevertheless do engage in dialogues with humans about the basic concepts that define the very fields in which power is exercised. As the definitions of rational and intelligent behavior change through the use of computers, these changed definitions have consequences for the social status of the women who once operated these machines. By prompting new questions about what intellectual capabilities are uniquely human, the use of these machines had large consequences for the basic idea of human dignity.[28] And by constantly expanding the range of problems that are said to be calculable, they consequently shrink the space of politically legitimate human agency.

In assessing the origins of computers—"thinking machines"—we must look beyond the usual cybernetic nexus of communications engineers, cognitive scientists, psychologists, and linguists, and instead consider political questions of individual and collective human reason that have figured prominently in American liberal thought from the Progressive Era through the Great Society. This is not to deny the contributions of cyberneticians, but rather to note that insofar as computers were machines designed to "think," the nature of thought itself remained contested. This dissertation situates these changing forms of knowledge work within the particular context of mid-century America. Debates about the nature of computing and knowledge were intimately connected to the discourse of administration from World War II through the end of the Great Society.[29]

A consequence of understanding computer architecture as an intervention in political culture and ideas of organization is that scientists, engineers, hackers, programmers, and system administrators emerge as political actors. This, in itself, is not a novel claim. However, in understanding these technologists as politically engaged, their significant contributions are in the systems that they design, the programs they write, and their analyses of their technical subject

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[27] Langdon Winner, "Do Artifacts Have Politics?" in *The Whale and the Reactor: A Search for Limits in an Age of High Technology* (Chicago: University of Chicago Press, 1986), 19-39.

[28] Jaron Lanier, *You Are Not a Gadget: A Manifesto* (New York: Alfred A. Knopf, 2010); Bruce Mazlish, *The Fourth Discontinuity: The Co-Evolution of Humans and Machines* (New Haven, Conn: Yale University Press, 1993); and Sherry Turkle, *The Second Self: Computers and the Human Spirit* (Cambridge, Mass.: MIT Press, 2001).

[29] Howard Brick, *Age of Contradiction: American Thought and Culture in the 1960s* (New York: Twayne, 1998); idem., *Transcending Capitalism: Visions of a New Society in Modern American Thought* (Ithaca, NY: Cornell University Press, 2006); Nelson Lichtenstein, ed., *American Capitalism: Social Thought and Political Economy in the Twentieth Century* (Philadelphia: University of Pennsylvania Press, 2006); Richard H. Pells, *The Liberal Mind in a Conservative Age: American Intellectuals in the 1940s and 1950s* (Middletown, Conn.: Wesleyan University Press, 1989).

matter—not necessarily in the overtly or superficially political statements that they may have made. With certain exceptions these individuals were not penning sophisticated political or economic analyses. The political analysis of science and technology ought to be more than little league cultural history. Fortunately their technical work has a richness that fully deserves examination, and the relationship between their technical-political interventions and the more directly political interventions of their contemporaries resonated with each other.[30]

This approach changes the stakes of the history of computing. No longer is it about explaining a particular path of technological development at the hands of military and corporate interests. Nor is it about how tech-savvy elements of the counterculture liberated computers from the Establishment. The stakes instead shift to how diverse forms of expertise were constructed and how autonomy was continually negotiated within increasingly formal systems. The historical dynamics of the man-machine hybrid are the foundation for demarcating the spheres of human agency/freedom and scientific/mathematical determinacy.

Reading the Dissertation

By making the Latourian move to see how both science and society are constructed through the experience of working with computers, this dissertation avoids the familiar categories of internalist and externalist. The location of the boundary between "internal" and "external" is precisely what is at issue. Therefore, the academic laboratory setting should not be taken to imply that the technological developments drive this narrative. Nor should the sections involving social theorists be taken to suggest that these determine the technical dimension to the story. Nor, finally, should the commingling of the two be taken to mean that the technology and the theory construct each other in some kind of interactive process. There are many points in this dissertation for which that is the case, but there are also many points when the theorists and the technologists are not in direct dialogue. Rather than make the concepts of the scientific and the social drive the narrative, this dissertation uses such terms as the outcomes of its narrative developments.[31]

This dissertation makes several arguments that build up to its central thesis. The first argument emphasizes the importance of industrial management for motivating the idea of computing as a generalized form of information processing. This generalization of the work that computers do did not emerge from the mathematical problems that motivated early scientific computing, nor was it a straightforward outcome of applying machines to military problems. The

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[30] For histories that do take technologies seriously as interventions in political theory, see Crowther-Heyck, *Herbert A. Simon*; and Matthew H. Wisnioski, "Engineers and the Intellectual Crisis of Technology, 1957-1973" (Ph.D. dissertation, Princeton University, 2005).

[31] STS readers may ask why it fails to live up to the narrative gymnastics of Latour's *Aramis*, to which the author can only plead that this is but a dissertation, with the narrow but virtuous aims of that genre. See Bruno Latour, *Aramis: Or the Love of Technology* (Cambridge, Mass.: Harvard University Press, 1996) On the larger question of whether *Aramis* points to a more fully realized synthesis of narrative form and content than traditional historical narratives, this author agrees. Hayden White's criticism that historical narrative remains mired in 19th century forms becomes even more urgent given the role of information theory in destabilizing contemporary narrative. See Hayden V. White, *Tropics of Discourse: Essays in Cultural Criticism* (Baltimore: Johns Hopkins University Press, 1985).

crucial centers of innovation were the tech-oriented management schools of the mid-twentieth century that brought together engineers, organization theorists, and practicing administrators.

The second and third arguments concern the changing role of managers amidst technological change. As computers handled increasingly sophisticated tasks and elementary decision-making, the work of purification involved more strictly defining the regime of creativity against that of routine (though of course this could never fully be achieved in practice). Therefore the professionalization of management within these schools required distinguishing two tracks: the acquisition of technical skills legitimated a lower form of managerial authority while the higher form of authority required those skills that remained beyond codification and explicit description. The exercise of judgment became reserved for top management, for whom trust in their judgment relied upon trust in their character. Trust in the decisions of skilled technicians (of lower social standing) required only trust in the skills themselves and in the technician's ability to have learned those skills.[32]

This pointed to a similar bifurcation in the forms of expertise in which a genteel form, involving the acquisition of deep experience and wisdom—almost a moral undertaking—sat uncomfortably alongside a newer model that involved acquiring knowledge, understood as atomized propositions. The justification for this latter form of expertise built upon the technical redefinition of information from the 1940s-1960s, though the suspicion of expertise that could not be formally expressed in terms of factual propositions and firm rules had a much longer lineage that motivated the creation of the management schools mentioned above.

The fourth argument is that the increasingly symbiotic relationship between human workers and their computers required a redefinition of what capabilities were uniquely human. Because computers, for all their flexibility, were not nearly as flexible as their human partners, this meant defining human capabilities in opposition to those of computers. The domain of human expertise was the negative image of the computers' domain. The result was a relative devaluation of the most easily formalizable aspects of intellectual work while increasing the value of creativity. At the same time, with the operations of computers having become increasingly important for skilled work, the capability to accommodate oneself to their demands became a very highly valued skill, with implications for the perception of human dignity in a computerizing world.

The final argument concerns the importance of bureaucracy for the libertarian turn in computing in the late 1960s and early '70s. While this moment has traditionally been understood as a flowering of individual ownership of computers and of loosely organized grassroots communities of users experimenting directly with machines, this dissertation suggests that this was by no means a repudiation of bureaucracy and hierarchical organization. Instead, thanks to the growing power of computers—whose development to that point had been so strongly influenced by organization theories and ideas of bureaucracy and administration—the growing availability of minicomputers meant the proliferation of such bureaucratic centers of power. The democratizing moment was not the result of transcending bureaucracy, but of growing it exponentially so that every hobbyist suddenly had at his or her fingertips a vast, rational

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[32] Meanwhile, trust in machines similarly required trust in the correctness of their code and trust in the robustness of their hardware—both of which were wide open for questioning.

administration of circuits. The ability of large-scale bureaucracies to mobilize individuals was not reduced, though the power of individuals grew substantially. The common view that the growth of computer power has flattened organizational hierarchies mistakes a symptom for the cause. What has mattered is the growth of the networks that can be mobilized. Computers are but one manifestation of these networks, but should not be taken as the only one. Certain well-positioned human actors went from being elements of systems to being administrators, but most did not.

The major argument that emerges from the concurrence of all of these is that a 20th century regime that encouraged the role of expert management as a way of impartially balancing competing claims and that encouraged a certain formalism reached a point, as a result of its own successes, in which the power of the formalism became separated from the wisdom of the expert. This constitutes the birth of the post-industrial moment and the resurgence of politics through the democratization of the formal power that had been the mark of managerial authority. Access to knowledge was becoming common. So too was access to basic information processing power. The claims of managers that their expertise was wholly rational never accurately described the full picture, but taken at face value it resulted in a negation of their actual authority.[33]

The first chapter begins with a brief survey of the late 19th century, focusing on the development of modern forms of management and the creation of the corporation as a system of organization. This dissertation is hardly the only study of computing to begin a century before the main work of computing took off. However, while the significance of the nineteenth century is often understood in terms of proto-computer technologies (whether actually built or not), such as the work of Charles Babbage or the communications technologies that made up “the Victorian Internet,” this chapter examines the creation of formal sciences of management as well as the varied responses to this development.[34] Its argument is that subsequent debates concerning the normative dimensions of computing can only be understood in terms of this earlier moment. Furthermore, it establishes the centrality of questions of administration and of organization for the early use and subsequent development of computers. That administrative work was among the first to be moved onto machines has been well established. However, the consequences of this orientation for the actual development of machines (as opposed to how it created an early source of customers) have not been adequately studied.

The second chapter turns more directly to the first computers. Their stories have been told many times before, though this iteration adds a twist. It examines the questions of early computer architecture from the perspective of maintaining discipline within the workplace. Managing the labor of computers required understanding the differences between how machines and workers followed instructions, fit into sociotechnical systems, and reacted to unanticipated circumstances. A consequence of this approach is a much better understanding of the different social meanings of computers, information, and related technologies and intellectual movements.

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[33] Compare this critique of the language of managerial authority to the critique of scientific authority in note 20, above.

[34] See, for example, Hyman, *Charles Babbage* and Dorothy Stein, *Ada: A Life and a Legacy* (Cambridge, Mass.: MIT Press, 1985); also see Tom Standage, *The Victorian Internet: The Remarkable Story of the Telegraph and the Nineteenth Century’s On-Line Pioneers* (New York: Walker, 1998).

Both the utopian forecasts of computerized societies and the dystopian ones were based upon thinking about computer as machines that were woven into the fabric of human life—not as merely technological marvels.

The direction of the dissertation changes in the third and fourth chapters, which deal more concretely with specific work done in the 1960s and 1970s. The third chapter considers the development of interactive computing at MIT, but does so through a detailed analysis of what interactivity meant and what it was supposed to accomplish. Interactivity was not an obvious development, nor was it obviously superior to other modes of computing. Instead, the continued appeal of interactivity involved understanding computers as mechanical analogues to human office workers. Given that these tended to be female clerical workers, the problem of defining the man-machine boundary took on deeply gendered meanings. Policing the boundary between human and machine required also maintaining a boundary between men and women.

The problem of defining the position of humans vis-à-vis computers took on greater urgency in artificial intelligence research, described in chapter four. The point here is that artificial intelligence was built upon a new theory of the individual as an information processor. By reading universalizable principles into the practice of human cognition, cognition moved beyond being a quasi-mystical, uniquely human capability into a sphere of scientific control. This in turn destabilized the foundations for maintaining *routine* intellectual work as an activity fit for human thinkers. As studies of industrial society in the 1960s began to confront the nascent concept of an "information age," defining the roles of "knowledge workers" required taking seriously these new models of the human as an information processor.

The final chapter returns to explicitly political questions through an examination of a particular effort to harness computer technologies for the administration of society and the development of a new form of social science. The reactions to this agenda were built upon the previous history of debating the cultural meanings of information technology. The chapter argues that these changing ways of mediating the relationship between humans and machines were intimately bound up with the larger transformations in American political culture during the 1960s and 1970s. The subsequent development of computing technologies took a very different turn for reasons that had as much to do with politics and culture as with technology.

Part I

The first two chapters of this dissertation explore the history of administrative theory and early developments in computation. Taken together, these chapters connect the postwar discourse of computing to a much longer concern with accountability in organizational decision-making. Among the many points of contact between computing and society, the relationship between calculation and decision-making stands at the heart of the dissertation.

The first chapter reaches into the late nineteenth century to connect the technologies of organizing information to the creation of the social system of modern management. American political culture has had an ambivalent relationship to management from its earliest days—valuing the efficiencies that can come from the managed enterprise, but suspicious of the concentration of power and the occupational regimentation that it creates. As the technologies of the office paved the way for industrial applications of computers, this critique of administration influenced early perceptions of computing.

Yet computing also drew upon a related, but distinct, strand in American culture concerning the division of labor. American developments in industrial engineering, including Taylor's Scientific Management and Fordist assembly lines, held as their goal the creation of the automatic factory. Computing fit into the ideal of automating mental work, and stirred up anxieties about the nature of mental labor and the skill involved in office work. The chapters in this section lay an essential foundation for understanding the subsequent development of computing technologies, described in the next section.

Chapter 1: Management Science and Administrative Machinery

Understanding the political valence of the new information technologies of the 1950s and 1960s requires first understanding the associations forged between the technologies of administration and democratic politics in the previous decades. The logic of administrative technologies could influence social thought in various ways. To some observers, applying the cool, disinterested logic of computerized data processing to social and economic problems meant a welcome transcendence of narrow self-interest and irrationality. To others, the extension of computerized data processing to more and more components of life suggested the excessive growth of a very particular notion of rationality. Computers were new, but these ideas had grown out of a long engagement with the relationship between administrative mechanisms and political action. At stake in the recurring debates about the promises of computing and its dangers was a basic question: how to balance the efficiencies created by the expert coordination of collective action against individual freedom and the pursuit of narrower self-interests. The development and application of computing technologies in the 1950s and ’60s was shaped by this line of questioning and the answers that had been given in the first half of the twentieth century.

The significant patronage of computing technology by defense interests has overshadowed the administrative origins of the technologies. The importance of the Ballistics Research Lab at Aberdeen, Maryland and of the early hydrogen bomb calculations at Los Alamos for early computers are well-known and uncontroversial. However, the subsequent transition from computerized data processing as a scientific tool to data processing as a central component of disinterested administration remains largely unexamined. There is no overt contradiction between military-centric origin stories and administrative ones; by World War II the military already faced formidable problems in manpower and logistics, and that bureaucratic behemoth grew even larger with the Cold War creation of the “national security state.” While no history of computing should—or even could—ignore the influence of the military, this story prioritizes a different set of actors: those concerned with the balance between individual freedom of action and a collective sense of social welfare. This was a diverse group, ranging from economic and political theorists to business managers, politicians, and engineers. The focus of the dissertation is therefore within the nexus of social and technological change, and it is therefore simultaneously a history of specific developments in computing technology, a social history of changing employment patterns, and an intellectual history of administrative theory.

Understanding mid-century computing requires understanding the relationship between engineering and management, which became closely intertwined in the “New Look” of management theory pioneered at Carnegie Tech and at MIT around 1950. The first chapter considers ideas about administration and management in the first half of the twentieth century.[1]

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[1] Today “management” is associated more with business, and “administration” with government, but the relationship between these terms was originally much closer, and the two will be used interchangeably in much of this chapter. When the differences begin to matter, it will be noted. On this point, see Peter F. Drucker, “Management as Social Function and Liberal Art,” in *The Essential Drucker* (New York: Collins Business Essentials, 2001), 3-13. Also consider Max Weber: “It does not matter for the character of bureaucracy whether its

The idea of management was articulated at a time of rapid industrialization, involving both technological innovation and organizational growth. The task of management therefore required the study of both the mechanical and human elements of the organization. The links between management and engineering made by Frederick Taylor have been studied extensively, but this chapter explores the popular reception of "scientific management" to understand how industrial engineering became a way of understanding social order. Business leaders and scholars identified two general trends in the industries of the early twentieth century: the consolidation of authority for planning at the top of the organization and the subsequent routinization of ordinary work, and the growing application of machines either to replace or standardize work done by employees. Advocates of administrative reform envisioned a career civil service that would execute policies without being compromised by politics—a bureaucratic machine.[2] This chapter follows the development of these ideas into a new field of "management science" in the 1950s. The significance of management science was its ability to describe the productive capacities of industry as being the product of generalized patterns of organization, rather than identifying it with the traditional factors of production: land, labor, and capital. As Peter Drucker put it, "it is the pattern that is actually productive, not the individual."[3]

After the creation of the Fordist factory, the management of an organization had to consider not only how to coordinate the actions of individual workers to achieve organizational goals, but also how to coordinate the work of humans with that of machines. The application of machines had several well-known economic advantages, and they could easily do tasks that were dangerous or difficult for humans. The greater use of machines within the factory challenged the place of human workers. By the 1950s the argument that work on assembly lines was fundamentally dehumanizing was met with the confident claim that technological developments could liberate these workers by replacing them with machines, literally de-human-izing industrial work. Between Ford's innovations in the 1910s and the automation furor of the 1950s, managers recognized that skilled work did not necessarily require skilled workers. Certain forms of skilled craftwork could be replicated through either technological or organizational means.

The new business schools of the 1950s defined management science in the language of institutionalism, behavioralism, and quantification. Yet they were challenged both by conservatives contrasting technocratic ideas of management against entrepreneurship, and by those critiquing the power relationships among managers, owners, and employees. This is crucial for understanding the later widespread fears of computers as creating unemployment and as part of the way that people spoke about "bureaucracy," "organization," and "technocracy" in the 1950s and '60s.

The management science of the 1950s was "scientific" insofar as the goal of the discipline was to identify general laws of organization rather than developing narrow expertise in

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authority is called 'private' or 'public.'" H. H. Gerth and C. Wright Mills, eds., *From Max Weber: Essays in Sociology* (New York: Oxford University Press, 1946), 197.

[2] A provocative analysis of civil service as a machine, though in a British context, is Jon Agar, *The Government Machine: A Revolutionary History of the Computer* (Cambridge, Mass.: MIT Press, 2003).

[3] Peter Drucker, *The New Society: The Anatomy of the Industrial Order* (New York: Harper, 1950), 22.

the operations of particular industries. Management science therefore built upon a familiar and long-standing distinction between "specialists" and "generalists." Specialists had an intimate knowledge of a narrow topic while generalists took in the big picture and were therefore suited for supervisory positions. This division was strongly associated with class; specialists hailed from the middle class professions, whose autonomy was based on possessing specialized knowledge, while the authority of generalists depended upon a classical education and the cultivation of character. Yet management science added a twist; it was a form of specialized knowledge—but a specialization in forms of organization and in the process of generalization itself.[4] Within the context of management science, the specialized knowledge and skills of the managed became another resource to be used by the organization while the responsibility for employing this knowledge and skill was reserved for the manager.

This chapter follows the dialogue between management theorists and engineers in the first half of the twentieth century: from the creation of political systems designed to insulate decision-making from personal whims, to the creation of administrative systems designed to insulate decision-making from the *person*; from the design of bureaucracies intended to efficiently execute managerial decisions and amplify managerial control, to the design of machines intended to efficiently execute the decisions and amplify the control of a more general class of users. These two broad claims seem contradictory: one meant to eliminate individual autonomy as far as possible through the creation of rigid procedures, and one meant to increase individual power by using an organizational and technological apparatus to extend his reach. Yet these applied to different groups of people. There were two distinct questions here, one descriptive and one prescriptive: how did management map onto ideas of mechanism, and should management take more or less mechanical forms?

The Creation of Management and Administration

The task of coordinating the actions of individuals with different skills, interests, and experiences is one whose significance only emerged in the mid-nineteenth century, due to the expansion of both private corporations and the state in those years. Of course, hierarchical organizations have existed for centuries, but there was something new about management in the second half of the nineteenth century.

According to business historian Alfred Chandler, the creation of modern corporate management was a response to the particular challenges faced by nineteenth-century railroad companies. Day-to-day rail operations required sophisticated forms of organization in order to communicate across large distances and across the many functional divisions in the company. High-profile railroad accidents vividly demonstrated the costs to life and property of inadequate organization, and the responsibility for designing safe, robust systems fell to salaried engineers.

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[4] Agar, *The Government Machine* contains an excellent analysis of how this division mapped onto class distinctions in England. Studies of trust in experts across different cultures (e.g., Sheila Jasanoff, *Designs on Nature: Science and Democracy in Europe and the United States* (Princeton: Princeton University Press, 2006), and Theodore Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton: Princeton University Press, 1994)) suggest a significantly different basis for trust in the American context, and in particular, a suspicion of claims to "generalist" expertise entirely.

They clarified the lines of communication within the organization to minimize contradictory instructions, developed distribution systems for the downward flow of information and reporting mechanisms for the upward flow of information, and also developed accounting systems to analyze business performance and communicate this information to potential investors. This last element was especially important given the large amounts of capital required to lay tracks and run trains. Thus the primary managerial innovations did not come from corporate owners, but from salaried employees with backgrounds in engineering. Market coordination by the "invisible hand" of free competition among small firms proved less successful than coordination via the "visible hand" of management within large firms. Yet this "visible hand" was created through a sequence of ad hoc, spontaneous innovations, rather than emerging through the implementation of some pre-existing intellectual blueprint.[5]

In addition to changing the way that information traveled within a company, these early managers also greatly increased the quantity of that information. During the first half of the nineteenth century, such work had been done on an ad hoc basis, either by the owner/manager, or by a small number of clerks who were responsible for all such operations. Accounting was almost always strictly for internal purposes, and written correspondence was reserved for situations where face-to-face contact was impossible. The mid-century creation of the back office established an occupational category dedicated to the production of systematic communications and control processes—a form of work that was not specific to the company's mission but was generally applicable to all similar firms. The creation of an organized administration simultaneously created the clerical worker; the human elements of the organization were created alongside the technologies of the office and the new genres of office communication.[6]

Observers at the turn of the century remained fully aware that the railroad and the telegraph had substantially changed the experience of space in the United States and made the country a decidedly smaller place.[7] Yet the administrative apparatuses that kept these

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[5] Alfred D. Chandler Jr., *The Visible Hand: The Managerial Revolution in American Business* (Cambridge, Mass.: Belknap Press, 1977). Note that these modern managerial practices existed before any coherent idea of "management" itself. On the existence of administration as a historically specific form of work, see Alfred D. Chandler Jr., *Strategy and Structure: Chapters in the History of the Industrial Enterprise* (Cambridge, Mass.: MIT Press, 1962), 8.

[6] An important analysis of developments in communications in this period is James R. Beniger, *The Control Revolution: Technological and Economic Origins of the Information Age* (Cambridge, Mass.: Harvard University Press, 1986). Beniger uses the modern (i.e., post-Claude Shannon) definition of 'information' to unify seemingly disparate strands of this era within an overarching history of ever-increasing information processing capabilities from the evolution of microbes to modern communications technology. Yet this technique imposes a unity that did not exist for the actors themselves until the equivalence of information, organization, and communication was claimed in the mid-twentieth century. JoAnne Yates fuses Beniger's insights to Chandler's institutional analysis in *Control Through Communication: The Rise of System in American Management* (Baltimore: Johns Hopkins University Press, 1989). Sharon Hartman Strom's *Beyond the Typewriter: Gender, Class, and the Origins of Modern American Office Work, 1900-1930* (Urbana, Ill.: University of Illinois Press, 1992) is also very insightful.

[7] For example, see Stephen Kern, *Culture of Time and Space, 1880-1918* (Cambridge, Mass.: Harvard University Press, 2003); Leo Marx, *The Machine in the Garden: Technology and the Pastoral Ideal in America* (New York: Oxford University Press, 2000); and Wolfgang Schivelbusch, *The Railway Journey: The Industrialization of Time and Space in the 19th Century* (Berkeley: University of California Press, 1986).

technologies running were invisible to their users. The new corporations reached across the country using networks of steel and telegraph cable, but they were supported by legions of clerks.

Businesses were not the only ones facing organizational challenges around the turn of the century. The federal government had also grown substantially in the late 19th century, and only began to operate on a truly national level in the early years of the 20th century.[8] Reformers wanted to create a permanent civil service in order to sever executive agencies from the spoils system and end the corruption that came from administrators remaining beholden to their patrons. Yet the spoilsmen, cynically or not, could tie that system to the principle of electoral democracy. They argued that the idea of a permanent civil service meant the creation of an entrenched, unaccountable body of experts who would not be responsive to the public will. The idea of administrative reform and the expansion of a career civil service carried more than a hint of European-style centralized government, rendering it suspect to 19th century democrats.[9]

A solution to this problem was to distinguish "politics" from "administration." Writing about political administration in 1887, Woodrow Wilson described the necessity of importing administrative techniques from European states and refining them for the American form of decentralized government. He contrasted an Anglo-American penchant for legislation and limits to executive power against a Continental tendency for well-administered centralized states. The new responsibilities that the state was taking on required more than just good laws; the American government needed expanded administrative capacities and a strengthened executive. Political questions and administrative questions had to be addressed separately.[10]

In analyzing this distinction, Wilson noted that administrators are not mere instruments or unthinking executors of legislation—a misconception based in an American tendency to focus on the question of *who* makes the rules rather than *how* they are implemented. On the contrary, administrators must necessarily make decisions in the course of doing their work. The distinction between politics and administration became one of determining general ends versus specific means.[11] The science of administration, while born from the particular conditions of centralized European states, was built upon basic principles whose truth and effectiveness could hold across cultures as easily as those of physics or other natural sciences. Wilson firmly believed that similar administrative capabilities could be implemented within the American political system, with its democratic ideals and heterogeneous population. Political decisions remained the domain of elected officials and voters, but administration was a specialized skill for implementing political decisions, regardless of what they were or how they were chosen.

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[8] However, as Paul Starr notes, one part of the government operated on a continental scale for much longer: the United States Postal Service, which, not coincidentally, was the central organ of communication in the nation. See Paul Starr, *The Creation of the Media: Political Origins of Modern Communications* (New York: Basic Books, 2004), 15.

[9] Stephen Skowronek, *Building a New American State: The Expansion of National Administrative Capacities, 1877-1920* (New York: Cambridge University Press, 1982).

[10] Woodrow Wilson, "The Study of Administration," *Political Science Quarterly* 2 (1887): 197-222.

[11] Ibid., 212.

It is important to keep in mind that these administrative reforms were not originally intended as pragmatic or technocratic attempts to improve the efficiency of government programs. Reform was first of all a moral issue, and only gradually did it become one of efficiency.[12] Meritocratic administration was salvaged by characterizing administration as an almost mechanical device for enacting the agendas of elected figures.

The distinction between administration and politics was reinforced by the political theorist Frank Goodnow. In 1900 Goodnow described the two main tasks of government as politics, the part that "has to do with policies or expressions of the state will," and administration, which "has to do with the execution of these policies." But he also acknowledged that in practice the two tasks would necessarily bleed into each other; political decisions had to consider their own implementation, while administrative decisions would necessarily touch upon sensitive political questions. Goodnow was most interested the interrelatedness of administrative and political work, and his primary concern was to understand what methods might insure a harmony between the two state functions.[13]

While the first large corporations in the mid-19th century had pioneered the practice of management, the theory of administration as the realm of the executive came from political theorists in the late 19th century who needed to create and defend an administrative apparatus that was free from political control while still remaining responsive to political will. The idea of administration at the turn of the century was self-consciously a hybrid of business and government; as Woodrow Wilson put it, "the field of administration is a field of business."[14]

The Progressive Era was marked by the growing strength of the professional middle classes who were now fighting to gain control of implementing these reforms. Their specialized knowledge and professional identities were to guarantee their objectivity, signifying loyalty to administrative concerns rather than to political parties or patrons. The expansion of education was to create the path into this new administrative elite.[15] The federal government, which until now had not been much of a presence in everyday life, quickly gained unprecedented responsibilities. Reformers simultaneously expanded their responsibilities and their ability to learn about the health of the state through statistics, surveys, and other such tools.[16] In the late

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[12] Dwight Waldo, *The Administrative State: A Study of the Political Theory of American Public Administration* (New York: The Ronald Press Company, 1948), 28.

[13] Frank J. Goodnow, *Politics and Administration* (New Brunswick, NJ: Transaction Publishers, 2003), 18.

[14] Wilson, 209.

[15] A classic treatment on Progressivism is Robert H. Wiebe, *The Search for Order, 1877-1920* (New York: Hill and Wang, 1967). On professions, see Andrew Abbott, *The System of Professions: An Essay on the Expert Division of Labor* (Chicago: University of Chicago Press, 1988) and Nathan O. Hatch, ed., *The Professions in American History* (Notre Dame, Ind.: University of Notre Dame Press, 1988), especially "The Profession of Government Service" by Don K. Price, 163-180, and "The Profession of Management in the United States" by Harold C. Livesay, 199-220. For more on Progressive Era culture, see Michael McGerr, *A Fierce Discontent: The Rise and Fall of the Progressive Movement, 1870-1920* (New York: Free Press, 2003).

[16] See Sarah Igo, *The Averaged American: Surveys, Citizens, and the Making of a Mass Public* (Cambridge, Mass.: Harvard University Press, 2007).

1940s, Dwight Waldo observed that through this Progressive emphasis on expert knowledge, "'management' or 'administration' thus becomes a thing-in-itself, a recognizable field of inquiry and expertise, 'a function that may be observed objectively and subjected to critical analysis.' It becomes a 'science.'"[17] Informal or ad hoc methods of coordinating individual efforts no longer seemed to be sufficient to coordinate the internal operations of large firms, or government programs.[18] Likewise, with specialized knowledge being the mark of middle-class expertise, and with professional identity guaranteeing one's commitment to the public interest, a specialized form of managerial knowledge secured the social position of middle-class salaried managers.

Yet the creation of management as a specialized profession confronted an older, genteel notion of business. The first graduate program in management began at Harvard University in 1908, intended to provide a structured education for managers. This was part of an expansion of professional education under the leadership of the university's president, Charles W. Eliot. Eliot and the other founding members of the business school, including economics professors Edwin Gay and Frank Taussig, and government professor A. Lawrence Lowell, viewed the growing wealth and power of Gilded Age robber barons with concern. Their vision of professional management education had a decidedly moral component, in that it sought to restore genteel manners and order to the vulgar business practices of the robber barons—and was also influenced by Eliot's dawning recognition that even Harvard graduates were entering the business world in growing numbers. A concentration in public administration at the business school was scrapped at the insistence of Lowell (Eliot's successor as university president in 1910), who believed that America did not need a permanent career civil service.[19]

For its first decade, the business school focused on teaching specific business skills, such as banking and accounting, and the general principles of Taylorism, though Frederick Taylor's belief in the importance of engineering education as preparation for management conflicted with

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[17] Waldo, *The Administrative State*, 56.

[18] The significance of this period was emphasized by a group of historians identified by Louis Galambos as contributing to an "organizational synthesis" of modern American history. Galambos further identifies the institutional economists of the 1960s (described later in this dissertation) as being among the primary influences upon these historians. Using the insights of the organizational synthesis to analyze institutional social scientists raises the question of whether these organizational challenges identified by Chandler, Wiebe, et al. in the 1960s and 70s led to the social sciences of the early 20th century, or whether these social sciences created the conditions for mid-century historians to understand the late 19th century in these terms. Chandler, for example, had studied under Talcott Parsons and had been steeped in the industrial theories of Frederick Taylor, Chester Barnard, and Herbert Simon, as well as the sociology of Max Weber. Chandler's powerful analysis of the late 19th and early 20th centuries as a period of corporate rationalization has established the historiography of that era on a foundation of Parsonian functionalism. See Stephen P. Waring, *Taylorism Transformed: Scientific Management Since 1945* (Chapel Hill, NC: University of North Carolina Press, 1991), 4. This dissertation seeks to critically engage with the historiography in order to question the cyclicality inherent in this process. For an overview of the organizational synthesis, see Galambos, "The Emerging Organizational Synthesis in Modern American History," *The Business History Review* 44 (1970): 279-290; and idem., "Technology, Political Economy, and Professionalization: Central Themes of the Organizational Synthesis," *The Business History Review* 57 (1983): 471-493.

[19] Robert Edwards Gleeson, "The Rise of Graduate Management Education, 1908-1970" (Ph.D. dissertation, Carnegie Mellon University, 1997), 22.

Eliot's equally strong conviction that a humanistic education was necessary.[20] From its beginning, the school tried to balance theory with applications. In 1919, Wallace B. Donham, the second dean of the school, began promoting the expanded use of the case method to demonstrate that the intellectual foundations of business management rested upon a unique form of professional judgment, rather than being merely applied economics. Donham's outlook was shaped less by the Progressive attack on robber barons and defense of education than by the belief (common to Republicans of the 1920s) that business leaders constituted a distinct managerial class and a natural social elite.[21]

By the late 1940s, however, certain engineering schools led by the Carnegie Institute of Technology and the Massachusetts Institute of Technology believed that there was a need for a new type of management education. The engineering schools' version of managerial education would renew the commitment to a *scientific* theory of management, as compared to the case method of Harvard and its peers. These new programs would use the social science of the first half of the century to develop rigorous theories of individual behavior within organizations, while also using a half-century of innovation in industrial process engineering to re-conceptualize the operations of firms. This appeal to a scientific form of management was a way of capitalizing on the authority of post-war science. Yet the idea of a scientific approach to management did also reflect a genuine faith that systematic inquiry into the operations of organizations would provide a fundamentally new way of thinking about administration. It is not surprising that the "New Look" of 1940s administrative science was based in engineering schools; engineers had figured prominently in theories of industrial organization in the first half of the twentieth century. The managerial innovations of the first half of the century were inseparable from the technologies of administration.[22]

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[20] Herbert Heaton, *A Scholar in Action: Edwin F. Gay* (Cambridge, Mass.: Harvard University Press, 1954), 73, and Daniel Nelson, *Frederick W. Taylor and the Rise of Scientific Management* (Madison, Wisc.: University of Wisconsin Press, 1980), 74-75. The larger point here is that a tension existed between competing sources of professional authority. Did the expert's authority come from absorbing knowledge and acting wisely as a result of this learning, or did it come from developing rigorous techniques that could be applied impersonally by a cadre of trained experts? Put another way: was expertise a function of learning, or of training? These issues will be revisited in the context of artificial intelligence in chapter 4.

[21] Gleeson, "The Rise of Graduate Management Education, 1908-1970," 12-45; William G. Scott, *Chester I. Barnard and the Guardians of the Managerial State* (Lawrence, Kans.: University Press of Kansas, 1992). A good overview of debates about political economy in the 1920s is Ellis Hawley, *The Great War and the Search for a Modern Order: A History of the American People and their Institutions, 1917-1933* (New York: St. Martin's Press, 1992).

[22] Daniel Nelson, "Scientific Management and the Transformation of University Business Education," in *A Mental Revolution: Scientific Management Since Taylor*, ed. Daniel Nelson (Columbus, Ohio: Ohio State University Press, 1992), 77-101; David F. Noble, *America by Design: Science, Technology, and the Rise of Corporate Capitalism* (New York: Alfred A. Knopf, 1977). The relationship between engineering education and business has been a point of contention among historians. For a nuanced study, see Christophe Lécuyer, "MIT, Progressive reform, and "industrial service," 1890-1920," *Historical Studies in the Physical and Biological Sciences* 26 (1995): 35-88.

Engineering Managerial Authority

By the end of the 19th century, managers had hit upon a unified framework for management, described by business historian Joseph Litterer as "systematic management." The general idea was to create administrative mechanisms to guarantee that work would be done correctly, on time, and with minimal waste. They accomplished this by creating standard channels for communicating information and standard processes for creating written records.[23] Systematic management was a broad movement, but it soon was dominated by the ideas of Frederick W. Taylor.[24]

Taylor was born in 1856 to a wealthy family in Philadelphia. Forced by poor health to turn down an education at Harvard, he joined Midvale Steel in 1878 while studying engineering at the Stevens Institute of Technology. At Midvale he began extensive studies of metal-cutting to devise the most efficient processes for working steel. These studies of metal cutting were important in their own right, but Taylor then began to apply his analytical techniques to study the physical work done by his employees. While previous management theories had been limited to defining lines of control and communication among employees, Taylor turned management into a system to directly control the pace and conditions of work itself.[25]

The first components of the system that would become known as Taylorism went virtually unnoticed. Taylor's motivation in adjusting work conditions was not at first about economizing. Much like the 19th century administrative reformers, he understood his work as a moral crusade against "soldiering," the tendency for workers to perform at less than full speed. Armed with a stopwatch, Taylor began measuring the rate at which his employees worked in 1882 and tried without success to simply order his workers to produce more. Yet his employees did not want to relinquish control over the conditions of their work. They banded together against the hard-driving Taylor. The workers formed a cohesive unit, earning similar wages and working under the same conditions, and anyone who either shirked or over-performed had to answer to his colleagues. Taylor hit upon the idea of offering variable wages, in which the per-unit wage increased substantially above a certain threshold of units produced. In this system workers would earn more money per day while still costing less per unit of output. This piece-rate wage system violated the understanding of fair wages, in which all workers doing similar work would earn the same wages, and therefore aroused significant opposition. Piece-rate wages set workers against each other.[26] Yet the significance of this wage control remained obscure to

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[23] Joseph A. Litterer, "Systematic Management: The Search for Order and Integration," *The Business History Review* 35 (1961): 461-476, and idem., "Systematic Management: Design for Organizational Recoupling in American Manufacturing Firms," *The Business History Review* 37 (1963): 369-391; Yates, *Control Through Communication*, 9-10.

[24] The literature on Taylorism is vast and still growing. What follows here focuses on one facet of Taylorism: the ability of the systems perspective to transcend binary class conflict.

[25] An engaging biography of Taylor is Robert Kanigel, *The One Best Way: Frederick Winslow Taylor and the Enigma of Efficiency* (New York: Viking, 1997).

[26] David Montgomery, *Workers' Control in America: Studies in the History of Work, Technology, and Labor Struggles* (New York: Cambridge University Press, 1979), 122-123.

other managers at first. Taylor presented the idea at the summer meeting of the American Society of Mechanical Engineers (ASME) in 1895, in a talk entitled "A Piece-Rate System, Being a Step toward a Partial Solution of the Labor Problem."[27] It received little attention.

The production levels set by piece-rate usually exceeded established practice. Taylor justified these levels through a systematic analysis of the work itself—the time studies that made the stopwatch the symbol of Taylorism. He carefully recorded the rate at which a worker could do a given task and used that as a baseline to extrapolate what a worker should achieve throughout a workday, given the proper incentives and time for rest. In *The Principles of Scientific Management*, published in 1911, Taylor demonstrated this principle with the example of a laborer known to posterity as "Schmidt." In his retelling, Taylor describes Schmidt as simple-minded but hard-working, willing to follow directions and carry greater amounts of pig iron in return for higher wages. He observed that

> in almost all of the mechanic arts the science which underlies each act of each workman is so great and amounts to so much that the workman who is best suited to actually doing the work is incapable of fully understanding this science, without the guidance and help of those who are working with him or over him, either through lack of education or through insufficient mental capacity. In order that the work may be done in accordance with scientific laws, it is necessary that there shall be a far more equal division of the responsibilities between the management and the workmen than exists under any of the ordinary types of management. Those in the management whose duty it is to develop this science should also guide and help the workman in working under it, and should assume a much larger share of the responsibility for results than under usual conditions is assumed by the management.[28]

Taylor denied that this system was based on any desire to exploit workers; he argued that his enlightened managerial practices would create the proper incentive for work, and would "give the workman what he most wants—high wages—and the employer what he wants—a low labor cost—for his manufactures."[29] As Taylor understood it, labor conflict was a result of workers allowing their immediate interests to overcome the social interest in cheap production. Piece-rate wages established by time studies would generate economic efficiency and thereby align the interests of management, labor, and the public at large. Arguing that consumers pay the wages of the workmen and the profits of the owner, Taylor claimed that "the rights of the people are therefore greater than those of either employer or employee. And this third great party should be

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[27] Samuel Haber, *Efficiency and Uplift: Scientific Management in the Progressive Era, 1890-1920* (Chicago: University of Chicago Press, 1964).

[28] Frederick Winslow Taylor, *The Principles of Scientific Management* (New York: Cosimo, 2006), 9-10.

[29] Ibid., 1.

given its proper share of any gain."[30] This seemed to be a reasonable compromise to him. It was received with less enthusiasm by his workers.

Taylor's system required that tools fit the task at hand. For example, having established an optimal load for workers to carry (21.5 pounds), he then selected different shovel designs for different materials so that the load per shovel remained constant regardless of the material. The system also required that employees be selected to fit the job at hand. In four years at Bethlehem Steel, Taylor cut the costs of handling steel in half by firing over 75% of its workers.[31]

While Scientific Management is most closely associated with Frederick Taylor, he had several colleagues in his analyses of work. Two of the most prominent were Frank and Lillian Gilbreth, who augmented Taylor's time studies with photographed motion studies. Frank Gilbreth then broke these complex motions down into a set of constituent basic motions, which he modestly named "therbligs." Gilbreth and Taylor had a difficult relationship, but time-and-motion studies became the most recognizable component of Scientific Management. Lillian Gilbreth, a psychologist, helped implement these management techniques.[32] Other associates included the Norwegian engineer Carl Barth ("even more of a Taylorite than Taylor," according to his son), who did statistics for Taylor; Henry Gantt, Taylor's assistant at Midvale, who ventured into politics with his "New Machine" movement; and Morris L. Cooke, a strong advocate of professional responsibility among engineers.[33]

Taylor and his disciples connected engineering and business management by treating the operations of human workers as analogous to the operations of mechanical components. Both types of operations could be broken down, systematically analyzed, and then reconstructed into a more efficient process. Responsibility for designing the work process was distinguished from the responsibility for executing the work process. Taylor often repeated that Scientific Management was not just a collection of techniques, but required an entirely new way of thinking about the practice of management.[34] Scientific Management was based upon the belief that the analysis of work was too complex for workers to handle; managers had to take responsibility for organizing work and workers then had to carry out their instructions. It removed discretion for the *how* of work from workers themselves, and consolidated it on the planning side. The resulting efficiency would, in theory, create an economy of abundance rather than one of scarcity, transforming

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[30] Taylor, 71. A provocative analysis of the growth of consumerism in this period is James Livingston, *Pragmatism and the Political Economy of Cultural Revolution, 1850-1940* (Chapel Hill, NC: University of North Carolina Press, 1994).

[31] Beniger, 295.

[32] Brian Price, "Frank and Lillian Gilbreth and the Motion Study Controversy, 1907-1930," in Nelson, ed., *A Mental Revolution*, 58-76. Frank Gilbreth, Jr. and Ernestine Gilbreth Carey wrote a popular memoir about the family, *Cheaper by the Dozen*. The book was adapted as a movie in 1950, earning a congratulatory letter from Norbert Wiener for its accurate portrayal of efficiency experts. See letter to Clifton Webb, 4/20/1950, Norbert Wiener Papers, MIT Archives and Special Collections, MC 22, box 8, folder "April 16-30, 1950."

[33] Kanigel, 332.

[34] Taylor, 67-68.

antagonistic relationships among management, labor, and the public into ones of shared interests, thereby quelling labor disputes and social unrest.

Taylor misunderstood his workers in assuming that they only wanted higher wages, a shortcoming that even his supporters acknowledged. Many workers most resented scientific management for taking away their ability to set their own work patterns.[35] At a labor meeting in Wisconsin, organizer Nels Alifas told Taylor that "the people of the United States have a right to say we want to work only so fast. We don't want to work as fast as we are able to. We want to work as fast as we think it's comfortable for us to work."[36] Yet what appeared to workers to be a straightforward desire to control their own working conditions appeared to other audiences to be an unacceptable promotion of narrow interests. Scientific management attracted widespread public interest in the 1910s and 1920s after Louis Brandeis, later a Supreme Court Justice, declared in the Eastern Rate Case of 1910 that scientific management could save the railroads $1 million per day, obviating their proposed rate hike. Brandeis presented scientific standards of efficiency as a way of protecting the public interest from the greed of both railroad managers and the unions. Taylor wrote Brandeis in admiration of his ability to garner support for the efficiency movement. "I have rarely seen a new movement started with such great momentum as you have given this one," he declared.[37] Muckraker Ida Tarbell declared in 1924 that "no man in the history of American industry has made a larger contribution to genuine cooperation and juster human relations than did Frederick Winslow Taylor.... He is one of the few—very few—creative geniuses of our time."[38]

Taylorism expanded the possibilities for efficient management by making a rigid distinction between the work of thinking and the work of doing. Scientific managers extended the division of labor into a regime where responsibility for planning remained fully distinct from responsibility for executing orders. At the same time, Henry Ford's factories integrated human work with mechanical work in his system of mass production. Ford used standardized and interchangeable parts, part of the "American System" of manufacture. By eliminating the variability of parts among different machines, managers therefore eliminated the job of the fitter, who made heterogeneous parts fit into a complete whole. Interchangeable parts and standardization moved the work of fitting into the design of the parts themselves. Instead of producing parts individually for a particular final product, or having to handle each piece using different techniques, standardization reduced the necessary number of tools and skills.[39]

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[35] See Montgomery, *Workers' Control in America*, and Livesay, "The Profession of Management in the United States," in *The Professions in American History*, ed. Nathan O. Hatch. For an important case of resentment against external efficiency experts telling workers how to do their jobs, see Hugh G. J. Aitken, *Taylorism at Watertown Arsenal: Scientific Management in Action, 1908-1915* (Cambridge, Mass.: Harvard University Press, 1960).

[36] Quoted in Kanigel, 520.

[37] See Daniel Nelson, *Frederick W. Taylor and the Rise of Scientific Management* (Madison: University of Wisconsin Press, 1980), 175.

[38] Quoted in Strom, *Beyond the Typewriter*, 127.

[39] David Hounshell, *From the American System to Mass Production, 1800-1932* (Baltimore: Johns Hopkins University Press, 1984).

Ford maintained that the assembly line did not eliminate skilled work, but instead created new forms of work, in planning and management, that required even greater amounts of skill. At the same time, he did observe that these skilled jobs constituted an ever-smaller proportion of the factory workforce. By 1924, "the remaining 95 per cent [of jobs] are unskilled, or to put it more accurately, must be skilled in exactly one operation which the most stupid man can learn within two days."[40] In Ford's factory, work could be broken up into its constituent parts and linked together with an assembly line. If the constituent steps were simple enough, they could be performed by machines. This was limited only to the simplest kind of mechanical operations, but in the long run such capital investments were far cheaper than paying wages indefinitely. Thus a link was forged between technological development and unemployment.[41]

In Ford's factories, the assembly line both set the pace for workers and eliminated the need for workers to physically move materials through the construction process. The inspiration for the assembly line, introduced in 1913, was the "disassembly line" of Chicago meatpackers.[42] With the assembly line, Ford recognized that his factories did not need to all produce entire automobiles; divisions within the company could create individual pieces that could then be fully assembled into complete automobiles later. Unlike the process of vertical integration, in which a corporation would purchase companies that supplied it with components, Ford's factories developed the other way: factories that created cars from scratch developed into a system of specialized units which would focus on individual components. While Ford's system was often compared to Taylor's, the two remained distinct.[43] Taylorism required analyzing workers to carefully identify their capabilities and aptitudes, and then tailoring their work accordingly. Fordism recognized that physical labor was another component of an industrial process to be standardized, and hired on the sole basis of whether or not the employee could do the work. Ford therefore recognized that the stripped-down tasks reserved for his factory workers did not, on the whole, require any great physical talents. Several of the remaining processes could be done by men missing arms or legs. And so they were.

Despite (or, more likely, because of) this concern for efficiency, turnover at Ford was very high, which made hiring very inefficient indeed. Ford's solution was to pay his employees far more than any of his competitors - the famous "$5 day" (which required investigations into

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[40] Henry Ford with Samuel Crowther, *My Life and Work* (Garden City, NY: Doubleday, Page & Co., 1924), 87.

[41] Amy Sue Bix, *Inventing Ourselves Out of Jobs? America's Debate Over Technological Unemployment, 1929-1981* (Baltimore: Johns Hopkins University Press, 2000).

[42] Ford, 81.

[43] A useful analysis of Taylor and Ford is in Hounshell, *From the American System to Mass Production*, 252-253. However, he makes the distinction between "Taylorism" and "Fordism" too sharp. The two systems shared several fundamental characteristics in regards to production itself. Their differences were in what industrial production meant for society at large. According to David Harvey, the crucial difference was that Ford recognized "that mass production meant mass consumption, a new system of the reproduction of labor power, a new politics of labor control and management, a new aesthetics and psychology, in short, a new kind of rationalized, modernist, and populist democratic society." See David Harvey, *The Condition of Postmodernity: An Enquiry into the Origins of Cultural Change* (London: Blackwell, 1989), 125-126. Taylor also extrapolated social lessons from the factory, though primarily in terms of expanding the role of expert leadership—in contrast both to Ford's democratic consumerism and the genteel elitism of traditional management.

the living conditions and habits of the workers to confirm their character before they could qualify—empowering workers as consumers meant confirming that they would make proper decisions with their money).[44] Unlike Taylor, who had antagonized workers in his quest to encourage higher productivity through piece-work wages and workplace competition, Ford set a standardized, high wage in order to retain employees. The major innovation of Taylorism was to treat personnel as movable parts to be configured in terms of industrial engineering, while the Fordist innovation was to standardize both human and physical inputs. Despite Ford's innovations on the assembly line, the company's management structure remained old-fashioned, using few of the modern developments in accounting and marketing. The rigidity of the company very likely led to its failures to compete adequately against Alfred Sloan's General Motors in the late 1920s and '30s.[45]

Office Work and Scientific Management

Administration began as a form of support work designed to improve the efficiency of the physical work that defined the 19th-century organization. Yet the quantity of administrative work grew substantially in the early part of the twentieth century, eventually changing the significance of administrative work for organizational goals. As corporations grew horizontally and vertically, the amount of information that a given company handled increased dramatically, and as these corporations were restructured, they created different patterns of communication. Workers within corporations often had to communicate across geographical space or functional divisions. While these had been handled earlier through personal relationships, the corporation of the early twentieth century was becoming large enough to make most such relationships impersonal. JoAnne Yates describes the new genres of communication pioneered by large corporations to structure communication in the absence of personal relationships. Instructions traveled downward in the form of circulars and training manuals (and magazines for employees as corporate welfare), while companies standardized the formats of reports traveling up the chain. Reports employed more graphs and tables and became much less verbose, eliminating the pleasantries of nineteenth century intra-office interactions with more bureaucratic formality.[46]

Clerks stayed in control of the greater quantity of documents circulating within the office by using new office technologies. These technologies allowed clerks to produce, reproduce, distribute, and store documents within a firm. As the technologies spread throughout American firms, they simultaneously created new groups of users and new forms of managerial communication. Typewriters, for example, became commercially available in the early 1870s, but exploded in number in the 1880s as firms realized that typewriting could be done at a far greater speed than handwriting, and as the gradual creation of typing courses simultaneously

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[44] Hounshell, 256-259.

[45] Thomas P. Hughes, *American Genesis: A Century of Invention and Technological Enthusiasm, 1870-1970* (Chicago: University of Chicago Press, 2004), 210-218.

[46] Yates, *Control Through Communication*, 95-97.

created skilled typists.[47] Most managers had written their own letters prior to the 1880s, though the creation of secretarial pools separated the function of drafting documents from that of producing finished ones. Dedicated secretarial pools not only sped up the production of documents, they also made the reading of documents faster by improving legibility and standardizing the formats of these documents.[48]

Several other office machines contributed to the work of the back office: simple mechanical calculators made mathematical work faster and more accurate, while cash registers made accounting that much easier. Perhaps the most high-tech artifact of late nineteenth century office machinery was Herman Hollerith's Electric Tabulating System, which read data stored on punched cards. Hollerith had been inspired by the control systems employed by the railroad. Train tickets, for example, contained a series of hole punches to indicate a passenger's identifying characteristics as well as all information about his voyage, while the signaling and switching systems along the railroad used a sequence of discrete events to determine a train's path. In Hollerith's tabulating system, a series of holes in cards marked data, and then the tabulating machine rapidly read the pattern of holes and performed simple mathematical operations accordingly.[49]

Hollerith's tabulating system was remarkably successful in speeding up the 1890 census. The 1880 census had run into trouble after its director, Francis Amasa Walker, collected so much information about the American populace and decided to generate such detailed statistics that it completely overwhelmed the capabilities of the Census Bureau. The census reports threatened to be obsolete before they could be completed. In desperation, the Secretary of the Interior decided in 1889 that the next census would require the Hollerith system. This demonstration of the technology by one of the federal government's most important statistical offices helped ensure the company's future success.[50]

Documents often needed to go to multiple recipients, and so point-of-origin duplication technologies also gained popularity during the same years, with carbon paper becoming the most common method after the publication of the report of President Taft's Commission on Economy and Efficiency in 1912.[51] Information retrieval also became important to the new firms of the late

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[47] To get a sense of the growth of the numbers involved, 15,000 typewriters were produced in 1886. In 1900, the number was ten times as high. Yates, *Control Through Communication*, 41-44.

[48] For a social history of the women working in these jobs, see Strom, *Beyond the Typewriter*, 273-415.

[49] For a description of how punched-card machines work, see Martin Campbell-Kelly, "Punched-Card Machinery," in *Computing Before Computers*, ed. William Aspray (Ames, Iowa: Iowa State University Press, 1990), 122-155.

[50] Beniger, 408-411. Hollerith's company later became IBM. For figures on the financial health of office machine companies, see James W. Cortada, *Before the Computer: IBM, NCR, Burroughs, Remington Rand, and the Industry That They Created* (Princeton: Princeton University Press, 1993). The 1890 census famously declared that the frontier had closed, even as it opened up new possibilities for government statistics on these new tabulating machines.

[51] Yates, *Control Through Communication*, 48-49. Administrative theorist Frank Goodnow, mentioned above, was a member of the commission.

nineteenth century. The system of vertical files, created in 1893 and modeled after library card catalogs, allowed for easy search and could be organized according to the needs of the firm.[52]

This variety of office machines and new office professionals allowed for more complicated and sophisticated forms of office work. The expansion of scientific management in industry required the application of scientific management to office work. The most prominent analyst of scientific management in the office was William Leffingwell, whose text *Scientific Office Management* discussed the physical layout of the office and strategies for keeping a clean desktop, as well as step-by-step instructions for such tasks as opening envelopes or sharpening pencils.[53] Not only had the back office been created within companies in order to produce, distribute, and file documents, but the actual labor within the office was also rationalized through the application of these machines, which standardized and routinized the practices of information management. The creation of these functional specialties among office workers was another form of the division of labor.

This was also a division of labor marked by gender. Gradations of authority within the office corresponded to this division by gender. Women's work remained limited to the clerical pools, where they remained largely interchangeable. The clerical profession had quickly become identified as feminine, even though prior to the 1880s clerks were almost all men. Sharon Strom identifies the 1890 census as a turning point; male employees resented the regimentation of its factory-like data processing, while women were eager for the opportunity to work and quickly gained a monopoly in expertise with office machines.[54] The gendering of clerical work as intrinsically feminine employed biological arguments about greater manual dexterity among women, as well as a suggestion that the skills of home economics provided an appropriate training for maintaining the health of the office. High status office jobs, such as that of the personal secretary, remained a masculine domain until the 20th century. Of the many jobs within the office, the work of the secretary was the least affected by specialization through scientific management. The varied work of the secretary, and the job's proximity to the boss, meant that it was originally deemed unsuitable for women's judgment. Secretarial work was eventually absorbed within the sphere of women's work through its requirement of deference to the boss. All of these office jobs involved being in a subservient position, but the job of the personal secretary was considered a higher form of work by virtue of it retaining some personal autonomy and a greater variety of tasks to perform.[55]

Both scientific management and the assembly line model strongly influenced the development of office work in the first half of the century. While the theories of scientific

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[52] Ibid., 63.

[53] William Henry Leffingwell, *Scientific Office Management* (New York: A. W. Shaw, 1917). For a detailed study of the technologies and practices of the information-rich life insurance industry, see JoAnne Yates, *Structuring the Information Age: Life Insurance and Technology in the Twentieth Century* (Baltimore: Johns Hopkins University Press, 2005).

[54] Strom, *Beyond the Typewriter*, 178.

[55] Margery W. Davies, *Woman's Place Is at the Typewriter: Office Work and Office Workers, 1870-1930* (Philadelphia: Temple University Press, 1982), 129-162.

management had been concerned with separating the tasks of thinking and doing, the growth of the office led to specialization and organization of work here too. Office work became inseparable from office tools, and office workers recognized that they too were subject to the logic of the assembly line.

Expanding the System

Taylorism provided the intellectual glue for the professionalization of engineers; the principles of scientific management corresponded with the traditional conceits of engineers in terms of valuing efficiency. Taylorism imbued mechanical and commercial efficiency with a sense of moral purpose.[56] The general concept of Taylorism encompassed a variety of fundamentally divergent beliefs concerning the purpose of engineering. Yet engineers of all political and social persuasions believed that a social vision unique to engineering was worth defining. Engineering would design individual incentives in order to transcend class divisions in modern society. Thomas Hughes has characterized this period as influenced by the construction of large technological systems that integrated technological and human elements.[57]

While many engineers were content to pursue the interests of the corporations that hired them, Morris L. Cooke became a vocal advocate of the social responsibilities of the engineering profession, claiming that the engineer's professional identity made him beholden to society as a whole rather than just to his employer. On behalf of the city of Philadelphia, he accused the municipal power utilities of charging excessively high rates and urged reform of the engineering societies, which he believed to have been captured by financial interests.[58]

While Morris Cooke's battles against the hidebound engineering associations and utilities were largely unsuccessful, the notion that the ideas of engineering could coordinate private economic interests to improve public welfare was commonplace around 1920. By far the most prominent advocate of this view was "The Great Engineer," Herbert Hoover, who had been unanimously elected as the first president of the Federated American Engineering Societies in 1920.[59] Hoover claimed that the widespread acceptance of the engineering perspective would result in "a new economic system, based neither on the capitalism of Adam Smith nor upon the

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[56] The connection between economic efficiency, industrial efficiency, and morality is emphasized in Haber, *Efficiency and Uplift*.

[57] Thomas P. Hughes, *American Genesis: A Century of Invention and Technological Enthusiasm, 1870-1970* (Chicago: University of Chicago Press, 2004).

[58] Edwin T. Layton, *The Revolt of the Engineers: Social Responsibility and the American Engineering Profession* (Baltimore: Johns Hopkins University Press, 1986), 154-178. Also see Bruce Sinclair, "Episodes in the History of the American Engineering Profession," in *The Professions in American History*, ed. Nathan O. Hatch.

[59] Layton, 189. The FAES was an umbrella organization for the particular engineering associations.

socialism of Karl Marx."[60] This system has been described as "associationalist" for being built upon trade associations and other voluntarist groups.[61]

Though Hoover distinguished the associationalist vision from both laissez-faire and socialism, the connections between prominent engineers and both business interests and socialists were often very tangled.[62] First of all, many of the engineers came from socialist backgrounds. One of the most prominent engineers of the day was Charles Steinmetz, the director of General Electric's research laboratory in Schenectady, NY and an avowed socialist. He outlined the relevance of the engineering perspective and its relationship to socialism in his 1916 book, *America and the New Epoch*.[63] Steinmetz argued that modern technology itself, in the form of the electric grid, created a powerful impulse toward socialism, whereas the age of steam had emphasized individualism. In an article from 1913, he argued that

> The relation between the steam engine as a source of power and the electric motor is thus about the same as the relation between the individualist and the socialist, using the terms in their broadest sense; the one is independent of everything else, is self-contained, the other, the electric motor, is dependent on every other user in the system. That means, to get the best economy from the electric power, co-ordination of all the industry is necessary, and the electric power is probably today the most powerful force tending towards co-ordination, that is cooperation.[64]

Walter Polakov, an engineer associated with Henry Gantt's New Machine movement, looked forward in 1918 to the widespread application of engineering efficiency: "With production simplified and power utilized to its fullest capacity, we could probably produce all we want in much less than six hours; and with distribution simplified we would have no trouble in securing the product for our own enjoyment." When his interlocutor asked if this was socialism, Polakov answered no, it was engineering.[65]

Yet perhaps the most vocal prophet of engineers as the administrators of the future was Thorstein Veblen in his 1921 book *The Engineers and the Price System*. Veblen observed that

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[60] Herbert Hoover, "Great Areas of Common Concern Between Engineers, Employers, and Employees," *Mining and Metallurgy* 1 (1920): 7.

[61] The fullest description of associationalism remains Hawley, *The Great War and the Search for a Modern Order*. For the significance of associationalism on science and technology policy during the 1920s, see David M. Hart, *Forged Consensus: Science, Technology, and Economic Policy in the United States, 1921-1953* (Princeton, NJ: Princeton University Press, 1998).

[62] William Leuchtenberg identifies several instances where Hoover's praise of free market principles had little resemblance with his actions. See his biography *Herbert Hoover* (New York: Times Books, 2009).

[63] Charles P. Steinmetz, *America and the New Epoch* (New York: Harper and Brothers, 1916). Ronald Kline, *Steinmetz: Engineer and Socialist* (Baltimore: Johns Hopkins University Press, 1992).

[64] Steinmetz, "The Future Development of the Electrical Business," quoted in Kline, *Steinmetz*, 216-217.

[65] Polakov quoted in James M. Jordan, *Machine-Age Ideology: Social Engineering and American Liberalism, 1911-1939* (Chapel Hill, NC: University of North Carolina Press, 1994), 64.

business owners preferred to stifle competition rather than to encourage it and that the logic of capitalist growth was such as to eventually destroy market competition altogether. The end point, with management having replaced competition, required the engineer's system of rational coordination.[66] While this bore a marked similarity to Marx's claim that capitalism contained within itself the contradictions that would lead to its own demise, Veblen was by no means advocating a Marxist dictatorship of the proletariat. His was an avowedly elitist collectivism in which trained efficiency experts organized production to streamline the national economy. Several leading liberals of the 1910s and early 1920s believed that corporate capitalism had already socialized economic production by destroying traditional ideas of ownership, a point made by Walter Lippmann in *Drift and Mastery*.[67] This was a vision of collectivism that had been filtered through the Progressivist faith in a technocratic elite.

The engineers remained critical of laissez-faire, but also remained some of big business's biggest allies. Most engineers were employed by the large corporations of the late nineteenth and early twentieth centuries and understood their jobs as improving the operations of their employers—the situation that Morris Cooke opposed. In general, however, whatever differences of opinion existed among the engineers, they advocated a form of corporatism that favored bigness in business, believing that the efficiencies of scale from integrated operations would be in the public interest.

Meanwhile, the cultural critic Lewis Mumford claimed that conflicts between labor and management, and the mechanization of society, were holdovers of a "paleotechnic" era of coal and steel for which the technical dimensions of industry encouraged the expansion of corporations and set the pace of industrial production. Concurrent with the narrowly technical innovations of the paleotechnic era were the changes in human self-understanding that followed from industrial order. The familiar figure of the industrial man, who ruthlessly economized and saw workers as means to productive ends, was as much an essential component of the paleotechnic era as the ribbons of steel that crossed Europe and America.

Mumford saw a new era of technics on the horizon; the "neotechnic" era, marked by the application of science to society, universal access to electricity, and rapid personal transportation through the automobile, promised a more humane and rational existence. The Mumford of the 1930s was an optimist. While technology had created inhuman industrial regimes, it also had the potential to bring about a new "Eden-like" existence as automated machines continued to eliminate drudgework. Furthermore, achieving the personal freedoms promised by neotechnics would require submitting to a more thorough social organization. Mumford directly compared the functioning of society to industrial processes and identified the need to submit to collective interests.[68]

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[66] John P. Diggins, *The Bard of Savagery: Thorstein Veblen and Modern Social Theory* (New York: Harvester Press, 1978).

[67] Walter Lippmann, *Drift and Mastery: An Attempt to Diagnose the Current Unrest* (Englewood Cliffs, NJ: Prentice-Hall, 1961). See also Howard Brick, *Transcending Capitalism: Visions of a New Society in Modern American Thought* (Ithaca, NY: Cornell University Press, 2006), 43-53, and Noble, *America by Design*, xx.

[68] Lewis Mumford, *Technics and Civilization* (New York: Harcourt, Brace and Company, 1934).

The Sciences of Social Management

Economic planning, or at least the rational organization of the economy, was a popular theme in the interwar years. Intellectuals were also concerned with understanding the relationship between individual action and society at large, particularly at the University of Chicago. The university was founded in 1891, following the new research ideal pioneered at Johns Hopkins, but in Chicago the tendency toward academicism was tempered by its location in the midst of America's second city. The tension between the interests of the individual and society was an organizing principle in several academic departments, from sociology to political science to biology. Administrators and faculty encouraged collaborations across departments, giving the university a unified sense of purpose that few others shared.[69]

Social scientists at Chicago between the two world wars wanted to tie together normative theories that had formed the old core of social analysis with empirical studies. Perhaps the most pressing question was how to understand the process of an individual making decisions within a broader social setting. One of the most creative members of the Chicago faculty was the economist Frank Knight, who began investigating decision making in his 1916 dissertation, published in 1921 as *Risk, Uncertainty, and Profit*. Knight distinguished between risks, events with knowable probabilities, and uncertainties, which remained fundamentally unknowable. It was the existence of this uncertainty that differentiated the real world of market interactions from that of economic theory, and this uncertainty also made necessary the work of the entrepreneur. Indeed, it was precisely the radical unknowability of uncertainties that had allowed robber barons to acquire their fortunes. Mere risks, by being calculable, could be commoditized. The entrepreneur navigated the world of uncertainty through the use of judgment, which remained beyond the bounds of rational calculation.[70] Scientific understandings of the world had brought some uncertainties within the realm of human control—turning them into risks—but Knight noted both the impossibility and undesirability of ever fully eliminating uncertainty. "We should not really prefer to live in a world where everything was 'cut and dried,' which is merely to say that we should not want our activity to be all perfectly rational," he wrote,

> But in attempting to act 'intelligently' we are attempting to secure adaptation, which means foresight, as perfect as possible. There is, as already noted, an element of paradox in conduct which is not to be ignored. We find ourselves compelled to strive after things which in a 'calm, cool hour' we admit we do not want, at least not in fullness and perfection. Perhaps it is the manifest impossibility of reaching the end which makes it interesting to strive after it. In any case we do strive to reduce uncertainty, even though we should not want it eliminated from our lives.[71]

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[69] Martin Bulmer, *The Chicago School of Sociology: Institutionalization, Diversity, and the Rise of Sociological Research* (Chicago: University of Chicago Press, 1984), 190. The idea of "cooperation" as a theme among the biologists at Chicago is explored in Gregg Mitman, *The State of Nature: Ecology, Community, and American Social Thought, 1900-1950* (Chicago: University of Chicago Press, 1992).

[70] Though he did not use the term "tacit knowledge," that concept fits Knight's description well.

[71] Frank H. Knight, *Risk, Uncertainty, and Profit* (Chicago: University of Chicago Press, 1921), 238.

The complete elimination of uncertainty would mean the elimination of judgment and the end of entrepreneurship as an art, as it would have become a risk with calculable costs.

Knight supported a laissez-faire market system on entirely non-consequentialist grounds. In fact, he went so far as to suggest that greater coordination within the market could potentially be more efficient and that the market created excessive and undeserved concentrations of power, for "it is clear that there is no technical (much less moral) equivalence between [having market advantages] and the right to their entire fruits in perpetuity, and to confer it on one's heirs and assigns forever—particularly when we consider the enormous element of pure luck in all operations of this sort."[72] Yet the market was uniquely the home of the entrepreneur, and for that reason Knight defended it. For Knight questions of political economy were primarily moral ones. Economics and social sciences were not strictly instrumental pursuits.

Knight's normative approach to economics was on the way to obsolescence. Even as other members of the Chicago economics department respected his work, they clashed on methodological issues. Chicago was home to Paul Douglas, Oskar Lange, and Henry Schultz, three of the most prominent mathematical economists who were also interested in empirical studies of the economy.

Members of the political science department emphasized a behavioral approach to studying individuals and groups. Under the leadership of Charles Merriam, the department sought both rigor and practical engagement with politics in the 1920s. Among the most significant works were Charles Merriam and Harold Gosnell's *Non-Voting* in 1924, which used extensive interview and survey data to examine why the expansion of suffrage resulted in lower voting rates, and Harold Lasswell's *Psychopathology and Politics*, which used Freudian psychoanalytic case studies to relate individual life histories to political engagement. Hunter Heyck describes this generation of the Chicago School as holding a "liberal managerialist" philosophy due to the tensions in their work between expert management and liberal democracy. Institutions were central to this school of liberal managerialism because they organized individual interests for socially productive ends.[73]

Early behaviorist social theory in the 1920s and early 1930s was intended to empirically test the propositions of traditional political theory.[74] Behavioralism began to be characterized by more grandiose philosophical claims in the 1930s and 40s following the arrival of German émigrés (including Hannah Arendt, Hans Morganthau, and Leo Strauss) with a more

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[72] Ibid., 180-181. Also see his 1923 essay, "The Ethics of Competition," in Frank H. Knight, *The Ethics of Competition* (New Brunswick, NJ: Transaction Publishers, 1997), 33-67. It is important to note the contrast between the "first Chicago School" of Knight's time and the "second Chicago School" of Milton Friedman and George Stigler (both students of Knight).

[73] Hunter Crowther-Heyck, *Herbert A. Simon: The Bounds of Reason in Modern America* (Baltimore: Johns Hopkins University Press, 2005), 44.

[74] Barry D. Karl, *Charles E. Merriam and the Study of Politics* (Chicago: University of Chicago Press, 1974), 297. It is important to distinguish *behavioralism* as an empirical method of social science building upon observable behavior, and *behaviorism*, a form of psychology associated with B. F. Skinner that emphasized how behavior was conditioned by stimulus-and-response.

philosophical style of political theory.[75] Theoretical concerns which had largely remained foreign to the American traditions of political science suddenly became significant. Behavioral science methodology was quickly conscripted into the positivist cause, by both its defenders and critics. Positivism remained associated with a dangerous secularism and a resistance to ultimate moral ends.[76] In general, however, the interwar political and social theory pioneered at Chicago was meant to be instrumental, concerned with understanding the relationship between changes in institutional and legal design, and changes in social practices.

New Concepts of Management

With business leaders vilified during the Great Depression, and with technology increasingly associated with unemployment, the analogy between modern business management practices and mechanical systems lost none of its negative associations, but did lose almost all of the positive ones cited by Veblen et al.[77] Cracks appeared in the façade of the unified idea of technocratic governance; industrial management was tarnished, though the idea of public administration was salvaged through the New Deal. The hope that a technocratic elite could transcend old distinctions between public and private interests began to seem naive. Critics—liberal, conservative, and socialist alike—raised new questions about the possibility of systematic, apolitical, technocratic administration.

One of the central, unresolved issues in the New Deal was the question of monopoly, and how to balance efficiencies of scale against concentrations of economic power. According to the engineering modernists, monopolies were not intrinsically bad; proper organization could make monopolies and cartels into vehicles for social progress. Advocates of the engineer's perspective divided into two camps: one promoting cartelization run by and for businessmen, and one promoting a public form of social planning. Against both of these groups were the remaining advocates of laissez-faire capitalism, who believed that excessive organization stifled competition.[78] Their debates about economic planning within the New Deal pitted trust-busters against social planners, leaving the business planners to organize via extra-governmental trade associations and through the use of management consultants. The profession of management consulting was largely created to get around the restrictions that limited overt business-led cartelization.[79]

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[75] John G. Gunnell, *The Descent of Political Theory: The Genealogy of an American Vocation* (Chicago: University of Chicago Press, 1993), 175-198.

[76] George A. Reisch, *How the Cold War Transformed Philosophy of Science: To the Icy Slopes of Logic* (New York: Cambridge University Press, 2005).

[77] On unemployment, see Amy Sue Bix, *Inventing Ourselves out of Jobs? America's Debate over Technological Unemployment, 1929-1981* (Baltimore: Johns Hopkins University Press, 2000).

[78] Ellis W. Hawley, *The New Deal and the Problem of Monopoly: A Study in Economic Ambivalence* (New York: Fordham University Press, 1995), 35.

[79] Christopher McKenna, *The World's Newest Profession: Management Consulting in the Twentieth Century* (New York: Cambridge University Press, 2006).

While the business-oriented management community believed that shared knowledge of best practices would strengthen business and, by extension, the community at large, the social planners instead maintained that the self-interest of the business cartels had created the nation's economic woes. The solution, however, was not to re-atomize the economy, as the laissez-faire crowd wanted. According to the planners, competition would create waste, destroy efficiency, and lower the overall standard of living.[80] Instead, the management of large organizations had to be reconfigured to achieve results that would benefit the public. One of the most significant intellectual contributions from the New Deal on this theme was Adolph A. Berle and Gardiner Means's *The Modern Corporation and Private Property*.

Berle and Means argued that the distribution of ownership in the modern corporation had fundamentally changed the function of ownership. Once upon a time, owners had an interest in protecting the value of their property and so they had been responsible for the long-term health of the corporation. However, Berle and Means noted that within publicly traded companies "the surrender of control over their wealth by investors has effectively broken the old property relationships and has raised the problem of defining these relationships anew. The direction of industry by persons other than those who have ventured their wealth has raised the question of the motive force of such direction and the effective distribution of the returns from business enterprise."[81] While stockholders remained the nominal owners of the corporation, they exercised a strangely passive form of ownership. Stockholders could not directly control their property, and the value of this property depended upon the actions of others, both inside and outside of the corporation. Yet this passive ownership provided something of value to stockholders: the ability to easily sell their ownership stakes. Managers, ostensibly managing on behalf of the owners, were left to call the shots.

Berle and Means claimed that "throughout the entire history of finance there is apparent a constant struggle so as to arrange matters that values anywhere may be made available anywhere. This involves two subsidiary processes: the first being a method for assigning recognized value to property; and the second, the devising of instrumentalities by which participations representing an interest in such properties may be created and made salable more or less universally."[82] Passive owners ceded control over their property to managers and let market mechanisms determine the price of their property. In return the symbols of their ownership became liquid. This new regime of distributed ownership removed the supervisory function of ownership. The wide distribution of ownership meant that an organized minority shareholder could wield effective control. Other owners had a purely passive role.

Writing in the midst of the Great Depression, Berle and Means concluded with an investigation into economic and legal solutions to corporate governance. Managers with small stakes in the welfare of the corporation employed their positions for self-aggrandizement,

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[80] Hawley, *New Deal*, 174-175. Even staunch advocates of laissez-faire, such as economist Frank Knight, conceded these points.

[81] Adolf A. Berle and Gardiner Means, *The Modern Corporation and Private Property* (New York: Harcourt, Brace and World, 1968), 4.

[82] Ibid., 248-249.

potentially at the expense of the health of the corporation or of the economy. Neither traditional economic incentives nor traditional legal rights seemed capable of guaranteeing enlightened management. Applying the profit motive to the corporate management would lead them to act on their behalf rather than the owners'. Making the property rights of distributed owners the primary consideration would not address managerial incentives at all. Inherited economic concepts no longer applied, for "these great associations are so different from the small, privately owned enterprises of the past as to make the concept of private enterprise an ineffective instrument of analysis. It must be replaced with the concept of corporate enterprise, enterprise which is the organized activity of vast bodies of individuals, workers, consumers and suppliers of capital, under the leadership of the dictators of industry, 'control.'"[83] Adam Smith's world of atomistic economic agents had been replaced by a network of a few large corporations whose operations were interwoven. Economic analysis would have to proceed at the level of the system. They concluded that "it is conceivable,—indeed it seems almost essential if the corporate system is to survive,—that the 'control' of the great corporations should develop into a purely neutral technocracy, balancing a variety of claims by various groups in the community and assigning to each a portion of the income stream on the basis of public policy rather than private cupidity."[84]

While these conclusions were an important piece of the planning movement during the New Deal, the intermediate stages of the argument are even more significant for connecting practicalities of corporate governance to new models of institutional analysis. Technocratic management had become important precisely because the categories of ownership and of influence had become so abstracted. With ownership diffused among shareholders, the supply of capital for the corporation depended only on the existence of buyers and of a market mechanism. Their particular identities did not matter. Likewise, 'control' of a company did not necessarily require a controlling financial interest in the company—what was owned was control over the operations of the organization. The suppliers of capital remained largely distinct from the operators of 'control.' As capitalists traded their liquid symbols of ownership in the market, the maintenance of the market remained the essential element for capital, while the insulation and protection of governing bodies from nominal owners was the essential element for management.

Chester I. Barnard, the head of New Jersey Bell and a supporter of the Hooverite associational model, responded to the New Deal by analyzing the conditions of organization. *The Functions of the Executive*, published in 1938, was characterized by a tension between his hard-won insights into the practical challenges of management and his abstract model of organization. An experienced manager, he had also spent several years in the Harvard Business School community with Wallace Donham, Elton Mayo, and the biologist-turned-sociologist L. J. Henderson, where he came to believe in the need for a rigorous foundation to the study of managed organizations. He began by recognizing that organizations form through the active cooperation of individuals. From the organizational perspective, an individual is just an agent who performs certain prescribed tasks. However, Barnard warned, each worker must also

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[83] Ibid., 306.

[84] Ibid., 312-313.

simultaneously be understood as an autonomous individual, whose needs may or may not align with those of the organization at any given moment.[85]

With this as his starting point, Barnard defined organization as "a system of consciously coordinated activities or forces of two or more persons."[86] He intended this analysis to be absolutely general, applying to private corporations, religious groups, public institutions, and so on. In keeping with this approach to the study of organizations, Barnard himself followed a similarly eclectic path from New Jersey Bell to the Depression-era New Jersey state relief agencies to the Rockefeller Foundation to the NSF.[87] The science of management, according to Barnard, was one of organization and coordination, understood in terms of general patterns and relationships. At the most fundamental level, lines of communication defined the channels of managerial power within the organization. Barnard's contributions to the study of communication were not limited to his insights in *The Functions of the Executive*; later, as the director of the Rockefeller Foundation, he commissioned Warren Weaver to write the famous introduction to the book publication of Claude Shannon's *Mathematical Theory of Communication*.[88] From his time at New Jersey Bell, Barnard maintained an interest in communications engineering along with his interest in communication as a social process.

Barnard created a typology of administrative scenarios, but maintained that the actual job of managing could not be simplified so easily. It still required personal judgment and flexibility, a point made clear in his lecture on how he defused a riot in Trenton, New Jersey in 1935.[89] His approach to organization was at once incredibly systematic in outlining the factors that affect the performance of the organization and delineating characteristics of the organization, while also accepting that such a systematic approach to organization runs up against the challenge of managing human beings. Ultimately, Barnard embraced the ambiguity in the balance between putting the system first and putting the individual first:

> I believe in the power of the cooperation of men of free will to make men free to cooperate; that only as they choose to work together can they achieve the fullness of personal development; that only as each accepts a responsibility for choice can they enter into the communion of men from which arise the higher purposes of individual and of cooperative behavior alike. I believe that the expansion of cooperation and the development of the individual are mutually dependent realities, and that a due proportion or balance between them is a necessary condition of human welfare. Because it is subjective with respect both to society

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[85] Chester I. Barnard, *The Functions of the Executive* (Cambridge, Mass.: Harvard University Press, 1938), 19-20.

[86] Ibid., 73.

[87] One of very few biographical treatments of Barnard is William G. Scott, *Chester I. Barnard and the Guardians of the Managerial State* (Lawrence, Kans.: University Press of Kansas, 1992).

[88] Letter from Warren Weaver to Claude Shannon, 1/27/49, in Claude Shannon papers, Library of Congress, Box 1, Folder 3. See Claude E. Shannon and Warren Weaver, *The Mathematical Theory of Communication* (Urbana, Ill.: University of Illinois Press, 1949).

[89] Chester I. Barnard, "Riot of the Unemployed at Trenton, NJ, 1935," in *Organization and Management: Selected Papers* (Cambridge, Mass.: Harvard University Press, 1948), 51-79.

> as a whole and to the individual, what this proportion is I believe science cannot say. It is a question for philosophy and religion.[90]

Barnard's skill in analyzing the factors of organization and in identifying the characteristics of organizational performance, taken together with his open-ended conclusions, made him a starting point for organizational theorists of dramatically different orientations. The chief lesson taught by Barnard was to take a broad perspective that encompassed the entire organization, without taking for granted its goals, its internal structure, or its elements. Organization itself was a tractable category of analysis.

Barnard, a practicing manager, deliberately kept his analysis abstract, recognizing that the challenge of actually managing an organization would always remain in the details. For him, management remained on a human scale. However, James Burnham, a former Trotskyist, took the argument from Berle and Means and carried it even farther. He accepted the basic distinction between ownership and management, but claimed that the two functions would eventually be consolidated within one group. Crucially, this consolidation would occur under the managers and not under the capitalists, the nominal owners. Burnham argued that the capitalists, as passive owners of corporations, had removed themselves from the production process, leaving managers unchecked. He claimed that "the managers' training as administrators of modern production naturally tends to make them think in terms of co-ordination, integration, efficiency, planning; and to extend such terms from the area of production under their immediate direction to the economic process as a whole.... the old-line capitalists ... appear to them as parasites, having no justifiable function in society, and at the same time preventing the managers from introducing the methods and efficiency which they would like."[91]

By the same token, "the masses seem to the managers stupid, incapable of running things, of real leadership ... [The managers] naturally tend to identify the welfare of mankind as a whole with their own interests and the salvation of mankind with their assuming control of society. Society can be run, they think, in more or less the same way that they know they, when they are allowed, can run, efficiently and productively, a mass-production factory."[92] As Burnham saw it, the expansion of management was a global phenomenon, seen in the Soviet Union, Fascist Italy, and Nazi Germany, and, albeit in a weaker form, in the New Deal.[93] Echoing Knight, Burnham recognized that the managerial revolution could increase efficiency, but at the potential cost of wedding industrial strength to oppressive state power.

Continuing this line of argument, the Austrian émigré economist Joseph Schumpeter lamented the passing of the entrepreneurial spirit amidst the growth of administration. He wrote

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[90] Barnard, *Functions of the Executive*, 296.

[91] James Burnham, *The Managerial Revolution: What is Happening in the World* (New York: John Day, 1941) 163. For the rightward drift of former communists in the 1940s, see Richard Pells, *The Liberal Mind in a Conservative Age: American Intellectuals in the 1940s and 1950s* (Middletown, Conn.: Wesleyan University Press, 1985), 76-83.

[92] Ibid., 163-164.

[93] For a nuanced comparison of these societies, see Wolfgang Schivelbusch, *Three New Deals: Reflections on Roosevelt's America, Mussolini's Italy, and Hitler's Germany, 1933-1939* (New York: Picador, 2007).

in 1942 that "to act with confidence beyond the range of familiar beacons and to overcome that resistance requires aptitudes that are present in only a small fraction of the population and that define the entrepreneurial type as well as the entrepreneurial function." The problem facing the continued health of entrepreneurship vis-a-vis administration, as he saw it, was that entrepreneurship and innovation were gradually rendering themselves subject to routine, and "the romance of earlier commercial adventure is rapidly wearing away, because so many more things can be strictly calculated that had of old to be visualized in a flash of genius."[94] It was difficult to reconcile the administrative ideal with the romantic captain of industry—the idea of administration was to substitute rules for personal whim, while that of romantic individualism was built upon unlimited personal agency.

The growth of bureaucratic corporations marked a moment within capitalism that remained imperfectly reconciled with that economic system's traditional justifications. Schumpeter acknowledged the significance of the growth of corporate capitalism, positing that even if large monopolies improved the operations of the economy, they still had a pernicious effect in destroying the social foundations of a capitalist society. For "even if the giant concerns were all managed so perfectly as to call forth applause from the angels in heaven, the political consequences of concentration would still be what they are ... the very foundation of private property and free contracting wears away in a nation in which its most vital, most concrete, most meaningful types disappear from the moral horizon of the people."[95]

The Science of Management

Postwar management science was defined by Herbert Simon's analyses in *Administrative Behavior*, published in 1947 and based on his doctoral dissertation at Chicago. Chicago remained the acknowledged center of political science into the 1930s and 40s, at which point its influence began to wane (in Simon's eyes, at least) after the first generation of behavioral political scientists had left, replaced by theorists in the mold of Leo Strauss. Simon studied with faculty from across the university, including political scientists Charles Merriam, Harold Lasswell, Harold Gosnell, and Leonard White; mathematical biologist Nicholas Rashevsky; philosopher Rudolf Carnap; and economist Henry Schultz. His graduate work focused on the behavior of individuals in complex organizations. This interest grew out of earlier experiences with the city government of Milwaukee, Wisconsin, where he recognized that different branches of the same organization tended to find different, often contradictory, solutions to problems. Simon understood that these different decisions could all be considered rational given the resources and goals of each agency. He wanted to understand the process by which organizations reached decisions given their particular circumstances. By taking seriously a distinction between facts and values, he noted that a science of administration could begin by explaining the consequences of certain value commitments, without taking a stand on those value commitments themselves. The purpose of an administrative science would be to determine the consequences of administrative decisions, not to judge the ethical desirability of administrative outcomes.

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[94] Joseph A. Schumpeter, *Capitalism, Socialism, and Democracy* (New York: Harper & Brothers, 1942), 132.

[95] Ibid., 140-141.

Administrative Behavior was strongly influenced by *Functions of the Executive*, from which it borrowed many of its fundamental definitions. Thus, for Simon the organization consists of "patterns of communications and relations" in which leadership is ascertained empirically by seeing how subordinates behave around the leader, and "in a very real sense, the leader, or the superior, is merely a bus driver whose passengers will leave him unless he takes them in the direction they wish to go. They leave him only minor discretion as to the road to be followed."[96] Simon identified decision-making as the fundamental process within organizations, claiming that decisions precede action. This led him to the elucidation of bounded rationality at the heart of his study. He recognized that individual actors do not act like the *homo economicus* of neoclassical economics; real humans do not optimize by rationally selecting across all of their options. Instead, they have limited options to choose from, limited knowledge of the consequences of their actions, and have different valuations of possible outcomes. Real decisions are made within constraints determined by the individual actor and by his or her organizational or environmental position. As Simon put it, "the behavior patterns which we call organizations are fundamental, then, to the achievement of human rationality in any broad sense. The rational individual is, and must be, an organized and institutionalized individual."[97] This was a New Deal liberal's defense of collective action, coupled with the recognition that technocratic administration could not eliminate a need for political decision-making.

Administrative Behavior was immediately recognized as a significant contribution to the study of organizations and of management. While *Administrative Behavior* was the most directly relevant to scholars of organization, it was not the only book to suggest a new foundation for a science of social behavior. In 1944 the mathematician John von Neumann and economist Oskar Morgenstern published their *Theory of Games of Economic Behavior*. This book dealt with the problem of creating rational strategies in competitive situations, and therefore touched upon several problems that Herbert Simon had been concerned with. He admitted that it was a book that he very much wished to have written. Von Neumann and Morgenstern hoped that a rigorous treatment of competitive strategies could provide a foundation for the further mathematization of economics. While game theory did eventually become a central component of microeconomic theory, it was first picked up by scholars in the new fields of management science and operations research.[98]

The postwar research agenda in economics emphasized the significance of quantification and the application of mathematics. Scholars have attributed this transformation to several different factors, from a post-Hiroshima "physics envy," to the growing influence of technical calculations for military patrons, to a protective mechanism for guarding the discipline's borders. While these tendencies toward abstraction and quantification have led to the easy caricature of these economists as cold and calculating, viewing humans and social behavior simply as

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[96] Herbert A. Simon, *Administrative Behavior: A Study of Decision-Making Processes in Administrative Organizations* (New York: Free Press, 1997), 186.

[97] Ibid., 111.

[98] See Philip Mirowski, "What Were Von Neumann and Morgenstern Trying to Accomplish?" in *Toward a History of Game Theory*, ed. E. Roy Weintraub (Durham, N.C.: Duke University Press, 1992), 113-147, and Robin E. Rider, "Operations Research and Game Theory: Early Connections," 225-239.

processes to be controlled, there were also opposing currents. Simon, for example, repeatedly claimed that the use of mathematics in social science was a tool to facilitate and clarify theorizing, rather than a straightforward and unambiguous description of human behavior from which additional facts could be deduced. His comments on the purpose of mathematics in economic theory are worth reprinting in their entirety:

> For me, mathematics has always been a language of thought. I don't know precisely what I mean by that (and explicating the meaning is today one of my important research goals), but I can try to explain. When I am working on a problem, I am sure that I do not usually think in words, but in terms of a more abstract representation that is perhaps partially pictorial or diagrammatic and partially symbolic. Mathematics—this sort of non-verbal thinking—is my language of discovery. It is the tool I use to arrive at new ideas. This kind of mathematics is relatively unrigorous, loose, heuristic. Solutions reached with its help have to be checked for correctness. It is physicists' mathematics or engineers' mathematics rather than mathematicians' mathematics.
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> For Tjalling Koopmans, it appeared, mathematics was a language of proof. It was a safeguard to guarantee that conclusions were correct, that they could be derived rigorously. Rigor was essential. (I have heard the same views, in even more extreme form, expressed by Gerard Debreu; and Kenneth Arrow seems mainly to share them.) I could never persuade Tjalling that ideas have to be arrived at before their correctness can be guaranteed, and that the logic of discovery is quite different from the logic of verification. I am sorry that he did not live to read and comment upon my recent work on the logic of scientific discovery. Perhaps we could have built a bridge across what seemed a great gulf that separated our attitudes toward mathematics. It is his view, of course, that prevails in economics today, and to my mind it is a great pity for economics and the world that it does.[99]

If Simon's views on the mathematical determinacy and the deducibility of human behavior are weaker than has generally been accepted, this does not mean that other mathematical economists and systems theorists did not buy into the unproblematic application of mathematical models as descriptions of human behavior. Both types existed in the late 1940s, though the mathematizers would eventually win out.[100]

These studies of administration were motivated by the belief that the growth of large, managed corporations signaled a fundamental change in social organization. Administrative

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[99] Herbert A. Simon, *Models of My Life* (Cambridge, Mass.: MIT Press, 1996), 106-107.

[100] One of the most provocative and vocal critics of the mathematical economists is Philip Mirowski. See his *Machine Dreams: Economics Becomes a Cyborg Science* (New York: Cambridge University Press, 2002). Will Thomas's dissertation on OR is a useful reminder that OR theorists were rarely the mathematical bogeymen of lore. However, in pointing out the methodological flexibility of the policy-oriented scientists, he misses the more hard-line approach taken in the universities. Gerald William Thomas, "A Veteran Science: Operations Research and Anglo-American Scientific Cultures, 1940-1960" (Ph.D. dissertation, Harvard University, 2007). For important background on quantification, see Theodore Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton, NJ: Princeton University Press, 1994).

scholars wanted to understand how organizations behaved, as well as the implications of administrative power for social order. Austrian émigré Peter Drucker, for example, claimed that this corporate form was a fundamentally new social arrangement, and one that altered the relationship between the individual and the organization. Production within a corporation involved workers performing simple tasks, which were then arranged in a complex pattern to create the final product. He claimed that "a product can only be made if the operations and motions of a great many individuals are put together and integrated into a pattern. *It is this pattern that is actually productive, not the individual*. Modern industry requires a group organization far exceeding in forethought, precision and cohesion anything we have ever witnessed." Productive power was not based in either machines or human labor, but in an abstract idea of the industrial process. "It is partly technical and theoretical: knowledge of principles and processes. Partly it is social: skill in the organization of men for work in a close group and in fitting together their operations, their speeds and their abilities. Above all, the new "skill" required is the ability to see, to understand and even to produce a pattern; and that is by definition imaginative ability of a high, almost of an artistic, order."[101] Drucker had made a crucial observation in identifying production with the creation of patterns, and the key managerial skill as understanding patterns. Even though his understanding of management as essentially humanist took Chester Barnard's analytical framework in a different direction than Simon did, Drucker's views were equally influential.[102]

The new science of management and administration had no shortage of critics. Those on the left attacked it for the sharp distinction it made between the managed and the managers. According to political theorist Dwight Waldo, "a striking feature in the upward and outward spread of the idea of applying science to organized endeavor, in the case both of public administration and scientific management, has been a disposition to draw a line between a 'lower' realm to which the New Method is applicable, and a 'higher' realm to which it is not ... In both disciplines the distinction has recently served the purpose of a fortified line suitable for either offensive or defensive operations."[103] Waldo understood the claims of management science as an ideological justification for a new governing class. Most forms of work were to be subject to the analyses of managerial or administrative science, while a select few forms of creative work remained under the control of the people who did it. This science justified keeping the managed under the scrutiny of the managers, while allowing the managers to maintain their own autonomy.

Waldo asked what an expertise in management-as-such meant. He noted that "we are not told why the will of the Administrative Technician is entitled to prevail in case of conflict. The Administrative Technician, it is asserted, is entitled to prevail because he is a 'specialist in generalization,' whereas meteorologists and stenographers are not. But 'specialist in

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[101] Peter Drucker, *The New Society: The Anatomy of the Industrial Order* (New York: Harper, 1950), 22-23.

[102] Nils Gilman, "The Prophet of Post-Fordism: Peter Drucker and the Legitimation of the Corporation," in *American Capitalism: Social Thought and Political Economy in the Twentieth Century*, ed. Nelson Lichtenstein (Philadelphia: University of Pennsylvania Press, 2006), 109-131.

[103] Waldo, 56.

generalization' is unexplained and unsupported. It is a feat of dialectic levitation."[104] The legitimacy of the administrative science rested upon its claims of identifying the shared interests of those administered and creating patterns of behavior to achieve those shared interests, without having any particular agenda of its own. Yet Waldo and other like-minded theorists, including Robert Dahl, accused administrative scientists of making normative claims of their own, whether or not they recognized it. Dahl pointed out that the distinction between ends and means was rarely as well-defined as administrative science suggested, echoing the original debates about administrative theory from fifty years earlier. The identification of "good" systems of organization with rational or scientific ones, Dahl claimed, only made sense in a very narrow range of situations where individuals "are so thoroughly dominated by the technical process (as on the assembly line, perhaps) that their individual preferences may safely be ignored."[105] According to these critics, administrative science provided a new justification for old relationships of authority.

The New Management Education

The Carnegie Institute of Technology began planning a program in Industrial Administration in the mid-1940s. By 1948, it was getting ready to open, and had to clearly differentiate itself from existing management schools, such as Harvard Business School. Rather than teaching a specific body of managerial techniques, the Carnegie program emphasized general skills, such as problem solving and how to adapt to technological change and new scientific knowledge. Graduates would learn specific skills on the job, after their Carnegie education had prepared them to acquire and use them most effectively. At a luncheon with business leaders in Pittsburgh at the Duquesne Club, Provost Elliott Dunlap Smith told his audience that "we are not seeking to train managers, but seeking to equip men to learn from later study and experience to become useful members of your organization and useful citizens ... Thus our students will have few proficiencies, but a broad, simple, thorough background in engineering, business, and management; and the habits of using fundamental knowledge in dealing with problems and in learning from experience."[106] This vision of business education was part of a university-wide "Carnegie Plan" for liberalizing engineering education.

The program would draw upon several distinct but interrelated content areas. First was the "social-humanistic" stem, to encourage the student to think systematically about business as a component of American society and to think about the responsibilities of business for American society, a crucial aspect of business education with the Great Depression having only recently ended and with the public image of business management not yet rehabilitated. The second stem, economics, focused on the relationship of the individual firm to the American economy as a

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[104] Waldo, 100.

[105] Robert Dahl, "The Science of Public Administration: Three Problems," *Public Administration Review* 7 (1947): 1-11, on 6.

[106] "Transcript of the Luncheon to present The Industrial Administration Program, Duquesne Club, Monday, March 15, 1948," Graduate School of Industrial Administration Records, Carnegie Mellon University Archives, box 3. Emphasis in original.

whole, for "today, the most difficult decisions of the industrial administrator are typically those of the relations between his firm and the economy (business cycles, labor unions, government controls), not the technical problems within his own plant." The third stem was devoted to engineering, to give managers the ability to keep up with technological innovation as it affects industrial processes, and the final stem was devoted to traditional business administration, built around quantitative skills for accounting and communication skills to deal with other people.[107] With the help of a $6 million donation from William Larimer Mellon, an executive at Gulf Oil and a member of one of the most powerful families in Pittsburgh, the school finally opened in 1949 under the leadership of Lee Bach, an economist trained at the University of Chicago.[108] Bach quickly recruited Herbert Simon, who was then teaching organization theory at the Illinois Institute of Technology. Simon hesitated at first, as he was enjoying the challenge of teaching political science to engineers and was trying to build up a social science program at IIT. He changed his mind in April 1949 and joined the team at Carnegie.[109]

According to Bach, Smith, and the other founders of the Graduate School of Industrial Management (GSIA, currently the Tepper School of Business), the pace of both technological and institutional changes made traditional management education obsolete. The goal was to create a science of management—to identify the fundamentals and constant features within the turbulent administrative environment of American business. This approach contrasted with the alleged overspecialization of students at traditional business schools, such as Harvard. Students there, claimed the GSIA's founders, did not learn a science of management; rather, they learned components of management, such as finance or marketing, and consequently failed to get a sense of management as a unified concept.[110] At Carnegie Tech, the claim of having created a scientific approach to management meant that their "specialists in generalization" would have formed the mental habits to systematically analyze the problems facing their organizations, develop creative solutions taking advantage of the latest technologies and social theories, and test out these solutions to determine whether they were indeed the most effective. The ideal of management science meant transcending the particular, historically- and functionally-situated understandings of practicing managers.[111]

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[107] Memorandum, "M.S. in Industrial Administration: Objectives and Program" 10/30/48 to Provost Elliott Dunlap Smith from Lee Bach, GSIA Records, box 3. The emphasis on understanding the human side of organizations surprised many of the engineers who enrolled in the program, recalls Bill Pounds, a GSIA alum and former Dean of the MIT Sloan School, in William R. Dill, "Herbert A. Simon and the Education of Managers," in *Models of a Man: Essays in Memory of Herbert A. Simon*, Mie Augier and James G. March, eds. (Cambridge, Mass.: MIT Press, 2004), 197-210, on 206.

[108] GSIA First Annual Report, 7/1/50, GSIA Records, box 1; Herbert Simon, "Lee Bach at GSIA" and W. W. Cooper, "G. L. Bach—A Beginning," Herbert A. Simon Papers, Carnegie Mellon University Archives, box 91.

[109] Lee Bach to Herbert Simon, 3/14/49; Simon to Bach, 3/18/49; Simon to Bach, 3/19/49; Bach to Simon, 4/23/49, Simon Papers, box 91.

[110] GSIA First Annual Report, 7/1/50, GSIA Records, box 1.

[111] On universal and situated knowledges, see Donna Haraway, "Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspectives," in *Simians, Cyborgs, and Women: The Reinvention of Nature* (New York: Routledge, 1991), 141-189.

The founders of the GSIA were consciously trying to create a new kind of business school. As Herbert Simon recalled 25 years later, "we felt like we were going to have the first business school that had academic respectability, scientific respectability, and we didn't think it needed to run like dead-headed old-fashioned business schools or we wouldn't have been here."[112] The school wanted to identify what was "truly fundamental" in management practices, and conducted research along these lines by forcing together interdisciplinary teams, valued not only for their breadth of knowledge, but also because they had to create their own vocabularies and assumptions rather than relying on pre-existing habits. With the primary focus of the GSIA being the development of fundamental management theories, and therefore being wary of recruiting traditional management scholars, the school attracted a remarkably heterogeneous faculty. The theoretical orientation of the program was compounded by its inability to find faculty who had both managerial experience and the GSIA view of management as a science. Said Simon, "You see, none of the people who came in came from a business school background. So we came in with an understanding that we were going to build a different kind of business school. That we were going to experiment and see where these new ideas led." The result was "a predominantly young faculty, consisting of men who have not become committed to the prosaic practices now found in most other business administration schools."[113] As self-proclaimed pioneers, the heads of the GSIA declared that it would be premature to expect their research to pay off immediately. Practical results would have to wait for several years—five at the earliest, possibly as many as twenty. It would also take some time to demonstrate the practical value of management science to the business world. According to Bach, the importance of academic management theory was that "as in the development of the physical sciences and engineering, educational institutions probably must take the lead in original basic research in the area of management, especially since much of this research will inevitably appear slow and impractical—even 'long haired.' Moreover, many findings may well be disruptive for long-established patterns of business operations and the men who stand for them."[114]

The first research programs at the school considered the question of control within organizations. One project dedicated to understanding intra-firm behavior was funded by the Air Force's Project SCOOP (Scientific Computation Of Optimal Programs), within the Air Force Comptroller's Office. SCOOP was formed in 1948 under the leadership of mathematician George Dantzig. The Air Force faced significant logistical challenges, and the Office of the Comptroller was responsible for coordinating this vast system. Dantzig and his mathematicians developed techniques for optimizing these logistics systems, processes known collectively as

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[112] Herbert A. Simon, oral history interview with Pamela McCorduck, 10/28/74, McCorduck Papers, Carnegie Mellon University Archives, box 2.

[113] Herbert A. Simon, oral history interview with Pamela McCorduck, 10/28/74, McCorduck Papers, box 2; Second Annual Report of the GSIA, 7/1/51, GSIA Records, box 1; Third Annual Report of the GSIA, 6/24/52, GSIA Records, box 1; Fourth Annual Report of the GSIA, 7/2/53, GSIA Records, box 1. Note, however, that Provost Smith, who also taught industrial relations at the GSIA, was an advocate of the Harvard case study approach.

[114] George Leland Bach, "Education for Management in a Mobilizing Economy," *Advanced Management* (May 1951): 7-9.

linear programming.[115] The Project SCOOP research at the GSIA involved studying the economics of operations at the sub-firm level, as well as the administration and organization of the firm. It therefore focused on the processes of control and communications within the organization, such as accounting, budgeting, and reporting systems—specifically on identifying the actual lines of control and quantifying their effects whenever possible. This research at the GSIA was organized by William Cooper.[116]

The second ongoing project, "Centralization vs. Decentralization of Accounting and Control Functions in Business," was funded by the Controllership Foundation. Here, too, research focused on the problems of accounting, budgeting, and other control functions within an organization, but also with understanding how these control functions were exercised at different positions within the organization. George Kozmetzky directed the project, with support from Herbert Simon, Harold Guetzkow, and Gordon Tyndall. Like Project SCOOP, this study brought together institutional studies with quantitative and economic analyses of the various operations within the firm, and with a healthy dose of psychology as well, to understand how individuals behave among these institutional configurations. The study focused far more on theory than on applications, and the final report was not what the Foundation wanted. In later years it became highly influential in accounting theory, once the "New Look" of management science was accepted more widely.[117]

These studies were part of a long-term focus on the behavior of institutions, reaching back to Simon's earlier work in *Administrative Behavior*. These studies also show Simon beginning to think about the behavior of the firm in the language of computers after his career-altering visit to RAND in 1952. A group of RAND scientists was studying the behavior of individuals within aerial defense stations, and Simon was brought in as a consultant. He began his life-long collaboration with the mathematician Allen Newell, and began to understand computers as the ideal machines for simulating human behavior. At first this understanding of computers as models of behavior served metaphorically, much as mechanical analogies had informed ideas of management in earlier decades. In a paper on budgeting from 1953, Simon observed that "in every organization in our society there is always some person or group of persons whom we regard as having the legitimate right to determine the organization's program and the way the program is carried out ... The problem of control is the problem of implementing

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[115] For information on the logistics in the Air Force, see David Hay, "Bomber Businessmen: The Army Air Forces and the Rise of Statistical Control, 1940-1945" (Ph.D. dissertation, University of Notre Dame, 1994).

[116] "Report of Progress (Second Year): Carnegie Institute of Technology School of Industrial Administration and the Department of the Air Force, Project SCOOP, Research Project for the Study of Intra-Firm Behavior," June 5, 1952, GSIA Records, box 6. Bill Cooper, one of the earliest members of the GSIA faculty, had dropped out of high school, and had abandoned his doctoral dissertation in economics at Columbia. He eventually completed it, but the dissertation, applying linear programming to economic theory, was rejected as unsuitably mathematical for an economics degree. At Chicago, Cooper had been the instructor for the one course Herbert Simon had taken for credit as a graduate student: boxing.

[117] William W. Cooper, "Memorial to Simon," in *Models of a Man*, 67-74. The published report is Herbert Simon et al., *Centralization and Decentralization in Organizing the Controller's Department: A Research Study and Report* (New York: Controllership Foundation, 1954).

that right."[118] Even as Simon's interests were moving away from organizational behavior, he worked with Jim March and Harold Guetzkow to produce one of the central texts on organizational behavior in 1958, and March continued this work in his collaboration with Dick Cyert (Dean of the GSIA during the 1960s, and the President of Carnegie Mellon University from 1972-1990) in *The Behavioral Theory of the Firm* in 1963.

The core of these institutional works was the novel understanding of bounded rationality pioneered by Simon. This idea had been at the heart of *Administrative Behavior*, and was then further clarified in two important papers: "A Behavioral Model of Rational Choice" in 1955, and "Rational Choice and the Structure of the Environment" in 1956.[119] These two papers drew heavily upon his studies of computers as models of behavior. Perhaps the most significant moment in this development, however, was the creation of the Logic Theorist machine in 1956, which will be discussed in the context of artificial intelligence in the next chapter.

The GSIA faculty had set out to revitalize the study of management, and the research done through these studies characterized the particular Carnegie "look"—paying careful attention to the control functions of organizations and the lines of communication within large, complex firms. Subsequent research programs, whether supported by military patrons, such as the Office of Naval Research, or supported by private foundations, such as the Ford Foundation, continued to broadly share this approach. By the mid-1950s, GSIA leaders could speak of entering a new phase in the school's growth, from a period of sheer innovation to one of consolidation and expansion.[120] Yet this institutional approach was becoming obsolete through the growing emphasis on mathematical finance, which was also being developed within the GSIA, by the economists Merton Miller and Franco Modigliani.[121]

Throughout this first decade, the GSIA maintained a reputation for highly original and interdisciplinary work on organizations, becoming a model for the reconstruction of business education, according to reports commissioned by both the Ford and Carnegie Foundations.[122] There were dissenting views, of course. In 1958, Peter Drucker questioned the entire organization of Management Science, because it "began with the application of concepts and tools developed within a host of other disciplines for their own particular purposes." He called

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[118] "Staff and Management Controls," 1953, Simon Papers, box 90, folder: American Academy of Political and Social Sciences.

[119] The papers are collected in Herbert A. Simon, *Models of Man: Mathematical Essays on Rational Human Behavior in a Social Setting* (New York: John Wiley and Sons, 1957).

[120] GSIA Second Annual Report, 7/1/51; Third Annual Report, 6/24/52; Fourth Annual Report, 7/2/53; Fifth Annual Report, 7/7/54; Sixth Annual Report, 6/22/55; Seventh Annual Report, 7/3/56, GSIA Records, box 1. A good overview of the GSIA in these years is Gleeson, 125-195.

[121] Franco Modigliani and Merton H. Miller, "The Cost of Capital, Corporation Finance and the Theory of Investment," *American Economic Review* 48 (1958): 261-297.

[122] Robert Aaron Gordon and James Edwin Howell, *Higher Education for Business* (New York: Columbia University Press, 1959); Frank C. Pierson et al., *The Education of American Businessmen: A Study of University-College Programs in Business Administration* (New York: McGraw-Hill, 1959).

for a new management science that "respect[ed] itself sufficiently as a distinct and genuine discipline" and that "[took] its subject matter seriously."[123]

Part of the GSIA's strength came from its intellectual diversity. Simon, the intellectual center of the program, held very strong views about proper methodology in the social sciences and the failures of classic economic orthodoxy, but he encouraged interesting work that contradicted his own views, including an important analysis of industrial planning at the Pittsburgh Plate Glass Company, produced in 1960 with support from the Office of Naval Research. It was co-authored by Charles C. Holt, an engineer-turned-economist from MIT; Franco Modigliani, an Italian economist; John F. Muth, a founder of the rational expectations school of economic theory; and Simon.[124] In later years the HMMS papers became very influential to industrial Operations Research. Holt attributed the strength of the forecasting models in HMMS to the basic disagreements between Simon's bounded rationality and Muth's theory of rational expectations: "The first [model] recognizes the limitations on data, analysis, and computation, and utilizes models of the world that are crude but robust. The second seeks to tap the power of more refined theoretical methods and relationships, but at some risk to common sense and loss of robustness. By drawing on these two approaches to the rational investigation of real-world relationships, research performance could be improved."[125] Such negotiations relied upon these scientists having a flexible attitude toward their mathematical models. Taken as literal descriptions of human behavior, they were incompatible, yet when the mathematics was understood as purely instrumental, without any claims about the underlying assumptions about human behavior, the revised model was quite powerful.

Yet this epistemic humility did not last. There were tensions between the organizational emphasis of Simon's group at GSIA and the formalisms of neoclassical economists. The difference between the two groups was based on fundamentally different assumptions about human rationality. While Simon insisted on a realistic model of human rationality as bounded, in which decisions were made through a process of choosing among a finite number of available options, the neoclassical model employed sophisticated mathematical models built upon the simplifying assumption of actors as capable of rationally calculating and maximizing their utility.[126] In the postwar years, this flavor of mathematical neoclassical economics became the predominant approach to the discipline.[127] Even if the tension between the two groups could be

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[123] Peter F. Drucker, "Can Management Ever Be a Science?" in *Technology, Management, and Society: Essays* (New York: Harper & Row, 1970), 191-200.

[124] Consequently, the papers are collectively known as "HMMS," collected in Charles C. Holt et al., *Planning Production, Inventories, and Work Force* (Englewood Cliffs, NJ: Prentice-Hall, 1960).

[125] Charles C. Holt, "Rational Forecasting, Learning, and Decision Making," in *Models of a Man*, 355-363, on 362.

[126] An important statement of methodology in neoclassical economics is Milton Friedman, *Essays on Positive Economics* (Chicago: University of Chicago Press, 1953). Friedman's assertion that the assumptions of a model are irrelevant to the usefulness of the results contrasts strongly with the views of Simon et al.

[127] A useful, but highly critical, history of this period in economics is Philip Mirowski, *Machine Dreams*. See also Michael Bernstein, *A Perilous Progress: Economists and Public Purpose in Twentieth-Century America* (Princeton, NJ: Princeton University Press, 2001).

productive, as with the HMMS study, many of the department's economists felt marginalized by the behavioralists' criticisms. Furthermore, the emphasis within economics on developing abstract mathematical theory conflicted with the pragmatic, problem-solving approach of others within the business school. Lee Bach, the GSIA's founding dean, repeatedly applied band-aids to hold the department together, but the gradual exodus of the main organizational theorists in the late '50s and early '60s left the department to the economists.[128]

The GSIA recruited a particular type of student. The school's educational program focused on a two-year M.S. degree, gradually expanding to allow for a research-based Ph.D. as well. As part of Mellon's original donation, the GSIA also sponsored executive education for practicing managers, though this was given far less attention than other activities in the school. The theoretical and research orientations of the school meant that the faculty generally had little interest in interacting with mid-career executives, other than to provide them with the vocabulary of the "New Look" of management science and perhaps to convince them to hire GSIA graduates. Given the school's stated mission of training generalists to become industrial leaders, and its commitments to scientific inquiry and an embrace of technology, Carnegie sought out students with bachelor's degrees in science and engineering, but with a stated interest in management. They faced stiff competition from the other new, technologically-savvy business school, the MIT School of Industrial Management.[129]

The School of Industrial Management (SIM, now the Sloan School of Management) was founded in 1951 with the support of the Alfred P. Sloan Foundation, and led by E. P. Brooks, a vice president of Sears, Roebuck. Much like the GSIA, the SIM wanted to capitalize upon its location at a major engineering school by fusing a general study of management with specific engineering methods. SIM began by bringing together an established program in production engineering with more traditional business subjects such as economics, accounting, and marketing.[130] Its status as an independent school signified the importance given to industrial leadership in the immediate post-war years.

While the point of reference for the social scientists at the GSIA was public administration, economic planning, and the politics of the New Deal, the coordination of the war effort was simultaneously leading to managerial innovations that would persist into post-war management practice. This military innovation, Operations Research, is a system that remains notoriously difficult to pin down. At its origin in World War II, it described the work done by mathematicians, scientists, economists, and others who collected data and performed statistical

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[128] Simon, *Models of My Life*, 164; letter from Simon to Lee Bach, 7/26/51, Simon Papers, box 91, Folder "Bach, G. L."

[129] In a promotional brochure about management science from 1958, the Ford Foundation wrote "Some of the outstanding centers of this movement are located in graduate schools of business. Not unnaturally, several of them happen to be attached to leading institutions of technological education." From "Industrial Anatomy," Simon Papers, box 99.

[130] Eli Shapiro, Edward L. Bowles, and Jay Forrester, "Development of a New Industrial Management Curriculum at MIT," 1/24/1956, Jay Forrester Papers, MIT Archives and Special Collections, MC439, box 53.

and mathematical analyses to assist military planners.[131] Following the end of the war, many of the scientists who had been involved with OR believed that the techniques that they had pioneered in the war could have industrial applications. The professionalization of industrial OR occurred amidst a proliferation of systems theories, making the distinctions among self-proclaimed operations researchers, cyberneticians, systems theorists, management scientists, and others difficult to identify.[132]

Yet for all the confusion over what precisely OR was, what remained significant was the fact that scientists and mathematicians had helped design military weapons and tactics. Many of these scientists continued to consult with the military after the war, out of a sense of patriotic duty, for the intellectual challenges involved, and for the close ties between academia and government that it promoted. These same scientists found that the same motivations applied to the industrial application of OR.

Philip Morse, a physicist from MIT, was one of the pioneers of Operations Research during the war, due to his work with the Anti-Submarine Warfare Operations Research Group (ASWORG). Morse was a highly entrepreneurial scientist, with wide-ranging interests and a knack for identifying up-and-coming areas of scientific research. Early in his career, he had recognized that he had a greater talent and interest in identifying worthwhile topics for scientific research and in building institutional support for these interests, than in doing research itself.[133] From 1951-1953, Morse launched a graduate program in Operations Research at MIT, founded a professional society with its own journal, and published the field's first American textbook, coauthored with George Kimball, a physicist who had briefly worked on the development of the atomic bomb at Columbia University before joining the wartime OR team.[134] Morse also continued to be active in physics during these years, publishing a textbook in theoretical physics with Herman Feshbach, a physicist who was later a very active member of the antinuclear movement and a founder of the Union of Concerned Scientists.

MIT was in a strong position to support an industrial Operations Research center. The Institute had close ties to industry, and from World War II it also had close ties to the military. It already had an OR contract through the Navy's Operations Evaluation Group, which Morse and his colleagues used as a springboard to propose a Civilian Operations Research Experiment (CORE). They pointed out that industry had been concerned with efficient production for years. Now that military Operations Research groups had developed a set of formal techniques to apply to optimization problems, these same techniques could be applied to industrial processes. Their experiment singled out a few companies for in-depth study. Not only would this gauge the utility of OR for private industry, but by being based in an academic setting, the members of CORE

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[131] Erik Peter Rau, "Combat Scientists: The Emergence of Operations Research in the United States During World War II" (Ph.D. dissertation, University of Pennsylvania, 1999).

[132] This is treated in Thomas, "A Veteran Science." A more general overview of postwar OR is M. Fortun and S. S. Schweber, "Scientists and the Legacy of World War II: The Case of Operations Research," *Social Studies of Science* 23 (1993): 595-642.

[133] Philip Morse, *In at the Beginnings: A Physicist's Life* (Cambridge, Mass.: MIT Press, 1977).

[134] Philip Morse and George Kimball, *Methods of Operations Research* (Cambridge, Mass.: MIT Press, 1951).

would be far enough removed from the day-to-day concerns of the company to reflect upon the techniques and develop general theories of industrial organization. The project's avowed short-term goals were to develop the basic theoretical structure of OR in civilian industrial settings, and to begin training OR professionals. Morse believed that OR would soon find permanent applications throughout society, for CORE would "develop fundamental knowledge of a new technique of organizational control ... it is to be expected that Operations Research will be an integral part of city planning, direction of manufactures, distribution, and transport." OR even fit into the reconstruction of postwar Europe, for MIT's operations researchers found it "conceivable that applications will be found in the rationalization of the industries of other countries, in the spirit of the Marshall Plan and related ventures."[135]

The growth of industrial Operations Research at MIT coincided with the creation of the School of Industrial Management. MIT had begun to expand its pre-existing industrial management program into a separate school within the institute, though the members of this new school had not yet fully worked out how the school would fit with the rest of the institute, or how it would fit within the wider world of management education. Operations Research, which was being expanded from a purely military research program into a much larger industrial one, seemed to be a good fit for MIT's new School of Industrial Management. OR's technical bona fides could bring rigorous, quantitative analysis into business education and thereby make MIT's management school uniquely suited to the institute's engineering culture. Furthermore, MIT's operations researchers emphasized that it was an intrinsically interdisciplinary pursuit, which made it a useful bridge between the new school and the rest of the faculty in engineering, science, and the social sciences. Yet E. P. Brooks, the dean of the new school, was not initially interested in claiming ownership of MIT's OR efforts, leaving the management of OR to Philip Morse. Morse, a member of the physics department, pointed out that OR was an application of the principles and methodologies of the physical sciences to problems of industry, making it inherently interdisciplinary. OR became a conduit through which scientists at MIT began working on traditional management topics.[136]

At this point Operations Research was not intended to be a new discipline, but rather was a methodology common to several fields of science and engineering. Morse organized his program such that graduate students could remain affiliated with their primary departments while working on this shared interdisciplinary pursuit, meaning that the OR Center was not competing against established departments or laboratories for students. Morse continued to receive ONR funding for this program and MIT continued to contract for the Operations Evaluation Group. Naval support for civilian OR came on the condition that the ORC not interfere with the OEG contracts. Morse himself was no militarist, having signed anti-war proposals in 1940, and continuing to express fears that military contracting left unchecked might warp the mission of the university. This was a particularly acute problem at MIT, which received a disproportionately

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[135] Memorandum for Mr. N. McL. Sage, Philip Morse Papers, MIT Archives and Special Collections, MC75, box 2, folder "Institute Cmte on OR".

[136] Memo from Philip M. Morse to Dean Eli Shapiro, May 10, 1955, Morse Papers, box 10, folder "ORSA Corres. 1954-5;" memo from T. M. Hill to Dean Brooks, May 10, 1954 and Memo from P. M. Morse to T. M. Hill, May 14, 1954, Morse Papers, box 10, folder "Orsa Corres." MIT Institute Professor John D. C. Little was the first graduate student to make this transition, shifting his interests from physics to OR and eventually to marketing.

large amount of military support in the early years of the Cold War. Yet Morse also would not turn down support for OR just because it came from military sponsors.[137]

The Managed Individual

The "New Look" for administrative theory was not being created within a vacuum. These new managerial schools were being created circa 1950, just as the Cold War was hardening. Managerial theories occupied a controversial ideological space in the early years of the Cold War. They were valuable to the extent that they improved industrial efficiency and administrative practices. However, some of these ideas about organization suggested that efficiency was located within social systems, rather than being the result of individual initiative. What was beneficial for industrial efficiency was not necessarily ideologically acceptable.

The creation of formal mathematical techniques for administration suggested that management was becoming a straightforward technical problem. Yet implementing these theories required greater computational power than was generally available. With the continued development of mathematical machines it seemed that "it is absolutely certain that if these rules are followed, they will lead to the best possible program, and it will be perfectly clear when the best possible program has been found. It is because the procedure follows definite rules that it can be taught to clerical personnel or handed over to automatic computers."[138] With management reduced to an exercise in optimization, there was no room left for the sort of ideological commitments that had marked the pre-war decades—part of a drift towards technocracy in the 1950s that Daniel Bell described as an "end of ideology."[139]

The seeming neutrality of administrative science surreptitiously distributed the values of the management throughout the organization, according to C. Wright Mills. Instead of relying upon aggressive, competitive personalities to lead large corporations, administrative bureaucracies internalized those traits and made them their governing principles. The individual men and women doing administrative work did not need to be aggressive themselves so long as the corporation was organized to produce those outcomes; "the men are cogs in a business machinery that has routinized greed and made aggression an impersonal principle of organization."[140] According to Mills, managers and their administrations "are not experts in charge of technology; they are executors of property."[141]

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[137] "Peace Resolution," spring 1940, Morse Papers, box 5, folder "American Association of Scientific Workers;" letter to Leslie Simon, June 12, 1952, Morse Papers, box 5, folder "Advisory Committee for the Ordnance Research and Development Division."

[138] Alexander Henderson and Robert Schlaifer, "Mathematical Programming: Better Information for Better Decision Making," *Harvard Business Review* 32 (1954): 73-100, on 75.

[139] Daniel Bell, *The End of Ideology: On the Exhaustion of Political Ideas in the Fifties* (Glencoe, Ill.: Free Press, 1960).

[140] C. Wright Mills, *White Collar: The American Middle Class* (New York: Oxford University Press, 2001), 109.

[141] Ibid., 103.

The general thrust of these arguments was that captains of industry had been self-directed men. They built corporations through their own initiative and shaped the world, for good or ill, according to their own vision. By contrast, individuals within modern organizations pursued their organizational goals without question, and operated strictly within prescribed boundaries. The image of the "organization man" became a common stereotype of the 1950s. In the influential study, *The Lonely Crowd*, sociologist David Riesman identified a widespread transition from an "inner-directedness" that characterized the entrepreneurial captains of industry to an "other-directedness" in which an individual's values were based upon the signals picked up from those around him, rather than from within. These were model subordinates with a potentially revolutionary appreciation for community, but they lacked the critical faculties and sense of personal responsibility of inner-directed men.[142]

The new management education was implicated in the increase of direction-following, other-directed organization men. Martin Bronfenbrenner, a visiting professor at the GSIA in 1955, recorded his thoughts about the program and its students for Bach: "The students seem disinterested in public policy issues, as compared to the Chicago or Wisconsin men. They are not, like most commerce students, aggressively pro-business; rather, like most engineering students, they just don't give a damn."[143] Robert Trueblood of the accounting firm Touche Ross told an audience at the GSIA in 1960 that there "is a tendency for GSIA men to be 'one-way directed' in their communication. To put it simply, we have found that most GSIA people communicate easily upwards—that is, with their superiors. They tend, however, not to communicate as effectively or as well with their peers or their subordinates."[144] Not only did the new administrative theories seem to depersonalize management, the new large administrations also seemed to cultivate a distinct personality type that was defined by its lack of individual will.

Kenneth Boulding, an economic polymath and winner of the 1949 John Bates Clark Medal, framed his ambivalence about the increasingly technical orientation of American business education in the language of the Cold War. In 1959 he contrasted "the market system" and "the budget system," while noting with some concern America's increasing movement toward the budget system. Recognizing that a pure market system was unworkable and that some degree of planning and budgeting was essential, Boulding claimed that "the capitalist societies drew the right inferences from the Marxist criticism and learned the right lessons from Marx, whereas the communist societies learned mainly the wrong things from him!"[145] Planning in the American economy was due to a "market-justified budget," which let the state correct for the inevitable failures of the market, and let corporations plan their internal operations. Yet Boulding worried about the allure of the budget system.

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[142] David Riesman with Reuel Denney and Nathan Glazer, *The Lonely Crowd* (New Haven: Yale University Press, 1950); William H. Whyte, *The Organization Man* (New York: Simon and Schuster, 1956). For an overview of social thought on this subject, see Howard Brick, *Transcending Capitalism: Visions of a New Society in Modern American Thought* (Ithaca, NY: Cornell University Press, 2006), 152-185.

[143] Gleeson, 214-216.

[144] Robert Trueblood, "An Employer Looks at GSIA," 1960. GSIA Records, box 4.

[145] Kenneth E. Boulding, "Symbols for Capitalism," *Harvard Business Review* 37:1 (Jan., 1959), 41-48, on 46.

> No matter how successful the market is in extending freedom and in lessening frustration, still nobody loves it … By contrast, the budget acquires a vicarious charisma from the organization which it coordinates. Organizations are superhuman, if not divine. They represent a power beyond that of the individual; they become colossi which throw bridges across straits and rockets into space. They attach to themselves the great virtues of loyalty, devotion, and self-sacrifice. They are watered with the blood of martyrs and nourished with the flesh of heroes. Our attitude toward them is deeply ambivalent; they destroy us and elevate us at the same time … It is little wonder that socialism has stirred the hearts of men as capitalism has not, and that it has filled the minds of men with its bright but deceitful dreams of the future.[146]

Even if the mathematical planning of OR was an acceptable form of the "market-justified budget," Boulding feared that long-range planning would be extended further into American society. The technocratic, utopian lure of planning and budgeting would ultimately restrict the freedom that it claimed to provide. As Boulding and others noted, echoing Schumpeter, the lure of planning was strongest for intellectuals and socially-minded individuals—precisely those that capitalism could least afford to alienate.[147] In this atmosphere of pessimism, Margaret Mead urged business leaders to think big and show that business could be an engine for social growth rather than being just a career.[148]

Following from David Riesman's *The Lonely Crowd*, Ted Levitt, a management theorist known for his work on marketing, argued that "the gradual elimination of the nineteenth-century economic prototype—in his most advanced form, the Horatio Alger captain of industry—will result in the atrophy of the capitalist spirit, with all that this implies about the future of capitalist society."[149] Levitt saw the entrepreneurial spirit of capitalism being replaced by a pathological fixation on stability. The bold captain of industry was being replaced by the cautious systems analyst, and creativity was being replaced by miniscule increases in the efficiency of existing business processes. Rather than being a spur to innovation, risk was being viewed as something to be eliminated. Business leaders were being trained as technicians rather than learning how to make large decisions under uncertainty. William Given, assessing the role of engineers in management for the *Harvard Business Review*, found a lower success rate than he expected.[150] Some claimed that they lacked the personality to manage people, while others claimed that their technocratic tendencies led them to technical fixes for all problems.[151]

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[146] Ibid., 47.

[147] Calvin Bryce Hoover, "Can Capitalism Win the Intellectuals?" *Harvard Business Review* 37:5 (Sep., 1959), 47-54.

[148] Margaret Mead, "Must Capitalism Crawl?" *Harvard Business Review* 40:6 (Nov., 1962), 8.

[149] Theodore Levitt, "The Changing Character of Capitalism," *Harvard Business Review* 34:4 (July, 1956), 37-47.

[150] William B. Given, Jr., "The Engineer Goes Into Management," *Harvard Business Review* 33:1 (Jan., 1955), 43-52.

[151] Frederic E. Pamp, Jr., "Liberal Arts as Training for Business," *Harvard Business Review* 33:3 (May, 1955), 42-50.

The critique of planning from laissez-faire capitalists was matched by a critique focused on social values. Citing both contemporary sociology and theology, Alvin Pitcher, a theologian at the University of Chicago and civil rights activist, urged restraint against "the great god production … surrounded in the pantheon of lesser deities of efficiency, competence, science, technical education, consumption, and advertising."[152] Pitcher longed for a society that consciously considered the welfare of individuals, rather than using efficiency as its sole criterion of value. Management theories developed in tandem with new mathematical techniques and computing machines, and both of these were implicated in the critiques of 1950s culture coming from social critics on both the political right and left.

Conclusion

While administrative theorists, such as Simon, believed that their scientific approach avoided the specter of "technocracy," their approach was not always interpreted so charitably. Allegations of technocracy persisted. Building a science of administration meant being concerned solely with the development of means rather than ends. This did not imply that managers were unconcerned with the question of ends or of values, only that such concerns did not fall within the scope of the science of administration. Simon maintained that the problem was not that the sciences were being imperialistic, but that critics asked more of the science than the scientists were capable of providing from their limited expertise.

The new management schools of the 1950s were motivated by the possibility of creating sciences of administration. Their goals were to train the future leaders of large industrial concerns by giving them the tools to analyze and rationally reconstruct industrial processes, manage labor, and understand the relationship of the firm to the wider economy. Through the influence of the systems approaches, the students within these programs were trained to think about individual firms as being embedded within larger economic and social structures. The science of administration dealt with how to efficiently achieve goals, not with the question of how to identify ultimate ends. Even as the individuals trained within these programs improved the operations of the firms in which they were employed, the suspicion remained that their style of management was too concerned with achieving pre-existing goals rather than actively setting the ends to be met.

The central problem for administrative theory was how to organize the human and mechanical elements of the firm in such a way as to create collective action, and furthermore to do so in a way that efficiently and effectively achieved certain ends. Due to their relationships with engineering schools and technical disciplines, these new management schools were interested in understanding the function of machinery in administrative processes. Social scientists had long been involved with administrative machines, such as the Hollerith tabulators used for the 1890 census. The new management scientists of the 1950s were well positioned to join computer research projects occurring within the universities, as the next chapter will discuss. Management science not only contributed to the technical content of the machines, but also to their social meanings; the public reception of computation would become largely

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[152] Alvin Pitcher, "The Importance of Being Human," *Harvard Business Review* 39:1 (Jan., 1961), 41-48.

indistinguishable from the critiques of administration. Even as the developments of computing machines and administrative theories mutually reinforced each other, so too would the suspicions of administration create a problem for the image of computing machines.

Chapter 2: The Logic of the Office

The growth of administration as an organizational form was closely related to the development of technologies to facilitate office work. Management science sought to discover the rules that allowed organizations to behave efficiently in order to make even the most complex organization run smoothly. The closely related field of industrial engineering likewise sought to simplify complex industrial processes by breaking them down into their constituent parts and to create a new division of labor between men and machines that would be more efficient and more economical.

After World War II (due in part to specific wartime technologies, but primarily to long-term developments in industry) these two disciplines became implicated in an even larger narrative concerning the role of humans vis-à-vis machines. Industrial management had raised the fear of humans being treated as machines, but such fears could only go so far; it was clear in the mid-century that many forms of work remained far from being mechanizable. The individuals who experienced the effects of mechanization most directly were those workers who remained on the margins. Higher status work was defined as that which remained beyond the capacity of machines.

Amidst widespread concerns about automation, many observers in the immediate postwar years believed that mental work remained safe. The growth of management science also suggested that such mental work would become more important in the years to come. Some simple decisions could be automated and some mathematical work could be done by machine, but most was left for humans. Yet two trajectories suggested that this state of affairs might change. First, the analyses of scientific management began to break complicated forms of mental work into simpler processes, as explained in the previous chapter. Second, new technologies, including computers, were being adapted to do more complex forms of work. As with the techniques of management, the techniques of computing were built upon theoretical understandings of the nature of computation and about the usefulness of the rational reorganization of mental work, and were developed in institutions dedicated to expanding the domain of the computable.

Thematic Issues

The stakes of this history were high. One of the major challenges facing industrial relations was the issue of trust, particularly because industrial work was increasingly done by working-class immigrants. Suspicion of “soldiering” motivated Frederick Taylor to eliminate worker discretion over his work processes. Henry Ford had required background investigations before workers could be eligible for the “$5 Day,” as explained in the previous chapter. Trust became even more of a concern as industrial engineers designed ever more elaborate systems for organizing industrial production. Machines were more reliable in the sense that they operated in precisely the same way all the time, but they were unable to exercise the situational judgment that allowed skilled workers to adapt to unforeseen problems. Automation seemed to entail a lack of robustness and flexibility.[1]

There were two ways to respond to this problem. Either automatic systems could build in feedback loops to allow the possibility of adapting to changes in their environments, or else a cadre of technicians could monitor the machines—acknowledging the importance of situational judgment but moving it to a localized office, higher within the organization. These strategies were compatible and many organizations pursued them both. These responses highlight the complex relationship between reliability (measured in terms of trust and in terms of regularity) and the exercise of independent judgment (an irregular process built upon trust), which mattered for workers facing the threat of unemployment as well as for the system designers.

These dynamics also point to a development in computing that will play out in the next two chapters. Automated systems and early computers were both designed to be "stupid" in a very specific way. They did what they were designed to do because they lacked the ability to do otherwise. While the final outputs of these systems could be marvelously complicated, their operations were deemed to be very simple: in the case of computers, little more than arithmetic and Boolean logic (the familiar system of AND, OR, and NOT). In this sense, computerization was part of a larger project of using modern technology to impose a straightforward legibility upon the world. This argument has been developed most clearly for the case of computers by Paul Edwards in *The Closed World*, though scholars have identified these tendencies as being part of modern science as far back as the Scientific Revolution.[2]

And yet, as several strands of twentieth-century thought have noted, this imposition of uniformity and universal order inevitably fails to account for many important features of a world that demands to be interpreted.[3] While the basic agenda of computer design in the 1940s and '50s retained its top-down structure, the only way to accommodate increasingly complex problems and greater computational demands was to continue tacking on additional capabilities. The result became increasingly baroque programs, whose internal complexity defied the original imperative that computing make the world legible. But this remains for later chapters.

Organization

This chapter begins with some observations on the current state of the history of computing and then takes a fresh look at familiar material: the origins of the computer, from organized mathematical work done by human computers through the wartime projects in Cambridge, Massachusetts and in Philadelphia that led to the Mark I and ENIAC machines. A brief survey of this familiar terrain shows that the questions of reliability and the distribution of

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[1] On the fragility of tightly coupled systems, see the work of organization theorist Charles Perrow, including *Normal Accidents: Living With High Risk Technologies* (Princeton, NJ: Princeton University Press, 1999).

[2] See, for example, Yaron Ezrahi, *The Descent of Icarus: Science and the Transformation of Contemporary Democracy* (Cambridge, Mass.: Harvard University Press, 1990).

[3] See James C. Scott, *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed* (New Haven, Conn.: Yale University Press, 1999).

skill pervade this work, and suggest that focusing on the wartime nexus of applied mathematics, electrical engineering, and easy money distracts us from a much longer history that situates these issues within the heart of business management and public administration. The military was a vital patron of computing, but an accidental one. A different set of concerns—not necessarily militaristic—had created the conditions for the computing revolution of the 1940s.

Indeed, in the years immediately following the war it was a group of spokesmen from the world of mathematically oriented management science who most strongly shaped the public perceptions of computing. Individuals such as the actuary/consultant/science writer Edmund Berkeley take on a greater significance for creating professional networks and for outlining a powerful vision for the future of computing. That the discourse of computing should be so heavily influenced by questions of labor and rational administration should no longer be surprising once we shift our attention to its industrial basis.

Familiar faces and problems from the history of computing take on new significances. Cybernetics is therefore important for articulating the dangers of taking the man-as-machine trope too far, and not only for being a science of holism that entranced the counterculture. The hoary question of whether computer architecture could shed light on the architecture of the brain, while important to the history of cognitive science, becomes important for the related problem of whether computers could be built to automatically do complex or creative intellectual work.[4]

This reframing of the major developments in computing makes clear the connections between the project of industrial automation and the project of computing. The possibilities of automation extended into the office as computing technology became more complex. Office automation created the same set of fears as industrial automation, but the recognition that this threat was to cognitive work amplified these concerns. The chapter concludes with some remarks on the state of computing historiography and where it might go from here.

Writing the History of Computers

The history of computers has been unabashedly Whiggish as early computing pioneers described their machines as the logical culmination of centuries of theorizing—specifically about mathematics and thought. A second strand of this history has emphasized its connections to calculating machines. Early computer promoters consciously tied their new machines to existing traditions of mathematics and philosophy, or to accounting practices, while emphasizing what made these machines a sharp break from the past.

Writing about computing has always been self-conscious about its historical position, and the mythologies that have arisen around computers have been an integral part of what computers are, no less than their hardware and software. Early histories of computing constantly looked back in order to legitimate the new discipline and connect with narratives of technological progress, while simultaneously looking ahead with claims about radical breakthroughs that were always just on the horizon. The idea of computation as such has helped create a shared language for the diverse scientific and technological communities working on various aspects of

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[4] Tara Abraham, "Cybernetics and Theoretical Approaches to 20th Century Brain and Behavior Sciences," *Biological Theory* 1 (2006): 418-422.

computing. Computation in the 1950s was not just about working with a particular set of machines; it was fundamentally about enacting a vision of the future. Studies of computing contributed to this project by connecting technical work to ongoing social concerns.

These Whig narratives suffer from the familiar shortcomings of that genre of historical writing. Yet the "machine-centric" history, the genre pioneered by the earliest computer researchers themselves, almost requires the teleological approach. By making the appearance of the first computers the crucial event of this history, the narrative is easily structured in terms of a prehistory that leads up to a crucial moment in the 1940s, and a subsequent history in which scientific computing, data processing, and theories of computation can each follow their own particular paths.[5]

Historian Michael Mahoney reminds us that these narratives treat the marriage of electronics and mathematics as a natural development culminating in modern computing, and therefore miss the many contingent aspects of this union. He instead suggests following the histories of these fields as they intersect with computers.[6] By highlighting the tangled and highly contingent relationships among the earliest computers and their many obvious antecedents—mechanical, intellectual, or social—Mahoney makes a convincing case for why the development of computers simply cannot be described in terms of a straightforward, linear development of any one of these strands. He suggests a far more complex picture, in which several strands of intellectual and social life interact in complex ways during the 1940s and 1950s in the design of the computer.

The machine-centric history fuses the mathematical and engineering stories, breathing the spirit of Babbage into the metal frame sculpted by Thomas Watson's boys. It raises the question of how this marriage was brokered. The answer, as usual, is money. And not just any money. Support came straight from the state, and more specifically, from the military—particularly filthy lucre for intellectuals. Aside from bringing the lofty ideational story crashing down to earth, the military-centric history has the benefit of answering the question of why these elements came together *at that moment*. The task of producing accurate and comprehensive firing tables, with the strong material support of the military, brought together the expertise in engineering and in mathematics that allowed the early computers to be built. Treating the patrons of computing as

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[5] Perhaps the most honest assessment of the history of computing from the perspective of practicing scientists came from Nicholas Metropolis and Gian-Carlo Rota, who described the "secret boredom" that accompanied the study of the machines and ideas of Babbage, Leibniz, and others. "Couldn't [the history of computing] be honorably replaced by a compact commemorative plaque listing in gilded letters the names of the pioneers who made the computer age possible, God bless their souls?" The accepted idea of the history of computing meant the recitation of intellectual milestones through the centuries. Writing in the early 1980s, when computers were becoming ubiquitous, they concluded that a critical history of these machines and ideas was vital to counter what they saw as an excessive fascination with novel machines and a concurrent decline of creative, critical thinking and a weakening of social bonds. See the preface and introduction to N. Metropolis, J. Howlett, and Gian-Carlo Rota, eds., *A History of Computing in the Twentieth Century* (New York: Academic Press, 1980). On the association between computer culture and individualism circa 1980, see the conclusion of this dissertation.

[6] Michael S. Mahoney, "The Structures of Computation," in *The First Computers—History and Architectures*, ed. Raúl Rojas and Ulf Hashagen (Cambridge, Mass.: MIT Press, 2000), 17-32.

important actors in this narrative keeps the importance of politics and institution-building up front. This has been a powerful approach in writing the history of computing.

If our primary goal is simply to narrate the creation of the earliest computers, this triadic structure of math, machines, and money does the job. It fails, however, to adequately explain how computing expanded from its limited origins to become a wide-ranging project, extending deep into science patronage agencies and into private industry, with independent academic programs and professional associations, and inspiring iconic cultural references. To be sure, the traditional narrative explains how key scientists (including the formidable John von Neumann) emerged from the war with a keen interest in computing, and how patrons continued to support the development of these machines. These are important.

But they do not go far enough. This focus on the personal connections forged within these wartime projects must be complemented with a serious study of how computing has been contextualized within a broader discourse of work and mental labor. This means once again treating the work of the ENIAC and Mark I as large-scale organized mathematics, rather than subordinating the work of these laboratories to a reified set of militarist priorities. This means refusing to get involved in the question of whether mathematics or engineering is more fundamental, but instead recognizing the stakes of these arguments in terms of institutional power. This means attending both to how individuals are using machines and how they are talking about them, and paying particular attention to the differences between these modalities.

Against these tendencies, Mahoney proposed *de*centering the computer by downplaying the importance of individual machines. “The computer is not a single device but a schema. It is indefinite. It can do anything for which we can give it instructions, but in itself it does nothing,” he argued. An obsession with machines has prevented us from properly recognizing the social context of computing. He observed that “the kinds of computers we have designed since 1945 and the kinds of programs we have written for them reflect not the nature of the computer but the purposes and aspirations of the groups of people who made those designs and wrote those programs, and the product of their work reflects not the history of the computer but the histories of those groups, even as the computer in many cases fundamentally redirected the course of those histories.”[7] Therefore, a more sophisticated historical understanding of computing would situate the various forms of computing in terms of the histories of the various communities who interact with computers. Such a history would respect the concerns of the actors on their own terms without forcing a retrospective coherence upon them. Mahoney’s proposal shifts from *a* history of machines to *multiple* disciplinary histories.[8] Yet this strategy makes the idea of computation so protean that it vanishes as a distinct concept.

Can we dispense with “computing” as an organizing concept? Are the many histories of computing so distinct that they must be kept separate? Even if computing could never be easily defined as a particular set of practices, it remained a coherent ideal indicating a broad consensus on ideas ranging from positivism to cognitivism and directly structuring computer research. Different computing communities used machines for different purposes, but they did maintain

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[7] Ibid., 18.

[8] Michael S. Mahoney, “The Histories of Computing(s),” *Interdisciplinary Science Reviews* 30 (2005): 119-135.

that some more fundamental theory of computing somehow explained or made possible their own individual projects. While we cannot remain satisfied with the Whig narrative of computer development, we also cannot ignore how the many overlapping concepts of "computing" have collectively structured computer research.

Avoiding both the oversimplification of machine-centrism and the fragmentation of disciplinary histories, this chapter presents a polycentric, networked history, in which competing centers of influence construct a shared discourse of computing, fusing a system for the division of mathematical work to a positivistic, deductive process of discovery, built upon a foundation of electronics. In this polycentric history the laboratories designing machines continue to matter, of course, but so too do the factories and boardrooms in which the relationship between labor and automation is worked out, and so too do the musings of theoreticians pondering the ultimate social significance of computing technologies. These diverse groups of actors were unified by a shared interest in uncovering the limits of "procedural rationality"—the notion that discovering and following proper rules was a guarantee of reliable and correct behavior, and, crucially, something that applied equally to machines, humans, and organizations.[9] Mahoney was correct that the computer's protean nature makes it fit into different contexts in different ways. However, it is the very idea of this universality that is at issue in this chapter.

The result is a history of computing that opens up new problems rather than answering old ones. The important issue of reliability is therefore not only a question of building better algorithms but of recognizing what is necessary, in each historical moment, to win the trust of the many interested publics. "Thinking machines" generated answers to well-posed questions, but without evincing the wisdom of human experts (whose cultural authority was becoming more tenuous in later years). Understanding machine intelligence therefore is not only about the similarities between minds and machines, but about recognizing how the literalness and interpretive inflexibility of computer programs could be marshaled as evidence for the overall intelligence and flexibility of computer systems. Different groups of computer developers and computer users disagreed on the specifics, but the evolution of debates on these points reflected the influence of many different constituencies.

Programs for Human Computers

Before turning to the moment in World War II when the major computing machines were built, an episode in organized mathematics showed the major concerns that shaped the emerging discourse of computing. This was part of a much longer history of organized computing work, ably explained by David Alan Grier in *When Computers were Human*.[10] In 1938, the Works

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[9] See Herbert A. Simon, "From Substantive to Procedural Rationality," in Spiro J. Latsis, ed., *Method and Appraisal in Economics* (New York: Cambridge University Press, 1976), 129-148.

[10] And a long history it is. As early as 1801, Gaspard Riche de Prony (inspired by reading about the division of labor in Adam Smith's *Wealth of Nations*) had used a team of unemployed Parisian hairdressers to calculate tables of logarithms. In 1822, Charles Babbage cited de Prony's work as an influence. No details of the organization of de Prony's project survive. See I. Grattan-Guinness, "Work for the Hairdressers: The Production of De Prony's Logarithmic and Trigonometric Tables," *IEEE Annals in the History of Computing* 12 (1990): 177-185. This section largely follows David Alan Grier, *When Computers Were Human* (Princeton: Princeton University Press, 2005).

Progress Administration (WPA) of the New Deal created a Mathematical Tables Project within the National Bureau of Standards, using large-scale mathematics work as a jobs program for the unemployed. The WPA supported scientific work as well as the infrastructural and cultural projects for which it remains best known. Science done under the auspices of the WPA tended to emphasize projects with large amounts of data collection and statistical analysis, and its Central Statistical Office worked with the National Academy of Sciences to identify the sorts of mathematical tables that would be of broad interest. Many of the human computers hired by the WPA had little formal mathematical education. The challenge for the directors of the Mathematical Tables Project was therefore to break down the computational work into tasks that were manageable for non-mathematicians, and to do so in a way that ensured accuracy.

Such tables were shortcuts to long, cumbersome calculations, and therefore the success of any given table depended upon being absolutely trustworthy. As the 19th century British astronomer John Herschel had put it, "an undetected error in a logarithmic table is like a sunken rock at sea yet undiscovered, upon which it is impossible to say what wrecks may have taken place."[11] Users of mathematical tables needed to have complete trust in the reliability of the numbers. Trust in the solutions depended on two variables: the trustworthiness of the overall program to generate numbers, and the trustworthiness of the individual calculators who crunched numbers. Likewise, trust in the method of calculation devolved into the issue of the trustworthiness of the project managers, who created these methods, and in the scientific patrons who vouched for the undertaking.

It is therefore significant that this project was not staffed by the nation's most prominent mathematicians, but rather was staffed by the unemployed. While a team of leading mathematicians would have had the immediate trust of the scientific community, the WPA project had to build the social foundation that allowed their calculations to be taken seriously. Mathematics as relief work could not attract any of the country's leading mathematicians. Instead, the WPA had to recruit from the margins of the mathematical community, settling on Arnold Lowan as its director—an immigrant from Romania, Jewish, trained as a chemical engineer, and teaching mathematics part-time in New York. Lowan recruited one of his students to take charge of day-to-day operations: Gertrude Blanch—also an immigrant (from Poland), and also Jewish, with a Ph.D. in mathematics from Cornell, yet underemployed as a clerical worker. Lowan and Blanch hired a team of six trained mathematicians, but the calculating work was given to the less-educated computers, who numbered several hundred by 1940. Blanch divided them into four groups, each assigned a specific arithmetic operation. Additional groups checked completed tables and constructed algorithms. Because these computers lacked formal training in mathematics, their instructions had to be made as explicit as possible.

The Mathematical Tables Project performed detailed and tedious calculations, and did so without compromising accuracy. Not only did complex procedures have to be broken down into individual arithmetic steps, with a fully specified sequence, these also had to be scrupulously checked. This meant doing a lot of math. Yet only the leaders of the project fully understood the math. The work done by calculators drawn from the ranks of the unemployed could be trusted

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[11] Quoted in Martin Campbell-Kelly and William Aspray, *Computer: A History of the Information Machine* (Boulder, Colo.: Westview Press, 2004), 8.

because they were only doing basic arithmetic. For example, negative numbers were written in red while positive numbers were written in black, with instructions on when to switch from one color to the other. A sign posted on the wall in group 1, dedicated to addition, instructed workers on the basics of their task:

> Black plus black is black.
> Red plus red is red.
> Black plus red or red plus black, hand the sheets to group 2.[12]

Through this detailed organization of mathematical work, even unskilled calculators could produce accurate mathematical tables, provided they were organized properly.

Lowan advertised the project among mathematicians and received some interest from two influential physicists studying machine computation: John von Neumann and Philip Morse. Morse helped the Mathematical Tables Project perform its first scientific calculation, using models constructed by the physicist Hans Bethe to determine the internal temperature of the sun. This calculation was published in 1940, followed shortly thereafter by the Project's *Tables of the Exponential Function* in 1941. These two results confirmed the significance of the Tables Project, and the capability of a well-organized team of workers (few of whom were individually proficient in mathematics) to do complex mathematical work.[13] By the end of 1941, with military concerns becoming more important than work relief, Lowan asked Morse to use his contacts to bring the Tables Project to the attention of the military. This attempt foundered on both the disreputability of the work relief aspect of the Project, and on the suspicion that Blanch and Lowan harbored Communist sympathies. Even though the Mathematical Tables Project performed useful calculations for the Navy's LORAN (long-range navigation) project in 1942, the Navy cited security concerns in deciding not to award it any more contracts. The Project had demonstrated that it could produce accurate and complete tables, and that these tables had scientific value. However, it could not entirely escape the aura of marginality that clung to its leaders (two Eastern European Jews, and one of them female), its workers (drawn from the unemployed), and its position as work relief.

That fall, a reorganization of the National Defense Research Committee created an Applied Mathematics Panel (AMP) led by Warren Weaver. Weaver expressed interest in the computational laboratories around the country, but found the Tables Project to have a unique expertise in managing large-scale computing projects. Ultimately, Weaver and Lowan devised a plan in spring 1943, moving the Mathematical Tables Project into the National Bureau of Standards, where it could contract for the AMP. This restructured project would be far smaller, but its computers would all have access to modern adding machines and mechanical calculators.[14] After the war, the Mathematical Tables Project became more closely integrated into the heart of the National Bureau of Standards, moving to Washington and losing its independence. The Project's informal supporters within the scientific establishment, John von Neumann (whose support had always been tepid) and Philip Morse (who had been far more

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[12] Grier, *When Computers Were Human*, 214.

[13] Grier, 218-219.

[14] Ibid., 253-255.

active in connecting Lowan with the administrators of government science programs) both foresaw that electronic computers would replace human ones, and when they failed to protect the Mathematical Tables Project as an independent entity, Lowan finally shut it down on September 30, 1949.[15]

Computing in Cambridge

The same issues of trust and legibility appeared in the major wartime projects. Howard Aiken's work on the Mark I has been described well by historian I. Bernard Cohen. Aiken had described his ideas as being motivated by Babbage's work (his original impulse was to avoid doing laborious calculations for his physics thesis), and IBM—already one of the major office machine companies—handled the actual design work.[16] In other words, Aiken's interest in the machine, as its user, was in having a machine do repetitive and tedious work, while IBM took an interest in the machine itself, due to its expertise in machine methods. Aiken had first turned to the Monroe Calculating Machine Company in 1937. Monroe's chief engineer, George Chase, strongly supported the project. According to Aiken, Chase "also foresaw what I did not. I did not foresee the application to accounting as coming out of it, and he did."[17] However, Chase could not persuade the management of Monroe to sign on, and so he encouraged Aiken to turn to IBM instead. After a series of negotiations, Harvard and IBM signed a contract for a machine on March 31, 1939, each party staking out its turf in computing. This device, known by IBM as the Automatic Sequence Controlled Calculator, and by Aiken (and the rest of the world) as his "Mark I" machine, would be used at Harvard strictly for scientific work, and the technical knowledge gained in building the machine would remain the property of IBM.[18]

As IBM did not foresee any immediate commercial applications of these machines, IBM President Thomas J. Watson instead asked for acknowledgement and publicity from Harvard to mark the significance of this new machine and for making it available to Harvard's scientists, and to foster closer ties between the two organizations in recognition of the intellectual work done at IBM. Yet media coverage of the event almost entirely neglected IBM's contribution to the creation of the Mark I, to Watson's great consternation.[19]

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[15] Ibid., 299; Philip Morse, *In at the Beginnings: A Physicist's Life* (Cambridge, Mass.: MIT Press, 1977), 274.

[16] On the problematic nature of this claim, see below.

[17] I. Bernard Cohen, *Howard Aiken: Portrait of a Computer Pioneer* (Cambridge, Mass.: MIT Press, 1999), 42.

[18] Ibid., 85.

[19] Charles Bashe, "Constructing the IBM ASCC (Harvard Mark I)," in *Makin' Numbers: Howard Aiken and the Mark I Computer* (Cambridge, Mass.: MIT Press, 1999), I. Bernard Cohen and Gregory W. Welch, eds., 65-75. For a personal account of how Aiken viewed his alumni who moved to IBM as traitors, see Frederick Brooks Jr., "Aiken and the Harvard 'Comp Lab,'" in *Makin' Numbers*, 137-142. Also note L. J. Comrie's review in *Nature* of the published Mark I operation manual: "One notes with astonishment, however, the significant omission of 'I.B.M.' in the title and in Prof. Aiken's preface…" in L. J. Comrie, "Babbage's Dream Come True," *Nature* 158 (1946): 567-568.

The Mark I had a remarkable ability to perform repetitive calculations quickly and accurately, which impressed those who needed to do large-scale data processing. This was the same reason that many academics failed to find it significant at all. At the dedication of the Mark I on August 7, 1944, reporters marveled at this “thinking” machine, though an editorial in the *New York Times* offered a dissenting vision of it as “the perfect scholar in a fascist state, answering questions but incapable of asking them.”[20] The prevailing tone among Harvard’s administrators (including the university’s president, James Conant) concerned the limitations of this new machine, rather than its great possibilities.

Even Aiken viewed his machine as important primarily for doing large-scale mathematics. While the Mark I could solve many kinds of problems, Aiken began printing tables of Bessel functions, earning the machine the nickname of “Bessie.” In time, Aiken’s Computation Lab became excessively insular, driven by Aiken’s preoccupation with table-making and neglecting the more diverse influences beginning to pull the computer in new directions.[21]

The Mark I’s first calculations were for the military, but this was because the most pressing problems of the early 1940s concerned the progress of World War II. Aiken had been called to service as a Naval Reservist in 1941, teaching at the Naval Mine Warfare School in Virginia. From here, he worked tirelessly to convince the Navy that his machine, under construction at IBM, could have applications for the war. Eventually he convinced the Navy’s leadership, who rented the Computation Lab from Harvard and put it under the jurisdiction of the Navy’s Bureau of Ships. In August 1944, the lab hosted a visit by the mathematician John von Neumann, who wanted solutions to certain partial differential equations describing compression waves moving inward from the surface of a sphere—implosion. It was only a year later, after the detonation of the atomic bombs over Japan, that the lab members learned what these calculations were for.

The significant contributions for the development of computing from Aiken’s project were incidental, growing out of the experience of doing routine work on the machine. Several naval officers and reservists with mathematical training joined the lab. These staffers, including Grace Hopper, Richard Bloch, and Robert Campbell, were the ones in charge of programming the machine. As they created coded instructions on tape, they realized that similar patterns of instructions recurred, and these stretches of tape were kept aside in libraries of subroutines.[22]

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[20] Quoted in Cohen, *Howard Aiken*, 125.

[21] Gregory Welch and Adam Rabb Cohen, “Aiken’s Program in a Harvard Setting,” in *Makin’ Numbers*, 163-181. Garrett Birkhoff recalled suggesting to Aiken in 1944 that his machine would “put the WPA tables out of business.” See Garrett Birkhoff, “Computing Developments, Cambridge, U.S.A.,” in *A History of Computing in the Twentieth Century*, 21-30.

[22] See Richard Bloch, “Programming Mark I,” in *Makin’ Numbers*, 77-109.

Computing in Philadelphia

The same issues arose in the case of the ENIAC, developed at the University of Pennsylvania's Moore School of Engineering by J. Presper Eckert and John Mauchly, working under contract for the Ballistics Research Laboratory at Aberdeen Proving Ground. The motivation for creating the ENIAC, as with other early machines, was the need to accurately perform repetitive calculations. The assistant director of the BRL in the early 1940s, Paul Gillon, recognized that the military needed more computational power to produce accurate firing tables for anti-aircraft gunners. The basic engineering problem was that gunners had to translate the location of their targets into coordinates for their guns, taking into account not only basic geometry, but also additional factors such as wind speed, temperature, humidity, and the motion of the target. Gillon approached the University of Pennsylvania, and the mathematician (and Reservist) Herman Goldstine began overseeing work there in September 1942. His wife, Adele Goldstine, recruited human calculators from the Women's Army Corps (WACs).

Two engineers at the Moore School, John Mauchly and J. Presper Eckert, were designing a machine to do rapid calculations. The seed of their project had been a conversation in 1940 between Mauchly and John Atanasoff, a physicist in Iowa who was working on a machine of his own.[23] Mauchly further developed the idea and sold Herman Goldstine on it, bringing a talented engineer, Eckert, onto the project. Eckert, Mauchly, and John Grist Brainerd of the Moore School began discussing a contract with Gillon in early April 1943, and signed a contract with the BRL on June 5, 1943. Gillon named this machine the Electronic Numerical Integrator And Computer, or ENIAC. This machine, like the Mark I, was the result of individuals with an interest in large-scale data processing getting together with a scientist interested in speeding up his own calculations.

Support for the ENIAC was controversial within the Ordnance Department, primarily because of the unreliability of the machine's electronic components. According to Goldstine, "Eckert fully understood at the start, as perhaps none of his colleagues did, that the overall success of the project was to depend entirely on a new concept of component reliability and on utmost care in setting up criteria for everything from quality of insulation to types of tubes." In order to run continuously for twelve hours without failure, the ENIAC's components needed to be more reliable than anything ever built, with failure rates of less than one in 10^{14} operations.[24] This was not only a matter of designing better components. Goldstine's point was that the complexity of the machine, and the fact that it used electronic components rather than mechanical ones, meant that each part had to be completely reliable. No human operator could verify that every component worked properly, nor could anyone automatically judge that the machine's outputs were correct. The only way to trust a machine as novel as the ENIAC was to have absolute faith in the workings of its individual components.

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[23] This point became the core of the case *Honeywell v. Sperry Rand* that declared Atanasoff the inventor of The Computer. See John W. Mauchly, "The ENIAC," in *A History of Computing in the Twentieth-Century*, 541-550. An account of the trial by a participant in the early research is Alice Rowe Burks, *Who Invented the Computer?: The Legal Battle that Changed Computing History* (New York: Prometheus, 2003).

[24] Herman H. Goldstine, *The Computer from Pascal to von Neumann* (Princeton: Princeton University Press, 1972), 153-154.

As with the Mark I, programmability for the ENIAC was an afterthought that only developed later through the experience of working with the machine. The problem was that programming the machine required rewiring it and setting switches and dials—essentially rebuilding the machine every time it ran a new procedure. The machine's speed could compensate for the time-consuming work of programming over several runs, but as early as 1943 Eckert and Mauchly recognized that the inability to easily program the ENIAC was a significant shortcoming. A progress report on December 31 described how "no attempt [had] been made to make provision for setting up a problem automatically. This is for the sake of simplicity and because it is anticipated that the ENIAC will be used primarily for problems of a type in which one setup will be used many times before another problem is placed on the machine."[25] Producing firing tables was one form of repetitive work, and the large-scale data processing that they envisioned for post-war industry would be similarly repetitive. Goldstine hired a mathematician, John Holberton, to direct the programming work, with a staff of six female computers.[26]

The reinterpretation of the ENIAC as a contribution to mathematics and to general intellectual life was driven by this question of reliability. While Eckert had been primarily concerned with the reliability of machine components, John von Neumann, intellectual polymath, used abstract design principles to decouple the question of the reliability of the machine design from the question of the reliability of the notoriously unreliable vacuum tubes. John von Neumann arrived at the Moore School in 1944, the result of a chance conversation with Goldstine at the train station by Aberdeen Proving Ground. Von Neumann spent the war as an advisor to the Navy Bureau of Ordnance, the Ballistics Research Laboratory, and the Los Alamos Laboratory, and was interested in testing the capabilities of the machines being built in Philadelphia and Cambridge. Distinct camps formed as von Neumann began to shift attention away from the technological features of the computer to its logical design. As Goldstine observed, "the group tended to split into the technologists—Eckert and Mauchly—and the logicians—von Neumann, Burks, and I. This was a perfectly natural division of labor, but the polarization was to become increasingly severe as time went on and was finally to disrupt the group."[27]

What von Neumann's project meant in practice was that design considerations would become driven by theoretical judgments. Eckert had seen the physical limitations of vacuum tubes as driving the overall design, while von Neumann wanted the form of particular components to be adapted to the top-down design (a point made even more explicitly in his highly influential EDVAC report).[28]

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[25] "ENIAC Progress Report," December 31, 1943, quoted in Nancy Stern, *From ENIAC to UNIVAC: An Appraisal of the Eckert-Mauchly Computers* (Bedford, Mass.: Digital Press, 1981), 75.

[26] Goldstine noted that four of these six married Moore School personnel, including Mauchly and Holberton. See Goldstine, 202. On the central role of women in the development of ENIAC, see Jennifer S. Light, "When Computers were Women," *Technology and Culture* 40 (1999): 455-483.

[27] Goldstine, 188.

[28] Von Neumann, "First Draft of a Report on the EDVAC," reprinted in Stern, *From ENIAC to UNIVAC*. J. Presper Eckert later claimed that describing the operations of the computer through the use of neuronal analogies

These different views of the ENIAC—Eckert's bottom-up machine in which overall reliability was determined by its weakest component versus von Neumann's top-down machine in which logical design was paramount—resurfaced in patent controversies. Sorting out competing claims to the ENIAC required making sharp judgments concerning what was important and fundamental. The two engineers on the project understood their machine to be a significant invention in its own right, while the logicians claimed that the truly significant innovation was the logical structure—something that no one had cared to define in the earliest stages of machine design. The two camps soon came into conflict, with each side questioning the importance of the other.

Interpreting the Wartime Machines

The great value of computing machines in 1945 was that they could do repetitive calculations very quickly. Nothing fancier than that. They could therefore easily be described in terms of a popular concept of the postwar years: automation. The word was first used by Delmore S. Harder, Vice President of Manufacturing at Ford Motor Company in 1946.[29] Harder originally meant it to describe the automatic handling of materials from one mechanical process to another. Ford, of course, had long been a pioneer in manufacturing, to the point that "Fordism" (along with Taylorism) had become synonymous with a particularly American style of manufacture. They were among America's most influential intellectual exports in the early twentieth century, as the previous chapter described.

Automation described a system of continuous flow that eliminated machine downtime and greatly reduced the need for human workers to run each individual process or move material between them. However, this kind of continuous flow was not useful or easy to implement for all industries. It worked well for very stable processes, in which production runs did not change much over time. This form of automation reduced downtime at the expense of simultaneously reducing flexibility.

While calculating machines performed math automatically, the connections between the architecture of these new machines and the architecture of automatic factories went deeper. Von Neumann's friend and fellow Hungarian physicist, Rudolf Ortvay, had sent him two letters in early 1941 suggesting this very point. According to Ortvay's letter of February 16, 1941:

> these days everybody is talking about organization, totality. Today's computing machines, automatic telephone exchanges, high-voltage equipment like cascade transformers as well as radio transmitter and receiver equipment, but also an industrial plant or an office are all technical examples of such organizations. I think there is a common element in all these which is capable of being axiomatized. I don't know if there has been an attempt in this direction? I am

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allowed von Neumann to circumvent the wartime secrecy that restricted his own ability to talk about the electronics. See J. Presper Eckert, "The ENIAC," in *A History of Computing in the Twentieth-Century*, 525-539, and J. Presper Eckert, oral history interview with Nancy Stern, 10/28/1977, Charles Babbage Institute, OH 13.

[29] See James R. Bright, *Automation and Management* (Cambridge, Mass.: Harvard University Graduate School of Business Administration, 1958), 4-5.

> interested in knowing this because I believe that if it is possible to sharply accentuate the essential elements relevant to the organization, as such, this would give an overview of the alternatives and would facilitate the understanding of such systems as, for instance, the brain.[30]

The analogy between brains and machines was a recurring motif, but Ortvay's letter to von Neumann suggested something even more important. Here was the idea that some language of organization could explain the workings of both biology and technology, and other things (such as social systems) too. The notion that something called "organization" made possible all natural and social systems was commonplace in the postwar years. As seen in the previous chapter, this was not a unique development of the postwar years—but after the war it gained additional traction.[31] Similar ideas of organization were expounded in postwar scientific management theories and in the scientific metalanguage of cybernetics. The idea that the development of computers had ramifications for organizing the social order, described below, was based on these varied approaches to understanding principles of organization.

"All the Language of Thought Will Be Calculable Like Mathematics": Edmund Berkeley and the Possibilities of Computing

One of the most influential and prominent advocates of the new computing machines came from the world of life insurance rather than from science or engineering. Edmund Callis Berkeley only spent ten months actually working on developing a major computing machine, during his wartime service at Howard Aiken's Computation Lab. However, Berkeley became a central figure in the field, particularly in making the technology applicable to a wide range of social problems. The individuals most closely tied to the first machines were in some ways *too* close to their machines to see the interest that they inspired among professional number crunchers.

Berkeley entered Harvard in 1926 and studied mathematics, which he enjoyed even though he did not believe that he had what it took to be a leader in that discipline. Of particular interest was a course in logic taken his sophomore year, taught by George Birkhoff. Berkeley believed that by using the language of logic one could solve social problems "by taking ideas out of language, operating on them precisely and mathematically, and then putting the ideas back into ordinary language, producing answers to problems. The result of this process would be the calculation of answers to arguments, instead of settling them by majority vote or by intuition, or according to who had the most power, etc."[32] Flirting with communism in those years, Berkeley had little patience for leaving important questions to be decided by what he perceived to be the

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[30] Translated from Hungarian in William Aspray, *John von Neumann and the Origins of Modern Computing* (Cambridge, Mass.: MIT Press, 1990), 180.

[31] For example, see the exchanges between H. Ross Ashby and Herbert Simon, and between Ed Berkeley and Warren Weaver below. This is also a central component of cybernetics. See Steve Heims, *The Cybernetics Group* (Cambridge, Mass.: MIT Press, 1991).

[32] Memo from Ed Berkeley to Ed Fredkin, 4/15/65, Edmund C. Berkeley Papers, Charles Babbage Institute, CBI 50, box 4, folder 63. Berkeley had expressed similar sentiments for decades.

shortsightedness of the American voting public. His fixation with the certainty of mathematics also reflected a personal childhood interest in "methods of achievement"—this was a method of reliably finding answers to pressing questions.

Upon graduating from Harvard in 1930, he joined Mutual Life Insurance as an actuary, and in 1934 moved to Prudential Life Insurance. While hardly a glamorous industry, insurance had long been at the forefront of using information technologies, and here Prudential was a leader.[33] At Prudential, Berkeley began applying Boolean logic to sort individual policyholders into distinct categories, and then to assess the effects of changes in policies to individual clients.[34] He often felt frustrated that his efforts to promote logic were not met with more enthusiasm. He blamed a lack of formal logic education on the part of his colleagues, and a general belief among the public that the subject was dull. He began to entertain the possibility of writing popular books on the application of logic.[35]

Yet, even as he was growing frustrated with actuarial work, he had multiple interests to balance. He briefly considered joining the labor movement. He systematically weighed the pros and cons of leaving his secure job at Prudential. "I want to defeat the vested interests, the private ownership of means of production requiring thousands of workers, racial intolerance, oppression of human beings by other human beings," he wrote in 1943, though he feared giving up the steady paycheck that supported his family. In any case, he decided that indulging in his interest in mathematics and logic was a distraction that he would have to give up.[36]

He jotted down those thoughts in the midst of World War II, during which he served as a Naval Reservist. He first did routine mathematical work at Dahlgren Proving Ground in Virginia, but then was transferred in August 1945 to work on the Mark II, an electromechanical computer within Harvard University's Computational Laboratory, which was being operated by the Navy's Bureau of Ships under the leadership of the mathematician (and Commander in the Naval Reserve) Howard Aiken. Berkeley found the atmosphere of Aiken's Computation Laboratory repressive, and its director authoritarian. He did admire Aiken's directness, however, and formed a close friendship with his second-in-command, Grace Hopper.[37] He left Harvard in April 1946 and returned to Prudential, having served less than a year aboard his commander's "ship."

The Prudential to which he returned had also been influenced by the experience of the war. In 1945, the city of Newark, NJ (where the company had its headquarters) faced severe

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[33] The essential book on this topic is JoAnne Yates, *Structuring the Information Age: Life Insurance and Technology in the Twentieth Century* (Baltimore: Johns Hopkins University Press, 2005).

[34] Boolean logic should be familiar to scholars in the humanities through the use of logical connectors such as AND, OR, and NOT.

[35] Bernadette Longo, "Edmund Berkeley, Computers, and Modern Methods of Thinking," *IEEE Annals in the History of Computing* 26 (2004): 4-18.

[36] "Intellectual Notebook for 1943," Berkeley Papers, box 48, folder 18.

[37] Kurt Beyer, *Grace Hopper and the Invention of the Information Age* (Cambridge, Mass.: MIT Press, 2009), 94. When Hopper was in the depths of depression, facing alcoholism and suicidal thoughts, Berkeley led the intervention effort. See Beyer, 204-207.

labor shortages, and the War Manpower Commission ordered Prudential to release 10% of its workers to fill the other needs of the city. Prudential instead successfully convinced the Commission to let the company keep the war work in-house by doing data processing for the Strategic Bombing Survey for Hap Arnold. All told, Prudential processed some 2 million cards, describing 150,000 attacks on 40,000 targets.[38] Several high-ranking members of Prudential spent the war years working directly for military computing projects, such as Arthur Wolf and Emerson (Jim) Cooley. Prudential emerged from the war with many connections between its personnel, the military, and the ranks of computer researchers.

The insurance companies had to recalculate their rates in 1947, following the passage of new regulations by Congress. Prudential decided that this data-intensive process would be an ideal moment to study what the company's information processing needs were, and what they were likely to become within a few years. Prudential created a new Methods Research Section, run by Jim Cooley and reporting to Harry Volk, who had led Prudential's efforts for the Strategic Bombing Survey. Edmund Berkeley was put in charge of researching the capabilities of the new digital computers.

Berkeley took an expansive view of this task, not only considering what computers could do for Prudential, but trying to understand more generally what their limitations and capabilities were. He wrote to Warren Weaver at the Rockefeller Foundation to request support for this undertaking, describing the current state of research on logic and computation as bringing us to a point "where all the language of thought will be calculable like mathematics." He further suggested that the Rockefeller Foundation target this nexus of logic, mathematics, and machinery for future support.[39]

To convince the rest of Prudential of his plans, Berkeley continued to describe the various types of computers that he studied in terms of pre-existing office operations. Thus, a programmed calculator "is really an automatic clerk … that can perform operations at speeds from 50 to 1000 operations a second, both night and day. It can remember and calculate; it can refer to rules and decide," while storing data on magnetic tape memory was akin to putting it in "files in cabinets" or "cards in drawers."[40] Eventually he brought a team to Harvard and borrowed time on the Mark I to run some trials on calculating bills. Berkeley concluded that switching to similar machines could save the company a quarter of a million dollars each year.[41] Prudential remained cautious because it had close ties to IBM, and IBM was refusing to build up a presence in computing that might jeopardize its lucrative business with punch card systems and tabulators.[42] Berkeley began working out a contract for a computer from the Electronic Control

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[38] Blair E. Olmstead, "Prudential's Early Experience with Computers," 3/15/78, Prudential Life Insurance, Berkeley Papers, box 10, folder 3B.

[39] Letter from Edmund Berkeley to Warren Weaver, 11/18/46, Berkeley Papers, box 8, folder 52.

[40] "New Machinery to Handle Information," 2/17/47, Berkeley papers, box 8, folder 53.

[41] "New Computing Machinery – Efficient Programming Capacity – Postulates," 6/2/47, Berkeley Papers, box 8, folder 55.

[42] James W. Cortada, *Before the Computer: IBM, NCR, Burroughs, Remington Rand, and the Industry they Created, 1865-1956* (Princeton: Princeton University Press, 1993).

Company (run by Eckert and Mauchly of ENIAC), and at a meeting of the Board of Trustees in July, 1947, administrative theorist Chester I. Barnard, standing in for an absent member, declared that the company "[couldn't] afford not to get involved" with digital computers.[43]

Berkeley continued to present his findings to the life insurance industry as a whole. In an article for the *Transactions of the Actuarial Society of America*, for example, he described the potential of these machines to transform actuaries into "engineers dealing with information machinery," and made them more familiar as the realization of the dreams of a former businessman—Charles Babbage.[44] The reaction to his proposals was mixed. Other actuaries recognized the significance of his proposals, but remained skeptical about the possibilities of implementing these ideas in the short run.[45] Berkeley began to feel that the relatively small world of actuaries was too narrow for his ambitious ideas about computers.

Attending a conference at Harvard in January 1947, he spoke to Samuel H. Caldwell, a professor of engineering at MIT who wanted to bring computing research out from the secrecy that the war had imposed upon it. Caldwell felt that his own research in computing was being eclipsed by that of Jay Forrester and his Whirlwind computer, which was being built with military support.[46] Berkeley similarly drew upon his experiences at Aiken's Computation Laboratory to suggest that computing laboratories could be too insular, and that the individuals working in them needed to form a larger community. Berkeley and Caldwell recruited other like-minded individuals interested in sharing ideas and formed the Eastern Association for Computing Machinery, which held its first meeting on September 15, 1947. By the end of the year it had become large enough to consider itself a national society, the ACM, with Edmund Berkeley as its secretary for the first six years. The council of the ACM had representatives from regional divisions as well as at-large positions to represent professional groups. It represented a highly ecumenical view of computing.[47] Yet the most prominent individuals in computing maintained their distance. John von Neumann wished Berkeley luck in forming the ACM, and even spoke at its December 1947 meeting, but declined to officially join. A friend tricked Howard Aiken into signing a registration form, and even paid his dues, though Aiken never participated in the ACM after that.[48] The individuals who led the ACM to articulate a vision of computing came primarily from the world of potential users, rather than from those closest to the technical details. For example, Aiken declared in 1954 that "everyone who was in any way

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[43] Olmstead, "Prudential's Early History."

[44] Edmund C. Berkeley, "Electronic Machinery for Handling Information, and Its Uses in Insurance," *Transactions of the Actuarial Society of America* 48 (1947): 36-52.

[45] William P. Barber, Jr., Edward H. Wells, and Edward A. Rieder, Discussion of "Electronic Machinery for Handling Information," Berkeley Papers, box 68.

[46] This conference was disrupted by Norbert Wiener's decision to protest military patronage of computer research, and while certainly not everyone agreed with him, his opinions on this matter were influential. See below.

[47] Atsushi Akera, "Edmund Berkeley and the Origins of the ACM," *Communications of the ACM* 50 (2007): 30-35.

[48] Beyer, *Grace Hopper*, 169.

connected with the original development of large-scale computers had in mind only one thing—namely, the construction of machines for the solution of *scientific* problems. … No one was more surprised than I when I found out that these machines were ultimately going to be instruments which could be used for control in business."[49] While Aiken's motivations came from the computational demands of his physics research, both his contacts at Monroe and at IBM had indeed recognized the business applications of the machine, as had Babbage, whose earlier work had inspired Aiken, and indeed, so too had his superiors in the Navy, who recognized the applications for large-scale data processing.

The departure of Harry Volk, the vice-president supervising the Methods Research Section, prompted Berkeley's resignation from Prudential in July 1948. Berkeley had long been thinking about going into business by himself as a consultant and writer, and he took this opportunity to do so. In 1949 he published his first successful book about computers aimed at a popular audience: *Giant Brains, or Machines that Think*. This book drew directly from his work at Prudential.

Giant Brains began with the briefest suggestion of an analogy between electric switches and neurons to justify the claim that computing machines might do something identifiable as thinking. However, while this analogy let neuroscientists use circuits to investigate neuronal behavior, Berkeley inverted this order. For him, the analogy implied that a complex electronic system could in principle perform the same operations as a real brain.

Much of the book is devoted to brief sketches of existing machines for handling information. As he described these different machines, he noted how all of them were, at the most basic level, doing something very similar: manipulating information according to established rules. Thus, systems of punch cards "move information expressed as holes on cards" while analog devices, such as the MIT Differential Analyzers, "move information expressed as measurements."[50]

Berkeley claimed a more general significance for these machines by associating them with developments in logic. He cited Claude Shannon's influential 1937 thesis, which described how circuit designs could perform basic logical operations—such as AND, OR, and NOT. This connection allowed Berkeley to claim for these devices the revolutionary potential of logic, which he had been proclaiming since the 1920s. For Berkeley, who remained suspicious of human limitations to the same degree that he was enamored by the necessity of mathematics and logic, the real danger was not that machines would fail at their job, but rather that they would succeed all too well. "A final degree of reliability is gained when most of the time the machine operates unattended," he wrote. "Then, there is no human operator standing by who may fail to do the correct thing at the moment when the machine needs some attention. In fact, the motto for the room housing a mechanical brain should become, 'Don't think; let the machine do it for you.'"[51] The crux of the matter was that humans had their own agendas and their own intellectual

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[49] Howard Aiken, "The State of the Art of Electronic Computers," talk at Harvard Graduate School of Business Administration, June 1954, reprinted in "Howard Aiken in his Own Words," in *Makin' Numbers*, 242.

[50] Edmund C. Berkeley, *Giant Brains: or, Machines that Think* (New York: John Wiley, 1949), 65.

[51] Ibid., 175.

shortcomings. Machines did only as they were instructed, with no capacity to disobey. As long as the instructions were sufficient, they would be perfectly reliable in their servility. Reliability grew out of mechanical limitations.

But the machines remained a hard sell. Berkeley asked two basic questions: "The first one is for any employee: What shall I do when a robot machine renders worthless all the skill I have spent years in developing? The second question is for any businessman: How shall I sell what I make if half the people to whom I sell lose their jobs to robot machines?"[52] The application of logical machines to industry would clearly increase productivity, he believed, but only if individuals set aside their own rational self-interest in favor of a planned economic system—planned with the help of his Giant Brains.

"Mechanical Labor Has Most of the Economic Properties of Slave Labor": The Problem of the Man-Machine Boundary in Cybernetics

The cybernetics movement broke down essential differences among humans, machines, and animals by redescribing them all in terms of universal processes of communication. However, with the old boundaries between humans and machines so permeable, what basis remained for protecting a special role for humans as social beings within a world of increasingly sophisticated machines? Cybernetics was originally a technical research program, but soon became a generalized metascientific framework for understanding the world in terms of endless processes of feedback. This technical project resonated with 1940s America and its concerns with both labor struggles and the morality of the emerging Cold War military order. The development of the cultural project of cybernetics shows a scientific community actively distancing itself from the concerns that shaped its origins, while continuing to influence the emerging discourse of computing.

Berkeley's faith in the power of logic and in the power of computers to autonomously guide decision-making can be contrasted against the views of Norbert Wiener, one of the major figures in shaping the cultural associations of all things computational—and its negative side in particular. Under the stern tutelage of his parents, Wiener obtained his PhD in philosophy from Harvard at the ripe old age of 18, and then studied in Cambridge, England under Bertrand Russell and G. H. Hardy. In 1918 he briefly worked for the mathematician Oswald Veblen at the Aberdeen Proving Ground in Maryland, and in 1940 once more applied his mathematical training to wartime calculations.

The crucial problem facing mathematicians studying ballistics was how to use the motion of a distant airplane to predict its trajectory, so that anti-aircraft gunners on the ground could fire at a distant, moving target. Ballistic tables calculated the trajectory of a shell given certain atmospheric conditions, while tracking used the motion of an airplane to predict its future position. Fire control brought together many of the most prominent engineers in the United States, and built upon a long-standing research program in control engineering. In World War II, these research programs grew to include the work of mathematicians and physicists, who integrated radar into feedback systems. Wiener helped MIT engineer Samuel H. Caldwell draft a

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[52] Ibid., 202.

proposal to integrate differential analyzers with the radar technology being developed at MIT, which was submitted to the fire control division of the NDRC in November 1940. He then teamed up with another engineer, Julian Bigelow, to build an antiaircraft predictor. As they studied aircraft motion, Wiener and Bigelow realized that the attempts of a pilot to maneuver the plane were delayed by the physical features of the plane itself, which the pilot had to take into account. The pilot and plane had to be considered as an integrated man-machine system, and the physical constraints of flight imposed certain regularities on the act of piloting.

Wiener and Bigelow produced a confidential report on "The Extrapolation, Interpolation, and Smoothing of Stationary Time Series" on February 1, 1942. Nicknamed "The Yellow Peril" for its stark yellow cover and forbidding mathematical contents, this book integrated control engineering with recent work in communications, all put in the language of statistics. However, Wiener felt that his methods were not being treated properly by the scientists in the MIT Rad Lab, and cut his ties with them in indignation. He found like-minded collaborators in the biological and social sciences. The Mexican biologist Arturo Rosenblueth joined Bigelow and Wiener to describe feedback processes that cut across the division between biological and mechanical phenomena. In 1943, the team of Bigelow, Rosenblueth, and Wiener published their paper on teleology, making purposive behavior central to feedback systems.[53] Wiener expanded his circle into the "Teleological Society" (so named by himself, John von Neumann, and Howard H. Aiken), and convened its first meeting on January 6 and 7, 1945 in Princeton, NJ. These continued in a more formal setting under the auspices of the Josiah Macy, Jr. Foundation in 1946, which brought together engineers, mathematicians, social scientists, and others interested in the possibility of using feedback as a metalanguage for science.[54] Some of the leading members of this group were neuroscientists, such as Warren McCulloch and his collaborator, mathematician Walter Pitts, who focused on the possibility of using the binary, "all or none" character of neuronal excitation to build a system capable of performing the operations of Boolean logic.[55] The machine-brain analogy became an important part of the new cognitive sciences, but had a further significance in suggesting that machines could produce the same forms of complexity as minds.

Yet even as Wiener formed a large discussion group to develop cybernetics, he also removed himself from the key developments in computing. The dropping of the atomic bombs on Hiroshima and Nagasaki appalled him and he decided not to support any further research with military applications. When Boeing approached him for assistance with missile guidance, he refused, and his letter was subsequently rewritten and published in *The Atlantic Monthly* as "A Scientist Rebels." Shortly thereafter, when Howard Aiken at Harvard convened his conference in January 1947 to discuss computers, Wiener publicly removed himself from the program, drawing attention away from the substance of the conference and angering Aiken. Yet these concerns

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[53] Arturo Rosenblueth, Norbert Wiener, and Julian Bigelow, "Behavior, Purpose, and Teleology," *Philosophy of Science* 10 (1943): 18-24.

[54] On these meetings, see Heims, *The Cybernetics Group*.

[55] See Tara Abraham, "(Physio)logical Circuits: The Intellectual Origins of the McCulloch-Pitts Neural Networks," *Journal of the History of the Behavioral Sciences* 38 (2002): 3-25.

over secrecy and military patronage of computing research in part motivated the creation of the ACM.

Wiener cemented his reputation as the father of a scientific movement the next year, with the nearly simultaneous publication of his manifesto, *Cybernetics*, and the public release of his "Yellow Peril." While the body of *Cybernetics* is full of equations and rather obscure for general readers, Wiener's introduction to the book makes clear his grim interpretation of the new science. He paid special attention to the potential applications of control engineering for industry, where he predicted that these technologies could be improved to the point where they rendered most semi-skilled labor obsolete and uncompetitive against machines. These new technological developments "[give] the human race a new and most effective collection of mechanical slaves to perform its labor. Such mechanical labor has most of the economic properties of slave labor… However, any labor that accepts the conditions of competition with slave labor, accepts the conditions of slave labor, and is essentially slave labor."[56] This development would render most workers economically superfluous, he said. The only remaining way to integrate ordinary people into a society that would not need and would not want their labor was "to have a society based on human values other than buying or selling. To arrive at this society we need a good deal of planning and a good deal of struggle."[57] Wiener's speculations reflect his lack of concrete industrial experience. Indeed, Wiener described the evolution of human-machine systems and feedback in strictly intellectual terms, citing Leibniz, Maxwell, and Gibbs as antecedents, while neglecting the recent history of control engineering, with which he was familiar due to his work at MIT.[58] Yet Norbert Wiener did recognize that by blurring the boundary between the capabilities of humans and machines, cybernetics suggested that one could potentially replace the other in the workplace.

The name "cybernetics" derived from the Greek word for "steersman" to emphasize the importance of feedback in control functions. Cybernetics itself became more diffuse than some of the other systems sciences, which developed into sophisticated technical research programs. Instead, cybernetics became more of a cultural phenomenon than the other systems sciences.[59]

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[56] Norbert Wiener, *Cybernetics: Or Control and Communication in the Animal and the Machine* (Cambridge, Mass.: Technology Press, 1948), 37.

[57] Ibid., 38.

[58] This history is the subject of David Mindell, *Between Human and Machine: Feedback, Control, and Computing before Cybernetics* (Baltimore: Johns Hopkins University Press, 2002). Most of Wiener's contemporaries writing about technology, including Lewis Mumford, also failed to recognize the long and complex history of human-machine interactions. See Mindell, 286-288. While arguments about "technological determinism" have long since become sterile and unproductive, the use of this idea among popular writing on technology remains strong. As the previous chapter noted, many socially-minded technologists used the form of new technologies to support their existing political beliefs (for example, describing power grids as naturally requiring social coordination, or alternatively, with cheap electricity empowering individual users). Taking man-machine systems seriously required abandoning the strong determinist position. The significance of cybernetics had much to do with destabilizing the naïve technological determinism among American intellectuals in the first half of the century.

[59] See Steve Heims, *The Cybernetics Group*. Also see Fred Turner, *From Counterculture to Cyberculture: Stewart Brand, the Whole Earth Network, and the Rise of Digital Utopianism* (Chicago: University of Chicago Press, 2006).

As Wiener became something of a cult figure in the following decades, his concerns had an outsized influence upon the public perception of the related sciences.[60]

The contrast with von Neumann is instructive. While the two remained close professionally, they were often seen as contrasting approaches to the world of systems sciences. Wiener recognized how close von Neumann's work was to his own, but pointed out the dangers of falling under the influence of game theory. He distinguished between "Manichaean" sciences and "Augustinian" ones. Manichaean science was driven by purposeful ends, and framed the object of study as a crafty enemy pitted in a contest of wills against the scientist, as in game theory. Augustinian science instead framed its object of study as fundamentally inscrutable rather than willfully malicious, making the scientist's struggle against an impersonal nature.

The Manichaean dimension of game theory (as Wiener put it) was appropriate as a political or military strategy, but it failed as a description of human behavior. Gregory Bateson, writing to Wiener, described the problem as he saw it: "What applications of the theory of games do is to reinforce the players' acceptance of the rules and competitive premises, and therefore make it more and more difficult for the players to conceive that there might be other ways of meeting and dealing with each other."[61] The proper frame for pure scientific research was in the Augustinian mode, recognizing that nature would resist interpretation, but that it would only do so passively and as a result of our own cognitive limitations. Game theory and the Manichaean sciences (which included the wartime cybernetics of fire control), along with the politicized and bureaucratized structure of Cold War science, shifted the pursuit of science to a Manichaean mode, according to Wiener. In a characteristic expression of Wienerian melodrama, he declared that "from the bottom of my heart, I pity the present generation of scientists, many of whom, whether they wish it or not, are doomed by the 'spirit of the age' to be intellectual lackeys and clock punchers."[62] The Manichaean state of Cold War science obscured the Augustinian nature

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[60] Taking Wiener's overheated social analyses at face value can be difficult, but they remain powerful emotional expressions of opposition to the hyper-rational structure of the postwar world. They held a powerful appeal for the early counterculture. They are analogous to the impact of the screeds of Ayn Rand for the right. On Rand, see Jennifer Burns, *Goddess of the Market: Ayn Rand and the American Right* (New York: Oxford University Press, 2009).

[61] Gregory Bateson to Norbert Wiener, 9/22/1952, Norbert Wiener Papers, MIT Archives and Special Collections, MC 22, box 10, folder 155. On the comparison between Wiener and von Neumann, a good starting point is Steve Heims, *Norbert Wiener and John von Neumann: From Mathematics to the Technologies of Life and Death* (Cambridge, Mass.: MIT Press, 1980) Heims sketches an excessively stark comparison between the two, but he makes important observations: namely that the political character of information technologies was quite distinct from the applications for which the technologies were used, or their specific technological foundations. On the use of technologies as political analogies, see Otto Mayr, *Authority, Liberty, and Automatic Machinery in Early Modern Europe* (Baltimore: Johns Hopkins University Press, 1986).

[62] Norbert Wiener, *I am a Mathematician: The Later Life of a Prodigy* (Garden City, NY: Doubleday & Company, 1956), 359-360. His anti-militarism and his cybernetic agenda for science were part of a coherent vision. For more on the distinction between the "Manichaean" and the "Augustinian" enemies, see Peter Galison, "The Ontology of the Enemy: Norbert Wiener and the Cybernetic Vision," *Critical Inquiry* 21 (1994): 228-266. Galison, however, places cybernetics firmly in the camp of "Manichaean" science, a claim that fits Wiener's descriptions of the current state of postwar science, but not his hope for a demilitarized and depoliticized cybernetics. Paul Edwards further develops this distinction by mapping out the spaces of the "Closed World" and "Green World." The agonistic nature of Wiener's "Manichaean" struggle takes place in the highly structured, rational environment of Edwards's Closed

of scientific research. Cybernetics, by suggesting fundamental unities between the search for order in the world and the systems (natural or social) that created order, and an end to Cold War politics, were Wiener's only hopes for doing proper science.

These themes have shaped our understanding of science well beyond Wiener's own time. Donna Haraway has emphasized the significance of cyborg ontology for its partiality and refusal to fit into essentialist categories. Even if cybernetics was born from militarism and industrial capitalism, for Haraway the cyborg opens up the potential for liberation by hopelessly entangling the organic and technological. It therefore cannot partake in essentialist discourse, and breaks free from the sterility of class- and gender-based politics. Haraway's analysis is not just a description of cyborg ontology; it is, as its title makes manifestly clear, a manifesto. She points out that the cyborg world "is about the final imposition of a grid of control on the planet, about the final abstraction embodied in a Star Wars apocalypse waged in the name of defence, about the final appropriation of women's bodies in a masculinist orgy of war" just as much as it is "about lived social and bodily realities in which people are not afraid of their joint kinship with animals and machines, not afraid of permanently partial identities and contradictory standpoints."[63] The cyborg's transgressions of the human/animal, organismal/mechanical, physical/non-physical boundaries matter because the ideal cybernetic subject then becomes fully capable of defining the conditions of its own existence. The necessity of the cyborg subject is due to an over-determination of existence for those who lack that fragmented identity.

Similarly, when Andrew Pickering describes the same liberating potential of cybernetics as an organic "dance of agency," and approvingly cites Gordon Beer's cybernetic principles of management, he misses that this liberation is only available to the leaders of such organization, not for those working at the middle levels.[64] Even when the ideas of managerial cybernetics were implemented in the socialist government of Chile under Salvador Allende, the stated goals of using cybernetic feedback to empower workers failed. Eden Medina sums up the experiment by noting that "these new technologies served to entrench further many of the management practices that had disempowered workers prior to Allende's presidency, rather than to bring about revolutionary change."[65] Cybernetics was no panacea.

World (characterized by game theoretic descriptions of human behavior and bureaucratized big science), while his vision of scientific research, in which the scholar confronts an "Augustinian" enemy, is the contingent meeting of scientist and nature within the open space of the Green World that a generation of cyberneticists found so compelling. See Paul N. Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America* (Cambridge, Mass.: MIT Press, 1996).

[63] Donna Haraway, "A Cyborg Manifesto: Science, Technology, and Socialist-Feminism in the Late Twentieth Century," in *Simians, Cyborgs, and Women: The Reconstruction of Nature* (New York: Routledge, 1991), 149-182, on 154. For a history of this "grid of control," see Paul N. Edwards, *The Closed World*. Edwards describes the position of the cyborg as "the only possibility of self-determination" within the contemporary world. See Edwards, 350. On the political dimensions of cyborgs, see Chris Hables Gray, *Cyborg Citizen: Politics in the Posthuman Age* (New York: Routledge, 2001), 29-31.

[64] Andrew Pickering, "Cybernetics and the Mangle: Ashby, Beer and Pask," *Social Studies of Science* 32 (2002): 413-437.

[65] Eden Medina, "Designing Freedom, Regulating a Nation: Socialist Cybernetics in Allende's Chile," *Journal of Latin American Studies* 38 (2006): 571-606, on 602.

The universalistic aspirations of computing and cybernetics required that its origins be framed in terms of the cold necessity of mathematics rather than in the embodied world of labor and battle. Cybernetics was not just about creating specific technologies (such as for fire control), but also about constructing a world that could be described in terms of communication. The appeal of cybernetics as a scientific research program derived in no small part from its claims of universality.[66]

This universality made cybernetics more fundamental than the logic that so captivated Edmund Berkeley. Understanding computers required understanding logic, though according to Wiener the use of the machines would invalidate the formalist program of his former professor, Bertrand Russell. “The science of today is operational,” he observed. “That is, it considers every statement as essentially concerned with possible experiments or observable processes. According to this, the study of logic must reduce to the study of the logical machine, whether nervous or mechanical, with all its non-removable limitations and imperfections.”[67] His social critique was built upon the intrinsic shortcomings of machine logic, coupled with what he perceived as the amorality (if not outright immorality) of the industries and states that would use them. For Berkeley the great problem was in not trusting the machines enough. For Wiener, it was in trusting them too much.

Excursus: The Tragedy of William James Sidis

Norbert Wiener’s ideas about the limitations of intelligent behavior and the dangers of separating thought from social context grew out of his own childhood experiences, and from those of a troubled childhood companion, William James Sidis. The tragic fate of Sidis served as an allegory for the creation of intelligent machines lacking a corresponding wisdom.

Sidis grew up in Cambridge, Massachusetts, where his father, Boris Sidis, was a respected psychologist and a friend of William James. Boris raised his son according to his theories about optimal psychological development. By the age of 11, young William James Sidis delivered a lecture to the Harvard mathematics department on the properties of four-dimensional objects. In a town full of precocious youths, he was one of the most spectacular. Harvard refused to let him matriculate until he turned 12 (a precedent set by Cotton Mather in 1674), part of a cohort of prodigies, including Norbert Wiener, Adolf A. Berle (later a member of FDR’s Brain Trust and co-author of *The Modern Corporation and Private Property*), Cedric Houghton (a Boston Brahmin who died young from a ruptured appendix), and Roger Sessions (a well regarded composer). Yet he did not enjoy his fame. He re-entered the newspaper headlines in 1919 for carrying a red flag during a May Day demonstration, and subsequently fled to New York City and disappeared. He renounced the very mathematical skills that had made him famous in the first place, and within a few years, this self-described “peridromophile” spent his time collecting streetcar transfers, writing a monumental catalogue of these transfers, and composing poems about Boston’s streetcars:

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[66] Geof Bowker, “How to be Universal: Some Cybernetic Strategies, 1943-1970,” *Social Studies of Science* 23 (1993): 107-127.

[67] Wiener, *Cybernetics*, 147.

From subway trains at Central,
  a transfer get, and go
To Allston or Brighton or
  to Somerville, you know;
On cars from Brighton transfer
  to Cambridge Subway east
And get a train to Park Street,
  or Kendall Square, at least.

In 1937 the *New Yorker* published a "Where Are They Now?" piece about Sidis written pseudonymously by journalist James Thurber.[68] Wiener suspected that the gossip surrounding Sidis drove him underground.

He resurfaced at MIT in the 1940s, where Philip Morse hired him to do calculations. After protesting that he could no longer do mathematics, Sidis finally agreed—on the condition that Morse give him step-by-step instructions so that he "wouldn't have to use any initiative or judgment of his own."[69] Morse was impressed by his work, which was always done flawlessly. Sidis's mood improved to the point where he began to open up to Morse, telling him about his interest in streetcar transfers and the history of the Native Americans. Seeing his evident mathematical skill, Morse gave him a new assignment—but provided only the function to be calculated and some very general instructions. Sidis promptly quit and vanished. Within a year he was dead from a cerebral hemorrhage.

Though cannot make too much out of the tragic fate of William James Sidis, his story dramatizes some connections, important to Wiener and others, between mathematical acumen and psychological wholeness. In the end, Sidis's mathematical talent became his curse and he could only bring himself to do only the most mechanical calculations, refusing to make any inferences on his own. He contrasted the pain of doing mathematics with the joy that came from his later interests in socialist politics and in the culture of Native American tribes. The tragedy of William James Sidis functioned as a parable of a divided self, in which emotion and calculative reason remained fundamentally at odds. For Sidis the roles of mathematician and man were not easily compatible.

Wiener rejected the widespread claims "that the martyrdom of young Sidis was a crime of Science" though he maintained that "who cannot understand Sidis, cannot understand me."[70] His first autobiography, *Ex-Prodigy*, focuses exclusively on his childhood and his unsettled thoughts about his upbringing. Wiener stopped short of criticizing his father and Boris Sidis for pushing their children, but gave a stern warning about the perils of shaping young lives in this way. "So you can make your child a genius, can you?" he asked. "Let those who choose to carve a human soul to their own measure be sure that they have a worthy image after which to carve it, and let them know that the power of molding an emerging intellect is a power of death as well as

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[68] Jared L. Manley, "Where Are They Now? April Fool!" *The New Yorker*, August 14, 1937, 22-26.

[69] Morse, *In at the Beginnings*, 142.

[70] Norbert Wiener, letter to Janet Rioch, 6/22/50, Wiener Papers, box 8.

a power of life."[71] The particularities of Wiener's upbringing forced him to recognize the shortcomings of reason and the importance of social existence—a crucial feature of his analyses of computing machines and cybernetics.

The Social Challenges of Automation

Cybernetic feedback and the promise of logical control were influential ideas and the industrial engineering community soon picked them up. As industrial engineers applied these computing concepts they laid the foundations for subsequent developments in information processing. Automatic number crunching fit into the automation fever of the 1950s, a movement that was strongly influenced by control engineering and the ideas of Norbert Wiener's cybernetics. Feedback systems had already been part of many technologies, but the crucial step in 1950s automation was to imagine the factory as a large mechanical system and make feedback part of its control. The idea of continuous flow expanded to include both the flow of *information* as well as the flow of material, and the processing of this information as well as the processing of material. This made the developments of control systems a new part of industrial design. Automation created a context for viewing programming as a practical tool. At the same time, it created new problems for defining the positions of workers and machines within the factory. Workers were confronted with new problems: what was skill? How could the ideal of a workerless factory support a consumer economy?

The 1950s discourse on automation drew upon the work of John Diebold, a consultant who had begun studying automation in earnest as a student at the Harvard Graduate School of Business in 1950. He studied under General Georges Doriot, known as the "father of venture capital" for founding the American Research and Development Corporation in 1946 with Karl Compton of MIT and Ralph Flanders, an industrialist and U.S. Senator from Vermont. ARDC's first major success was its investment in Digital Equipment Corporation (DEC) in 1957. Diebold pursued a research project with Doriot to identify the most promising developments in automatic controls, for which he sought advice from engineers and scientists at MIT, including Norbert Wiener.[72] Diebold emphasized that automation was not just a new technology of manufacturing; it would ultimately require a fundamentally new way of thinking about the nature of work and production. Automatic controls were a necessary component of an automated factory, but this did not exhaust the possibilities of automation. He described the use of automation in steel mills, the prototypical example of heavy industry. While many companies had implemented such half-measures as automatic furnace door openers, these methods did not fundamentally re-imagine the process of steel making. They kept old ways of batch processing, but made them marginally faster and more standardized. Instead, he singled out three companies—Scovill Manufacturing Company, Republic Steel Corporation, and the Babcock & Wilcox Tube Company—for having created continuous casting processes by 1948, a truly novel form of production. This process worked by passing molten metal through a mold and allowing it to cool. As it was gradually

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[71] Norbert Wiener, *Ex-Prodigy: My Childhood and Youth* (New York: Simon and Schuster, 1953), 136.

[72] Letter from John Diebold to Claude Shannon, 11/3/50, Claude Shannon Papers, Library of Congress, box 1, folder 8.

withdrawn, the hardened metal acted as a plug for the remaining molten metal, making the casting process continuous rather than batched. By constantly measuring the temperature of the steel and its chemical composition, and by adjusting the rate at which the plug was withdrawn, different kinds of steel could easily be produced without requiring new batches.[73]

For Diebold, the advantage of these systems was that they reformulated the job of producing steel and brass in terms of new technology rather than fitting the technology to preexisting methods. That more conservative approach was understandable given the very large capital investment required to overhaul a process like steel making. However, in the long run companies could recoup the money in a few years.

The case of steel gave a clear example of how that quintessential industrial process could be reimagined. Yet even continuous casting did not exhaust the possibilities of the new control technology. Diebold suggested that automation could reshape even the stock exchange. By using electronic record-keeping to match buyers and sellers, the physical trading floor could be eliminated. The relationship between investors and brokers would remain the same, as would the work of brokers themselves. However, the work of traders could be substantially reduced, which could then bring savings to brokerage firms and to individual investors.[74] The automation of the stock exchange could more easily achieve the promise of automatic controls. The problem of handling materials (the original point of automation) became far simpler when that material was nothing more than information itself.

The central challenge for automation was to strike the right balance between flexible and streamlined. Highly standardized processes could be easily automated by being totally inflexible. At the other extreme, two physicists, Eric W. Leaver and John J. Brown, had suggested in 1946 that machines could be imagined strictly in terms of functions rather than products. These functional machines could be linked together in any number of ways, and would therefore be highly flexible. Yet Leaver and Brown's system, being truly general-purpose, would be far too expensive to be practical.[75] Diebold suggested a middle way. By starting from bundles of commonly associated functions, a factory could build in some flexibility without losing the savings from automation. These trade-offs defined the challenge of making automation practical.

James R. Bright, a professor at the Harvard GSB, provided one of the most thorough analyses of how automation fit within a history of mechanization. This was the type of qualitative research that schools such as Harvard continued to do, even as the newer management schools focused on mathematical methods. Bright and his colleagues devised a 17-part scale ranging from "the hand" through "hand tools" and various forms of "powered tools," up to devices that could read signals and respond, culminating in machines that "anticipated required performance and adjusted accordingly." Most industrial work remained in the middle stages of his scale.[76] In addition to the scale of automation, Bright also suggested measuring the span and

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[73] John Diebold, *Automation: The Advent of the Automatic Factory* (New York: Van Nostrand, 1952), 33-36.

[74] Ibid., 46-50.

[75] Eric W. Leaver and John J. Brown, "Machines Without Men," *Fortune* (November 1946): 165-166, 192-204.

[76] Bright, 39-56.

penetration of automation—the degree to which the varied tasks of the factory (such as clerical work and delivery of finished goods) and the supporting steps for an operation (such as designing tools) were automatized.

Bright concluded that automation was both less and more important than commonly believed. The fantastic visions of worker-less factories were far from being a reality and few factories had implemented automation consistently. Most firms took a sharp dive in productivity when they implemented automation, and took months or years to return to their pre-automation production levels. Yet eventually most companies realized significant savings. Companies made trade-offs: expanding production, increasing quality, and lowering labor costs, but adding new operating constraints and requiring more thorough maintenance.[77] The significance of automation was that it encompassed a set of technological innovations as well as new ways of thinking about the organization of business. Economist George Schultz observed that the first Industrial Revolution was one of "energy and power" while the "second Industrial Revolution" was one of "control over processes and operations."[78] This referred to the work of Taylor as well, while holding out for future developments in information technology.

Automation also bore upon large-scale dynamics in the economy. One of the major social concerns in American capitalism in the 1950s was the growth of large corporations that potentially wielded excessive economic and political power. In this, the role of automation was ambiguous. Insofar as automating factories required large capital investments, it would continue to favor the consolidation of large corporations. However, to the extent that automation made materials cheaper and reduced operating costs, it could promote decentralization.

As Wiener considered the role of cybernetics in postwar American life, his concern for the fate of labor led him to contact Walter Reuther and the Union of Automobile Workers in 1949.[79] Wiener warned that a fully automatic factory would have a devastating effect upon the future of employment for autoworkers.[80] Automation threatened to remove control over industrial production from workers and place it entirely within the hands of a technological elite. He considered simply refusing to write about automation and cybernetics, as he had done with military research several years earlier. However, he ultimately decided that the ideas were "in the air" and that even if he did not write, "they were bound to reappear in the work of other people, very possibly in a form in which the philosophic significance and the social dangers would be stressed less."[81] The problem, as Wiener saw it, was that the imperatives of technological growth that these elites favored did not necessarily correspond with the values of ordinary citizens.

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[77] Ibid., 85.

[78] George P. Schultz and George B. Baldwin, *Automation: A New Dimension to Old Problems* (Washington, DC: Public Affairs Press, 1955), 5.

[79] Wiener's interest in labor grew out of his brief stint as a journalist covering labor issues in Lawrence, Massachusetts. He also credited this job with teaching him how to write. See Wiener, *Ex-Prodigy*, 266-268.

[80] Norbert Wiener to Walter Reuther, 8/13/1949, Wiener Papers, box 7, folder 102.

[81] Wiener, *I am a Mathematician*, 308.

Wiener published a more accessible study of cybernetics in 1950, titled *The Human Use of Human Beings*. After repeating his claim from *Cybernetics* that automation relegated labor to the status of slavery, he castigated industry for its shortsightedness in pursuing automation: "It is perfectly clear that this will produce an unemployment situation, in comparison with which the present recession and even the depression of the thirties will seem a pleasant joke. … However, there is nothing in the industrial tradition which forbids an industrialist to make a sure and quick profit, and to get out before the crash touches him personally."[82] Yet between the publication of *The Human Use of Human Beings* in 1950 and its revision in 1954, Wiener became more optimistic that his attempts to sound the alarm were paying off.

Contra Wiener's dire predictions, economist Eugene Staley of Tufts University suggested that there was not in fact a fixed amount of labor to be done, which machines would take over. "There are some very able economists in the same building with him at the Massachusetts Institute of Technology," he began, "and a little journey down the corridor for some internal communication could have saved [Wiener] from setting down his rather naïve remarks on technological unemployment."[83] Yet this sense of automation as leading to unemployment was pervasive—by 1962, John F. Kennedy announced that he "regard[ed] it as the major domestic problem, really, of the Sixties—to maintain full employment at a time when automation, of course, is replacing men."[84] Managers became especially interested in automation as a cost- and labor-saving system during the brief recession that followed the end of combat in Korea, and Ford's automated engine factory in Ohio was of particular pride to the company. When UAW leader Walter Reuther made a trip to the factory, a manager pointed to the machines and quipped, "not one of these machines pays union dues." Reuther quickly noted, "not one of them buys new Ford cars, either."[85] Reuther recognized that unemployment was not the only problem caused by automation. The use of machines to do skilled work threatened to create a gulf between unskilled workers and the engineers responsible for designing machines, and that threatened both class mobility and the ease with which the middle class became consumers.

Diebold took Wiener's concerns more seriously, acknowledging that the history of industrialization had resulted in the degradation of manufacturing work. However, rather than seeing automation as a further stage in this story of decline, Diebold saw automation as freeing workers from their chains: "To a great extent the jobs in which the worker is tied to and paced by the machine will be taken over by other machines. The worker will be released for work permitting development of his inherent human capabilities."[86] This same sentiment was expressed more pithily by journalist David Woodbury: "automation at its best is the enslavement

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[82] Norbert Wiener, *The Human Use of Human Beings: Cybernetics and Society*, 2nd ed. (Garden City, NY: Doubleday, 1954), 162.

[83] Quoted in Diebold, 157.

[84] Report to the President by the American Foundation on Automation and Employment, 11/30/1962, Berkeley Papers, box 44.

[85] Nelson Lichtenstein, *The Most Dangerous Man in Detroit: Walter Reuther and the Fate of American Labor* (New York: Basic Books, 1995), 291.

[86] Diebold, 162.

of machines, that human slavery may be truly ended."[87] Diebold went on to observe that "in an odd and entirely unexpected way, automation may bring us back to the human and psychological values of the self-respecting craftsman."[88] Preaching the benefits of automation meant acknowledging that the factory system was both dangerous and damaging to workers. Automation solved the dehumanization of modern industry by literally removing humans from the production process.

Indeed, for Diebold, as for other proponents of automation, the problem was what to do once the material struggle for existence ended. Once automation provided for the basic material necessities, individuals could focus on the artistic and spiritual sides of life. Automation held the key to success in the Cold War by building our defensive capabilities while leaving American citizens free to work, live, and prosper in the civilian sphere.[89] It also held the key to development in the third world by bringing a rapid improvement in material conditions without excessively disrupting patterns of life—all the benefits of industry without the social dislocations of industrialization.[90] While optimistic, Diebold admitted that there were many uncertainties. There remained the questions of who would own and manage these automated industries and how an effective distribution of goods could be achieved.[91]

Yet the labor problem remained a real one. The poles of this debate were familiar. Advocates of automation claimed that more advanced technology would require more responsibility on the part of workers, and would improve their skills. Labor leaders instead

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[87] David O. Woodbury, *Let ERMA Do It: The Full Story of Automation* (New York: Harcourt, Brace and Company, 1956), 10.

[88] Diebold, 164.

[89] This is similar to President Eisenhower's reliance upon nuclear weapons as a way to limit the need for a large standing army. See John Lewis Gaddis, *Strategies of Containment: A Critical Appraisal of Postwar American National Security Policy* (New York: Oxford University Press, 1982). Indeed, the scale of nuclear weapons, and the speed with which an attack could come, was one important motivation for automating detection and warning systems. H. Rowan Gaither of the Ford Foundation, who had helped found RAND and had produced the 1957 Gaither Report of Eisenhower's Science Advisory Council, cited this as a major reason for Ford Foundation support of decision-making systems.

[90] On this strand of development as foreign policy, see David Ekbladh, *The Great American Mission: Modernization and the Construction of an American World Order* (Princeton, NJ: Princeton University Press, 2010). Eugene Staley, mentioned above, was a key figure in shaping this agenda. See Ekbladh, 65-69.

[91] Of course, these questions of how goods are to be allocated and who controls the means of production are perhaps the two most basic questions of political economy. It is difficult to understand how a sophisticated analyst such as Diebold could push them aside. As became clearer in later years, the ideal of the automatic factory created a significant problem for economic thought. Economic theories designed to allocate scarce resources seemed inadequate to many theorists as automation suggested the arrival of an age of plenty. The central problem became how to allocate a sudden surplus of time and goods, and typical solutions included the development of inner cities and third world countries, as well as support for the arts and for spirituality. These ideas and their connection with the Great Society will be explored in chapters 4 and 5. Diebold was more perceptive than most of contemporaries in seeing how easily rising material prosperity could be a challenge to social order. He concluded his book with the warning that "it is indeed hard to provide a society in which increased material welfare truly benefits man rather than cheapens him. Strong moral leadership and men of good will are sorely needed, as much now as always." See Diebold, 175.

feared that the sophistication of automatic machinery meant a consequent devaluing of skilled workers. Machinists could be replaced by machine operators who were less skilled and more replaceable. James Bright (of the Harvard Graduate School of Business) found that the evidence supported labor. He suggested that skill requirements peaked for complex hand tools but then began to drop off. Contrary to the most vocal advocates of automation, "the progressive effect of automation is first to relieve the operator of manual effort and then to relieve him of the need to apply continual mental effort. At times the mental effort is increased because of the alertness and over-all responsibility required. Eventually, safety devices and various recording and signaling systems are added to reduce or eliminate this demand."[92] George Schultz (later Ronald Reagan's Secretary of State) and his colleague, George Baldwin, made an important distinction: "automation will not upgrade people; it will only upgrade jobs," before adding that this would eliminate the dull, routine jobs created by the assembly line.[93]

Labor relations became a central concern in the immediate postwar years, when the problem (in the helpful phrase of General Motors CEO and future Eisenhower Secretary of Defense Charles E. Wilson) was "Russia abroad and labor at home."[94] Union membership increased from 9 million to 15 million during the war, and strikes were rampant in the years immediately following the war's end. In 1947 General Electric demonstrated its record-playback system, in which the movements of a skilled machinist were recorded onto an analog machine, which could then be played back to create copies of the machinist's work, accurate to 1/1000 of an inch. Meanwhile, a system of numerical control was being developed in the Servo Lab at MIT under Gordon Brown and William Pease, using digital instructions to control machine operations. It was unveiled in 1952 and refined in the following years using the Whirlwind computer. While both systems could be equally promising in terms of the quality of output, they differed in how they mediated the relationship between workers and managers. The record-playback system used workers' skill as the foundation of the system. Numerical control instead translated the complex physical movements of skilled workers into a sequence of operations described formally and mathematically. It abstracted the workers' knowledge and translated it into a language intelligible only to the designers of the N/C systems.[95]

At stake here was control over the factory floor. Unions wanted to protect good wages, but their more far-sighted leaders (including Reuther) recognized that control was fundamental and that deskilling meant that workers would become more easily replaceable. Skilled work required skilled workers, and the possession of skill had to remain in the control of workers

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[92] Bright, 188.

[93] Schultz and Baldwin, 11.

[94] David F. Noble, *Forces of Production: A Social History of Industrial Automation* (New York: Oxford University Press, 1984), 45.

[95] Noble, *Forces of Production*.

themselves.[96] Labor leaders recognized that the N/C system was dangerous precisely because it replaced inarticulate skill with a set of precise instructions and quantifiable measurements.

These points were further explored by Kurt Vonnegut in 1952 in his first novel, *Player Piano*, in which American society is organized by a small group of engineers and bureaucrats. Automation and the growing efficiency of industrial processes provide regular growth in the standard of living even as machines have eliminated the need for most human workers. Computers have reached a point where they determine all production levels by taking into account thousands of possible factors, such as the supply of materials and demand for goods. Computers further dictate the course of each citizen's life, compiling test results to determine one's aptitudes and thereby opening up or closing off educational and professional opportunities. Vonnegut had worked at General Electric, home of the record-playback system of automation, and he worked many aspects of GE's corporate culture into the world of the novel. He wrote to Norbert Wiener, praising his critiques of industrial production in *Cybernetics*.[97] By making explicit the connections between man and machine, cybernetics sounded the alarm for dehumanization within the industrial system.[98]

The plot of the novel was inspired by other dystopian novels, such as Aldous Huxley's *Brave New World* and Yevgeny Zamyatin's *We*. Like them, this one also celebrated resistance to a totalitarian system. A group of Vonnegut's engineers join a doomed resistance movement against the system precisely to demonstrate that action outside of the system is possible, even if ultimately futile. In an ironic twist, no sooner do the mechanically-savvy rebels destroy the machines than they begin tinkering with the parts and building devices to perform the task more efficiently. Vonnegut contrasts the intellectual thrill of devising these machines with the danger that an uncritical acceptance of them creates. The danger was not the machines themselves; the real danger was the tendency to overvalue them and their work.

Automating Mental Work

Computers, as machines manipulating information, seemed to many analysts to be ideal for automating clerical work. Indeed, the earliest applications of computers to industry involved routine number crunching and cataloguing, automating existing methods of doing this work.[99]

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[96] The essential look at skill in the context of 20th century scientific management and automation remains Harry Braverman, *Labor and Monopoly Capital: The Degradation of Work in the Twentieth Century* (New York: Monthly Review Press, 1974).

[97] Vonnegut inadvertently offended him by having a character in the novel named "Von Neumann," who remained a friend of Wiener's despite being his intellectual sparring partner. Letter from Kurt Vonnegut to Norbert Wiener, 7/26/52, Wiener Papers, box 10. The contrast between von Neumann and Wiener has been commonly invoked in the literature (see above), and as Vonnegut's use of it shows, was quite apparent even in the 1950s.

[98] Wiener's ambivalence on this point is emphasized in N. Katherine Hayles, *How We Became Posthuman: Virtual Bodies in Cybernetics, Literature, and Informatics* (Chicago: University of Chicago Press, 1999), 84-112.

[99] Russell B. McNeill, "Mechanizing Paper Work," *Harvard Business Review* 26 (1948): 492-512; Robert A. Shiff and Arthur Barcan, "The New Science of Records Management," *Harvard Business Review* 32 (1954): 54–62; Charles P. Bourne and Douglas C. Engelbart, "Facets of the Technical Information Problem," *Datamation* 4 (1958): 6–12.

While Diebold and other automation consultants stressed that automation required rethinking operations, few managers were in a position to do so with computers. Many of them "considered [computing] to be in the nebulous area occupied by atomic energy, the theory of relativity, and other scientific phenomena."[100]

Yet they could innovate when necessary, as in the familiar case of the ERMA system. Bank of America, by 1945 the largest bank in the country, faced a crisis in check clearing. In the decade from 1943-1952, check volume had doubled from 4 billion/year to 8 billion. The bank predicted that in 1955 this volume would be increasing by 1 billion checks per year. This problem was compounded by the fact that most checks had to pass through at least two banks, which took several days. On a typical day in 1950 there were nearly 70 million checks being processed by hand (in a country with 150 million people). The bank's president claimed that within a few decades check clearing would require employing the entire adult population of California .[101] The need for careful handling of checks and the repetitive nature of the work meant that banks suffered very high turnover rates. In 1950, S. Clark Beise, a vice president at Bank of America, began talking to Thomas H. Morrin at Stanford Research Institute in Palo Alto about designing a system to automate check clearing. This arrangement was unique because this case of technological innovation was driven by the user rather than by the tech companies. Furthermore, because the major tech companies (including IBM) believed this to be an impossibly difficult undertaking, the design work was done at SRI, an industrial research lab spun off from Stanford University.

Bank of America's check system had to read data and do bookkeeping reliable to the last penny. As SRI designed the system, they realized that some fundamental reorganization of the bank's accounts had to accompany the machinery. In 1955 Bank of America announced their new "Electronic Recording Machine—Accounting," or ERMA. The official unveiling on September 22 was dubbed "ERMA Day."[102]

Once SRI's prototype was unveiled, major technology companies became interested in building the system. The bank selected General Electric, to GE's surprise. GE was not a major player in the computer market at the time. Their proposal was written on letterhead from the "Industrial Computer Section" of GE, which had the unfortunate problem of only existing on paper. Robert R. Johnson later said that he wrote the proposal as "an interesting exercise and good for experience." But the bank felt that GE's inexperience in the field would make them more flexible than more established companies like IBM, and that the company would protect its reputation by committing enough resources to make the project succeed. Barney Oldfield of GE understood that the ERMA contract would be a big project with which to launch a major computing effort within the company. With the contract in hand, but without the requisite technical staff, GE had to scramble.

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[100] Ralph W. Fairbanks, "Electronics in the Modern Office," *Harvard Business Review* 30 (1952): 83-98.

[101] Joseph Weizenbaum, "AAAS Commission on the Year 2000: Social Implications of the Computer," 11/22/1968, Robert Fano Papers, MIT Archives and Special Collections, MC 413, box 7.

[102] Amy Weaver Fisher and James L. McKenney, "The Development of the ERMA Banking System: Lessons from History," *IEEE Annals of the History of Computing* 15:1 (1993) 44-57. The word "Accounting" was added to its name when Bank of America decided that ERMA sounded more approachable than ERM.

Oldfield quickly set up GE's Palo Alto team to design ERMA, leaving George Jacobi and Robert Johnson in charge, and hiring Joseph Weizenbaum to lead the programming of the system. GE made some significant modifications to the SRI design, ultimately producing a system that used magnetic ink and optical character recognition to read checks, systems to quickly sort them, and programs to adjust account balances, monitor holds placed on checks, and protect against errors. The completed ERMA system was unveiled in 1960 at a celebration hosted by General Electric Theatre's own Ronald Reagan.[103]

This form of office automation was immensely suggestive to practicing managers. Not only could this type of automation improve the operations of information-intensive industries (such as banking and insurance), but the very development of systems theories for industrial production meant that companies were devoting more resources to information work, such as economic forecasting and analyses of their assembly lines.

If the back office was recast as the "nervous system" of the organization, the principle of rationalization involved finely dividing up the work done here too. Robert Katz, in a study funded by the Alfred P. Sloan Foundation, described three distinct skill sets for different levels of white-collar workers. Low-level office workers performed routine tasks, and needed very particular technical skills in order to do them well. Middle management needed interpersonal skills to lead a team and convey instructions from higher-ups, and to report on his team's work to top management. The leaders of the firm needed a third set of skills—the conceptual skill to understand how each division fit into the whole, and how the company related to its industry and to the broader society. The task of the top manager was not merely to impose order, but to broadly conceptualize the linkages among different components of the corporation and between the corporation and the outside world.[104]

Managers of industrial companies began to include feedback and cybernetic ideas into the way they described their management strategies in the 1950s. Frank Abrams of Standard Oil of New Jersey, writing in the *Harvard Business Review*, described the corporation as an important component of a larger social system, and as such was responsible to American society as a whole. The manager faced many conflicting demands on his attention and had to act as a steersman of his "man-made instrument of society" by using both intelligence and sound moral judgment.[105]

A manager was an easy fit for the role of steersman, but the mathematical techniques being created as management theory's "New Look" offered him guidance. Operations Research was particularly influential. Robert McNamara and his "Whiz Kids" at Ford demonstrated the utility of mathematical tools for analyzing production. Economist Robert Solow, initially skeptical of the ability of operations researchers and others to contribute to American business, eventually conceded their usefulness: "Longhairs—typically PhDs with no business training or

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[103] James L. McKenney and Amy Weaver Fisher, "Manufacturing the ERMA: Lessons from History," *IEEE Annals of the History of Science* 15:3 (1993), 7-26.

[104] Robert L. Katz, "Skills of an Effective Administrator," *Harvard Business Review* 33 (1955): 33-42.

[105] Frank W. Abrams, "Management's Responsibilities in a Complex World," *Harvard Business Review* 29 (1951): 29-34. Note that James Bright identified the oil companies as some of the most fully automated.

experience—are getting into business more and more, supplying not only the technology of machines or processes, but also a new general technology linked to decisions … At its best it is a systematic approach to a whole business as an integrated operation, an analysis of the interrelation of all its parts."[106]

Managers lacked clear definitions of OR. Scientific management along Taylorist lines had been a common element of the managerial toolkit well before World War II and many managers simply saw operations research as a continuation of Taylorism.[107] Yet OR analysts claimed that they had a distinctly new view of the corporation. Within OR, "operations are considered as an entity. The subject matter studied is not the equipment used, nor the morale of the participants, nor the physical properties of the output; it is the combination of these in total, as an economic process."[108] The central insight claimed by OR was that the overall health of an organization was more than the sum of its parts. Counterintuitively, they found that making an individual process more efficient could harm the effectiveness of the entire process.

Operations Research claimed to have a solution to this problem: mathematical programming. The heart of this component of OR was George Dantzig's simplex method, developed in 1947 for the Air Force. It was computationally intensive and far too unwieldy for practical use until the widespread introduction of the computer. The mathematical nature of OR was a strong selling point; its promoters stressed that for an OR analysis, "it is *absolutely certain* that if these rules are followed, they will lead to the best possible program; and it will be perfectly clear when the best possible program has been found. It is because the procedure follows definite rules that it can be taught to clerical personnel or handed over to automatic computers."[109] The authors of this article implicitly equated clerical workers and machines, an equivalence that would become even more significant in the next decade, as these clerical workers felt their jobs imperiled by the development of new computers.

The epitome of this rational planning via computer occurred in Jay Forrester's *Industrial Dynamics*, published in 1961. Forrester was one of the leaders of the Whirlwind computer and SAGE (the Semi-Automatic Ground Environment) at MIT, and the inventor of magnetic core memory.[110] After managing the Whirlwind project, Forrester turned his attention away from military matters and towards management. *Industrial Dynamics* was nothing less than a grand synthesis of the many different components of the systems sciences. Drawing upon statistics, cybernetics, game theory, and empirical studies of industry, Forrester's work was a direct

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[106] Robert Solow, "Operations Research is in Business," *Fortune* 54 (1956): 148-152.

[107] John J. Caminer and Gerhard R. Andlinger, "Operations Research Roundup," *Harvard Business Review* 32 (1954): 132-136.

[108] Cyril C. Herrmann and John F. Magee, "'Operations Research' for Management," *Harvard Business Review* 31 (1953): 100-112.

[109] Alexander Henderson and Robert Schlaifer, "Mathematical Programming: Better Information for Better Decision Making," *Harvard Business Review* 32 (1954): 73-100. Emphasis in the original.

[110] For a history of SAGE, see Thomas P. Hughes, *Rescuing Prometheus* (New York: Pantheon Books, 1998), 15-67.

contribution to the managerial planning tradition, intended to give managers more time for creative thought by facilitating decision making.[111]

Applying OR to management meant rationalizing industrial processes. It was a way of continuing to bring responsibility for oversight of industrial work within the back office. These methods did not address the rationalization of work within the office, however. The fundamental component of mental work was making decisions, and only in the simplest cases could that be reduced to the type of control functions that a system like ERMA could handle. True automation of the office had to be built on an understanding of decision-making.

The many analogies between the brain and computers (or between the brain and the office) laid foundations for a way of thinking about office automation in terms of psychology. Here, the work of Herbert Simon and his colleagues at Carnegie Tech was crucial. Simon recognized that a theory of organizational behavior required a theory of individual rational decision-making, and that a theory of rational decision-making required a theory of organizations. Having laid the groundwork for the general study of organizations in *Administrative Behavior*, Simon then wanted to turn to the process of decision-making by an individual in particular environments. Simon was not satisfied with the neoclassical fixation on utility curve maximization as a useful model of individual choice. By comparing rational choice in humans with the behavior of microscopic organisms, he replaced the concept of maximizing utility with one of "satisficing." Simon abandoned unobservable utility functions for an empirically grounded process of comparing environmental conditions against measurable satisfaction levels.[112] Human actors generally cannot know a priori what course of action will produce a maximally good result. Instead, Simon argued, they only seek to satisfy their appetites, which themselves vary with time according to the environment—a model of feedback.[113] The theories expressed in the 1956 articles "A Behavioral Model of Rational Choice" and in "Rational Choice and the Structure of the Environment" were strongly influenced by von Neumann and Morgenstern's *Theory of Games and Economic Behavior* and H. Ross Ashby's *Design for a Brain*.[114]

Some of the most important research on individual decision-making within complex systems took place at the RAND Corporation (Research and Development) in Santa Monica, California, which had been created in December 1945 as a joint project of the Air Force and Douglas Aircraft. The context for the creation of RAND was as a think tank for the Air Force,

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[111] Jay W. Forrester, *Industrial Dynamics* (Cambridge, Mass.: MIT Press, 1961).

[112] Herbert A. Simon, "Rational Choice and the Structure of the Environment" in *Models of Man: Mathematical Essays on Rational Human Behavior* (New York: John Wiley and Sons, 1957), 261-273.

[113] Herbert A. Simon, "A Behavioral Model of Rational Choice," in *Models of Man*, 241-260.

[114] Simon, *Models of My Life*, 114. When asked by Bernard Berelson of the Ford Foundation to describe what works most influenced his studies of organizational behavior, he cited only Chester Barnard's *Functions of the Executive* as coming from that discipline. His other inspirations were H. Ross Ashby's *Design for a Brain*, von Neumann and Morgenstern's *Theory of Games and Economic Behavior*, the McCulloch-Pitts paper, "A Logical Calculus of the Ideas Immanent in Nervous Activity," and the work of the Chicago mathematical biologist, Nicholas Rashevsky. Herbert Simon to Bernard Berelson, 6/18/56, Herbert A. Simon Papers, Carnegie Mellon University Archives, box 99.

where the latest developments in systems theory could be used to design Air Force strategy for a future in which technology would play an ever-larger role. RAND occupies a central position in histories of the Cold War, as a quick standby for the rationalization of warfare and the creation of the military-industrial-academic nexus. But RAND's military-oriented systems analysis also had a close relationship to the innovations in management theory. Above all else, RAND positioned itself as having a particularly scientific approach to analyzing sociotechnological systems and public policies. While initially having a strong military orientation, these concerns did not define the boundaries of Project RAND.[115]

Simon came to RAND in 1952 at the invitation of four analysts—William Biel, Robert Chapman, John L. Kennedy, and Allen Newell—studying the behavior of individuals within aerial defense warning stations. The members of this Systems Research Laboratory admired Simon's work on rationality within organizations. The group soon realized that they did not yet have an adequate framework for describing the behavior of individuals in these circumstances, and the project was deemed a failure. However, it launched a partnership between Simon and Allen Newell to study information processing in individuals. The laboratory was also revived and spun off as the Systems Development Corporation, which would train personnel for the Distant Early Warning System.[116]

Simon was fascinated with the computers at RAND and wrote to Lee Bach, the Dean of the GSIA at Carnegie Tech, "when I return, I shall be a real strong computer proponent, although I haven't the slightest idea as to what I would compute on such a toy if I had one."[117] Yet through his studies of decision-making, he encountered the suggestive cybernetic analogy between mind and machine. For Herbert Simon, the question of whether or not the architecture of the computer resembled that of the brain was largely beside the point. What mattered was that the computer provided a material stratum with which to investigate the properties of mind, and that the machine might eventually produce the same kinds of behavior as a mind.

Simon and Newell decided that symbolic logic would be an ideal test case for studying information processing, as the content of logic was a set of abstract symbols devoid of semantic meaning. They believed that more complex ideas could still be translated into this symbolic language, but logic remained a first step. Their decisive breakthrough was in December 1955, when Simon and Newell were back at RAND (joined by Cliff Shaw) at the invitation of Merrill Flood. Their system, named Logic Theorist, was designed to derive proofs from Russell and Whitehead's *Principia Mathematica* using methods similar to those employed by human logicians. Accuracy, rather than efficiency, was the primary goal. It worked by analyzing what sort of logical move would be most appropriate given the current state of the proof. Logic Theorist was implemented on the JOHNNIAC computer at RAND, named after John von

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[115] See David A. Hounshell, "The Medium is the Message, or How Context Matters: The RAND Corporation Builds an Economics of Innovation, 1946-1962," in *Systems, Experts, and Computers: The Systems Approach in Management and Engineering, World War II and After*, Agatha C. Hughes and Thomas P. Hughes, eds. (Cambridge, Mass.: MIT Press, 2000), 255-310.

[116] Simon, *Models of My Life*, 168. On the importance of DEWS as part of a cybernetic "grid of control" over the world, see Paul Edwards, *The Closed World*.

[117] Herbert Simon to Lee Bach, 7/22/54, Simon Papers, box 91.

Neumann.[118] While waiting for the machine, Newell, Simon and Shaw translated the computer instructions into sets of simple instructions to be implemented by a group of human processors. Simon's family members and graduate students were given program instructions to be executed when instructed. This mathematical assembly line generated logic proofs without anyone doing anything more than simply following instructions. Said Simon, "Here was nature imitating art imitating nature. The actors were no more responsible for what they were doing than the slave boy in Plato's *Meno*, but they were successful in proving the theorems given them."[119]

Bertrand Russell was quite pleased by the ability of the computer to reproduce his work, and wrote to Simon to note, "I wish Whitehead and I had known of this possibility before we both wasted ten years doing it by hand. I am quite willing to believe that everything in deductive logic can be done by a machine." One proof was even simpler than the one in *Principia Mathematica*, a feat that greatly impressed him and suggested that a simple program could generate truly new proofs.[120]

While subsequent discussions of Logic Theorist's importance would focus on the fact that the proofs were generated by a machine, the demonstration of the principles of Logic Theorist with a mental assembly line highlight that the physical implementation was beside the point; what mattered was that the heuristic processes—the acquisition of which constituted "learning" for math students—had been performed by an organized group of people who individually did not know the entirety of the process. Previously, mathematical proofs could only be generated by mathematicians who contained all of the intuitions within one head. Simon and Newell instead used heuristics as a sort of assembly line for discovery. Crucially, they did not only see Logic Theorist as being a generator of proofs. They acknowledged simpler methods for generating proofs, though rejected them as not being based upon a generalizable method of discovery.

The process of mathematical discovery remained mysterious, even with Newell and Simon's innovations. Marvin Minsky, for example, openly recognized the difference between how mathematicians did their work and how they believed they did it: "Mathematicians never talk about how they think about mathematics, and they worship their creativity as a God-given gift. They're hypocritical about teaching students because on the whole they believe that you can't teach students to be mathematicians—some of them have it or they don't…" Instead, he found that artists made far better sources of information about the creative process because they were "not superstitious about creativity; they're very concerned with it, and most of the artists I've talked to don't think much of the theory of talent, and they admit they learn things by

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[118] Willis H. Ware, *RAND and the Information Evolution: A History in Essays and Vignettes* (Santa Monica, Calif.: RAND Corp., 2008).

[119] Simon, *Models of My Life*, 206-207.

[120] In his second letter to Simon, Russell quipped that "the facts should be concealed from schoolboys. How can one expect them to learn to do sums when they know that machines can do better?" Bertrand Russell to Herbert A. Simon, 2/11/56 and 9/21/57, Simon Papers, box 114.

looking at other people's work and thinking about it and asking them how you do things, and so forth."[121]

Simon sketched the implications of the Logic Theorist for the study of management in a talk for the 1957 meeting of the Operations Research Society of America in Pittsburgh. Simon began the talk by noting how "Operations Research has had more to do with the factory manager and the production-scheduling clerk than it has with the vice-president and the Board of Directors."[122] The problem was that the highest levels of management faced unstructured problems. They handled precisely those problems that were not amenable to the type of systematic study where OR excelled. Yet Simon and Newell's creation of the Logic Theorist suggested that automating heuristic processes could create systems for general problem solving.

A common criticism of office automation was that machines could do the work of a low-level clerk, but could never perform the complex and creative thinking required of chief executives. Simon not only disagreed with this assessment but went one step farther. He claimed that machines could do creative work just as well as humans and brazenly made four predictions:

1. That within ten years a digital computer will be the world's chess champion, unless the rules bar it from competition.
2. That within ten years a digital computer will discover and prove an important new mathematical theorem.
3. That within ten years a digital computer will write music that will be accepted by critics as possessing considerable aesthetic value.
4. That within ten years most theories in psychology will take the form of computer programs, or of qualitative statements about the characteristics of computer program.[123]

These predictions would later come back to haunt them, becoming the first in a series of bold predictions on behalf of artificial intelligence that would not come to pass. Indeed, mathematician Richard Bellman declared that "anyone who has examined the formidable difficulties in the formulation and recognition of problems, the construction of criteria, and the prescription of policies, much less the programming of machines to accomplish some of these tasks, will deplore braggadocio of this type."[124] Yet these four points explain the broad scope of the Newell-Simon research program; automating heuristics would produce intelligence in all of its messiness and unpredictability. Rather than simply being an imposition of order to reach a

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[121] Quoted in McCorduck, *Machines Who Think*, 204.

[122] Herbert A. Simon and Allen Newell, "Heuristic Problem Solving: The Next Advance in Operations Research," *Operations Research* 6 (1958): 1-10, on 5. The distance between Simon's cautious statements about the limitations of rule-directed behavior and his brazenly optimistic outlook on the future of AI remain striking.

[123] Ibid., 7-8.

[124] Richard Bellman, "On 'Heuristic Problem Solving,' by Simon and Newell," *Operations Research* 6 (1958): 448-449. Simon and Newell defended the predictions as part of their professional obligation to analyze the social implications of their field of expertise in "Reply: Heuristic Problem Solving," *Operations Research* 6 (1958): 449-450.

pre-established goal, automating heuristic reasoning would allow computer systems to create novelty out of the cold logic of the machine.[125]

Yet by making these strong claims on behalf of heuristic artificial intelligence, Newell and Simon opened themselves up to charges that they were reducing intelligent behavior to a top-down process of logical manipulations of symbols. Their agenda for AI recognized the significance of the external environment for decision-making, and focused on mathematical demonstrations precisely because those seemed easiest because they did not require an understanding of context. The goal was to use machines as an intermediate step to understanding human problem solving, rather than seeing the machines as ends in themselves. The differences here between Simon and Newell and the cyberneticists are smaller than usually believed.[126] The English cyberneticist H. Ross Ashby had expressed to Simon his conviction that they were working on similar projects: “It is my firm belief that the principles of ‘organization’ are fundamentally the same, whether the organization be of nerve cells in a brain, of persons in a society, of parts in a machine, or of workers in a factory. … Reading your papers has only confirmed this hunch, for it is obvious that we are thinking on closely parallel lines.”[127]

Simon and Newell’s agenda of “human information processing” contrasts strongly with the more traditional logical program of their contemporary, Hao Wang, for example, who described computing machines as “persistent plodders” rather than truly intelligent. According to him, this plodding and lack of creativity is precisely what made them ideal logicians: “Logicians had worked with the fiction of man as a persistent and unimaginative beast who can only follow rules blindly, and then the fiction found its incarnation in the machine.”[128] Wang expressed precisely what made it so difficult for others to place logic at the apex of human reason. Yet while he implemented this vision of logic in the machines (and was very successful at it), Simon and Newell tried to reproduce the actual creative processes—and were tarred with the same brush as Wang, without generating the results of their colleague.[129] Artificial Intelligence was not only a product of the usual cybernetic nexus of communications engineering, psychology, and mathematics; it drew as much from the political program of understanding the behavior of individuals within groups and environments as it did from conventionally technological sources. At stake in the development of mechanical intelligence was the question of whether the difference between manual labor and creative intellectual work was merely one of degree, or whether there was something qualitatively different.

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[125] On heuristics, see George Pólya, *How to Solve It* (Garden City, NY: Doubleday, 1957). Pólya had been one of Newell’s professors at Stanford.

[126] For example, Andrew Pickering contrasts the cybernetic “dance of agency” with Simon and Newell’s AI, which he portrays as being excessively concerned with representing ideas about intelligence. See Pickering, “Cybernetics and the Mangle,” 420. By contrast, Robert Boguslaw described the Newell, Simon, and Shaw system as a “creative alternative” to more formal systems. See Robert Boguslaw, “Systems of Power and the Power of Systems [1965],” in Alan F. Westin, ed., *Information Technology in a Democracy* (Cambridge, Mass.: Harvard University Press, 1971), 419-431, on 422.

[127] H. Ross Ashby to Herbert Simon, 7/23/53, Simon Papers, box 91.

[128] Hao Wang, “Toward Mechanical Mathematics,” *IBM Journal of Research and Development* 4 (1960): 2-22.

[129] See Donald MacKenzie, *Mechanizing Proof: Computing, Risk, and Trust* (Cambridge, Mass.: MIT Press, 2001).

Reflecting on the Origins of Computing

Howard Aiken cited Charles Babbage not only as a historical antecedent for his work, but also as a direct inspiration. Yet Aiken's engagement with Babbage seems quite superficial, in that the initial design of the Mark I ignored some features that Babbage actually had described.[130] Ed Berkeley, who had worked with Aiken, continued to repeat the homage to Babbage as a way of making the computer seem more familiar to non-technical audiences. The machine was not actually some complicated device for physicists, he claimed, but was *really* designed by a simple businessman a century earlier. Mathematicians from England, such as L. J. Comrie, lamented that the country that had produced Babbage, and later Turing, had failed to accomplish what the Americans had. The years between Babbage and the 1940s were the dark ages of computing.[131] Today the invocation of Charles Babbage as the father of computing is commonplace.[132]

But what kind of father was Babbage? His Difference Engine was never completed in his lifetime, as he lost interest in it in favor of his more complicated Analytical Engine, and the government dropped its support for lack of any concrete applications.[133] Yet even with the more sophisticated Analytical Engine, Babbage's interests remained rooted in his theories of administration rather than in abstract mathematics, as both Simon Schaffer and Jon Agar have explained.[134] The idea of the Difference Engine as a device for the manufacture of numbers was based on Babbage's efforts to systematically analyze the logic of the factory system. According to his *On the Economy of Machines and Manufactures* from 1835, "the master manufacturer, by dividing the work to be executed into different processes, each requiring different degrees of skill or of force, can purchase exactly that precise quantity of both which is necessary for each process."[135] The same analysis and rationalization could be applied to mathematics.[136] Babbage's relocation of "intelligent behavior" into the machine mirrored the ongoing concentration of planning and intellectual work within the management of the factory.

Babbage described his Difference Engine to the prime minister in 1852 as a machine that would extend the rationalization of the factory system to the intellectual work of bureaucracy. The government eventually stopped supporting his work even as it continued to develop the

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[130] Cohen, *Howard Aiken*, 61-72.

[131] L. J. Comrie, "Babbage's Dream Come True," *Nature* 158 (1946): 567-568.

[132] For example, the major archive in the United States for the history of information technology is named the Charles Babbage Institute, which was founded in Palo Alto in 1977, and is currently located in Minneapolis. See http://www.cbi.umn.edu.

[133] The Difference Engine was finally built in 1991. For a demonstration recorded at the Computer History Museum in Mountain View, Calif., see http://www.youtube.com/watch?v=KBuJqUfO4-w (accessed 4/10/2010).

[134] See Simon Schaffer, "Babbage's Intelligence: Calculating Machines and the Factory System," *Critical Inquiry* 21 (1994): 203-227, and Jon Agar, *The Government Machine*.

[135] Charles Babbage, *On the Economy of Machines and Manufactures* (New York: New York University Press, 1989), 175-176.

[136] On the idea of "algorithm" as a unifying theme for Babbage's varied interests, see I. Grattan-Guinness, "Charles Babbage as an Algorithmic Thinker," *IEEE Annals in the History of Computing* 14 (1992): 34-48.

analogy between bureaucracy and mechanism, formalized in the Northcote-Trevelyan reforms of 1854 that split the civil service into a higher grade of generalists and a lower one of specialists who could be entrusted to faithfully execute their orders.[137]

Babbage's significance remains intimately bound up with the industrial world of 19th century England, but the invocation of his name in mathematically-oriented histories is done quite explicitly to suggest a generality for the idea of computing that liberates it from the world of 1940s and '50s America, aerial defense, IBM, and Prudential Insurance. Yet, it is no coincidence that so much of the inspiration for computing in the United States in these years came from administrators and accountants rather than from logicians. Charles Babbage may well be considered the father of computing, but not in the way that his children understood it.[138]

Histories of computing have emphasized the distinction between hardware and software, between the physical machine and the non-physical information. Yet rather than privileging one over the other, we must recognize the two as necessary complements of each other. Tracing the idea of information processing to Leibniz or to Smith brings us no closer to understanding computing, nor does tracing the origins of this machine back to the abacus. Modern computing is a product of the mid-twentieth century due to a confluence of ideas about organization and rationality and due to the creation of specific technologies. The physical manipulation of these new machines created new understandings of computing; programmable machines were different in important ways from ones that had hard-wired instructions, and the speed of electronics also fundamentally altered the experience of using computers and the types of problems they could solve.

Different groups of computer researchers emphasized different aspects of computing, yet regardless of whether they focused on the machines or the theory, they all remained in dialogue and created a coherent discourse of computing. Thus, Simon and Newell's Logic Theorist relied on a novel process for doing math while Hao Wang instead relied on the speed of electronics. One wanted to model the machine on how mathematicians, as humans, actually worked (relying on hunches, suspicions, and other illogical activities), while the other modeled his programs on how logicians described the ideal ways of doing math.[139]

The boundaries of computing were not sharp; rather than being a well-defined technology it was a large cluster of ideas and machines and institutions that altered the trajectories of

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[137] The ambiguity in this analogy was important. The "mechanical" character of the bureaucracy made it more trustworthy (see chapter 1), while the decision not to press the mechanical analogy too far (as Babbage's device did) let the civil servants retain their expertise and substantial autonomy over their working conditions. See Agar, 43. In tracing the metaphor of the bureaucratic machine, Agar describes British debates about the "Prussianization" of the civil service, and the importance of keeping the executive powers of the civil service subordinate to the legislature.

[138] One exception to this interpretation of Babbage as mathematician and logician among computer pioneers was the political scientist, Herbert Simon, who used Babbage to identify the social sciences as the origins of computing. Against the traditional view, he claimed in his provocative 1957 ORSA talk: "physicists and electrical engineers had little to do with the invention of the digital computer—the real inventor was the economist Adam Smith." Simon and Newell, "Heuristic Problem Solving," 2.

[139] For more on the differences between the Newell/Simon project and Wang's project, see Daniel D. McCracken, "A Progress Report on Machine Intelligence," *Datamation* 6 (1960): 10.

everything that came near it. The intellectual influences of computing were diverse—from the rational management of work to the relationship between brains and minds to the limits of axiomatic foundations of mathematics. The mechanical aspects included anything that manipulated information (digital or analog) and ultimately anything that incorporated processes of feedback. The flexibility of computers is usually attributed to their logic circuits, where the materiality of the machine meets the versatility of symbolic logic. Yet this misses one important point: the state of mathematics and organizational theories in the 1940s were both already ripe enough to be taken in the many different directions that the machines allowed. It was not only the nature of the machine that allowed for contested meanings; the many points of contact between computing and the rest of American culture meant that it would be influenced in many, often contradictory, ways.

Part II

The previous chapters have examined the history of administrative and managerial theories as well as the connections between computing and automation. The next two chapters make the connections between these two strands explicit. These connections existed on two levels: the use of computer systems as governing social interactions among groups of users, treated in chapter three, and the promise and danger of modeling individual behavior after models of rationality built into computers, the subject of chapter four.

A fundamental approach taken in this section is to blur the distinctions between the design of technologies and the creation of institutions and social norms. Demarcations between "the technological" and "the social" remain important in both chapters because they continue to function as powerful rhetorical devices within debates about the social significance of computing. The problematic notion of technological agency is another pervasive theme in the pages that follow. In these cases, the seeming power of inanimate actors is something deliberately constructed, which shows both the solidity of objects and the magnitude of the learned incapacity leading to passivity in the face of technological change.

These chapters describe the basic problems of working among computers, as they were first discussed in the late 1950s and 1960s. These problems remain unresolved in the following chapters—and, indeed, they remain so today. They culminate in the third section of this dissertation, in a conflict over different visions of a computerized society that was simultaneously tied to broader political and economic debates in American society. The final section thus updates the histories from the first chapters and brings them into the final quarter of the twentieth century.

Chapter 3: Interacting With Machines

The immediate aftermath of World War II was a clear turning point in the history of American science and technology. The far-reaching effects of this period ranged from the creation of the atomic bomb, radar, and computers to the institutional empowerment of scientists and patronage agencies; from the expansion of science education and the international superiority of American universities to the heightened social position of scientists. Much of the contemporary historical research on this era concerns the surprising and complex interactions among these transformations.[1] A powerful lesson from this scholarship is the permeability of the seemingly fundamental boundary between the scientific and social aspects of this history. To take a familiar example, the concretely technical work of building an atomic bomb in the Manhattan Project forged ties between a generation of scientists and the national security apparatus with repercussions that would last for decades and that would shape both the institutions of American physics as well as the content of the science itself.

The postwar history of computers differed significantly from that of the nuclear establishment. The most immediate difference concerns the organization of research: the Manhattan Project brought together nearly all American scientists with an interest in and knowledge of nuclear physics, while computing research at the end of World War II had produced two important, though substantially different, machines, with no defined community of computer scientists. As explained in the previous chapter, the very notion that some fundamental science of “computing” described the operations of both of these machines was only gradually being articulated in the immediate postwar years. The meaning of the nuclear weapons establishment was stable, as all of its members (human or otherwise) had passed through the institutional bottleneck of the Manhattan Project. By contrast, the field of computing remained wide open. Its boundaries were only dimly perceptible and its core remained poorly defined.

These differences suggest that the state of 1950s computing simply cannot be described with the sort of specificity that is possible for more narrowly defined subjects.[2] However, specific computing projects can be studied. The warnings from the previous chapter continue to hold; understanding the development of computing technologies requires simultaneously investigating technical innovations and the changing social meanings of the work done by these machines. This chapter focuses on the development of interactive, time-shared computing at MIT, considering it as an example of “heterogeneous engineering” in which the technology, the social norms governing users, and the networks of patronage tying the institute to its state

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[1] For a good summary of the current state of the literature, see Hunter Heyck and David Kaiser, “Focus: New Perspectives on Science and the Cold War,” *Isis* 101 (2010): 362-366.

[2] The ease of identifying precisely what is or is not a computer seems to be disappearing as of this writing. The notion that the term “computer” refers unproblematically to easily identifiable mainframes or personal computers may be a relic of the 1970s-90s. The state of 1950s and 1960s computing may be surprisingly relevant for contemporary technological questions.

patrons and its corporate partners all contributed to the operations of the computing system.[3] The institution building at MIT also functioned as a way to educate and train students—leading to the creation of a coherent educational program. Zooming out to the national level, professional societies helped the computer research communities from other universities (particularly Carnegie Tech, MIT, Stanford, and the University of California at Berkeley) coalesce around a shared idea of computing.

Time-sharing was an important technological development that reshaped the relationships between groups of computer users and their machines. By promising access to a shared machine mediated only by a supervisory program, it confronted both the culture of traditional industrial data processing (with dedicated technicians handling the machines) and the culture of unmediated computer access found in other research labs such as MIT's growing AI community. This meant a new role for the computer user who no longer interacted with the machine through the medium of trained technical experts, and who had to make some concessions to the fact that this machine was being shared among many users. The distinction between higher (more abstract, done by men) and lower (actually interacting with machines, done by women) forms of programming vanished, and with it vanished the positions—not quite clerical, not quite managerial; technical, but not entirely intellectually respectable—that provided women with a leading role in early computing. The tenuous position of women in computing was based on the perception that programming was an extension of stenography—a perception that eroded with the growing responsibilities of now-predominantly-male computer professionals.

Computers in these years remained closely tied in the public imagination to the large organizations that used them, as explained in the previous chapters. This association meant that debates about the development of American political economy often implicitly influenced the shifting meanings of innovations in computing. In the late 1950s and early 1960s, social science theorists used the growing bureaucratization and technocratic leadership of American corporations and government to signal a profound change in the character of American capitalist democracy. The choice between personalizing computing through time-shared systems (analogized to public utilities) or through minicomputers (understood in terms of personal property) took on an ideological dimension shaped both by technical ideas of computational efficiency and by political ideas of how to situate individual users within the community.

The idea of man-machine interaction thus had significance well beyond the immediate question of what interface should exist between computers and their users. Wrapped up in the varied forms of man-machine interaction were such questions as: who should have access to the machines? How autonomous would these computer experts be? By what authority did they have any power, and to whom were they responsible? Computer scientists did not always address these questions directly. However, there were many indirect points of contact where the immediate work of computer design addressed these questions of power, and where cultural notions of authority shaped technical work.

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[3] See John Law, "Technology and Heterogeneous Engineering: The Case of Portuguese Expansion," in W. E. Bijker, T. P. Hughes, and T. J. Pinch, eds., *The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology* (Cambridge, Mass.: MIT Press, 1987), 111-134.

This chapter begins with a brief synopsis of MIT's first foray into military computing with the Whirlwind computer. This provides essential background for the rest of the chapter, which examines MIT's efforts to create institutional support for computing. This came in several stages. First, computer researchers had to actively develop an interest in computer methods. Computers had been valued for their ability to solve important problems but had not been seen as deserving much attention themselves. This began to change, through the influence of two groups of people: academics one step removed from the labs where computers were actively built and designed, and the first generation of computer students who were interested in computers themselves and not merely in what computers could do for others.

MIT wanted to create a true computing community, which required serving users in a timely manner while also building a self-sustaining network of engineers, theorists, administrators, and patrons. The specific character of the computing systems at MIT drew upon its particular configuration of human and technological components, creating a new mode of interactive computing and articulating a certain social theory of computing. Looking beyond MIT, the development of computer science as a discipline was simultaneously a way of defining the role of computers in modern society and a way of defining an agenda for research. Because the creation of computer science followed from the turn to general computer methods, to understand the social significance of computers we must look at the specific work that they did rather than the distant ends for which they were directed. This brings the immediate relationship between user and machine into the foreground while pushing the ultimate goals of patrons into the background. Consequently, the matter of control in computer science becomes one of mastering an obstinate electronic partner rather than one of imposing a uniform legibility on the world.

Patronage and the Military

While the earliest computer research programs remained under military patronage, the situation began to change in the postwar years as scientists organized their own laboratories and projects. These scientists created the institutional support structures for a self-supporting academic discipline as well as the technological and intellectual core of the subject. This process had started during the earliest stages of military computing as researchers abstracted from their immediate problems to understand the nature of work, information, and communication.

The close relationship that developed between the military and scientists in the years after World War II is by now well understood, though the implications of this relationship for the science remain debated. This is particularly important for computing, which has had a long and complex association with the military. The usual question concerns the degree to which scientists tailored their work to fit the priorities of their military patrons.[4] The less commonly asked

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[4] The classic statement remains Paul Forman, "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960," *Historical Studies in the Physical Sciences*, 18 (1987): 149-229. Dan Kevles questioned the terms of this argument by denying the existence of any natural development from which the actual course of events deviated. This has the historiographically advantageous position of being both perfectly true and also failing to resolve the heart of the matter, which is that by embracing military patronage, scientists tied themselves to the military (and national security more broadly) in myriad complex ways. See Dan Kevles, "Cold War and Hot Physics: Science, Security, and the American State, 1945-1956," *Historical Studies in the Physical and*

question is how the influence of scientists altered ideas of warfare and command-and-control within the military itself.[5]

The history of computing is plugged into both of these questions. Less thoroughly militarized and regulated than the nuclear establishment, the world of computing was nevertheless flooded with military money. Yet this was due as much to military strategists incorporating new ideas from communication theory as it was due to the military influencing the priorities of computer scientists. This new understanding of command-and-control is described well by Paul Edwards, for whom the "closed world" of Cold War computing was built upon a very particular, and particularly seductive, fusion of militarism and engineering.[6]

Exhibit A in the case for computing as fundamentally militarized is the Whirlwind computer and the SAGE (Semi-Automated Ground Environment) aerial defense system, built at MIT in the decade after World War II.[7] Whirlwind was originally designed as an analog flight simulator for the Navy, however that changed when Jay Forrester (who had been in charge of the project at the MIT Servomechanisms Laboratory) decided to pursue digital computation instead. Forrester had previously worked on Numerical Control systems in the Servo Lab.[8] From this experience he gained a greater appreciation of the power of automatic digital control systems. He recognized that a digital computer could be a general-purpose machine, and so Whirlwind was converted into a research project for digital computing.[9]

Whirlwind was part of MIT's embrace of Cold War, military-funded research. These research connections had been developed during World War II, and they continued into the postwar years. While military patronage had allowed both the Mark I and the ENIAC to be built

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Biological Sciences, 20 (1990): 239-264. A detailed study built around understanding the influence of the military on computer research is Arthur L. Norberg and Judy E. O'Neill, *Transforming Computer Technology: Information Processing for the Pentagon, 1962-1986* (Baltimore: Johns Hopkins University Press, 1996).

[5] See Martin van Creveld, *Command in War* (Cambridge, Mass.: Harvard University Press, 1985), 232-260. Also see Stephanie Young, "Power and the Purse" (Ph.D. dissertation, University of California, Berkeley, 2009).

[6] Paul N. Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America* (Cambridge, Mass.: MIT Press, 1996). Much scholarship in science and technology studies focuses on how scientific knowledge has functioned to increase the state's ability to render the world legible. For important examples, see Yaron Ezrahi, *The Descent of Icarus: Science and the Transformation of Contemporary Democracy* (Cambridge, Mass.: Harvard University Press, 1990) and James C. Scott, *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed* (New Haven, Conn.: Yale University Press, 1999). However, the remaining chapters show computer science going in the opposite direction—expanding the possibilities of computing at a dizzying rate, leaving the state without a monopoly on the ability to impose meaning on the world.

[7] What follows is a compressed history of SAGE, highlighting its connections to work going on elsewhere at MIT. For more thorough treatments, see Thomas P. Hughes, *Rescuing Prometheus* (New York: Pantheon, 1998), 15-67; Martin Campbell-Kelly and William Aspray, *Computer: A History of the Information Machine* (Boulder, Colo.: Westview Press, 2004), 141-152; and Edwards, *The Closed World*, 75-111.

[8] On Numerical Control, see chapter 2, and see David F. Noble, *Forces of Production: A Social History of Industrial Automation* (New York: Oxford University Press, 1984).

[9] Fernando Elichirigoity, *Planet Management: Limits to Growth, Computer Simulation, and the Emergence of Global Spaces* (Evanston, Ill.: Northwestern University Press, 1999), 46-47.

during the war, both machines had origins in scientific research problems. Whirlwind was purely a product of military priorities. Though Forrester took Whirlwind in a different direction, its ties to the military were reinforced when it became the heart of the SAGE project to coordinate a vast aerial defense network.

The seeds of SAGE lay in MIT's series of summer studies on defense questions. Jerrold Zacharias, a physics professor, organized these studies as interdisciplinary collaborations to think through problems of national interest. The goal was to think freely about defense (on the government's dime) rather than to create new weapons or institutions. The first such study, Project Hartwell (1950, led by Zacharias), dealt with anti-submarine warfare, while the second study, Project Charles (1951, directed by F. Wheeler Loomis of the Rad Lab), dealt with using computers to centralize defense information processing and led to the creation of MIT's Lincoln Laboratory, which focused on research for defense, and which ultimately housed Whirlwind.[10]

MIT professor George E. Valley, Jr. organized a committee for the Air Force Scientific Advisory Board to think about aerial defense in 1950, suggesting that a network of automated information processing centers could monitor the input of radar arrays to provide comprehensive coverage of American air space. Valley needed a computer, but was warned against using the Whirlwind for his aerial defense system. Whirlwind had a reputation as a frivolous project, in which the technical work done had little bearing on the project's stated goals. In the words of its historians, Kent Redmond and Thomas Smith, "the tail had passed through and beyond the point of wagging the dog and had become the dog."[11] Yet, despite the warnings, Valley had nowhere else to turn. Furthermore, Forrester was having problems with his patrons in the Navy and looked forward to shifting to Air Force support.[12] In the cybernetic language of the project, Whirlwind became the nervous system of SAGE.

In fact, SAGE owed at least as much to ways of thinking particular to industrial engineering as it did to military influences. Forrester, in the introduction to his *Industrial Dynamics* of 1961, described how this work blurred the distinctions among military command-and-control, automation, and industrial engineering. Looking over the decade between the creation of Whirlwind and *Industrial Dynamics*, he observed that "In a mere ten years these automatic decisions [automatic threat evaluation, weapon selection, friend or foe identification, alerting of forces, or target assignment] were pioneered, accepted, and put into practice. In so doing, it was necessary to interpret the 'tactical judgment and experience' of military decision making into formal rules and procedures."[13] This effort to transform inchoate judgments into formal processes and thereby make them automatic (whether done by machine or by reliable workers) was one that extended deep into 20th century industrial thought, as seen in the previous

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[10] For MIT's ties to military research, see Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993).

[11] Kent C. Redmond and Thomas M. Smith, *Project Whirlwind: The History of a Pioneer Computer* (Bedford, Mass.: Digital Press, 1980), 46.

[12] For the ONR's involvement with Whirlwind, see Mina Rees, "The Computing Program of the Office of Naval Research, 1946-1953," *Communications of the ACM* 30 (1987): 832-848.

[13] Jay Forrester, *Industrial Dynamics* (Cambridge, Mass.: MIT Press, 1961), 17.

chapters. Parallels certainly existed within the organization and discipline of the military, but the impetus came from the history of industrial engineering.

Institutional Computing at MIT

The importance of machine methods in the study of computing—the part that would be considered computer science today—had to be deliberately promoted to patrons who understood the significance of computers in terms of the problems that they could solve. Computers were valuable because they helped scholars study more important phenomena. The notion that the study of computers could be valuable in itself took longer to cultivate. This early history of academic computing, shows computer scientists balancing the needs of both their patrons (both government agencies and private corporations) and their home institutions with their own intellectual interests. The construction of a computing community at MIT required simultaneously designing new computer systems and engineering new organizations and institutional norms.

Philip Morse took the first step in organizing computation for the wider MIT community by creating a Center for Machine Computation in 1951. In Morse's vision, half of Whirlwind's computing time would be dedicated to its contracted Air Force work, while the rest would be available for research into computing methods. He requested support from the Office of Naval Research, one of the major sources of science funding, to study machine hardware and to develop techniques for numerical analysis, and to train students (both graduate and undergraduate) to use these machines.[14] This established a pattern of supporting research into computing methods in tandem with expanding the computing services available to those using computers as scientific tools. The intellectual respectability of computers grew out of investigating their uses as mathematical tools, not from an a priori appreciation of their complexity.

By 1954, however, military computing patrons were starting to demand results and cut back on their lavish funding during the post-Korean War recession. Morse asked MIT Chancellor Julius Stratton to create policies to protect computer access; MIT needed to be more proactive in planning its computing facilities. Morse noted that MIT had two centers of computing: Lincoln Laboratory, which housed the Whirlwind machine (used for research), and the Office of Statistical Services, which did the Institute's data processing. Both centers charged for computer access, and this access would be prohibitively expensive for students without the support of a body such as the ONR. Morse suggested that the two computing centers at MIT could be merged to save money on personnel. He recognized that other labs might purchase or rent computers as needed, but remained adamant that some basic level of computer access be guaranteed throughout the institute. "The Institute <u>must</u> make arrangement for the direct financing of

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[14] Philip Morse letter to ONR, 2/16/1951, and memo to N. McL. Sage, 2/20/1951, MIT Computation Center Records, MIT Archives and Special Collections, AC 62, box 1.

computing equipment adequate to train the next generation of engineers and scientists," he wrote.[15]

Training in computer methods was essential training for the future. Morse emphasized the significance of computers for MIT's educational mission. Many businesses valued hiring graduates with computing experience, and computers were necessary to train students for the technical jobs of the 1950s and beyond. IBM in particular wanted graduates with computer skills and was eager to invest in universities that could provide this training. Cuthbert C. Hurd of IBM had suggested to Morse that the company would be willing to donate a computer in order to train young engineers. Morse proposed creating a consortium of local schools, with MIT at the head, in order to split the costs of running the machine and to make a more compelling case for support from IBM and from science funding agencies.[16] He urged MIT's administration to take the initiative and reach out to both IBM and other universities and colleges in New England.

It was still unclear what the ultimate significance of computers was. Was it primarily an administrative tool? A tool for scientific research? Given this uncertainty, responsibility for computing remained dispersed throughout MIT. Morse, writing with Eli Shapiro, Dean of the School of Industrial Management, argued in 1955 that the SIM should be the center of computing at the Institute. While scholars from every department were using computers in their own work, SIM's expertise in studying both the technologies of automation and the social consequences of these technologies made it a natural focal point for computing and a natural home for the administration of MIT's computational resources.[17] Despite Morse's backing, this was not to be—partly out of inertia among scientists and partly due to apathy among some of the industrial management faculty. Instead, the particular challenges of running a computing center would gradually push MIT's computing leaders to create their own institutional support networks.

IBM donated a 704 machine to the Institute in 1956, placed in the newly established Computation Center which took over the task of being the school's central computing facility. This center was nominally run by Morse, but in practice was run by his two assistants, Frank Verzuh and Fernando Corbató.[18] The arrangement gave ten hours of access to the machine every night to IBM, seven hours of access to the members of the New England consortium, and the remaining seven hours to MIT. Morse articulated two crucial goals in the Computation Center's funding applications: to do scientific research that required the machine's unprecedented

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[15] Memo from P. M. Morse to J. A. Stratton, 12/28/1954, and "Requirements for Electronic Computing Equipment for Training and for Educational Research at the Institute," MIT Computation Center Records, box 1.

[16] Memo from P. M. Morse to J. A. Stratton, 12/28/1954, and Memo from P. M. Morse to President James R. Killian, 7/20/1955, MIT Computation Center Records, box 1.

[17] Letter from Eli Shapiro and Philip Morse to Rowan Gaither, 10/27/1955, Jay Forrester Papers, MIT Archives and Special Collections, MC 439, box 53.

[18] Corbató had previously done some work on Whirlwind with a fellow physics graduate student and Morse advisee, John Little, who later became Institute Professor for his work on marketing. The range of Morse's influences can be seen here, with Little becoming a leader in Operations Research, while Corbató became a leader of the burgeoning computer community.

computing power, and to do research on computer methods.[19] These two goals were fundamentally interrelated. The research into computer methods would make the machine a more versatile tool, while the questions asked by researchers using computers would suggest new directions for increasing computer power.

The study of computer methods was still done in the service of practical work, and was not yet a field of mathematical significance. But this was slowly beginning to change. The staff of the Computation Center had begun to push back against the popular idea that computers only followed instructions and therefore could not generate any truly novel results. The fact that computers were constructed by people did not mean that those people could automatically predict their behavior. As Fernando Corbató wrote to the NSF in 1957, "a computer of the size of the 704 has an order of complexity such that its designers literally are not aware of all of its capabilities. Consequently it is important that research into these capabilities be carried on; such research, revealing easier ways of obtaining results and new methods of operation, may at times go farther in increasing machine serviceability for the general user than can the providing of a new machine with increased speed or capacity."[20] Computers were complex enough that they did not simply and straightforwardly "do as they were told." Or rather, they only did as they were told—but in doing so they uncovered startling implications of their starting premises. Clever engineers learned how to make them do novel tasks from simple programs. Discovering the capabilities of these machines required sustained study even though they were built upon simple and intelligible electronic and logical foundations. These were human creations whose capabilities exceeded the capacities of human understanding. Those working with computers began to recognize that the study of these machines had to be approached in a way analogous to natural science.[21] Computers were turning out to be far more complicated and more powerful than expected.

Morse was particularly interested in exploring what computing could do for the social sciences and for Operations Research. Scientists and engineers had been quick to recognize the usefulness of computing machines, but that was because they were close to the communities of electrical engineers and mathematicians who were building them. Morse suggested that an even greater potential existed for the application of computing machines to research in the social sciences. Such research projects perhaps needed different mathematical tools than did those in the physical sciences, and possibly needed to handle fundamentally different kinds of inputs and

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[19] "The Application of Digital Computers to Social and Operational Problems; Opportunities and Needs," 3/7/1956, MIT Computation Center Records, box 1.

[20] "Proposal for NSF Grant for Support of Research at the MIT Computation Center into Methods for Making High-Speed Digital Computers Easier to Use by the Occasional User," 1957, MIT Computation Center Records, box 1.

[21] In this respect, the unruliness of computers characterizes them with Norbert Wiener's "Augustinian Enemy" from chapter 2—intransigent due to their fundamental unintelligibility, rather than being intransigent because they have been actively designed in that way.

outputs.[22] The suggestion was vague, but the promise of computing methods seemed boundless. The potential of this powerful tool suggested that it was time to think outside the box.

These systems could only be created by bringing social scientists who were deeply interested in methodology together with engineers and mathematicians. It would require training social scientists in the basic operations of computing machines. In a letter to the Rockefeller Foundation, Morse set forth the three major programs needed to make computers useful for social sciences: first, understanding machine logic so that the "rules of thought for the machine" could come into greater correspondence with the forms of inference used by social scientists. Second, developing ways of storing, organizing, and manipulating large amounts of information—a project that would involve close collaboration with librarians.[23] Third, using computers to develop new forms of experimentation in the social sciences, which drew the attention of psychologists (including veterans of the Psychoacoustics Lab, such as George Miller and J. C. R. Licklider), economists (doing both econometrics and modeling), and the scholars at MIT's Center for International Studies (where Ithiel de Sola Pool and Theodore Baer were studying redistricting).[24] The connections between the social sciences and computing were not fully developed, but the potential for growth seemed clear.

Ideals of Interactivity

MIT maintained two machines for doing research into computation, each with its own mode of operations. At first, users could reserve blocks of time and run the IBM 704 directly. This allowed users to adjust their programs if they were not written correctly, though they had only a short window in which to fix them. The culture of computing generally frowned on tinkering with programs; the ideal was to write the program correctly the first time through, but in practice this was rarely possible. Bugs indicated failures of forethought. Tinkering meant that much of one's allotted time on the machine was spent idling.

Demand for the machines grew even more as new programming languages made it easier to learn how to write for the machine. The creation of the Fortran (Formula Translation) language in 1957 was a significant turning point in expanding access to computing.[25] However, this demand meant that the backlog of requests for time grew uncomfortably long. Rather than giving users access to the machine directly, the Comp Center staff switched to a batch processing

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[22] See "The Support of Machine Programming Staff for the Utilization of Electronic Computers in Social Science," "The Application of Digital Computers to Social and Operational Problems; Opportunities and Needs," and "Proposal for Research into the Effects of Automatic Control and Machine Processing of Operating Data on Social and Industrial Operations," MIT Computation Center Records, box 1.

[23] While librarians were quick to seize upon the application of computers to their field, this interest was not always returned. Norbert Wiener dismissed the utility of computers for cataloguing by emphasizing the importance of the tacit knowledge of librarians. See letter from Norbert Wiener to David K. Maxfield, 3/17/50, Norbert Wiener Papers, MIT Archives and Special Collections, MC 22, box 7, folder 114.

[24] Letter from Fernando Corbató to Norman Buchanan, 11/29/1956, MIT Computation Center Records, box 1.

[25] M. Mitchell Waldrop, *The Dream Machine: J. C. R. Licklider and the Revolution that Made Computing Personal* (New York: Penguin, 2001), 165.

system in which users submitted programs that were run sequentially on the machine by trained operators. Users could pick up their results later, often after a delay of 24-36 hours. The computer ran around the clock, though if a program was written incorrectly the user would not find out until several hours later and would then still need to submit a new, corrected request (and once again wait for results). The time needed to write programs, run them, debug, and get usable results meant that the use of the computer was impractical for projects with short deadlines, such as in undergraduate instruction, and that the difficulty of getting results from the machine deterred both graduate students and faculty members from doing otherwise interesting research.[26]

While the Comp Center switched the IBM 704 to batch processing, MIT's TX-0 (the descendant of the Whirlwind, based at Lincoln Laboratory) retained a culture of direct access. Users had to sign up weeks in advance for an hour of time, during which they had the machine at their fingertips. The two computing cultures that developed around these machines persisted through the coming years, and the physical separation of the two groups created many of the fundamental divisions within ideas about computing at MIT.[27]

The two computing groups maintained unique identities. The circle around the TX-0 grew into the core of MIT's Artificial Intelligence community, while the members of the Computation Center, using the IBM 704, designed the architecture of computer programs and operating systems. The AI community became famous for its hackers who took a freewheeling approach to the machines, in contrast to the more bureaucratic work of those concerned with operating systems. There was a certain consistency between the cultures of the computer labs and the types of work done within them. Yet these designations were not rigid; scientists often had idiosyncratic interests. For example, John McCarthy, one of the fathers of AI, led the effort to create a system of time-sharing at the Comp Center (discussed below). Jay Forrester had gone from being the architect of the Whirlwind (described only somewhat tongue-in-cheek by DEC founder Ken Olson as the "first minicomputer") to being one of the biggest users of data processing at MIT for his Systems Dynamics research in the business school, and remained a late convert to time-sharing.[28] Wesley Clark, one of Lincoln Laboratory's main computer scientists and a proponent of the earliest personal computers, remained a critic of both time-sharing and networking as late as the 1990s.[29] Individual attitudes toward developments in computing varied considerably.

The transition from sharing large computers to using personal computers remains one of the defining moments in the history of computing, and histories often revolve around this issue.

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[26] Fernando Corbató, oral history interview with Steven Webber, 2/1/2006, Computer History Museum Oral History X3438.2006.

[27] John McCarthy, oral history interview with William Aspray, 3/2/1989, Charles Babbage Institute, OH 156. The work of the AI community is emphasized in chapter 4.

[28] For Ken Olson's claim, see Robert R. Everett, "WHIRLWIND," in Metropolis, Howlett and Rota, eds., *A History of Computing in the Twentieth Century*, 365-384, on 384. For Forrester's acceptance of time-sharing, Robert M. Fano, oral history interview with author, 2/20/2009.

[29] See Wesley Clark, oral history interview with Judy E. O'Neill, 5/3/1990, Charles Babbage Institute, OH 195.

While there were certain ideological differences in MIT's computing cultures, these differences do not map onto an easy narrative of computers becoming more "free" or more "personal."[30] Rather than pitting "personal" computing against "centralized" computing, the salient distinction ought to be between computing as a matter of erasing the distance between user and machine (at the potential cost of wasting computer resources), versus computing as a matter of instituting rules to govern a community of users (at the potential cost of making the computer-as-machine more obtrusive).

Time-sharing at MIT began with a memo sent from John McCarthy to Philip Morse on January 1, 1959. McCarthy described how the batch processing system was inadequate for the current uses of the computer. That system made sense for programs that would be written once and then run repeatedly. Such applications could include much of commercial information processing, where the logic of automation favored making large capital investments to reduce labor costs and to increase standardization. However, McCarthy suggested that programming (at least in academic settings) had shifted toward a system of small programs that were written to be used only a few times.[31] Given that approach to programming, the time spent debugging was a significant cost that could not be recouped over the long run. Higher-level programming languages were important for making programming intuitive, and thereby reducing the need to debug, but a further simplification would come from simply reducing the turnaround time at the Computation Center.[32]

McCarthy's idea of time-sharing was not entirely novel. Christopher Strachey, a computer scientist from England, had also discussed a form of time-sharing as a means to simplify debugging, and McCarthy later claimed that he had been thinking about the general idea of time-sharing since 1955.[33] He described the idea as being "in the air" by 1959—though this did not make it uncontroversial. The significance of his memo was not only that it described a

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[30] The idea that minicomputers liberated computing from the mainframes was (and remains) widespread. See Fred Turner, *From Counterculture to Cyberculture: Stewart Brand, the Whole Earth Network, and the Rise of Digital Utopianism* (Chicago: University of Chicago Press, 2006). For an anthropological view of computing culture and this notion of freedom, see Chris Kelty, *Two Bits: The Cultural Significance of Free Software* (Durham, NC: Duke University Press, 2008). For the standard historiography and the transition to personal computing, see Martin Campbell-Kelly and William Aspray, *Computer: A History of the Information Machine* (Boulder, Colo.: Westview Press, 2004), and Paul Ceruzzi, *A History of Modern Computing* (Cambridge, Mass.: MIT Press, 2003). While the pioneers of minicomputing worked in the 1960s and 70s, this idea remained powerful for marketing the first personal computers for mass audiences. See the iconic 1984 Mac commercial (directed by Ridley Scott, following shortly after his *Blade Runner* of 1982) at http://www.youtube.com/watch?v=OYecfV3ubP8 (accessed 8/15/2010). However, this individualistic notion that personalized computing requiring personal ownership simply was not the only way to imagine interactivity.

[31] It is significant here that McCarthy also created the LISP programming language for AI research. LISP was designed for interactive use, and so the problem that McCarthy described was one that remained important for his research.

[32] Memo from John McCarthy to P. M. Morse, "A Time Sharing Operator Program for our Projected IBM 709," 1/1/1959, Philip Morse Papers, MIT Archives and Special Collections, MC 75, box 3.

[33] John McCarthy, "Reminiscences on the History of Time-Sharing," *IEEE Annals of the History of Computing* 14 (1992): 19-24. See also M. Mitchell Waldrop, *The Dream Machine: J. C. R. Licklider and the Revolution that Made Computing Personal* (New York: Penguin, 2001), 163-164.

novel way of organizing computer use; its more immediate significance was as a response to a very specific crisis with MIT's scarce computational resources.

To address this scarcity more systematically, Stratton created a Long-Range Computation Study Group at MIT in 1960 to look ahead and create a computational regime that would remain adequate for the foreseeable future. The group was led by Philip Morse, Al Hill (a former director of Lincoln Laboratory), and Bob Fano. Yet Morse and Hill were not on speaking terms, and so this group failed to do any meaningful work. Instead, responsibility for crafting the report fell to a working group chaired by Herb Teager. This group, too, had its problems. After extended discussions, Teager, known for being steadfast in his views and for compromising only with great difficulty, produced a massive report that was opposed by every other member of the group. Teager's recommendation was to purchase an IBM STRETCH machine in the short term, though his ultimate vision for time-sharing was so expansive that some other committee members worried that it could never come to fruition. The sense of urgency was felt most strongly by McCarthy, who took a lead role in writing an alternative report signed by all members of the group aside from Teager and Wes Clark.[34] The alternative report suggested that MIT could create a more customized machine by working together with vendors, and that MIT should immediately implement a form of time-sharing.

This report invoked the "Memex" from Vannevar Bush's influential 1945 *Atlantic Monthly* article, "As We May Think," as its ideal. The Memex was essentially a machine to store information that could be retrieved in various ways through a series of associations.[35] The report gave a three-year window for restructuring the Institute's computing systems in order to allow the Institute to upgrade its machines soon (the Institute was already planning to upgrade its IBM 704 to a newer 709, but the committee wanted to look ahead to the possibilities of a custom-made system in years to come), while still allowing time for a systematic rethinking of what it could mean to work with computers. This report assumed throughout that the Institute would proceed with time-sharing, and further observed that just as the difficulty of reliably accessing machines and the slow turnaround time deterred potential computer users, increasing access would bring this unmet demand into the open and would likely even increase demand further. Implementing time-sharing successfully meant not only getting the most powerful computer possible, but also identifying and acting upon every possibility for reducing runtimes and optimizing memory use. Creating more intuitive languages, designing better I/O devices, and training users were all crucial steps in building a time-shared system.[36] MIT needed to build a computer culture alongside the technical system.

The idea of time-sharing was straightforward. Given that electronic computers operated so quickly, a computer processor could cycle through multiple users, performing pieces of every process in turn, and still respond to the user in real time. Users retained the illusion of having a

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[34] McCarthy oral history interview with Aspray.

[35] On the Memex, see Vannevar Bush, "As We May Think," *The Atlantic Monthly* (July 1945): 101-108. The Memex has recently been described as a form of hypertext. For example, see Geoffrey C. Bowker, *Memory Practices in the Sciences* (Cambridge, Mass.: MIT Press, 2005), 115.

[36] "Report of the Long-Range Computation Study Group," April 1961, J. C. R. Licklider Papers, MIT Archives and Special Collections, MC 499, box 7, pp. 5-12.

machine to themselves, assuming that they were working with small programs. A user could idle at his or her terminal without wasting valuable computer time—a major problem with the previous system. This did, however, mean that some fraction of the computer's memory and processing power would be spent on this task of coordination, or "supervision," as it was called. For time-sharing to work, the costs of administering the system had to be more than made up for by the more efficient application of computer time.

This meant not only creating an efficient "supervisor" to schedule tasks and allocate memory among multiple users, but also figuring out how to reduce redundant operations while keeping the necessary memory protection safeguards (which required a certain amount of built-in redundancy). Because this system was designed specifically for users wanting to interact with the machine in real-time, large computer programs (for which the user had no expectation of receiving an immediate response) could be run batch-style during the graveyard shift. This suggested an analogy that would motivate subsequent research in computer systems at MIT: that the task of managing the processing power of the Computation Center was much like the problem of balancing load on the power grid.[37] Processing power remained a scarce communal resource.

The report also cited the most recent updating of Bush's Memex idea: J. C. R. Licklider's vision of man-machine symbiosis. Licklider had spent the war in Cambridge, Mass., working at the Rad Lab and the Psychoacoustics Lab alongside his friend and fellow psychologist, George Miller. Trained as a psychologist, Licklider's wartime work consisted of studying the effects of noise on human communication. By the end of the war, his colleague Walter Rosenblith observed that "there is hardly any size or shape of Procrustus bed on which Lick has not stretched, clipped, tilted or even differentiated speech before listening to it." The result of this research was a significant contribution "to the rejuvenated and Wienerized Science of Communication."[38]

From these studies of noise and communication, Lick became deeply interested in Claude Shannon's studies of information theory and became a regular member of Norbert Wiener's circle of cyberneticists. At the same time, he cultivated his interdisciplinary ties through regular membership in MIT's defense summer studies.[39] He began consulting for the Air Force and participated in several research programs dedicated to integrating pilots and airplanes into seamless systems.[40]

He and Miller decided that they should spread out, with one remaining on the MIT campus and one remaining at Lincoln Laboratory. A coin toss settled their fates; Lick was to return to MIT. In his role as an educator and faculty member there, he found that most of his

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[37] Ibid., 38-40.

[38] "Dr. J. C. R. Licklider Receives Biennial Award at State College Meeting," *Journal of the Acoustical Society of America* 22 (1950): 882-883.

[39] A wide-ranging biography of Licklider, placing him within the many currents of information theory, psychology, and computing, is Waldrop, *Dream Machine*.

[40] J. C. R. Licklider, "Theoretical Aspects of Research on Man-Machine Systems," 5/30-31/1957, Licklider Papers, box 6.

time was spent doing mundane tasks that were only tangential to research and teaching. He conducted what he described (in Taylorist terms) as a "time and motion analysis" and found that clerical and routine activities took up 85% of his working time, leaving, by his count, only 15% of his time for creative thinking and active decision making. By bringing his experiences in research to bear upon his studies of man-machine systems he articulated a vision of man-computer symbiosis to integrate the power of the computer as an information processor with the creative functions of the human mind.[41]

He contrasted "symbiosis" with the more familiar models in which machines simply extend human capacities (as eyeglasses or canes do) or in which humans assist automated systems do the work that cannot easily be automated (as in the most advanced factories). Rather, Licklider presented symbiosis as a historically-specific system for a division of mental labor between humans and machines. In his language of signal processing, the basis for this division of labor was that "men are noisy, narrow-band devices, but their nervous systems have very many parallel and simultaneously active channels. Relative to men, computing machines are very fast and very accurate, but they are constrained to perform only one or a few elementary operations at a time. Men are flexible, capable of 'programming themselves contingently' on the basis of newly received information. Computing machines are single-minded, constrained by their 'pre-programming.'"[42] This agenda was particular to 1960 because Licklider did not want to dismiss the claims made by AI proponents concerning the future development of machines.[43] The boundary between men and machines remained a moving target.

The position of the machine in this symbiosis was as a partner whose logical power aided the user's creative or critical thinking. The ability of computers to help solve certain classes of well-formed problems was ultimately less important than helping computer users uncover the implications of certain lines of thinking, or to organize information related to particular problems. It was a form of symbiosis in which the human partner remained in control, though ultimately, according to Licklider, the mental distance between the human mind and the machine would grow vanishingly small.

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[41] J. C. R. Licklider, "Man-Computer Symbiosis," *IRE Transactions on Human Factors in Engineering*, HFE-1 (1960): 4-11, reprinted in *In Memoriam: J. C. R. Licklider, 1915-1990* (Palo Alto: Digital Equipment Corp., 1990), 1-19.

[42] Ibid., 6.

[43] Teager's report to the Long-Range Computing Study Group used the future trajectory of AI research to cast additional uncertainty into predicting MIT's future computing needs: "The achievements of [AI] research are constantly moving the border line demarking the area of what is more economically achieved by machine … The range of present substitution of machine for human intellectual activity is constantly expanding and as of today could include much of the formal mathematical manipulations used in science and engineering which are both symbolic and numerical in character." The minority report's insistence on Licklider's vision of symbiosis indicated an understanding that the aspirations of AI did not eliminate the need to design a new system for the interim. See MIT Long Range Computation Study Group, *MIT Computation — Present and Future* (1961), MIT Archives and Special Collections, p. 15.

Supervising Users

The debates about time-sharing were going on while MIT was planning its centennial celebration in 1961. Amidst the festivities was a conference dedicated to exploring the social and economic implications of computing, organized by management professor Martin Greenberger. The talks ranged widely, covering everything from programming methodologies to artificial intelligence to the future of libraries and universities. One of the most significant presentations was John McCarthy's analysis of MIT's time-sharing system. Following a standard description of what time-sharing meant, McCarthy concluded with a prediction about "management and the computer of the future" at Greenberger's urging: "If computers of the kind I have advocated become the computers of the future, then computation may someday be organized as a public utility, just as the telephone system is a public utility. We can envisage computing service companies whose subscribers are connected to them by telephone lines. Each subscriber needs to pay only for the capacity that he actually uses, but he has access to all programming languages characteristic of a very large system." After describing some potential applications of this computing utility, he drily concluded that "the computing utility could become the basis for a new and important industry."[44]

While many computer users were interested in the idea of time-sharing, resistance remained among the largest computer companies. The association of computing with clerical or administrative work meant that many professionals, particularly those associated with commercial data processing, did not see sufficient demand for interactivity. Gene Amdahl of IBM was typical in this respect. In his comments following McCarthy's talk he questioned "whether every individual user will desire to operate his own console. Not everyone wishes to operate his own typewriter, for example."[45] IBM's conservatism on this point led to friction with MIT. Relations between the two organizations were already strained by a patent controversy concerning Jay Forrester's magnetic core memory, though IBM continued to believe that its dominance of the computing field (which was built upon its SAGE contracts) made it a natural partner for MIT. Upon hearing about MIT's plans to implement time-sharing, IBM openly questioned the demand for such a system, and encouraged MIT to study whether the faculty

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[44] This conclusion was one of the most commented-on parts of an already highly publicized conference. Indeed, the published proceedings of the conference took its title from the text of McCarthy's talk. John McCarthy, "Time-Sharing Computer Systems," in *Management and the Computer of the Future*, ed. Martin Greenberger (Cambridge, Mass.: MIT Press, 1962), 220-248, on 236. Greenberger's claim about inspiring this segment is from Martin Greenberger, interview with author, 4/10/2009. Greenberger later published an essay, expanding on the computer utility concept. See Martin Greenberger, "The Computers of Tomorrow," *The Atlantic Monthly* (May 1964): 63-67.

[45] Gene M. Amdahl, in "Time-Shared Computer Systems," 238. A further question was how skilled computer users needed to be. Grace Hopper described her frustrations with programmers after designing COBOL, a language much like English. "Having discovered that programmers could not write in English after we carefully had made programs so that they could write in English," she left the actual coding to teams of female typists. "They know that they are the ones who are really programming the computer," she concluded. Grace M. Hopper, "A New Concept in Programming," in *Management and the Computer of the Future*, 250-287, on 286.

actually wanted this. The members of the computing study group criticized what they perceived as IBM's stalling. John McCarthy moved to Stanford in frustration.[46]

MIT and the Computation Center had not stood still during these negotiations with IBM. Following a very rudimentary demonstration during the summer centennial conference, MIT demonstrated its Compatible Time-Sharing System (CTSS) in November 1961. Fernando Corbató led the development of CTSS, along with Marjorie Merwin Daggett and Bob Daley.[47] At the heart of CTSS was a "supervisor" that cycled through programs, giving each more or less time, depending on how long the user had waited and how large the program was. The supervisor also moved user programs in and out of memory, and ensured that no user could corrupt another user's data. The queuing system lowered the priority of programs when they ran and increased the priority of programs that waited.[48] The system was wasteful in a sense because it continually had to load the next program into memory before running it. The system for memory allocation was designed to hold as much of an idle process in memory as possible to reduce the need to reload it later on.[49] The challenge of designing CTSS was to create a system for allocating both computer memory and processor time to a diverse group of users and programs while keeping each user's data and programs separate from those of other users.[50] It was, in other words, the framework for a constitution governing the Computation Center's users.[51]

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[46] Later, McCarthy suggested that in this respect, Teager's report may have been correct: the decision to go with the IBM STRETCH would have kept IBM involved from the outset, rather than encouraging the bad relations that developed between the two institutions. John McCarthy interview with William Aspray.

[47] Fernando Corbató, Marjorie Daggett, and Robert Daley, "An Experimental Time-Sharing System," *AFIPS Conference Proceedings* 21 (1962): 335-44.

[48] The problem of allocating computing resources, such as memory and processor time, drew directly on the mathematical tools developed to move information and materials through physical networks.

[49] J. H. Saltzer, "CTSS Technical Notes" and F. J. Corbató et al., *The Compatible Time-Sharing System: A Programmer's Guide* (Cambridge, Mass.: MIT Press, 1963).

[50] J. B. Dennis, "Program Structure in a Multi-Access Computer," MAC-TR-11.

[51] While it is tempting to think about this as a system designed strictly for a group of human users, it is important to bear in mind how the structure of programs constrained user behaviors, and to consider CTSS as a system with both human and mechanical actors. The computer's superhuman speed and the complexity of its operations gave it, at minimum, the appearance of agency—though the degree to which computers could be understood to have agency was debated. Regardless of where one stands on the question of machine agency, the important point is that the workings of the computational parts of the Center were not under the complete control of human users, nor were the actions of humans completely determined by the structure of programs or machines. Therefore, the most productive position is to view this as a complex assemblage of human and computational components, as in the actor-network theory of Bruno Latour et al. See Bruno Latour, *The Politics of Nature: How to Bring the Sciences into Democracy* (Cambridge, Mass.: Harvard University Press, 2004).

The theory does important work here by pointing to the question that animates so much debate about the interaction between computers and society: where does responsibility lie? If human actions are strictly determined by the structure of computers, does that mean abandoning our responsibility? Must we argue, yet again, about the absurd notion of "technological determinism?" If computers only do what they are told, and if responsibility for actions lies with the humans who design, program, and use them, do we presume an impossible ability to foresee the workings of complex systems? Such questions animate the ongoing debates about the possibility of creating

While earlier utility systems (such as the telephone and electric grid) relied upon large numbers of operators to control access to the central system, the time-shared computing utility could redistribute computer resources at the speed of the machine, so as not to waste valuable computer cycles. The mathematics developed by operations researchers to study the flow of physical goods within transportation systems and warehouses applied to the information processing going on in the computer. The time-sharing system implemented a sophisticated scheduling algorithm to handle the variety of tasks demanded by users. McCarthy estimated that this would reduce the lag time between submitting a program to the machine and receiving a response from as much as 36 hours down to as little as one second.[52] Yet others argued that this failed to address the real problem of limited access to computers. Excessive demand for computing could cripple a time-shared system as easily as a batch-processing system. Through the early 1960s the growing appeal of time-sharing was not based on any unequivocal success stories.

The diverse needs of users and of programs posed a problem when viewed individually. This was what had led to batch processing in the Computation Center—putting programs in a standard format and running them sequentially to optimize the use of the processor. Individual users given direct access to machines would waste time and computing power, and would need different amounts of processing power at different times. Yet, as Fernando Corbató noted, "each user of the system asynchronously initiates jobs of arbitrary and indeterminate duration which subdivide into a sequence of processor and channel tasks. It is out of this seemingly chaotic, random environment that we finally arrive at a public utility-like view of a computation center. For instead of chaos, we can average over all the different user requests to achieve nearly total utilization of all resources."[53] CTSS, properly designed, could be a general framework for balancing resources, given a reasonably uniform population of users. Part of the construction of the system therefore meant constructing the user group whose individual variations would average out into a uniform load. Users had to be educated about their usage, and charges for computer time could be adjusted to match demand.

"artificial intelligence," but they are also at the heart of locating individual responsibility within computerized systems.

A willingness to consider some form of machine agency forces us to recognize the impossibility of purely social solutions to the problems of organizing computer systems. It should also encourage us to recognize the impossibility of purely technical solutions. Moreover, it suggests that the boundary between the "social" and "technological" is permeable, and that much of the substance of these arguments concerning the place of computing in society is fundamentally about locating that boundary. As should be clear by the preceding chapters on management systems and on automation, the location of this boundary has important ramifications for the economic and political power of system designers vis-à-vis users, for the classification of system members as acting subjects versus component objects, and for determining the grounds on which controversies are settled.

[52] Memo from John McCarthy to P. M. Morse, 1/1/59.

[53] F. J. Corbató, "System Requirements for Multiple Access, Time-Shared Computers," MAC-TR-3, p. 3.

Gender, Skill, and Authority Within Computer Systems

The accessibility of computer programming fed into debates about the role of skilled work in data processing. A growing importance of computer systems meant a growing potential for conflict between the technological gatekeepers of these systems and traditional organizational leaders. Reflecting on her experience with the military, Grace Hopper had noted that “one of the difficulties with programmers is that the symbolisms that they have invented leave management and systems analysts ignorant of what is going on. When it recently became possible to use the English language to write programs, an Air Force colonel was heard to say, ‘Now we can take back command of the Air Force from those damned programmers.’”[54] Improving individual access to computers would protect the prerogatives of traditional leaders from what many feared would be a usurpation by technological experts. But this democratization of access was tempered by the growing importance of these systems and of their gatekeepers.

Meanwhile, Ida Hoos challenged the views of automation advocates such as John Diebold and claimed that office automation and the introduction of computers were resulting in a net destruction of jobs and a deskilling of the workforce. This effect extended into most levels of the corporation, from the secretarial staff to management. The only ones who directly benefited were the computer developers themselves, the “EDP [electronic data processing] Elite” standing on the periphery of the business. Unconcerned with traditional industrial relations, “for them, the basic ingredient of success is efficiency and not popularity … Theirs is an electronico-centric universe from which emanate the waves of change.”[55] Hoos worried about the empowerment of unchecked technocrats. Having been invited in to streamline the operations of the firm, the EDP division gained autonomy due to the inability of management to understand their work. Low status jobs, often held by women, were eliminated without the creation of new ones.

This reflected a larger trend. As the discipline of computer science came into its own, there was a significant shift in terms of who did computational work. The act of directly programming the machine had been framed as an extension of clerical work, which was considered women’s work.[56] Programming was divided in two stages: men, typically trained in mathematics, created high-level instructions for the machine, which women then translated into the particular language of the machine. This was widely understood to be a straightforward translation from English to machine instructions.[57] Writing in languages closer to the machine was precisely the sort of drudgework that was expected of women programmers, and women were believed to have the superior manual dexterity that would let them program more easily.

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[54] Hopper, “A New Concept in Programming,” 284-285.

[55] Ida Russakoff Hoos, “When the Computer Takes Over the Office,” *Harvard Business Review* 38:4 (July, 1960), 102-112, on 109.

[56] The basis for this was mentioned in chapter 1. See Sharon Hartmann Strom, *Beyond the Typewriter*. Also see Margaret Lucille Hedstrom, “Automating the Office: Technology and Skill in Women’s Clerical Work, 1940-1970” (Ph.D. dissertation, University of Wisconsin, Madison, 1988).

[57] Consider this note from a book proposal by Jay Forrester and Robert Everett: “The misunderstandings about the difficulty of problem programming will be considered and the point made that the actual coding for the machine operation is a purely clerical task once the problem to be solved has been fully and completely specified.” Jay W. Forrester and Robert R. Everett, “Book Proposal,” 11/4/48, Forrester Papers, box 110.

Educated women were encouraged to become experts in computing, both because of their supposedly natural inclinations to do this type of work and because this was a form of expertise that seemed distinct from that of their male colleagues. This situation did not last; within a few short years a new generation of male engineering students would recognize the intellectual challenges of working with computers and claim the work as their own—shifting the gender dynamics of the computer world to the hyper-masculine one that it is today. Yet, during this brief window, computing offered real opportunities for women. A pamphlet from the American Federation of Information Processing Societies in 1968 went so far as to declare that "in probably no other field can women get ahead as easily and go so far as they can in information processing."[58]

There were important technical accomplishments from this unacknowledged form of expertise. Workers in the AI tradition had long lamented that they could hardly claim to have created "intelligence" so long as programs could not themselves create new programs. Meanwhile, women studying programming began to do just that by thinking about how to automate their own work rather than by thinking about how to create a more general form of intelligence from scratch. Grace Hopper, the creator of some the first compilers, described how the experience of doing low-level work could inspire innovation: "Programmers couldn't copy things correctly. … Programmers could not add. There sat that beautiful big machine whose sole job was to copy things and do addition. Why not make the computer do it? That's why I sat down and wrote the first compiler. It was very stupid. What I did was watch myself put together a program and make the computer do what I did." Compilers automatically translated higher-level programs into language for the machine, dramatically reducing the work of the women who had previously been assigned to this task. Hopper credited a less famous companion, Betty Holberton, with creating the first system to automatically generate specific data-sorting programs given a set of technical specifications.[59]

Yet the gendering of computing did not depend only on the associations among clerical work, computing, and women. It built upon much deeper notions of skill. These associations worked in contradictory ways, creating a situation where the gendered meanings of computing were not predetermined by the nature of the technology, but were instead contested and open to continual renegotiation. On the one hand, computer work was associated with stenography and clerical work—which had been framed as women's work in the early twentieth century. On the other hand, it was also associated with rationality and abstraction—masculine virtues.[60] One thing remained fixed within these ambiguous mappings: the association of skilled computer work

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[58] "Computer Careers," 1968, American Federation of Information Processing Societies Records, Charles Babbage Institute, CBI 44, box 13.

[59] Grace Hopper oral history with Angeline Pantages, December 1980, Computer History Museum, X5142.2009. Note Hopper's language. The word "programmer" refers to those building programs abstractly, not those directly handling machines.

[60] See Judith Halberstam, "Automating Gender: Postmodern Feminism in the Age of the Intelligent Machine," *Feminist Studies* 17 (1991): 439-460. Halberstam suggests that as computers began to seem autonomous, and the difficulty of programming became apparent, computers became agents of chaos and disorder—mysterious in the same way that nature was, and similarly in need of domination.

(however that was defined) as men's work. The skill involved in women's work went unacknowledged while men fought to preserve the skilled character of their work. Skill was simultaneously a way of preserving their economic status, a way of protecting autonomy over the work process, and a source of pride. Hence, the fear that tinkering with computers precluded planning and foresight yielded to a growing recognition of the skilled nature of programming as men entered the field.

Automation, as a threat to skilled labor, had been a feminizing influence in the factory by subordinating assembly line workers to machines and by shifting the work force to include more women. Where computers were seen as tools of intellectual automation they were understood to be appropriate for women and vaguely intellectually suspect. Meanwhile, computing was assuming an ever-larger importance for the first generation of students to grow up with computers. Programmers during the war and in its immediate aftermath had mostly been women with traditional mathematical educations, who could develop an expertise in computing machines because much of the traditional mathematical community found the machines vaguely disreputable.[61] Yet the engineering side of computing was predominantly masculine, and this was the nucleus of the student culture surrounding computers. Computer users began to describe their interactions with their machines in sexualized language. Given the difficulties of getting access to computers, computer students described sneaking out to spend all night with machines.[62] Nor was this limited to the undergraduate hackers. Licklider, speaking at the MIT Centennial, recounted a joke about the perceptions of computer scientists: "One of my friends has a sign on his wall which says, 'Put your clothes back on, lady, I'm a computer man.' The point is that once you get anywhere near something like the man-machine system … you know you really want it."[63]

The dramatic reversal in the gendering of computers remains to be adequately explained. However, part of the explanation rests on this shifting definition of what it meant to work with computers. Opportunities for women in computing were based on the suspicion that calculating machines were mere shortcuts for real thinking. The greater respectability of the subject in later years removed the stigma that had allowed women to make computing their own. As computers ceased to be seen as simplistic machines for executing straightforward programs, and began to be understood as the junior partner in a symbiotic relationship, the relationship between programmer and machine became one of mastery rather than simple application.[64]

Computer systems could therefore be gendered in two ways. Computer systems as part of bureaucracies were considered part of a feminine realm: without much intellectual substance and earning an intellectual affinity with secretarial work. This form of computer work was also considered ideologically suspect as a form of bureaucratization that represented constraints on

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[61] See Jennifer S. Light, "When Computers Were Women," *Technology and Culture* 40 (1999): 455-483.

[62] See Steven Levy, *Hackers: Heroes of the Computer Revolution* (Sebastopol, Calif.: O'Reilly Media, 2010).

[63] J. C. R. Licklider, "The Computer in the University," in *Management and the Computer of the Future*, 180-217, on 213.

[64] Sherry Turkle, *The Second Self: Computers and the Human Spirit* (Cambridge, Mass.: MIT Press, 2005), 183-218.

individual action. Meanwhile, the hackers who used computers as their toys represented a more individualistic and traditionally masculine strand of computing culture. As computing became more personal, and as the idea of directly interacting with computers became more prominent, the space carved out by women programmers shrank considerably. The authority of computer experts and the intellectual respectability of computer science grew hand-in-hand.

Hoos extended her critique the next year, building upon the increasingly firm connections among gender, class, and skill. She emphasized how the newly-empowered efficiency experts not only lacked the necessary training to deal with employees as people with a full range of human needs, but were actively discouraged from thinking in those terms—what she described as “trained incapacity.”[65] Her extended analysis of automation in the office drew on James Bright’s earlier study of automation, and she drew many similar conclusions about the skill gap created in automated offices. She observed that the traditional distinction between physical and mental work was becoming less important than that between skilled and unskilled. Skilled factory work required just as much technical expertise and independent judgment as skilled office work, while unskilled work of both types was considered repetitive, unimaginative, and ripe for replacement by machines. Looking at the bigger picture, Hoos described the semi-skilled jobs in both factories and offices as entry points into the middle class and wondered what would happen to class mobility as these jobs vanished.[66]

The goal of increasing office automation to the point of eliminating jobs was taken quite seriously. During the early 1960s Herbert Simon saw a way for computers to automate low-level managerial decision-making. Routine types of decisions involved well-defined goals and operating procedures, which made them programmable. The set of inputs was known, the set of outputs was known, and the range of possible actions was constrained. These sorts of decisions could be complex but ultimately they were reducible to an algorithmic treatment. While they were traditionally the domain of middle management, Simon believed they could be automated and handled by machine. He defended the claim that these computerized systems could make good decisions and that their forms of information processing counted as “thinking.”

Other types of decisions were neither routine nor well defined. For these sorts of problems, goals could be more malleable, and the range of possible actions could be unbounded. Though a computer could not solve these sorts of problems directly, the method of heuristic problem-solving, based on the concept of satisficing, could be a powerful mental aid to a top manager.[67] The extreme endpoint of management as a function of technical skill and of a quantitative social science capable of being computerized was management by computers. However, certain forms of unstructured problems still required the skill and vision of a human manager. Forecasting the potential of computerized decision systems meant drawing very traditional distinctions based on perceptions of the skill involved in decision-making.

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[65] Ida Russakoff Hoos, *Automation in the Office* (Washington, DC: Public Affairs Press, 1961), 16-18.

[66] Ibid., 57-58.

[67] Herbert A. Simon, *The Shape of Automation for Men and Management* (New York: Harper and Row, 1965). See the next chapter for a more extended analysis of this movement.

Two basic possibilities for computer use had been articulated by the 1960s. During the 1950s it seemed possible that these "Machines that Think" could bring the logic of the assembly line to the work of the mind. The computer in this sense was a tool of bureaucracy, standardizing the ways in which workers processed information.[68] As these bureaucratic tasks multiplied in the 1940s and '50s, this mode of information processing grew ever more attractive. Yet computational tools were also marketed as helping users organize information and thereby make better decisions. The distinction between interactive and automated computing was built upon a distinction between two types of mental work: one kind fit for humans, and one fit for machines.

Building the Infrastructure of a New Economy

While MIT grappled with the Long Range Computation Study Group's recommendations about how to implement actual time-sharing, Licklider was finding new avenues for expanding his notion of man-computer symbiosis. In 1957 he joined the Cambridge firm Bolt Beranek and Newman, a consulting group that had started out doing conventional acoustical studies but was gradually developing an expertise in computing. BBN recruited from among the faculty and researchers at MIT and Harvard and soon brought McCarthy, Minsky, and Morse into its orbit as well.[69] Licklider did not stay long at BBN. In 1962, Jack Ruina of the Department of Defense's Advanced Research Projects Administration hired him to head both its behavioral sciences program and its information processing section (the Information Processing Techniques Office, or IPTO). These were both low priority at ARPA and so Licklider was relatively free to do as he chose.[70]

Licklider's decision to join ARPA was based on his recognition that the problem of command and control was related to his vision of man-computer symbiosis, which had developed from the confluence of thinking about thinking and about military man-machine systems. As he recalled his meeting with Ruina, "the problems of command and control were essentially problems of man-computer interaction. I thought it was just ridiculous to be having command control systems based on batch processing. Who can direct a battle when he's got to write the program in the middle of the battle?"[71] Yet his concern with information technology also drew upon an even broader critique of industrial society. "If all the Industrial Revolution

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[68] For a striking visual representation of this theme and its connection to computerization, note the introduction of the computer in Orson Welles's 1962 adaptation of Kafka's *The Trial*.

[69] BBN's centrality to the history of computing remains important, though it has not received the same sustained study that more obvious institutions in the computing community (such as MIT, IBM, or ARPA) have. To get a sense of its significance, see Janet Abbate, *Inventing the Internet* (Cambridge, Mass.: MIT Press, 2000).

[70] Jack Ruina, interview with William Aspray, 4/20/1989, Charles Babbage Institute, OH 163. Corbató suggested that the impetus for upgrading command-and-control systems came from the critical communications failures of the Cuban Missile Crisis. See Corbató oral history interview with Webber.

[71] J. C. R. Licklider, oral history interview with Arthur Norberg and William Aspray, 10/28/1988, Charles Babbage Institute, OH 150.

accomplished was to turn people into drones in a factory, then what was the point?" he was known to ask.[72]

Lick decided to concentrate ARPA funding in a few national centers. Funding decisions were based on the reputations of individuals rather than on specific projects; the idea was that talented researchers could run interesting projects, but good ideas would not generate results without talented scholars. Given his ties to MIT, Lick made the Institute one of the major recipients of ARPA largesse.[73]

In the fall of 1962 Lick assembled MIT's computer experts and unveiled his agenda for the future of computing research. The faculty members, representing diverse computing interests, could not agree on a direction for computing research. Not even ARPA's financial backing could impose a coherent vision or convince a faculty member to take responsibility for organizing the community. Shortly before Thanksgiving, during a train ride to Washington, DC, Licklider spoke with his colleague, Robert Fano, an expert in information theory and communications engineering who had spent the previous year on sabbatical at Lincoln Laboratory to learn about computers. Fano seemed an ideal candidate to run a new computing initiative at MIT. The only other credible senior faculty member to lead such an effort was Philip Morse, who was already overextended. Fano had the further advantage of being in the inner circle of the Dean of Engineering, Gordon Brown. Recognizing that he was the only one on the MIT campus with the clout, the expertise, the personal connections, and the time, Fano returned to Cambridge and began speaking to the Dean of Engineering and the university administration about directing a new research project.[74]

Fano organized this new group as a "project" due to the administrative requirements attached to formal "laboratories." Interested faculty could affiliate with his project without threatening the established laboratories (such as the Research Laboratory for Electronics) or the academic departments. In this stage of the history of computing, the notion that the study of computers could support an entire community of scholars did not yet exist. After a brief period of being known informally as "Fano's Folly," the project was christened Project MAC—an acronym for both Multiple-Access Computer (reflecting its lineal relationship to the earlier development of time-sharing and CTSS) and Machine-Aided Cognition (reflecting the strong influence of J. C. R. Licklider's ideas about symbiosis).

MAC ran an informal summer school in 1963 to demonstrate the principles on which the system was organized, and then organized a more formal research structure using the ARPA contract.[75] This contract covered both the work in time-sharing run by the group that had built

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[72] Quoted in Waldrop, *Dream Machine*, 98.

[73] Arthur L. Norberg and Judy E. O'Neill, *Transforming Computer Technology: Information Processing for the Pentagon, 1962-1986* (Baltimore: Johns Hopkins University Press, 1996). The significance of the relationship between ARPA and MIT remains a point of some debate. The scientists themselves did not see ARPA as pushing a militaristic agenda in these years. The most immediate effect of ARPA patronage was to concentrate support in a few key institutions while leaving smaller programs with far less funding.

[74] Robert M. Fano, interview with author.

[75] "Proposal for a Research and Development Program on Computer Systems to ARPA, 1963," 1/14/1963, Licklider Papers, box 7.

CTSS and the work on artificial intelligence going on around Marvin Minsky's group. The two groups had little interaction and their work was largely done independently.[76] The group studying time-shared computer systems, however, was looking ahead to a new model for computation, built around the idea of thinking of computing in terms of public utilities. This system, part of Project MAC, was known as the Multiplexed Computation Service (Multics).[77] While ARPA money supported this research, the agency did not demand any short-term returns on its investments and the scientists on the contract could count on continued government support.

As the MIT computing community searched for a vendor who could design a system more compatible with the Multics vision, they gradually converged upon General Electric. GE's relatively small footprint in the computing world worked to its advantage, as it had in winning the earlier ERMA contract from Bank of America. The GE-MIT collaboration was partly brokered by Joseph Weizenbaum, a young professor at MIT who had earlier been the lead programmer for ERMA.[78]

The decision to go with General Electric was not made lightly. IBM remained the dominant player in the computing market and the company believed that the MIT contract was theirs by right. Furthermore, IBM was in the midst of revamping its mainframe business with the release of System/360—known as its "$5 billion gamble"—and so the Multics decision came at a critical time for the company. The rationale for System/360 had been to bring order to the profusion of computers on the market by creating standards that would make machines and peripherals all compatible. However, IBM was too rigid to accommodate what MIT saw as a highly experimental system and opposition to time-sharing remained high within its corporate culture. GE joined MIT as a partner in Multics, with Bell Labs rounding out the program.

A basic problem facing the idea of time-shared, interactive computing was that it ran in the face of the prior development of these machines. The great advantage of computers had been that they performed calculations automatically, without the need for continuous human input. Complex programs linked together multiple-step processes while logical branches allowed programs to respond to different circumstances. The creation of new Input/Output devices allowed the machines to read in and print out information automatically. The need for human operators was due to the imperfect realization of automation and the perennially forward-looking computer scientists anticipated the eventual obsolescence of these tasks, which contributed to

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[76] Upon stepping down as chair of the Electrical Engineering Department in April 1966, Peter Elias warned MIT president Howard Johnson "I cannot visualize how my successor will survive a situation in which two large groups, each with the necessary complement of faculty and graduate students, run two computer science educational programs or fail to agree on running one." Letter from Elias to Johnson, 4/1/1966, Morse Papers, box 3. Indeed, in recognition of the relative independence of the AI community, an unofficial third meaning of MAC was "Minsky And Corbató." The next chapter will consider the work done in Artificial Intelligence and its implications for organizational psychology and ideas of rationality.

[77] "Project MAC, Proposal for Continuation of Research, 1965," 12/14/1964, Licklider Papers, box 8.

[78] See Fernando J. Corbató, oral history interview with Arthur L. Norberg, 4/18/1989 and 11/14/1990, Charles Babbage Institute, OH 162.

their low status.[79] By contrast, the stated goal of time-sharing was to work in real-time with the machine. This challenged what was understood to be the efficient use of the machine and the efficient use of the human operator's time. Companies such as IBM understandably saw it as a trivial use of powerful machines. MIT's decision to go with GE shocked IBM management, who were belatedly recognizing that time-sharing had real appeal. In 1966 IBM hastily built support for time-sharing into their new 360/67 machine.[80]

The goal of Multics was to create a computing system that would be available to remote users, similar to the telephone and electric grids. Thinking about computing as a utility meant increasing reliability to the point that users could tap into the service whenever needed, without needing to worry about whether or not they would have access. Fano cited the disruptiveness of the New England blackout of 1965 as driving this point home.[81] The utility concept also suggested that users would have access not only to raw processor cycles, but also to central memory banks full of both generally relevant data and programs. As Licklider put it, "a data base contains data. An information base contains any or all kinds of information, particularly including procedure as well as data."[82]

Multics was designed with an eye out for the practicalities of working with the system. This emphasis came from keeping the research groups and the support groups in close contact. The Multics staff was aware that their system was ahead of its time and they consciously thought about how their work might shape future business practices. "In general it should be said that multi-access computer systems represent a totally different class of tool from any that has existed heretofore," wrote Richard Mills, "one should expect from the outset that the management and accounting practices associated with them must be correspondingly different. It is possible that thoughtful and open-minded consideration of the issues will lead to radical departures from long-established business tradition."[83] Needless to say, the Multics team saw these "radical departures" as beneficial. The implications of Multics for business and for society were likened to those of the telegraph and of electrification. And given that Multics was a partnership of MIT with Bell Labs and General Electric, this identification of computational utilities as a technological evolution from earlier networks of electricity and telegraphy and telephony seemed natural.

Multics included not only the physical infrastructure but also the programs to help users navigate the system. The automation of the computer utility (and the development of computing systems within MAC) was complicated by having two conflicting motivations. Office

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[79] It is important to note here that programming the machines was not considered difficult or skilled work. The valuable intellectual work in programming was understood to be at the most abstract levels. This distinction had both class- and gender-based implications, as described above.

[80] See Emerson W. Pugh, Lyle R. Johnson, and John H. Palmer, *IBM's 360 and Early 370 Systems* (Cambridge, Mass.: MIT Press, 1991).

[81] Robert M. Fano, interview with author.

[82] J. C. R. Licklider, "Command of Procedures," 1964, Licklider Papers, box 7.

[83] Richard Mills, "Management Problems of Multi-Access Computer Systems," 7/13/66, Morse Papers, box 3.

automation reflected its origins in industrial management by emphasizing the creation of routines and regularity—constraining what individual users could do for the sake of the system. The model of symbiosis, on the other hand, sought to position the computer as the user's personal intellectual assistant. The challenge therefore was to integrate the efficiency that came from automation with the personal control over the machine that symbiosis suggested. Licklider and Robert Taylor (Licklider's successor as director of IPTO from 1966-69) introduced the idea of a digital servant, named after the AI researcher Oliver Selfridge, to be the user's agent within the computer utility. "The OLIVER is, or will be when there is one, an 'on-line interactive vicarious expediter and responder,' a complex of computer programs and data that resides within the network and acts on behalf of its principal, taking care of the many minor matters that do not require his personal attention and buffering him from the demanding world."[84] They were describing a digital secretary, as they themselves acknowledged, whose function was to free up time for the human behind the machine. Even in something as basic as communication there were routine elements that could be automated away in the name of efficiency.

Bob Fano, assessing the significance of Multics and MAC in general in 1971, claimed that the truly important feature of the project was in expanding computing beyond the small circle of those who were most interested in the machines themselves. By making computers accessible to those who cared about them primarily as tools for doing other work, these individuals could push the applications of computing in directions that their creators had never expected. For him, "the importance of a multiple-access system operated as a computer utility is that it allows a vast enlargement of the scope of computer-based activities, which can in turn stimulate a corresponding enrichment of many areas of our society."[85]

By the end of 1971, the Multics staff understood that that their challenges were not merely technical. They understood that "in a few years it will be clear that we are as vitally dependent upon the informational processing of our computers as upon the growth of grain in the field and the flow of fuel from the well. We are already totally dependent on computers in the field of banking." Creating reliable computer systems meant simultaneously improving the technical components and the social organization of computerized organizations—and the interface between the two in particular. An essential prerequisite to reliable computer systems was "solv[ing] the problem of communication between people and computers on a 'meaning-to-meaning' level." The ultimate goal was to improve the mental coupling of humans and machines, while ensuring that such systems "have the property that we can know with mathematical certainty that they will function as intended."[86]

What did this all mean? Expanding access to computing by means of a Multics-like utility represented an effort to democratize computing no less than did the simultaneous

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[84] J. C. R. Licklider and Robert W. Taylor, "The Computer as a Communication Device," *International Science and Technology* (1968), reprinted in *In Memoriam: J. C. R. Licklider, 1915-1990* (Palo Alto: Digital Equipment Corp., 1990), 21-41, on 38.

[85] Robert Fano, "Highlights of the Multics System," 1/18/1971, Fano Papers, box 2.

[86] "The Future of MAC," 12/6/1971, Laboratory for Computer Science Records, AC 268, MIT Archives and Special Collections, box 19.

marketing of minicomputers.[87] Multics was not only a technological system that grew out of a particular economy of computer processing scarcity, it was also a social vision in which the distribution of computing power did for mental labor what the distribution of electrical power did for manual labor. It was a vision in which the communal availability of shared data continued that annihilation of distances across space and time that telegraphy had begun a century earlier.[88] With Multics came the realization that the radical implications of computing were not limited to the futuristic forecasts of AI researchers (described in the next chapter)—a group whose work was perpetually on the brink of coming to fruition at some point that always remained just beyond the horizon.

And what happened to Multics? Its vision of expanding access to computing through centralized utilities was met by a competing vision of expanding access to computing by marketing smaller machines. This debate was occurring at computing research centers around the country. Reflecting on the decisions made at Stanford, Ed Feigenbaum noted that "the mini-computer revolution was essentially inevitable. It was going to happen—it was going to affect Stanford. We chose not to let it happen in 1965; we opted for a strong centralized computation center idea because of the presumed economies of scale of the large central machine, in hardware, in our ability to attract gifts and discounts, and so on."[89] In part, this was an issue of ideology, pitting centralized systems that maintained their efficiency by balancing loads against a computing market in which users maintained direct access to small machines, privileging individual ownership over social computing. In part, it was a simple issue of availability. DEC had begun marketing minicomputers and hobbyists were learning how to program calculators and simple machines such as the Altair. Users could directly own and manipulate their own machines within vibrant hobby groups.[90] Multics, as a system, was in perpetual redesign. The argument for efficiency also ceased to be compelling as computer power grew exponentially. While centralized systems could, in principle, more efficiently use processor power, the growth described by Moore's Law meant that the wasted power became a non-issue.[91]

The development of time-sharing and of the computing utility analogy required a transformation in what it meant to work with computers: shifting away from an understanding of computers as numerical engines, and toward a different understanding of computers as remarkably flexible intellectual tools. This transformation had several contributing causes. One, surely, was the military's interest in real-time command-and-control—though ideas of

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[87] And, indeed, promoters of computer utilities as democratizing computer access relied on arguments that were very similar to those made on behalf of rural electrification in the early years of the 20th century, and which are even more similar to those made today on behalf of improving broadband internet access in rural communities.

[88] A sense of the community that developed around Multics and the devotion that its developers felt is apparent by perusing the website http://www.multicians.org.

[89] Ed Feigenbaum, oral history interview with Pamela McCorduck, 6/12/1979, Charles Babbage Institute, OH 14.

[90] This is the subject of a growing literature on computer hobbyists, briefly treated in chapter five.

[91] Paul E. Ceruzzi, "Moore's Law and Technological Determinism: Reflections on the History of Technology," *Technology and Culture* 46 (2005): 584-93. However, this exponential growth of computing power did not happen automatically—demand for minicomputers and research in electronics fed off each other.

command-and-control owed as much to developments in communications sciences as these sciences owed to command-and-control. A more immediate cause was the attempt to rationally distribute scarce computer access to a diverse group of users, which led to the creation of complex supervisory systems.

The arguments of the 1960s, seemingly long since settled, may not be dead. Contrary to the persistent claims of its evangelists that "technological progress" is monotonic, the dormant issues raised by Multics were brought back to life in the 1990s and 2000s with the growth of the Internet.[92] Important contemporary questions—about privacy and data security, the economics of access to processor power, and copyrights and the ownership of works of art in an age of electronic reproduction—all assumed a central importance in the development of Multics.[93]

From Computer Systems to Computer Science

The tensions within the computing community made it difficult for MIT to construct a coherent intellectual and organizational framework for thinking about computing. In the summer of 1966 Robert Fano suggested that the Institute had to reorganize its computing environment. He suggested a more formal laboratory organization, centrally located on campus so that affiliated scholars could retain close ties to their home departments located in every one of the Institute's five schools. The first challenge in creating the lab would be to locate it so that it did not lose its close ties with either the engineering or the management side.

The second challenge was to create a laboratory that could bridge the research gap between the Multics group and the AI community. Not only did they use different machines (a GE-645 versus a PDP-6), but they also took completely different approaches to regulating computer access. While the Multics system was a highly regulated computing environment, the AI community built its own *In*compatible Time-Sharing System (ITS) as a rejection of the CTSS/Multics approach. As Fano warned, "two different, major computer systems evolving without intimate collaboration between their system programming staffs can create communities of users separated by a significant intellectual wall. This danger is very real." The differences between these two groups extended to the very students and personnel attracted to the labs, as will be discussed more fully in the next chapter.[94]

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[92] Compare statements made circa 1990 by two computer scientists who had worked at MIT in the 1950s and were on MIT's Long-Range Computation Study Group (described above). Jack Dennis, a developer of CTSS: "Now, my view is that what has happened is that [microprocessors] simply injected a ten-year or twenty-year pause in the evolution of computer systems. Now that we're going back to workstations and servers, all of the same issues are in front of us again, and solutions have yet to be worked out. The solutions will be along the lines we were thinking about back in the early '60s." Wesley Clark, a developer of Whirlwind: "I think networks are a mistake. They don't work." See Jack Dennis, oral history interview with Judy E. O'Neill, 31 October 1989, Charles Babbage Institute, OH 177, and Wesley Clark, oral history interview with Judy E. O'Neill, 3 May 1990, Charles Babbage Institute, OH 195.

[93] A fuller exploration of these themes is in chapter 5. For the contemporary significance of these topics, see Lawrence Lessig, *Code, Version 2.0* (New York: Basic Books, 2006).

[94] Letter from R. M. Fano to Committee on Computation, 7/14/66, Morse Papers, box 3.

The third challenge was to integrate the communities of computer researchers with the Institute's administrative computer service groups, running an IBM 360/67. For this, he cited Fernando Corbató's dual position as both a research leader and the director of the Computation Center as crucial. Collectively, these arguments were leading to a formal recognition of an academic program in computer science, located within the Electrical Engineering Department. Training the next generation of computer researchers and creating a body of skilled computer users to maintain the systems required building a formal educational program in computer science. Only in the 1970s would the department formally rename itself "Electrical Engineering and Computer Science," following the lead of the University of California, Berkeley.[95] This effort to build a standard computing curriculum at MIT was part of a broader national movement to make sense of computing.

UC Berkeley had only with great difficulty integrated computing into engineering through the efforts of electrical engineering professor Lotfi Zadeh. The campus Computing Center was run by the mathematician Abe Taub, who had been recruited from the University of Illinois on the condition that computer science eventually gain departmental autonomy. The electrical engineering faculty was ambivalent about whether to include the computer scientists. Computer researchers, for their part, were ambivalent about joining electrical engineering, which required all students to take courses in surveying, drafting, and other traditional engineering subjects. Yet only the most theoretically-minded computer scientists had any interest in joining Taub and the mathematicians.[96] Taub's center and the electrical engineering department, led by Zadeh, were essentially at war from 1963-1973. In 1967 a botched attempt at a Solomonic compromise led to the computer science community on campus being formally split in two, divided between the newly-renamed department of Electrical Engineering and Computer Science located within the College of Engineering, and a fully separate department of Computer Science within the College of Letters and Sciences, closely aligned with the mathematics department. The schism lasted for five years before the university shut down the splinter group, and CS became a semi-autonomous division of EECS.[97]

Computer scientists at Berkeley had to define themselves within a spectrum between engineering and abstract mathematics. Berkeley was not the only institution with this division, but the acrimony there radicalized a number of the faculty on this issue. Michael Harrison, a professor in Berkeley's Computer Science department, later unsuccessfully tried to encourage a similar schism while a visitor at MIT.[98]

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[95] Karl L. Wildes and Nilo A. Lindgren, *A Century of Electrical Engineering and Computer Science at MIT, 1882-1982* (Cambridge, Mass.: MIT Press, 1985), 354-365.

[96] The one exception to this rule was Martin Graham, who worked on hardware, and who was invited by Taub to run the Computer Science department and bring about a détente. See Martin Graham, oral history interview with Andrew Goldstein, 8/9/1991, IEEE History Center.

[97] Lotfi Zadeh, oral history interview with author, 11/12/2009; letter from Martin Graham to Alan Perlis, 7/20/1971, Alan Perlis Papers, Charles Babbage Institute, CBI 64, box 1.

[98] Fernando J. Corbató and Robert M. Fano, oral history interviews with author, 2/20/2009.

National efforts to organize curricula for computer science increased significantly in the late 1960s, as the professional societies got involved in what had been previously been organized at individual schools. The undisputed spiritual leader of this movement was Alan Perlis of Carnegie Tech. Perlis had earned his Ph.D. in mathematics from MIT and was hired by Carnegie in 1956 (after a stint at Purdue) to run its Computation Center. His stature was such that in 1961, when MIT was attempting to create what would become Project MAC, Morse had only one suggestion for its director: Alan Perlis. Morse saw no other figures with comparable experience and stature, creating the vacuum filled by Fano.[99]

Perlis, together with Allen Newell and Herbert Simon, formed the heart of Carnegie's computing community. The university's Computation Center was founded with the same Mellon family support that launched the Graduate School of Industrial Engineering, with additional research support from the Ford Foundation. For several years, the GSIA remained the heart of Carnegie computing, building on its position as the nation's most tech-savvy business school.[100] In 1956, the deans of the GSIA and the engineering school requested the donation of an IBM 704 with the same conditions as MIT.[101] In 1961, the university began to create a unified academic program in computer science, based in an interdisciplinary Systems and Communications Science graduate degree, run by Newell.[102]

The creation of this interdisciplinary graduate program provided a home for graduate students interested in computer research. However, it lacked full departmental status, which meant that the program faculty did not have full discretion over its budget or over hiring decisions. When his old GSIA colleague, Charles Holt, wrote to him in 1963 about the computing environment at the University of Wisconsin – Madison, Simon observed that Carnegie was taking the unusual step of creating a distinct computer science department. This would give the Carnegie computing community control over budgets, hires, and students. But there was a danger: "if a separate department develops, however, the Lord help us if it goes the direction of departments of statistics—i.e., into the esoteric and irrelevant," he fretted.[103] Independence could breed isolation and insularity.

ARPA funding helped establish a Research Center for the Study of Information Processing in 1964, existing alongside the service-oriented Computation Center. Newell and Perlis described a very broad research agenda, in which "the intertwining of concern with artificial systems and natural systems—whether artificial and natural languages or artificial and natural problem solving systems—is mutually beneficial, and will continue to characterize the

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[99] Memo from Peter Elias to Gordon Brown, 5/9/1961, MIT Computation Center Records, box 2.

[100] For an example of the GSIA's technological innovativeness, see Kalman J. Cohen et al., *The Carnegie Tech Management Game: An Experiment in Business Education* (Homewood, Ill.: Richard D. Irwin, Inc., 1964).

[101] Letter from G. L. Bach and B. Richard Teare to J. C. McPherson, 7/22/1956, Herbert A. Simon Papers, Carnegie Mellon University Archives, box 102.

[102] Letter from Paul Armer to G. L. Bach, 8/22/1961, Allen Newell Papers, Carnegie Mellon University Archives, box 62.

[103] Letter from Herb Simon to Charles C. Holt, 11/11/63, Simon Papers, box 102.

approach of the proposed center."[104] Finally, in 1965, a gift of $5 million from Richard King Mellon allowed Carnegie Tech to launch the new department of computer science, run by Alan Perlis (who was simultaneously head of the mathematics department).[105] Carnegie computer scientists came from the GSIA, mathematics, psychology, and the engineering programs. This interdisciplinarity was valuable, according to Newell, precisely because individuals from such varied backgrounds had no pre-existing shared scientific language or ideas. Instead, they had to build their discipline from scratch.[106] Carnegie was unique in the unity of its computer science program. It was saved from internal conflict, unlike Berkeley, in part because the three leaders of the program—Newell, Perlis, and Simon—held positions throughout the university, and because so much of the reputation of Carnegie Tech was built upon its work with computers.

The idea of a coherent project of computer science only began to develop once the first departments and professional societies were already established. Allen Newell, Alan Perlis, and Herb Simon wrote in the journal *Science* in 1967 that, quite simply, "computer science is the study of computers."[107] The background for this was an ongoing project of the Association for Computing Machinery (the ACM) to create a blueprint for university curricula in computer science.[108] The ACM took a broad view of what constituted computer science, spurring several angry replies to the effect that computers, as man-made artifacts, could not properly be the subject of scientific inquiry, or claiming that the subject matter of this new science was the more fundamental concept of "information" rather than "computers." Typical of this view was Edsger Dijkstra, a Dutch computer scientist who later relocated to the University of Texas — Austin, who claimed that defining computer science as a study of computers was akin to defining astronomy as the study of telescopes. In reply to these critics, the team of Newell, Perlis, and Simon defended computer science as the study of computers, which therefore naturally included all related phenomena, including mathematical logic, the design of hardware, and programming methodology. They denied the naturalness of any contemporary science—were transuranic elements "natural?" Were synthetic polymers? That computers were constructed should be no obstacle to their being understood as proper objects of scientific inquiry.

No one did more to cultivate the shared basis for computer science than Alan Perlis, who became famous for articulating interesting problems facing the field and for making connections among otherwise disparate subjects. According to Newell, Perlis became the first winner of the

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[104] Proposal for a Center for the Study of Information Processing submitted by Carnegie Institute of Technology to the Advanced Research Projects Agency of the Department of Defense, April 27, 1964, Graduate School of Industrial Administration Records, Carnegie Mellon University Archives, box 7.

[105] Memo from R. Cyert, 2/15/1965, GSIA Records, box 7; Letter from Austin Wright to Alan Perlis, 12/7/1972, Perlis Papers, box 1.

[106] Allen Newell, oral history interview with Arthur L. Norberg, 6/10-12/1991, Charles Babbage Institute, OH 227.

[107] Allen Newell, Alan J. Perlis, and Herbert A. Simon, "Computer Science," *Science* 157 (1967): 1373-1374.

[108] "Curriculum '68: Recommendations for Academic Programs in Computer Science," *Communications of the ACM* 11 (1968): 151-197.

Turing Award of the ACM in 1966 for these contributions to shaping the social character of the field, rather than for the work on compilers that was actually cited by the ACM.[109]

Perlis insisted that computer science stood on it own and had no need to be subordinated to either math or electrical engineering. In a 1967 paper entitled "Computer Science is Neither Mathematics Nor Electrical Engineering," he claimed that the goals of computer science were fundamentally distinct from those of its allied disciplines, and that understanding the operations of computers required knowledge that was fully distinct from that of electronics or mathematical logic. Perlis also claimed a central importance for the subject, as it affected everything from modern scientific research to social planning. He admitted that the influential science advisor George Kistiakowsky had a point in fearing "that an ever-expanding use of computers for the solution of scientific problems might change the nature of the problems that active scientists choose for study, and thus change the whole nature of scientific research," but suggested that the solution was to maintain bridges between computer scientists and other academics, as well as giving computer scientists broad educations.[110] By emphasizing that the study of computational phenomena were at the heart of computer science, he denied any essentialistic basis for the subject. Reflecting on the first ten years of CMU's department, he observed that the goals of computer science would change along with the state of the technology: "there is no such thing as truth in this field. There's only fun, the privilege to explore, to understand man."[111]

Herb Simon most directly addressed the methodological problem of how to situate the study of artificial phenomena with respect to the natural sciences. This was an issue that Simon had begun to think about in his work as an administrative theorist in the 1930s and '40s. Through the 1950s he published articles on the methodology of the social sciences, collected in his 1957 volume, *Models of Man*, and in 1962 Simon published an influential essay on "The Architecture of Complexity" that suggested a theory of organization as a way to understand various complex phenomena, in both natural and built systems. Simon's next major contribution to this question was a set of lectures at MIT published as *The Sciences of the Artificial* in 1968, which emphasized design as the characteristic feature of this science.[112]

The idea of scientific disciplines was being significantly rethought in the 1960s, thanks to a burst of metascientific studies, and the debates among computer scientists about the foundations of their discipline benefitted from the new intellectual space provided by the works of Thomas Kuhn and others. Computer scientists (particularly those from the influential Carnegie camp) took as their disciplinary goal the study of ever-changing technological objects, rather than strictly natural phenomena. They emphasized the value of play over the search for Universal Truths. Through the process of consciously forming a self-contained academic discipline—fully aware that this was essential for securing budgetary autonomy, control over

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[109] Allen Newell oral history interview with Arthur L. Norberg.

[110] Alan Perlis, "Computer Science is Neither Mathematics nor Electrical Engineering," 1967, Perlis Papers, box 3.

[111] Alan Perlis, Keynote Speech, 10th Anniversary of Carnegie Mellon Computer Science Department, Perlis Papers, box 4.

[112] Herbert A. Simon, *The Sciences of the Artificial* (Cambridge, Mass.: MIT Press, 1996).

personnel decisions, and establishing common curricula—computer scientists were actively creating a fundamentally new kind of scientific discipline.

Taylorism Redux

Even as computer scientists tried to mark the boundaries of their discipline, debates over the significance of computers within American society continued unabated. At the end of the 1960s, management theorist Thomas Whisler described the connections between computers and organizational forms. He predicted that the introduction of computers into organizations would centralize control as well as reduce both the span of control and the number of layers within the organization. This centralization was a consequence of allowing managers to handle more information. Middle managers typically suboptimized within their sphere of control without knowing how these decisions influenced other areas of the organization. Improving the work of one sector often involved making decisions that were detrimental to the organization as a whole. The tools of Operations Research and Management Science were effective at addressing precisely these problems, as even those skeptical toward the new techniques admitted.[113] Following Ida Hoos, Whisler observed a transfer of authority from traditional departments to the computer and systems experts who built and maintained these tools.[114]

Whisler acknowledged that these tendencies reflected the particular implementations of the companies that he studied rather than an essential tendency of the technology. "The new technology is so powerful that it could probably make either a decentralized or a centralized system work better. The preference of management, however, seems to be to exercise as much control as is technically and economically feasible; with such a bias, the most likely trend will be toward greater centralization," he wrote.[115] The observed results of bringing computers into organizations reflected both the properties of the machines as well as organizational norms.

Turning his attention to the workers who interacted with computers, Whisler observed a puzzling trend in which the jobs demanded more responsibility even as the work was routinized. "*In computerized systems, it is critical that employees perform in precisely the way that the system demands at precisely the time required*," he observed. "Although 'skills' seem to have been programmed out of the job in one sense, the net effect appears to be that of asking for a greater input, a greater commitment from the individual. … the job is defined as having been upgraded. If this interpretation is correct, imposing a man-machine system on the office has precisely the same effects as 'scientific management'—classical Taylorism—did in the factory years ago."[116] This tendency followed from the increasing centralization that computers allowed.

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[113] Peter Drucker had noted in an assessment of Management Science in 1959 that "in some cases the best way to strengthen the system may well be to *weaken* a part—to make it *less* precise or *less* efficient. For what matters in any system is the performance of the whole." See Peter F. Drucker, "Thinking Ahead: Potentials of Management Science," *Harvard Business Review* 37:1 (Jan.,1959), 26, emphasis in the original.

[114] Thomas L. Whisler, *The Impact of Computers on Organizations* (New York: Praeger Publishers, 1970), 66-69.

[115] Ibid., 106.

[116] Ibid., 140. Emphasis in the original.

Computerized systems within organizations built their rules of use into the structure of the programs, creating a sense that the technology was responsible for this change. Whisler corrected this misperception by pointing to the design decisions as being equally responsible. By building these rules and responsibilities into the system itself, it represented a new stage in the management styles of Taylor and his disciples.

Whisler further described how the use of these computer technologies made the exercise of authority less visible. He argued against the claim that the use of these machines in organizations would level hierarchy, for "with computers present, … much of the hierarchy can be built into the programs themselves. Programs then 'decide' when and how certain activities will take place after information is fed into them. Hierarchy, as some forecast, may not disappear but just become less visible. Coordination, based upon predetermined agreed-upon rules, may become depersonalized and 'hidden' in computer programs."[117] Once again, Whisler pointed out that much of the traditional job of management now fell within the scope of systems design. The job of coordinating teams of workers was built into programmed systems—though these programs were usually based closely upon existing protocols. While Hopper, Hoos, and others suggested that making computers easier to understand and access would reverse the centralization of authority among managerial and technological elites, Whisler disagreed. The preliminary results from organizations working with time-shared systems showed these trends getting even stronger.

Understanding computer systems as they were used in practice required understanding organizations. The introduction of a computer system within an organization heralded the creation of a new center of power. The authority associated with computer expertise grew out of previous claims to authority made by advocates of scientific management, as scholars of organizational behavior were quick to point out. Yet, despite these historical precedents, the form of authority that came from computerized systems had one substantially new feature: the system of rules and protocols was embedded within the technology rather than in face-to-face interactions, shifting the location of the exercise of power from a social sphere to a technological one. This had further implications for how different occupational groups interacted within the computerized organization.

The association between computers and organizational forms also existed within the machines themselves. Different regimes for governing the interaction between users and machines relied upon different programming. Operating systems were one medium between users and the operations of the machine, along with high-level programming languages and physical interfaces. Technological innovations governed how individuals and machines interacted, as well as how users interacted in large organizations. The linkages between humans, between machines, and between humans and machines all stitched together the organizations of the 1960s and beyond. One cannot understand the historical dynamics within these organizations by thinking purely in social terms or purely in technological terms.

Observations about the connection between technological expertise and institutional power reflected a wider ambivalence with the direction of the expanding American economy. This extraordinary economic growth was one of the crucial characteristics of the postwar

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[117] Ibid., 154.

decades, yet it was accompanied by significant changes in the organization of the American workforce—including the growth of large corporations and the expansion of government intervention in the economy. The anti-communism of the 1950s was moderated in the next decade by an increasingly common attitude that suggested convergences between the American and Soviet systems. These theories maintained that neither society was communist or capitalist, but should best be described as something new, and thoroughly bureaucratic. The fear that bureaucratic managerialism was choking off laissez-faire forms of capitalism had been a staple of conservative arguments through the early years of the century. Yet certain strands of liberalism celebrated this trend, leading Daniel Bell to label the era one of the “end of ideology,” and John F. Kennedy to describe the function of politics as technocratic in an environment of broad consensus about the purposes of government.[118] The industrial system was characterized by top-down, technocratic planning and reflected the broad appeal of both Taylorist scientific management and a wide-ranging welfare state. Bureaucratization was the essence of convergence.

There were many correspondences between the forms of computers and visions of the American economy, in which the computers could be understood as electronic instantiations of bureaucracy. On the one hand, the advances in the organization of information that computers made possible could be understood positively as leading to greater productivity or negatively as leading to intolerably rigid forms of control. The social valances of these different forms of information technology did not come directly out of the laboratory. They also reflected broad concerns about the direction of American economic development.

Computer science grew out of these varied debates about the nature of computers and about what constituted significant scientific work. Because the core of computer science was based on machines as they were used, different groups of scholars could organize work around competing visions of what computers could or should do. This meant that the work of computer scientists acted as a bridge between the technical capabilities of machines and the social need for computing. However, this also meant that the boundary between the inner world of computer science and the outer world of the workplace became a site of continued controversy.

The attribution of social meanings to these technologies made it impossible to fully separate these two categories. Computers were alternatively seen as the ultimate tools of bureaucratization, or as the expressions of a liberal faith in the universality of reason—a universality that extended even to machines. Between these two poles were the machines themselves: blank slates built upon sophisticated electronics, pure potentialities whose capabilities could only with great difficulty be discerned.

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[118] Daniel Bell, *The End of Ideology: On the Exhaustion of Political Ideas in the Fifties* (Glencoe, Ill.: Free Press, 1960). For more on the history of technocracy as a third way between laissez-faire and socialist ideologies, see chapter 1.

Chapter 4: Plans and the Structure of Society

This chapter uses the development of artificial intelligence as a window into the changing concept of human intelligence. It traces both the creation of new models of human nature and the attempts to assemble cybernetic individuals into a future society. Artificial intelligence matters for this dissertation as a way of grappling with the question of locating boundaries between man and machine rather than as a means of addressing the familiar question of whether or not machines can be said to "think"—a subject about which much has already been written.[1] The development of artificial intelligence is therefore treated here within the wider context of creating complex sociotechnical systems for formal reasoning because it suggested the possibility of a bridge between specifically human capabilities for thought and programmed machine behavior.[2] Such computerized systems for reasoning suggested new ways of creating social organizations that could effectively analyze information, reach decisions, and act. However, the quest to create artificial intelligence ran into significant problems that forced its adherents and its critics to critically examine how these systems represented knowledge about the world and how they acted upon such information. By focusing on the ways in which artificial intelligence dealt with fundamental issues in social thought, this chapter provides a point of contact between the technology of AI and the developing discourse of science studies, which has been important in generating sophisticated sociopolitical studies of technology. Although the work of artificial intelligence seems removed from the questions animating mid-century management theory, it was in fact instrumental in challenging the legitimacy of existing organizations in the late 1960s.

The chapter begins with a network of scientists operating at the boundaries of economics, psychology, and computer science. These scientists tried to fuse the science of Claude Shannon's information theory to social theories of communication. The crucial point here is that these scientists understood themselves to be creating a new scientific discipline, albeit one that never fully cohered into the traditional framework of having independent academic departments, journals, and conferences. The chapter will offer some thoughts as to why this attempt to define scientific boundaries failed and what effect this had on the attempts to use these sciences as the foundation for a new theory of human nature.

It then gives a brief overview of the origins of artificial intelligence that situates these problems within their institutional contexts and within the larger environment of computing. This analysis of artificial intelligence focuses on its critics, whose arguments rested in part on technical considerations, but more importantly on the basic questions of what uniquely defined

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[1] For some standard histories that address this angle of AI, see Daniel Crevier, *AI: The Tumultuous History of the Search for Artificial Intelligence* (New York: Basic Books, 1993), and Pamela McCorduck, *Machines Who Think: A Personal Inquiry into the History and Prospects of Artificial Intelligence, 25th Anniversary ed.* (Natick, Mass.: A K Peters, 2004).

[2] On such sociotechnical systems see the work of Thomas P. Hughes, such as *Networks of Power: Electrification in Western Society, 1880-1930* (Baltimore: Johns Hopkins University Press, 1993).

intelligence and how human sociability contributed to the unique power of human cognition. In this way, the technical project of designing artificial intelligences contributed directly to the project of articulating what was essential about the human experience, particularly insofar as individual intelligences operated as part of larger organizations or in spite of them. The project of building AI was so provocative because the relationship between knowledge and technology within AI had important ramifications for the basis of authority within the relationship of man and machine—a relationship that was extending beyond the laboratory and into offices, military command centers, and ultimately homes. These debates about artificial intelligence contributed to the analysis of scientific knowledge in the 1950s, 1960s, and beyond, as the next chapter explains.

The third movement in this chapter is to examine the discourse of a "New Economy" in the 1960s. The major claim of this chapter is that the technological developments in computerized decision-making and management information systems, coupled to new ways of explaining the generation and transmission of knowledge, contributed to new social visions. In particular, the chapter analyzes the changing role of individuals within the corporations and institutions of this new era and articulates how these systems of social organization were predicated upon both the architecture of machines and the design of man-machine systems. The justifications for these patterns of social organization ran counter to those that had been developed earlier in the century (and which were described in chapter one). The result was that the social responses to the computing developments of the 1950s and 1960s contributed to the delegitimization of managerial authority. This forms a bridge between the bureaucratic, top-down form of computing in the 1950s and the libertarian, bottom-up form of personal computing from the late 1960s on.

The chapter concludes by situating this history amidst larger thematic issues in Science and Technology Studies. While the economic transformations in the 1960s demanded serious analysis, the central role of technology in this story meant that the most compelling analyses had to take technology into account. An important strand of STS directly addresses these issues, and this chapter explains how STS performs a vital role in the study of recent American history by providing a basis for analyzing agency within complex sociotechnical systems.

Solving Problems and Creating New Ones

The idea that research in psychology, cognitive science, and the behavioral sciences was beginning to identify a common methodology and subject matter in the 1960s was one supported by scholars, research institutes, and patronage agencies (both public and private). The impulse for unification had been a recurring element in twentieth century science, ranging from the positivistic agenda for a "unity of science" to post-war focuses on communications, control, and systems, as expressed in both the cybernetic movement and in the search for "general systems" theories.[3]

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[3] On the "unity of science" movement among the positivists, see George A. Reisch, *How the Cold War Transformed Philosophy of Science: To the Icy Slopes of Logic* (New York: Cambridge University Press, 2005). On cybernetics as a meta-science see Steve Heims, *The Cybernetics Group* (Cambridge, Mass.: MIT Press, 1991), and Geof Bowker, "How to be Universal: Some Cybernetic Strategies, 1943-70," *Social Studies of Science* 23 (1993):

Post-war behavioral science provided a framework for unifying the sciences broadly concerned with the human. Hunter Heyck has described two distinct moments within post-war social science patronage. The first moment involved the consolidation of the field along behavioralist lines, resulting in the greater use of mathematics and formal models. Research was based at institutions such as the Carnegie GSIA, the Harvard Department of Social Relations, and Michigan's Institute for Social Research, and funded through the support of foundations such as Ford and Rockefeller. The second moment involved a greater reliance on federal patrons such as the National Science Foundation and the National Institutes of Health, and led to specialization and fragmentation among researchers as those government patrons demanded tangible returns on their investments.[4]

This section begins with an analysis of that part of the emerging behavioral sciences concerned with the relationship between representations of behavior and performance at the moment in the late 1950s when the first behavioral science regime began to bleed into the second. This abstruse topic matters because the study of these faculties within the human was concurrent with the creation of new machines and new corporate forms whose successes depended on precisely the issues of how knowledge was represented within systems and how those systems performed complex behavior in response.[5] It also provides an explanation for how the development of communications sciences influenced the social meanings of science in the late 1950s that provides an alternative to the usual Sputnik-centric narrative in which political crisis led to radical shifts in public perception of science. This chapter instead argues that many of the actors seeking to justify the authority of large organizations (both public and private) found this science's definitions of top-down planning and atomistic information to be useful.

The psychologist George Miller pinpointed the crucial moment in this narrative as September 11, 1956, at a Symposium on Information Theory at MIT. That day Miller presented his article on "The Magical Number Seven," describing certain cognitive limitations, while Noam Chomsky described his "Three Models of Language" and Newell, Simon, and Shaw demonstrated their Logic Theorist, which had previously shaken up that summer's AI conference at Dartmouth.[6] That same year, psychologists Jerome Bruner, Jacqueline Goodnow, and George Austin published their *Study of Thinking*, which identified the heuristics used by individuals

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107-127. On "General Systems Theory," see Deborah Hammond, *The Science of Synthesis: Exploring the Social Implications of General Systems Theory* (Boulder, Colo.: University of Colorado Press, 2003).

[4] See Hunter Crowther-Heyck, "Patrons of the Revolution: Ideals and Institutions in Postwar Behavioral Science," *Isis* 97 (2006): 420-446.

[5] The general idea of a tension between representation and performance as typical of the 1950s can be seen in the work of Beat writers, such as Allen Ginsberg and Jack Kerouac. That their slightly younger spiritual descendents, the circle around Ken Kesey and Stewart Brand, took such a great interest in communications technology may be indicative of continuing interest in these matters. The significance of performance also figured prominently in the work of abstract expressionists such as Jackson Pollock, invoked by the Congress of Cultural Freedom as a symbol of American individualism. I suggest that there are parallels between the debates concerning the significance of these works and those concerning the validity of the strong AI project.

[6] Howard Gardner, *The Mind's New Science: A History of the Cognitive Revolution* (New York: Basic Books, 1985), 138. The 1956 AI conference is described below.

when confronted with novel problems and was built upon earlier work that had been influential for the design of Logic Theorist. These contributions came from different disciplines with only the loosest ties among them. However, several of the more broad-minded scholars perceived the possibility of creating a new science located precisely at the intersection of these diverse worlds of cybernetics, informatics, and cognitive science by focusing on the importance of communication.

Among the proponents of this proto-science was Kenneth Boulding, an unorthodox economist (and founding member of the Society for General Systems Research in 1954) who recognized the significance of information and communication while spending a year at the Center for Advanced Study in the Behavioral Sciences (CASBS) in Palo Alto in the mid-1950s.[7] Boulding described this science as the study of images, representations, and communication, which he christened "eiconics." His major influences in shaping the study of eiconics were Chester Barnard's *Functions of the Executive*, Norbert Wiener's *Cybernetics*, and Claude Shannon's *Mathematical Theory of Communication*.[8] This set of works is strikingly similar to a list of intellectual influences provided by Herbert Simon to Bernard Berelson at the Ford Foundation in 1956: Barnard's *Functions of the Executive*, H. Ross Ashby's *Design for a Brain* as a contribution from cybernetics, Warren Pitts and Walter McCulloch's papers as a contribution from information theory, von Neumann and Morgenstern's *Theory of Games of Economic Behavior*, and Nicholas Rashevsky's work in mathematical biology.[9] These works collectively described the ubiquity of information transmission and related the behavior of complex organisms and institutions (whether organic, mechanical, or social) to the most basic processes of sending, receiving, and decoding messages.

The content of this new science (according to Boulding, Simon, and George Miller, among others) uniquely described a domain of inquiry between the generalized abstraction of mathematics and the particularities of the strictly empirical sciences. This meant that eiconics (to keep Boulding's term) provided a scientific language that was able to reflect upon itself and could describe not only the facts of the natural and social world, but also the process through which observed facts, entrenched habits of mind, value commitments, and social situations all contributed to create a total representation of this world. This science therefore allowed one to study how ideas are communicated, learned, and assimilated into one's worldview and, at the same time, to systematically force scientific inquiry to reflect upon its own presuppositions. Boulding's summary of the problems created by an unreflective science remains strikingly vivid:

> The acid of science which has eaten away so many ancient images now is seen to turn on the image of science itself. The white-coated high priest of truth: austere, objective, operational, realistic, validating, is degraded to the status of the servant of a subculture, trapped in the fortress of its own defended public image, and straining the grains of truth through its own value system. As the physicist

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[7] The Center was created using funds from the Ford Foundation, and merged with Stanford University in 2008.

[8] Kenneth Boulding, *The Image* (Ann Arbor, Mich.: University of Michigan Press, 1956), 153.

[9] Letter from Herbert A. Simon to Bernard Berelson, 6/18/1956, Herbert A. Simon Papers, Carnegie Mellon University Archives, box 99.

> dissolves the hard table into whirling atoms, so the communication and information theorist dissolves the hard fact into messages filtered through a value system. Like Hume, we pale before the abyss of skepticism toward which our logic leads us relentlessly, but from which we draw back horrified, incredulous at incredulity. Like Hume, also, we go off and have a good dinner and then we feel better. We put philosophy into the back of the filing cabinet and shut it tight and return to the cheerful and ordinary business of life "believing where we cannot prove." From the abyss of reason we turn again to clutch at the slender rope of faith.[10]

Against this dispiriting view of science consuming its own foundations, Boulding noted with a certain optimism that "just as two negatives make a positive, so having illusions about an illusion would seem to be almost the same process as finding out the truth about truth."[11] He further observed that eiconics established a distinction between "messages from nature" and "symbolic messages" to replace the common fact/value distinction. Within the study of images, both "facts" and "values" were to be subordinated to the category of received messages subject to analysis on the basis of "consistency, coherence, survival value, stability, and organizing power." Because the holistic power of the image as an organizing principle in turn structured how messages were received, "the way in which the total image grows determines or at least limits the directions of future growth. In this growth process, however, the factual and the valuational images are inextricably entwined."[12]

Boulding's contribution was to clarify the independence of the image for behavioral scientists. The image, and the processes of communication and translation that shaped it, could be examined without reference to specific phenomena and personalities. Eiconics not only studied how messages were passed, but, crucially, how they were interpreted with respect to previous messages. This was useful for thinking about how others might see the world in different ways. However, it was taken up by others working at the CASBS in the late 1950s as one component of a larger theory that would explain how representations of problems would structure plans for problem solving. George Miller, Eugene Galanter, and Karl Pribram, a triumvirate of psychologists, supplemented Boulding's analysis of "Images" with a related analysis of "Plans." Their analysis began with an analogy that recalled Simon's 1956 ORSA address: "The notion of a Plan that guides behavior is, again not entirely accidentally, quite similar to the notion of a program that guides an electronic computer."[13] Put more formally, they defined a Plan as "any hierarchical process in the organism that can control the order in which a

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[10] Boulding, *The Image*, 171-172.

[11] Ibid., 172-173.

[12] Ibid., 174-175. Boulding concluded this analysis of the content of "eiconics" by reinforcing the self-sufficiency of the study of the Image: "We can never examine the correspondence of the image with reality, whether in the field of value or in the field of fact."

[13] George Miller, Eugene Galanter, and Karl Pribram, *Plans and the Structure of Behavior* (New York: Holt, 1960), 2. Simon's four predictions at the ORSA meeting were mentioned in chapter 2. His fourth prediction was that theories of human behavior would be articulated in the language of computer programming.

sequence of operations is to be performed."[14] This definition of planning therefore focused on the level of strategy as opposed to tactics. Plans were intended to be modular, meaning that a high-level plan could execute sub-plans as needed to achieve its goals. They cited Taylorist motion studies as the purest example of strictly plan-driven behavior. In the Taylorist ideal, every motion of every worker would be choreographed to accomplish a task with a minimum of waste. Variations might enter as circumstances changed, but these would be systematically determined by the efficiency expert in order to best accomplish the overall goals. However, the crucial innovation of Miller, Galanter, and Pribram was that their plans could incorporate within themselves sub-plans that could be invoked or not, as circumstances in the environment demanded. Their plans could branch and adapt. The rigid specification of action in the Taylorist system neglected the flexibility that a hierarchy of plans could accommodate. The cybernetic understanding of behavior employed a set of programs or tools to be used when they fit into the image of the world.

However, Miller, Galanter, and Pribram found that the analogy of the computer program was much better suited for explaining the behavior of organizations than for explaining the behavior of individuals. This was because organizations remained goal-directed and had no independent existence beyond the fulfillment of those goals.[15] Individuals, on the other hand, devoted their mental resources to creating rich images of the world and of their places in it, with the creation of formal plans being a decidedly secondary concern.[16]

Plans fell into two broad categories. The first, and simpler, category was that of systematic plans, or algorithms. While such a plan could definitively be proven to work, there was no guarantee that it would be efficient. Indeed, many such plans were plodding, slow, and strikingly inefficient.[17] The growing power of machines meant that these plans actually could be implemented for certain real-world problems. However, there remained classes of problems for which the plodding, systematic approach would remain too slow because the number of options to test became prohibitively large. Chess was a paradigmatic example, as the problem of projecting ahead several moves and counter-moves created a universe of options that was simply too large. The method of using heuristics created shortcuts that offered feasibility at the expense of certainty.[18]

Miller, Galanter, and Pribram employed two heuristics from the Logic Theorist and the General Problem Solver, Newell, Simon, and Shaw's early machines. The first heuristic was that of means-ends analysis, which first measured the difference between the existing state of the world and the desired end, and then selected procedures designed to reduce that difference. This was repeated until the difference was eliminated. The second type of heuristic was called the

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[14] Ibid., 16.

[15] An insight they derived from Chester Barnard's definition of an organization as the conscious coordination of effort to achieve a shared goal. See Barnard, *Functions of the Executive*, 73.

[16] Miller et al., 100.

[17] Recall Hao Wang's description of the ideal logician as a "persistent plodder."

[18] Recall from chapter two that the need for accuracy in calculation motivated the design of early machines.

"planning method," which consisted of reducing a given problem to its bare essentials, finding a solution to this simplified, abstract problem, and then using that as a high-level plan to guide the solution of the real-world problem. The value of these heuristics was not only that they could solve formally intractable problems, but also that they could generate additional plans, unlike the plodding solutions of algorithmic plans.[19] The similarity between "plans" as a framework for understanding psychology and "plans" as a way of building useful heuristics in machines led Newell and Simon to accuse Miller, Pribram, and Galanter of intellectual theft.[20] The psychologists protested that these ideas had been percolating in their minds for some time, and were based on the same few sources cited by both Boulding and Simon as leading to this new science.[21]

Miller, Galanter, and Pribram studied planning within machines to understand human problem solving, much as Newell and Simon did. They acknowledged that the fundamental task of the scientist was to represent the natural world—to work on the level of the image. However, computers offered a platform with which to experimentally re-enact the processes of problem solving and of creating images. Together, *Image* and *Plans* formed the basis for a new understanding of cognition—one that was built upon an analogy to the use of computing machines. The key parallel at this stage of cognitive science was not between electronics and neurons (as had been the case earlier), but the fundamental processes of communicating between images and plans in humans and machines.[22] As this chapter explains, the centrality of the planning method had ramifications not only for the development of artificial intelligence, but also for broader questions of social organization. The analogy drawn by Miller, Galanter, and Pribram concerning the centrality of plans for computers and organizations as contrasted with the centrality of images for individuals is crucial; as theories of organization and of intelligence drew upon the planning model, they also inspired a reaction against planning as the central component of thinking and acting in the individual.

Boulding had raised the stakes by arguing that research in communication theory was rendering the positivistic picture of scientific knowledge unsupportable. During the ideological struggle against communism in the 1950s, these academic debates took on a charged political meaning. The positivistic notion of science, based upon a unitary knowledge of the world, supported the goal of planning a society in accordance with that unitary truth. His understanding

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[19] For a thorough analysis of these heuristics, see Allen Newell and Herbert A. Simon, *Human Problem Solving* (Englewood Cliffs, NJ: Prentice-Hall, 1972).

[20] An irony was that Simon had claimed in his 1956 ORSA talk that the future of psychological theories was to be described in the language of computer programs. When Miller et al. did precisely this it came a bit too close to Simon's interests for comfort. Simon's heuristics were generated from studies of humans solving problems, and then became a basis for Miller et al. to rethink problem solving on computational lines, creating a virtual cycle whose circularity went unacknowledged.

[21] Herbert A. Simon, letter to George A. Miller, 5/10/1959; George A. Miller, letter to Herbert A. Simon, 5/12/1959; Herbert A. Simon, letter to George A. Miller, 5/19/1959, Herbert Simon Papers, box 109; Karl Pribram, letter to Allen Newell, 5/20/1959; Allen Newell, letter to Karl Pribram, 6/2/1959, Allen Newell Papers, box 62.

[22] For analogies between electronics and neurons, see John von Neumann, *The Computer and the Brain* (New Haven, Conn.: Yale University Press, 1957).

of scientific knowledge (untethered from the reality of world, as expressed in note 12, above) supported a vision of markets and democracy. Boulding described the Image as the organic product of an individual living in society and trying to make sense of the world. Eiconics suggested to him an intellectual humility and an embrace of pluralism because different individuals could construct such varied images from the world. It fit with his defense of a pluralistic market system against what he described as a planned budget system. "No matter how successful the market is in extending freedom and in lessening frustration, still nobody loves it … By contrast, the budget acquires a vicarious charisma from the organization which it coordinates. Organizations are superhuman, if not divine. They represent a power beyond that of the individual … It is little wonder that socialism has stirred the hearts of men as capitalism has not, and that it has filled the minds of men with its bright but deceitful dreams of the future."[23]

The argument of *Plans*, by contrast, suggested that all problem-solving—and possibly all behavior—could be described through fully articulated plans. This had clear implications for the ongoing project of office automation, in which successively complex forms of mental work would be analyzed and reconstituted in the most efficient ways with as many components automated as possible. Miller, Galanter, and Pribram had implicitly reinforced this opposition between individuals and organizations by observing that individuals primarily think on the level of Images rather than Plans, while organizations and computers operate on the level of Plans rather than of Images. The association among the image, organicism, and the individual on the one hand, and among plans, mechanization, and bureaucratic organizations on the other was firmly established. And yet the research plan at this intersection of cognitive science and artificial intelligence was to use the planning model as a window into human psychology.

Articulating Intelligence

Even as the behavioralists were turning toward the sciences of communication, a loose collection of mathematicians and computer scientists was creating the research program known as artificial intelligence. Artificial intelligence and the behavioral sciences built upon each other and many individuals contributed to both fields. The defining moment for this new field was a conference at Dartmouth in the summer of 1956 organized by John McCarthy, Marvin Minsky, Nathaniel Rochester, and Claude Shannon, and supported by the Rockefeller Foundation.[24] The purpose of the conference was to lay the groundwork for bringing distinct mathematical studies of pattern matching together into a coherent intellectual program.[25]

The term "artificial intelligence" lumped together work that was done for very different ends and according to very different principles. The name itself proved to be a bone of contention. John McCarthy needed a name for the subject of his conference and selected

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[23] Kenneth E. Boulding, "Symbols for Capitalism," *Harvard Business Review* 37 (Jan., 1959): 41-48, on 47.

[24] "A Proposal for the Dartmouth Summer Research Project on Artificial Intelligence," J. McCarthy, M. L. Minsky, N. Rochester, C. E. Shannon, 8/31/1955, Claude Shannon Papers, Library of Congress, box 11.

[25] A sense for what the conference organizers believed would be the heart of this new study can be gleaned from a collection of papers assembled earlier that year. See C. E. Shannon and John McCarthy, *Automata Studies* (Princeton: Princeton University Press, 1956).

"artificial intelligence" without any great reflection. Arthur Samuel thought that the word "artificial" trivialized their work while others feared that it created unwarranted associations with the problem of understanding human intelligence, a problem that many were at least tangentially interested in. Newell and Simon, for example, referred to their work as "complex information processing" instead.[26] These differences of nomenclature often signaled differences of methodology; the singular term "Artificial Intelligence" should not obscure the diverse approaches taken by researchers. Newell and Simon (with J. C. Shaw of RAND) presented their Logic Theorist and claimed (none too modestly) they had already solved the problem that the conference was supposed to define. Though by no means unknown within the larger world of computing, they approached the general topic of machine learning as outsiders, informed as much by problems in administration as by problems in mathematics. According to Newell and Simon, their arguments about "complex information processing" fell on deaf ears until they gave a clear demonstration of its possibilities.[27]

In the absence of any clear idea of how to make a truly intelligent machine (or even agreement on what intelligence was), the primary criterion for good AI research was achieving interesting results. Ultimately researchers wanted to describe the process of creating intelligent behavior, but until then they would be content with acquiring the tacit knowledge of how to work with computers while developing a body of empirical results. To this end, they studied highly artificial scenarios and invited undergraduate "hackers" to play with their machines. These were necessary preconditions to developing properly scientific understandings of computer intelligence. This also reflected generational differences between older researchers who continued to privilege pencil and paper work (with actual computer time reserved for implementation) and younger researchers who valued experimenting with the machine. While similar dynamics within the larger field of computer science did lead to an increasing formalization of research, this process was far slower in the AI laboratories. Years later, invoking a crudely Kuhnian concept of scientific paradigms, several AI researchers—including Marvin Minsky and Joseph Weizenbaum at the MIT AI Lab and John McCarthy, who had left MIT for Stanford in 1962—suggested that the lack of a paradigm for AI made their work less than fully scientific, as described below. Play was a proto-paradigmatic stage of scientific work.

While some AI leaders, such as John McCarthy, understood the challenge of AI primarily as a logical problem, and some, such as Newell and Simon, made broad claims about psychology based on their computational work, there remained a deep ambiguity within the community concerning AI's position as a science. The desire to simulate existing human intelligence and the desire to create high-performance—but fully fictitious—machine intelligence existed in conflict in the work of Newell, Simon, Minsky, McCarthy, Feigenbaum, and other AI leaders. That this tension was never resolved led to some consternation among these scientists, though others insisted that an AI that took itself too seriously would be excessively narrow. AI's greatest strength was that its practitioners could be psychologists, engineers, or logicians as the situation demanded. Being "scientific" carried certain social authority, but the most successful researchers in AI impishly embraced their ambiguous position. Weizenbaum described his skepticism to

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[26] Crevier, 49-50.

[27] Herbert Simon, letter to Pamela McCorduck, 8/12/1975, Simon Papers, box 108.

strong AI in a 1962 article, "How to Make Machines *Appear* Intelligent" by denying that the question of whether or not his program was intelligent was the right one to ask. Instead, he pointed out (following Turing) that "intelligence" was a quality that we attributed to the behavior of a machine and not something inherent in it.[28]

The MIT AI Lab had an opportunity to redefine itself in 1970 when it formally separated from Project MAC due to budgetary constraints. In defending the intellectual importance of AI, Minsky's team believed that the problem with "mainstream" computer science was that it either focused too much on formalism to the exclusion of practical knowledge about what computers could do, in the case of the mathematical theory; or it focused too much on building working systems without adequately theorizing what these systems should be doing in the first place, as with the Multics group. Minsky and his colleague Seymour Papert instead described the cutting-edge research in AI as involving the creation of highly abstracted, artificial test cases, or "micro-worlds." Within these highly constrained micro-worlds, researchers actually could build systems that exhibited complex behavior. Of course, these operated within entirely artificial conditions, but Minsky and Papert declared that "the category true/false is less important than fruitful/sterile. Naturally the final goal must be to find a true conclusion. But, whether logicians and purists like it or not, the path to truth passes mainly through approximations, simplifications, and plausible hunches which are actually false when taken literally. And little good can come from a mind or a machine whose only ability with respect to these fruitfully false statements is to detect their falsity, abandon them and try some other route."[29] The path to truth necessarily passed through a realm of fantasy. Artificial Intelligence, as its name suggested, would continue to straddle that boundary.

A recurring motif in the attempts of various computer scientists to define the unique core of their discipline is the emphasis on design, or artifice—what Marvin Minsky described as a science of form itself. The standard by which this science had to be judged was the utility of these forms and not the truthfulness of the models used or of the data. Some, such as Simon and Newell, held to the belief that AI could teach them about human cognition even as they maintained that the principles of design developed through computer architecture would be a crucial component of future science.[30] Others, such as Simon's student, Ed Feigenbaum, became "knowledge engineers" designing "expert systems" that could solve certain well-defined problems in certain technical domains. In both cases, the significance of the work done by thinking machines depended upon the form of the problem to be solved. While the nature of machine intelligence remained controversial, AI research generated new insights into the forms of problems and knowledge domains.

Simon's strong claims about the power of his models led to accusations that he was a fundamentalist for the project of reducing all thought to simple information processing, a charge

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[28] Joseph Weizenbaum, oral history interview with Pamela McCorduck, 3/6/1975, Pamela McCorduck Papers, Carnegie Mellon University Archives, box 2.

[29] Marvin Minsky and Seymour Papert, "Proposal to ARPA for Research on Artificial Intelligence at MIT, 1970-71," J. C. R. Licklider Papers, box 10. Emphasis in the original.

[30] See Herbert A. Simon, *The Sciences of the Artificial* (Cambridge, Mass.: MIT Press, 1996).

most famously leveled by the philosopher Hubert Dreyfus. However, there was a basic ambiguity in Simon's many statements about his models. He emphasized that they were to function as tools to inspire thinking rather than as descriptions of the world from which further claims could be deduced, and claimed that the point of his AI machines was to uncover what he described as "the whole set of sly tricks which humans use in lieu of computing power."[31] At the same time, he also remained convinced that his model of intelligent behavior as a form of information processing appropriately described the processes creating human intelligence.

AI, while a narrow field, connected to several broader themes of 1950s American scientific life. It remained intimately related to the larger social concerns about "Images" and "Plans" discussed above. The centrality of coherent images and of structured plans in this new science came under attack from a loose confederation of philosophers, anthropologists, and sociologists. This involved a high degree of linguistic slippage; while plans had been built for the specific goal of solving well-defined problems, they were then taken to be general models for thought. AI was a weighty topic to be analyzed by the burgeoning metascientific disciplines while continuing to speak directly to the nature of science itself, much as the work of Boulding and Miller did. A key figure in shaping these conversations about the limits of planning was Hubert Dreyfus, then at MIT. In the course of teaching philosophy to computer science students at MIT, Dreyfus claimed that "what was interesting was to see how much what [the students] were doing was just what Plato and so forth had been doing and that they were right—that they were doing that more rigorously, more clearly and more definitively than any philosopher had ever done it and that if philosophy really was what people from Plato through … [Kant] thought it was, they were the real inheritors of philosophy." From this he reflected on Heidegger's claim "that cybernetics is the last stage of metaphysics. And then I realized more and more that I was against them and against philosophy."[32] Dreyfus's points are well taken in light of the strongest claims made on behalf of AI, but this was a research field where the gap between its ambitious rhetoric and its modest projects could not be bridged so easily.

Dreyfus secured a position as a visiting philosopher at RAND, where his brother Stuart worked as a mathematician. Using the ideas of phenomenologists such as Heidegger and Merleau-Ponty, philosophers of language such as Wittgenstein, and critics of formal scientific methods such as Michael Polanyi, Dreyfus produced a booklet in 1965 entitled "Alchemy and Artificial Intelligence," later expanded as *What Computers Can't Do* in 1972, and *What Computers Still Can't Do* in 1992. The decision to publish "Alchemy and Artificial Intelligence" was deeply controversial within RAND, where many of the scientists he criticized were based. The team of Newell and Simon came in for a particularly heavy drudging, and Simon vociferously demanded that RAND not lend its imprimatur to the work of the Dreyfuses. At MIT, Seymour Papert and Marvin Minsky made it clear that Dreyfus was not to be engaged as a legitimate critic of the science.[33] Dreyfus did find support from an unexpected source: the management guru Peter Drucker, who was famously ambivalent about the growing formalism of

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[31] Herbert Simon, oral history interview with Pamela McCorduck, 11/6/1974, Pamela McCorduck Papers, box 2.

[32] Hubert Dreyfus, oral history interview with Pamela McCorduck, 7/21/1976, Pamela McCorduck Papers, box 1.

[33] Hubert Dreyfus, oral history interview with author.

mid-century management education and who helped him expand the article into a book and find a publisher.[34] Drucker had a long-standing interest in the relationship of technological change for business management. He was also openly skeptical about the tendency of management research and education to become increasingly technical.[35] He approached management theory from a background in continental philosophy and defended more philosophical traditions of business management from the encroachment of mathematical tools.

The attack on the pretensions of the AI community was based upon three major foundations, which Dreyfus described as the psychological assumption, the epistemological assumption, and the ontological assumption.[36] In attacking the psychological basis of AI, Dreyfus criticized the cognitive scientists' elision of the neurobiological level and the phenomenological one. Their assumptions about how the stuff of worldly experience was translated into the firings of neurons were often unexamined, and when they were made explicit they were incoherent. Yet the remaining two arguments were even more decisive, said Dreyfus.

His epistemological argument attacked the centrality of rules and procedures in AI. He said that the faith in rule-governed behavior was a misunderstanding of the observation that rules could be found to describe physical behavior after the fact, because this did not imply that the behavior was governed by any pre-existing plan. In other words, the form of scientific laws describing behavior (in physics, say) was not equivalent to that of rules governing computer behavior. Drawing on the experience of linguistics, he described how analytical rules were inadequate to describe the way that language is performed, for "if the theory then requires further rules in order to explain how these rules are applied, as the pure intellectualist viewpoint would suggest, we are in an infinite regress. Since we do manage to use language, this regress cannot be a problem for human beings. If AI is to be possible, it must also not be a problem for machines."[37] This directly attacked the planning models of Miller, Galanter, and Pribram.

The final argument—that of ontology—challenged the belief that atomistic logical propositions had meaning devoid of context. Dreyfus emphasized that intelligent behavior could only be understood in terms of a situation that gave it meaning. Ever the philosopher, he extrapolated some large consequences from this analysis of AI and from AI's failure to meet the predictions of Simon and others. According to Dreyfus, "these failures must be interpreted as empirical evidence against the psychological, epistemological, and ontological assumptions. In Heideggerian terms this is to say that if Western Metaphysics reaches its culmination in

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[34] Hubert Dreyfus, oral history interview with author. Drucker had learned about Dreyfus and his work through a "Talk of the Town" piece in *The New Yorker* in June 1966.

[35] On this point, Drucker made a cryptic comment in 1997: "I consider the American research university of the last 40 years to be a failure." He suggested that too much effort was being spent on technically adept but irrelevant research, and too little time on fundamental education. Quoted in Dennis Normile, "Schools Ponder New Global Landscape," *Science* 277 (1997): 311.

[36] Dreyfus also argued on a fourth level, the biological, based on assumptions about neuronal behavior. These were based on the state of neuroscience of his day, and he gave this argument less weight.

[37] Hubert L. Dreyfus, *What Computers Still Can't Do: A Critique of Artificial Reason* (Cambridge, Mass.: MIT Press, 1992), 203.

Cybernetics, the recent difficulties in artificial intelligence, rather than reflecting technological limitations, may reveal the limitations of technology."[38] But this was not simply a problem of technology—what did these limitations suggest for a society increasingly built around technological understandings of cognition, communication, and socialization?

These big consequences lent urgency to his analysis. Dreyfus argued that the cost of continuing to follow an erroneous understanding of human reason would be great.

> If the computer paradigm becomes so strong that people begin to think of themselves as digital devices on the model of work in artificial intelligence, then, since for the reasons we have been rehearsing, machines cannot be like human beings, human beings may become progressively like machines. During the past two thousand years the importance of objectivity; the belief that actions are governed by fixed values; the notion that skills can be formalized; and in general that one can have a theory of practical activity, have gradually exerted their influence in psychology and in social science. People have begun to think of themselves as objects able to fit into the inflexible calculations of disembodied machines: machines for which the human form-of-life must be analyzed into meaningless facts, rather than a field of concern organized by sensory-motor skills. Our risk is not the advent of superintelligent computers, but of subintelligent human beings.[39]

Whatever the merits of this claim, it recognized an important point: the fecundity of the computational metaphor was making it *pre*scriptive and not merely *de*scriptive.

Dreyfus was not the first to question the possibility of artificial intelligence. A simpler argument came from the mathematician Mortimer Taube, who explained that the use of computers to solve some formally simple problems did not imply that all problems were solvable. Drawing upon Gödel's work in logic, he explained that while certain problems could be solved using formal methods, the rules of logic were themselves outside these bounds.[40]

Dreyfus found few converts within the AI community. However, Joseph Weizenbaum did take him seriously and became a critic of AI.[41] Weizenbaum had gained some notoriety at the MIT AI Lab for his work on ELIZA (designed from 1964-1966), a conversational program. He had written a version of ELIZA to carry out dialogues that mimicked a session with a therapist.[42]

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[38] Ibid., 227.

[39] Ibid., 280.

[40] Mortimer Taube, *Computers and Common Sense: The Myth of Thinking Machines* (New York: Columbia University Press, 1961).

[41] Weizenbaum had struck several of his associates as a different sort of computer scientist. Allen Newell described him to Ithiel de Sola Pool as "an intellectual. In this he differs (positively, as far as I'm concerned) from many engineers and programmers." See Allen Newell, letter to Ithiel de Sola Pool, 10/24/1966, Newell Papers, box 63.

[42] A sample conversation, from Joseph Weizenbaum, "ELIZA—A Computer Program for the Study of Natural Language Communication between Man and Machine," *Communications of the ACM* 9 (1966): 36-45:

The public reaction to this program shocked him in three ways. First, he was appalled that practicing psychiatrists believed that the direction of their profession was toward greater automation, and that someday a program like ELIZA could conduct real therapy sessions with real patients. His second shock was that even knowledgeable users of the program quickly felt a deep attachment to their artificial analyst, revealed their deepest thoughts to it, and felt genuine emotions during this conversation. The final shock had to do with the exaggerated claims for ELIZA's significance made by social critics. This suggested to him that observers wanted to believe the most fantastical things about computing technology and that there was little critical restraint when talking about computers.[43]

Weizenbaum's transition from ELIZA and experimentation in the early 1960s to his moralism at the end of that decade reflected his perception that the AI community was confusing the importance of constructing interesting computer systems with the importance of constructing those that accurately re-enacted human problem solving. The successes of toy problems bred an unhealthy sense of hubris among researchers and an unhealthy credulity in the public.

The decisive problem with computers, according to Weizenbaum, was moral, not technical. In describing the application of automatic and potentially intelligent programs to questions of social interest, he maintained that "the question is not whether such a thing *can* be done, but whether it is appropriate to delegate this hitherto human function to a machine."[44] Weizenbaum's argument required fundamental differences between the nature of human existence and that of machines. Drawing upon his work with Dreyfus, he argued that what made fundamental problems fundamental was a sense of immediacy that was essentially non-rational and something that even the most sophisticated machine simply could not understand. "What could be more obvious than the fact that, whatever intelligence a computer can muster, however

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TELL ME MORE ABOUT YOUR FAMILY
My mother takes care of me.
WHO ELSE IN YOUR FAMILY TAKES CARE OF YOU
My father.
YOUR FATHER
You are like my father in some ways.
WHAT RESEMBLANCE DO YOU SEE
You are not very aggressive but I think you don't want me to notice that.
WHAT MAKES YOU THINK I AM NOT VERY AGGRESSIVE
You don't argue with me.
WHY DO YOU THINK I DON'T ARGUE WITH YOU
You are afraid of me.
DOES IT PLEASE YOU TO BELIEVE I AM AFRAID OF YOU
My father is afraid of everybody.
WHAT ELSE COMES TO YOUR MIND WHEN YOU THINK OF YOUR FATHER
Bullies.
DOES THAT HAVE ANYTHING TO DO WITH THE FACT THAT YOUR BOYFRIEND MADE YOU COME HERE

[43] Joseph Weizenbaum, *Computer Power and Human Reason: From Judgment to Calculation* (San Francisco: W. H. Freeman, 1976), 5-7.

[44] Ibid., 207.

it may be acquired, it must always and necessarily be absolutely alien to any and all authentic human concerns? The very asking of the question, 'What does a judge (or a psychiatrist) know that we cannot tell a computer?' is a monstrous obscenity."[45] The question of whether the task of judging could be given over to a computer was one that had arisen during a debate with John McCarthy.[46] For Weizenbaum, McCarthy's confidence (and through him, the entire AI community) that all work of the mind could be reduced to information processing—applying given rules to a body of established facts—represented the danger that purely instrumental reason would lead to the inability to speak in the languages of morality or spirituality.

The vitriol between the AI camp and its critics requires some parsing. It is hard to reconcile the two sides of Simon—the author of *Administrative Behavior*, a persistent critic of the neoclassical school of economics and the creator of bounded rationality, and simultaneously the high priest of strong AI. It is certainly not this author's intention to adjudicate a fifty-year old debate, though it seems clear that there were certain failures of communication. While the debates about artificial intelligence focused on the most extreme positions possible on either side, the connection between the questions raised by artificial intelligence and those raised by organizational politics clarifies these positions.

Simon recognized that while the question of *organizational design* had been the link from the study of administration to that of computation, his link from artificial intelligence back to business had shifted to *strategy*. Hierarchies of plans governed the behavior of programs just as they did for organizations.[47] His insights into bounded rationality had described how individual reason was limited by the larger systems in which it is embedded. His subsequent work on heuristics gave him an insight into the strategic level—and the construction of those bounds on individual rationality. As one whose social ideals were formed in the New Deal, Simon retained a faith that proper structures could align private interests with public welfare; the framework of top-down means-ends analysis was one that connected his social beliefs with his technological ones.

For Dreyfus, the implications of artificial intelligence for organizational questions remained decidedly secondary. He was, however, directly concerned with the implications of using artificial intelligence as a model for understanding human cognition. This question similarly motivated Joseph Weizenbaum. Both had a distinctly moral edge to their analyses, and both kept their concern at the level of the individual. Drucker, who backed Dreyfus and remained wary of Simon's efforts in management, distrusted the top-down models that stripped work of its

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[45] Ibid., 226-227.

[46] McCarthy, however, disputed Weizenbaum's use of this quote (and others). See John McCarthy, "An Unreasonable Book," 1976, available online at http://www-formal.stanford.edu/jmc/reviews/weizenbaum/weizenbaum.html (accessed 3/14/2011).

[47] Wrote Simon, "In the course of a conversation one day with a Japanese businessman, who was trying to find out where interesting things were being done in the United States, I suddenly realized that the work of real interest for the future of management information systems was not being done under that label at all, but under such labels as "strategic planning." After I had made that translation, things looked a good deal less bleak." See letter from Herbert A. Simon to James D. Koerner, Alfred P. Sloan Foundation, 6/28/1972, Simon Papers, box 115.

meaning. The division between these two camps generally reflected a difference between a top-down and a bottom-up orientation.

Dreyfus later admitted that the vitriol in these debates might have been due to the recognition on both sides that their philosophical principles were fundamentally at odds and that each one's successes threatened the other. The nature of thinking was crucially important to both Simon and Dreyfus, and they had each built their reputations as thinkers upon dramatically different descriptions of thought. For Simon, cognition was nothing but the logical manipulation of symbols, while for Dreyfus such a view was a hopelessly limited view of the richness of being in the world.[48] On a less lofty level, the dispute generated theatrics that heightened the stakes for the individuals involved. After Dreyfus noted that a ten year old had beaten the most sophisticated chess machine of the day, Mac Hack, Seymour Papert organized a showdown between Dreyfus and the machine. Dreyfus lost. The ACM ran a headline declaring: “A Ten Year Old Can Beat the Machine: Dreyfus. But the Machine Can Beat Dreyfus.” According to Simon, the lesson for Dreyfus was that “MacHack behaved not like an “omniscient computer” (to quote you [Dreyfus] out of context), but like a frail and sometimes desperate humanoid—even, shall we say, as you and I.”[49]

The claims made on behalf of artificial intelligence were not strictly technical. Instead, support for the strong AI agenda also represented a statement of belief that the processes of reasoning were based upon rules that could be executed regardless of who or what was doing so. As such, the possibility of machine reason implied a universality of reason that supported basic principles of equality. During a talk about the impossibility of artificial intelligence, Dreyfus had claimed that the fundamental differences between the physical embodiment of humans and computers implied fundamental differences between human and computerized intelligence. Pamela McCorduck, a writer and the wife of computer scientist (and head of the Carnegie Mellon department) Joseph Traub, protested that similar arguments had historically used the physical differences between the sexes to deny that women had equal intellectual capabilities.[50] McCorduck also denied that a proper socialization was necessarily a good or humane thing. Conversing with RAND computer scientist Paul Armer, she asked “how decisions might have been made by people who weren't socialized the way decision makers have been socialized? You know, to get a decision-maker to turn around his views on women, you have to beat him over the head.”[51] During the 1960s and 1970s, as civil rights movements were at their height, such debates about universality and cultural particularism were part of the intellectual atmosphere. Artificial intelligence provided a test case for universalizing reason.

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[48] McCorduck, *Machines Who Think*, 237.

[49] Herbert Simon, ““Cool It, Friend!” (An Open Letter to Hubert Dreyfus),” undated, Simon Papers, box 108.

[50] Pamela McCorduck, conversation with Marvin Minsky, 2/7/1977, McCorduck Papers, box 2. The point is repeated in McCorduck, *Machines Who Think*, 238. McCorduck comes closest to giving the “official” history of artificial intelligence even though the book remains a very personal account. As in the work of Diana Forsythe, described below, the best accounts of the power and the limitations of impersonal machine reason involve the authors navigating complex personal relations with the subject matter.

[51] Pamela McCorduck, conversation with Paul Armer, 12/13/1974, McCorduck Papers, box 1.

But the case for dignity could also rest on a denial of this universalism.[52] The poet Adrienne Rich had attended a talk by Simon on the strong AI program. She responded to the idea of machine learning with a poem, "Artificial Intelligence." In the poem she seized upon the differences between the perfect cognition of the thinking machine and the corporeal intelligence of the human poet, as captured in a chess match.

I'm sulking, clearly, in the great tradition
of human waste. Why not
dump the whole reeking snarl
and let you solve me once and for all?
(*Parameter:* a black-faced Luddite
itching for ecstasies of sabotage.)

Still, when
they make you write your poems, later on,
who'd envy you, force-fed
on all those variorum
editions of our primitive endeavors,
those frozen pemmican language-rations
they'll cram you with? denied
our luxury of nausea, you
forget nothing, have no dreams.[53]

Rich's poem, with allusions to masochism, illness, and the irrational, manages to convey a respect for the uncanny calculating power of mechanical intelligence while questioning whether such perfection is even capable of being a source of poetry and genuine creativity at all. Her suggestion is that the process of forgetting is essential to artistic creation, and that the need to create derives from the necessary limitations of human capabilities.

The basic question was whether machine intelligence reflected the interests of the powerful, having been built by elite engineers and for the purposes of either the military or large bureaucracies, or whether the unsociability of machines freed them from the stifling conformity of received opinion to grasp more fundamental truths. The solution to this type of question lay beyond the power of even the most powerful machines to solve. What chance did mere mortals have?

New Workers in a New Economy

Arguments about the nature of artificial intelligence bled into arguments concerning the organization of the mid-century corporation. The critics of artificial intelligence questioned

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[52] Joan Scott clearly summarized the stakes of universalism: "Is universalism a genuinely inclusive concept, violated only in practice, or is it inherently exclusionary, a way of (mis)representing a set of particularistic normative standards as if they were neutral?" See Joan Wallach Scott, *Gender and the Politics of History* (New York: Columbia University Press, 1999).

[53] Adrienne Rich, undated letter to Herbert Simon, Simon Papers, box 113.

whether formal procedures could ever truly replicate the experience of human coping in the world. On the other hand, advocates of artificial intelligence defended the universality of reason and claimed that valid thought could be performed by machines as well as by brains. The similarity between programs and organizations brought the controversies of artificial intelligence to bear upon planning at the level of the system. Advocates of social planning emphasized the interconnections within the social system and claimed that corporations ought to be run as sub-systems whose operations should be geared to the maximization of social welfare rather than the maximization of corporate welfare, which could be detrimental to society overall.[54] Their critics, on the other hand, maintained that corporations had a unity of purpose that could be rationally maximized, unlike either individuals or society as a whole. Indeed, trying to satisfy additional social goals ran up against the specialization of labor that made corporations so effective at doing their work and socially valuable as a consequence.[55] The analogy between computers and organizations meant that both could be discussed within the same framework of planning. The question of whether society as a whole fit into the same category remained unresolved.

The central position of technology in analyses of social change was not just due to the importance of the technology itself. These dialogues were facilitated by a set of institutions and publications that encouraged dialogue among scientists, engineers, economists, and cultural critics. Such dialogues, of course, were not unique to the 1960s. Earlier social theorists certainly understood that technological change had consequences for economic and social developments, and technologies such as the electric grid and the railroad had been subject to probing analyses by Veblen, Mumford, and others. However, these connections became more explicit with the assortment of foundations, journals, and academic associations operating in the 1960s. Technology was no longer just another field in which social movements could play out; technology had moved to the center of a social theory that was more concerned with the instruments of bureaucracy than with the inevitability of class struggle.

The major philanthropic foundations—including Ford, Rockefeller, and Sloan—supported research into the various elements of postwar industrial society, bringing together technical and social contributions. These foundations represented the core tenets of moderate liberalism and became targets for the right-wing in its "revolt against the vaguely pluralistic, empiricist, putatively 'value-free' tradition of social science underlying liberal 'consensus' politics and political culture."[56] As the beginning of this chapter described, the research into applying cognitive science and computing paradigms to psychology was largely done (in its early stages) with the support of these foundations. In the latter stages, which involved creating

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[54] For example, see Adolf A. Berle, Jr., "The Corporation in a Public Society," in Melvin Anshen and George Leland Bach, eds., *Management and Corporations, 1985: A Symposium Held on the Occasion of the Tenth Anniversary of the Graduate School of Industrial Administration, Carnegie Institute of Technology* (New York: McGraw-Hill, 1960), 63-98. The influential management guru Peter Drucker also argued that managers should consider the role of their organizations within the wider social realm.

[55] A classic statement to this effect is Milton Friedman, *Capitalism and Freedom* (Chicago: University of Chicago Press, 1962).

[56] Alice O'Connor, "The Politics of Rich and Rich: Postwar Investigations of Foundations and the Rise of the Philanthropic Right," in Nelson Lichtenstein, ed., *American Capitalism: Social Thought and Political Economy in the Twentieth Century* (Philadelphia: University of Pennsylvania Press, 2006), 228-248, on 231.

more concrete applications, the government's science funding agencies took up much of the slack.

The foundations also supported the interdisciplinary research centers that were intended to bring scientific insights to bear on social problems and to apply trenchant social criticism to new technological developments. Such centers included the young Center for Advanced Studies in the Behavioral Sciences, as well as initiatives run by the established American Association for the Advancement of Science such as its Commission on the Year 2000.[57]

This style of scientific/objective/technocratic administration was not only studied by its adherents within the liberal establishment. The intellectual core of the nascent conservative backlash grew out of the rejection of this technocratic impulse. Among the cadre of not-quite-yet neo-conservatives who founded such publications as *The Public Interest* (whose first issue prominently featured analyses of automation and technological unemployment by Robert Solow and Robert Heilbroner), the major article of faith was that the scientific approach to public policy was fundamentally incomplete, based upon utopian aspirations and flawed notions of human nature.[58] Daniel Bell, one of that journal's founders, assessed what he first described in 1962 as "post-industrial society" and argued that it represented a return to the political from the naïve faith in apolitical technocratic problem solving. And, as he concluded, "this is how it should be."[59] The variety of human interests and of moral values required subordinating technical solutions to the give and take of politics.

On the left, critiques of the liberal establishment began to invoke Weber with greater frequency and Marx with less—an outcome of C. Wright Mills's deep engagement with Weber and, perhaps, a consequence of lingering anti-communist hysteria.[60] But this orientation had deeper roots in the fertile soil of the early twentieth century, when industrialization coincided with both the rapid growth of the American state and the professionalization of the social scientific community and, perhaps, an earlier phase of anti-communist hysteria. One influential group of academics and journalists described a "Triple Revolution" in 1964 that fused technological changes in employment to both the nuclear stalemate of the Cold War and the growing movement for civil rights. According to them, military spending diverted funds and attention from social problems, and fed the technological developments that led to unemployment, which in turn most severely affected minorities. Crucially, the authors of this

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[57] The report is Daniel Bell, ed., *Toward the Year 2000: Work in Progress* (Boston: Beacon Press, 1968).

[58] While Solow and Heilbroner differed in their assessment of the connection between technological development and unemployment, they both agreed that the profession of economics had failed to engage with a topic of great public interest. See Robert M. Solow, "Technology and Unemployment," and Robert L. Heilbroner, "Men and Machines in Perspective," *The Public Interest* 1 (Fall 1965): 17-36.

[59] Daniel Bell, *The Coming of Post-Industrial Society: A Venture in Social Forecasting* (New York: Basic Books, 1976), 364-367.

[60] See Daniel Geary, "C. Wright Mills and American Social Science," 135-156, in *American Capitalism*, and Howard Brick, *Transcending Capitalism: Visions of a New Society in Modern American Thought* (Ithaca, NY: Cornell University Press, 2006).

manifesto argued that the problems created by these revolutions could only be solved by taking a broad approach that simultaneously tackled economics, rights, and security.[61]

These developments in American social theory matter because the critiques of state legitimacy were based on the increasing bureaucratization and impersonality for which computers remained the prime example. The many venues for putting social scientists in dialogue with technologists encouraged a greater degree of sophistication in these analyses. The most immediate concern for these intellectuals was not lofty, abstract principles of freedom, rationality, efficiency, or any of that. It was the matter of jobs and the balance of work between humans and machines. Those larger, abstract concerns quickly entered these analyses, however, and the question of technological unemployment could not be dissociated from the issues of dignity, authority, and autonomy.[62]

Managers valued computing machines for bringing the efficiencies of the assembly line to the machinery of bureaucracy—but left unchecked, the growth of these large-scale systems of machines and technologies threatened the jobs of the rank-and-file. The development of computers as automatic thinking machines in the 1960s was inspired by the belief that many elements of these large technological systems could be removed from the realm of human activity and brought entirely within the machines themselves. Routine, repetitive, or time-consuming drudge work would be assigned to the machine, as could potentially dangerous work, while the human user could concentrate on the interesting tasks—those that required the exercise of human judgment. According to James Bradburn, an executive at Burroughs, "for all its benefits, 'mass production' has tended to yoke millions of people to repetitive jobs requiring a fraction of their ability. Computers offer the hope of removing this corrupting influence on our national character."[63] While increasing efficiency may have come at the expense of dehumanizing work, computer research into interactivity had been intended to break this connection.

Herbert Simon sketched the implications of the new information sciences for the function of management in a short book from 1960, *The New Science of Management Decision*. This work clearly connects his interests in the organization of complex institutions, individual problem solving, and the creation of formal systems for managing work. It revolves around the distinction between *programmed* and *non-programmed* decisions, which are defined by whether a decision is made by following a fully specified plan, or whether it is made by a "general

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[61] The Ad Hoc Committee on the Triple Revolution, "The Triple Revolution," *Liberation*, April 1964, 9-15. The membership of this group includes a Who's Who of the left: Donald G. Agger, Dr. Donald B. Armstrong, James Boggs, Lois Fein, W. H. Ferry, Maxwell Geismar, Todd Gitlin, Philip Green, Roger Hagan, Michael Harrington, Tom Hayden, Robert L. Heilbroner, Ralph L. Helstein, Frances W. Herring, Hugh B. Hester, Alice Mary Hilton, Irving Howe, Everett C. Hughes, H. Stuart Hughes, Gerald W. Johnson, Irving F. Laucks, Stewart Meacham, A. J. Muste, Gunnar Myrdal (with reservations), Linus Pauling, Gerard Piel, Michael D. Reagan, Bayard Rustin, Ben B. Seligman, Robert Theobald, John William Ward, William Worthy.

[62] As the critiques of AI suggest, it was precisely the attempt to make these analyses subject to strict cost-benefit accounting and quote-rational/efficient-unquote concerns that was at issue. As much as planners tried to bring order to these studies, the fuzzy/abstract/normative elements continued to pervade these debates.

[63] James Bradburn, "An Executive Voice in Datamation," *Datamation* 4 (1958): 24-25.

capacity … for intelligent, adaptive, problem-oriented action."[64] In traditional organizations, formal procedures describe the realm of programmed decisions, while the non-programmed decisions are made through the use of judgment—a vaguely defined term. Simon's notions of procedural and substantive rationality not only made a sharp distinction between a managerial form of expertise and a narrowly specialist one, but identified the former with the core mission of computer science. During this Golden Age of AI, the significance of studying computer operations was nothing less than an attempt to understand the organization of knowledge itself.

Simon recognized the power of what he referred to as "Gresham's Law of Planning," in which the expansion of formal procedures drives out opportunities for the exercise of judgment. Because many important problems would remain beyond the reach of formal procedures, creative and unstructured problem solving required special institutional protection.[65] At the same time, research into both human problem solving and into artificial intelligence continued to lift the veil from judgment and demystify it. This suggested that the distinction between programmed and non-programmed decisions was not a *necessary* one, but rather one of economics—of continually determining where resources should be expended to translate instinct into procedure and where human actors should be left alone to do their work.[66] The best outcome, and a realistic option, was to use formal processes as aids to open-ended thinking, along the lines of Licklider's symbiosis. The programmed methods of problem solving would be an intellectual toolkit for the human thinker, who could then employ his or her mastery of these tools to work on complex problems.[67] The expansion of programmed decision-making tools need not mean the end of creative thinking. It would instead provide greater structure for human problem solvers, who could continue to use their creativity and judgment to work within this structure, but who could use structured methods to solve increasingly complex problems. Simon predicted that certain responsibilities of middle management, which tended to focus on programmed decisions, would be automated, but organizations would continue to require hierarchies and distributed responsibilities. In a later essay, Simon described this position as being a technological radical but an economic conservative.[68]

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[64] Herbert A. Simon, *The New Science of Management Decision* (New York: Harper & Row, 1960), 6.

[65] Ibid., 13.

[66] Bruce Mazlish described this recognition that the function of intelligence was not our uniquely capability "the fourth discontinuity"—the fourth major displacement from our understanding of ourselves as occupying a special position within the natural order. The other displacements he cited were the Copernican shift to heliocentrism, Darwinian evolution as an explanation for human origins, and Freud's discovery of the unconscious. See Bruce Mazlish, *The Fourth Discontinuity: The Co-Evolution of Humans and Machines* (New Haven, Conn.: Yale University Press, 1993).

[67] Simon, *The New Science of Management Decision*, 33-34.

[68] Herbert A. Simon, *The Shape of Automation for Men and Management* (New York: Harper & Row, 1965), xi-xiii. Note that Simon's use of the term "economic conservative" does not refer to his position on questions of economic policy, in which he remained a staunch liberal in the mold of the New Deal. His language means that he believed that computers are capable of solving problems with the same degree of sophistication that humans employ, while still being skeptical that these developments would lead to a profound restructuring of social life and workplace patterns.

A consequence of this, according to Simon, was that the continued development of sophisticated computing machines and complex procedures for programmatically solving problems need not result in the sort of dehumanization that critics like Dreyfus feared. Instead, Simon observed that this stage of mechanization could enhance the possibilities of leading an authentically "human" existence. Previous stages of mechanization meant designing the environment in a way that reduced the comparative advantage of humans. For example, the use of automobiles required vast networks of paved roads that were designed for the particular requirements of those machines, while the automated factories of the first half of the century tied workers to the pace of the assembly line. By contrast, the use of heuristic problem solving techniques on computers meant that these machines could be used to help solve unstructured problems, subordinating machines to human concerns. At a conference in 1960 celebrating the tenth anniversary of Carnegie Mellon's GSIA, Simon looked ahead twenty-five years to 1985 and declared "that we will have the technical capability, by 1985, to manage corporations by machine; but that humans, in 1985, will probably be engaged in roughly the same array of occupations as they are now. I find both of these predictions reassuring."[69]

The reduction in the amount of work that would need to be done by humans in an automated industry created concerns among workers that their own jobs might be the ones to be automated away. Memories of the Great Depression were still fresh, as were the arguments that equated labor-saving technology with increased unemployment.[70] Yet economic theorists contended that even if these technologies did increase unemployment, the gains in productivity would be so large that the rewards of automation could be shared with the newly unemployed. There was a widespread sentiment, most clearly expressed by John Kenneth Galbraith but shared among economists and sociologists of all ideological persuasions in the early 1960s, that industrial growth was slowly but surely rendering the notion of scarcity irrelevant. The accepted premises of economics seemed to be anachronisms.[71]

In this new economic dispensation the critical economic factor would be the production and distribution of knowledge, rather than of any material good.[72] The problem was no longer how to produce enough material goods, but rather knowing what goods to produce, how to distribute these goods, and how to innovate. The crucial worker in the new economy would be a member a "New Class."

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[69] Herbert A. Simon, "The Corporation: Will it be Managed by Machines?" in *Management and Corporations,* 17-55, on 52.

[70] For an overview see Amy Sue Bix, *Inventing Ourselves Out of Jobs? America's Debate over Technological Unemployment, 1929-1981* (Baltimore: Johns Hopkins University Press, 2000).

[71] John Kenneth Galbraith, *The Affluent Society* (Boston: Houghton Mifflin, 1998); Howard Brick, *Transcending Capitalism: Visions of a New Society in Modern American Thought* (Ithaca, NY: Cornell University press, 2006); Robert Collins, *More: The Politics of Economic Growth in Postwar America* (New York: Oxford University Press, 2000).

[72] The classic statement is Fritz Machlup, *The Production and Distribution of Knowledge in the United States* (Princeton, NJ: Princeton University Press, 1962). However, Machlup employed a very broad definition of "knowledge" that was broken down in terms of the ends to which knowledge was applied rather than in terms of the nature of the knowledge. It therefore included the value of gossip for socializing, among other things. See Machlup, 21-22. Invocations of a "knowledge economy" used very different, more restrictive, definitions.

Galbraith tried to broaden the world of American liberalism, to inject it with a moral cause that went beyond technocratic tweaks.[73] In his *Affluent Society* of 1958, he suggested that the received lessons of economics derived from an age of scarcity and were not necessarily appropriate for setting economic policy amidst the unprecedented wealth of 1960s America.[74] He attacked the assumption that efficiency in production was the central problem for American industry. With the essential human needs met—such as food, shelter, clothing, and transportation—production was done to satisfy manufactured wants. Indeed, he claimed that the production of new needs was as essential a component of industry as the production of goods to satisfy those new wants. He added a third engine of the modern economy to supplement the manufacture of wants and the manufacture of goods: the extension of credit with which to continually acquire the goods to satisfy these wants. Galbraith concluded by observing that affluence and the satisfaction of basic human needs created an opportunity to develop the public wealth of the nation. The emphasis that the business community placed on increasing the efficiency of production was immaterial to the basic problems of American life in the 1960s.[75] At the same time, the salient distinction within the American workforce was between those who merely worked and those for whom work was a means of personal development. The members of this "New Class" demanded interesting, creative work, and devoted their energy to eliminating drudgery—a noble goal, and one that might allow all workers to find greater satisfaction in their jobs while freeing their creative potentials.[76]

In 1966 Lyndon Johnson's National Commission on Technology, Automation, and Economic Progress issued its report on the role of public policy in addressing unemployment.[77] The commission suggested that the problem of unemployment was caused by productivity growing faster than the economy as a whole, and was therefore a problem of policy, not of technology. Good policies would allocate the dividends of productivity growth in order to encourage economic growth, and to prevent unemployment from being concentrated in certain communities, such as among African-Americans or within isolated rural areas. By looking at unemployment patterns, they found that opportunities for the most skilled white-collar workers were growing, while unskilled laborers and minorities were suffering the most.[78]

To most observers versed in economics, the problem of unemployment due to productivity gains was temporary. Herb Simon chided the more breathless critics of automation:

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[73] Kevin Mattson, "John Kenneth Galbraith: Liberalism and Cultural Critique," in *American Capitalism*, 88-108, on 90.

[74] Richard Parker, *John Kenneth Galbraith: His Life, His Politics, His Economics* (New York: Farrar, Straus and Giroux, 2005), 280.

[75] Galbraith, *The Affluent Society*, 243-254.

[76] Ibid., 249-253.

[77] The full membership of the committee: Howard R. Bowen (Chair); Benjamin Aaron; Joseph A. Beirne; Daniel Bell; Patrick E. Haggerty; Albert J. Hayes; Anna Rosenberg Hoffman; Edwin H. Land; Walter P. Reuther; Robert H. Ryan; Robert M. Solow; Philip Sporn; Thomas J. Watson, Jr.; Whitney M. Young, Jr.

[78] *Technology and the American Economy: Report of the National Commission on Technology, Automation, and Economic Progress* (Washington, DC: US Government Printing Office, 1966).

"Insofar as they are economic problems at all, the world's problems in this generation and the next are problems of scarcity, not of intolerable abundance. The bogeyman of automation consumes worrying capacity that should be saved for real problems—like population, poverty, the bomb, and our own neuroses."[79] Contra Galbraith, scarcity remained a fact of economic life, and there remained more work to be done. However, the question of affluence was directly related to the problem of defining the role of specifically human labor.

The report concluded with a proposal to create a planning board to determine how the productivity generated by modern technology could be harnessed to solve public problems, including unemployment and the poverty endemic to certain regions of the country. The report stood as a powerful expression of the Great Society's faith in rational systems for decision-making. And yet its confidence masked some underlying anxieties. The new information technologies seemed to be having large consequences for the organization of the workplace. The recurring language of a "New Economy" run by a "New Class" indicated that the old assumptions could no longer be accepted uncritically.

An economy that recognized the production of knowledge as its engine required paying greater attention to the universities. Perhaps the most articulate examination of the transformation of the university came from the University of California's Clark Kerr, who published *The Uses of the University* in 1963. Kerr had been active in industrial relations and had co-authored a significant study of pluralism within the industrial system. He was attuned to the multidimensional changes—in social relations, in individual psychology, in the relative power of different economic sectors, etc.—that industrialization had wrought.[80] From *The Uses of the University* onward, Kerr attempted to parse the significance of the "knowledge industry" and of the university's role within it.[81]

The invocation of a "knowledge society" or a "new economy" involved an important transition in what constituted valuable knowledge. Understanding the significance of the production of knowledge meant creating new metrics for measuring knowledge. Fritz Machlup's generous definition of knowledge had been too broad to be truly workable. By restricting the definition to atomistic nuggets of information, its constituent parts could be more easily measured and assessed. Derek Price, for example, conducted extensive studies of scientific literature in order to describe the growth of the profession and the knowledge that it generated.[82]

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[79] Herbert A. Simon, Letter to the Editor, *New York Review of Books*, 5/26/1966.

[80] Clark Kerr, John T. Dunlop, Frederick H. Harbison, and Charles A. Myers, *Industrialism and Industrial Man: The Problems of Labor and Management in Economic Growth* (New York: Oxford University Press, 1964). See also Paddy Riley, "Clark Kerr: From the Industrial to the Knowledge Economy," in *American Capitalism*, 71-87.

[81] Kerr memorably concluded that "what the railroads did for the second half of the last century and the automobile for the first half of this century may be done for the second half of this century by the knowledge industry: that is, to serve as the focal point for national growth. And the university is at the center of the knowledge process." See Clark Kerr, *The Uses of the University* (Cambridge, Mass.: Harvard University Press, 2001), 66. For Kerr's application of his ideas at Berkeley, see Mary Soo and Cathryn Carson, "Managing the Research University: Clark Kerr and the University of California," *Minerva* 42 (2004): 215-236.

[82] See Derek de Solla Price, *Science since Babylon* (New Haven, Conn.: Yale University Press, 1961), and idem., *Little Science, Big Science* (New York: Columbia University Press, 1963).

According to Jean-François Lyotard, writing in the late 1970s, this redefinition involved "a thorough exteriorization of knowledge with respect to the 'knower,' at whatever point he or she may occupy in the knowledge process. The old principle that the acquisition of knowledge is indissociable from the training (*Bildung*) of minds, or even of individuals, is becoming obsolete and will become ever more so."[83]

These atomistic meanings of information drew upon the developments in communication theory from Claude Shannon through the 1956 MIT symposium and the models of learning employed by the computer science community. The competing vision of knowledge and expertise as fundamentally enmeshed within a wider set of cultural practices was not one amenable to industrial-scale production. Would expertise require experience, or could it be a simple accumulation of information?[84] As Kerr lamented in later editions of *The Uses of the University*, what place remained for the humanistic ideal within the modern university?[85]

Technology was at the heart of this new knowledge economy, though futurologists offered conflicting interpretations of what lay ahead. A recurring question, however, concerned the place of ordinary individuals within this new economy. As economists debated the nature of unemployment and its connection to technology, Paul Goodman criticized the expansion of government programs that treated citizens as functionally "useless." These newly useless citizens were told to enjoy their newfound leisure time but were barred from doing any meaningful work because the work for which they were trained was automatically deemed meaningless.[86] Such arguments pointed to larger problems with the liberal framework of social management and technological expertise.

The patterns of authority within the knowledge economy had to be built upon a different foundation than authority within industrial corporations had been. The sociologist Robert Merton suggested that there was systemic distrust concerning corporations—and institutions in general—which reflected "a sense of dissatisfaction on every side with the organizational pressures on men."[87] Adolf Berle denied that the technical developments in management were the primary driver of economic growth following the New Deal; instead, the crucial element had been "the introduction of a measure of social ethics and human morality into the business system."[88] Berle

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[83] Jean-François Lyotard, *The Postmodern Condition: A Report on Knowledge* (Minneapolis: University of Minnesota Press, 1984), 4.

[84] A model of intelligence that various AI projects, such as Douglas Lenat's Cyc, tried to implement. After Newell and Simon's General Problem Solver became passé, Cyc became Dreyfus's target.

[85] Even though Kerr may never have actually likened the modern university to a "knowledge factor," as Mario Savio famously alleged, the protesters recognized an essential kernel of truth: with more claims for the economic importance of knowledge production, the university would face additional pressures to reconstruct its research mission along the lines of industrial efficiency.

[86] Paul Goodman, "The Empty Society," in *The Triple Revolution: Social Problems in Depth*, ed. Robert Perrucci and Marc Pilisuk (Boston: Little, Brown and Company, 1968), 645-659, on 652-653.

[87] Robert K. Merton, "The Corporation: Its Coexistence With Men," in *Management and Corporations*, 57-61, on 60.

[88] Adolf A. Berle, Jr., "The Corporation in a Public Society," in *Management and Corporations*, 63-98, on 68.

further argued that the continued utility of corporations within a democratic society relied upon these corporations working to promote the general ends selected by that society. The development of greater technical sophistication was of secondary importance.

By the end of the decade John Kenneth Galbraith would describe the set of large corporations as forming a "technostructure" that operated according to its own rules and its own logic. The key to understanding the technostructure was that while it putatively worked for the welfare of shareholders and the public at large, it was in reality run by an elite group of managers who marshaled economic arguments to justify their authority. Furthermore, adapting an argument made by the economist Robin Marris that managers would lead the firm to benefit themselves, Galbraith argued that corporations would prioritize stability over rapid growth. They therefore needed to focus on planning in order to control growth and minimize risks.[89] The expansion of these capabilities was made possible through the use of new management information systems.

The form of top-down planning employed by social planners and by corporate managers was directly implicated in the transformations of the workplace. From the perspective of office workers in organizations that were beginning to use computers, these machines represented a new imposition of authority.[90] Unlike traditional forms of authority within the workplace, this one did not rely on personal contact. It was a form of authority in which the human relations were obscured, and the exercise of power occurred indirectly, mediated by machines. The grievances of these office workers therefore matched up with the social critiques of communitarian theorists about the alienating effects of mass society; here the alienation was effected by managerial applications of communication technology. The arguments of office workers to the effect that both technology and management strangled their sense of autonomy and threatened their livelihoods drew directly upon the old line that systematic management undermined both entrepreneurship and personal responsibility. In that earlier moment, the professional responsibilities of management were checks against the whims of owners. In this later moment, office workers felt themselves to be simultaneously empowered and constrained. The appropriation of elite arguments by office workers represented a common attitude that the developments of technocratic management from the early 20th century no longer had the same authority.

The significance of automation for the middle class therefore went far beyond the immediate fear of unemployment, as traumatizing as that was, and tied into a much longer debate about the sources of the authority of the professions. Trust in impersonal rules and procedures, based on divorcing individual expertise from the particular character of the expert, came crashing against the possibility of building this impersonal expert knowledge into machines. Not only were jobs at the lower end of the middle class under threat, the justification for professionalizing

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[89] John Kenneth Galbraith, *The New Industrial State* (Boston: Houghton Mifflin, 1969), 166-178. See also Robin Marris, *The Economic Theory of "Managerial" Capitalism* (New York: The Free Press of Glencoe, 1964).

[90] See Thomas L. Whisler, *The Impact of Computers on Organizations* (New York: Praeger Publishers, 1970), and Shoshana Zuboff, *In the Age of the Smart Machine: The Future of Work and Power* (New York: Basic Books, 1988). While these books were written nearly twenty years apart, many of the basic points concerning the nature of authority in computerized organizations carry over between the two.

the technical functions of middle management was also gradually being eroded. Even if the reality of automation-driven unemployment was never as dire as critics feared, the very possibility of office automation and the formalization of intelligent behavior marked the beginning of the end for the early 20th century reorganization of labor, ownership, and management.

AI as STS

While the communities of economists, sociologists, and political theorists would continue to critique this arrangement, such analyses only went so far. If these changes truly were related to a more fundamental reconfiguration of expertise in terms of cognitive science, information theory, and computer architecture, then effective analysis had to proceed on those terms. The questions asked by STS from the 1960s on directly addressed the substance of the debates concerning AI and automation. Putting STS in dialogue with AI contributes to understanding this broad structural transformation.

The claims made by proponents of the strongest versions of artificial intelligence embroiled these researchers in a lengthy dispute with the growing community of STS scholars. These conflicts began to emerge in the 1960s as the rationalist claims of the cognitive scientists and artificial intelligence researchers confronted the radical epistemological claims of ethnographers and other social theorists. As the next chapter will explain, this was part of a broad-based fracturing of the behavioralist consensus within the social sciences. There was more to this, however. The strongest claims made by artificial intelligence researchers were not just academic arguments to be debated around the seminar table. They cut to the heart of the critical enterprise in academia and addressed basic questions about the changing foundation for authority within organizations engaged in computerization and about the vocabulary of planning at the intersection of technology and economics.

Though the engagement with AI would only pick up in the 1980s, the STS scholars working in those years examined the consequences of the growing information-centrism of the period from the late 1960s through the 1980s.[91] The most important work on this subject was Lucy Suchman's pioneering investigation into the role of plans in creating the complex behavior of interactive machines in *Plans and Situated Actions*, published in 1987. Her work was fully informed by contemporary anthropological research in ethnomethodology. The analysis of man-machine interaction therefore began with a contrast between European styles of navigation and those of Micronesians. In this schematic, the European navigator created a plan at the outset, and then hewed closely to that plan, regardless of atmospheric conditions, ocean currents, or the vicissitudes of the voyage. The Micronesian navigator instead had only a general idea of the destination, and fit his or her actions to the local conditions during the voyage. Of course, these systems of navigation could not be mutually exclusive. A skilled navigator (whatever his or her point of origin) had to take both the local conditions and an overall plan into account. The

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[91] When compared with note 5, above, we might agree with the words of both Marshall McLuhan and Hegel to the effect that while the artist's "antennae pick up these messages before anybody," the scholars of technology write "only with the falling of dusk."

centrality of practice (as opposed to theory), and of non-western practices in particular, characterized this genre of anthropological research.[92]

Suchman's concern was that information technologies were being built on the model of the idealized European navigator without so much as acknowledging the necessity of the Micronesian approach. Computer designers had made the mistake of taking the claims of "European navigators" at face value without noticing the "Micronesian" practices that allowed for actual navigation. According to her, "as projective and retrospective accounts of action, plans are themselves located in the larger context of some ongoing practical activity. And as commonsense notions about the structure of that activity, plans are part of the subject matter to be investigated in a study of purposeful action, not something to be improved on or transformed into axiomatic theories of action."[93] She reversed the hierarchical relationship between plans and actions taking place within immediate situations by insisting that plans are merely resources for coping with complex situations.

The dichotomy between European and Micronesian navigation mirrored an existing distinction within the world of informatics. As mentioned in chapter two, the early social critics of computing divided into those who maintained a more top-down, logic-centric understanding of the role of communication and those who celebrated a more organic form of communication, as the cyberneticians did. By the late 1960s, the argument that computing represented the height of instrumental rationality was difficult to sustain among those actually working with machines. Marvin Minsky had playfully observed that "when a program grows in power by an evolution of partially understood patches and fixes, the programmer begins to lose track of internal details, loses his ability to predict what will happen, begins to hope instead of know, and watches the results as though the program were an individual whose range of behavior is uncertain."[94] His MIT colleague Bob Fano was less optimistic in observing that "as the complexity of computer software keeps increasing, a point will be reached—and we are close to it now—at which no individual will understand in sufficient detail how each particular computer system operates to be able to assess the validity of what it does in a particular situation … we will be in the unfortunate position of having to accept on faith what computers do or suggest to us…"[95]

If practicing computer scientists openly admitted amongst themselves the limits of understanding their programs, this message was often lost in public understandings of computing. Suchman's work has had a lasting influence because it built from the concrete question of how to design intelligent machines that interact with users and expanded the scope of its concerns to identify "the status of planning as itself a form of culturally and historically

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[92] Lucy Suchman, *Human-Machine Reconfigurations: Plans and Situated Actions, 2nd Edition* (New York: Cambridge University Press, 2007).

[93] Ibid., 69.

[94] Marvin Minsky, "Why Programming is a Good Medium for Expressing Poorly Understood and Sloppily Formulated Ideas," in M. Krampden and P. Seeitz, eds., *Design and Planning II* (New York: Hastings House, 1967), quoted in Weizenbaum, *Computer Power and Human Reason*, 235.

[95] R. M. Fano, "On Increasing the Availability and Quality of Knowledge-Based Services," Fano Papers, box 14.

situated activity."[96] At the heart of Suchman's work was the problem of distinguishing behavior that is actually governed by rules as it is being performed from behavior that can retrospectively be described as following rules. This has been a significant approach to the study of science, appearing, for example, in Bruno Latour's invocation of the Roman god Janus to distinguish the messiness of "Science in Action" from formal descriptions of science.[97]

The easy contrast between a mechanical/abstract/Western pattern of action and an organic/situational/non-Western vision obscured the actual work done within AI labs, which rarely remained on such a high level of abstraction. Alison Adam clearly distinguishes the actual work done within AI research from the fanciful claims of building artificial minds. According to Adam, the real work of designing artificial intelligences involves far more bricolage than has typically been appreciated.

Even so, Adam's critique of the gendered assumptions underpinning artificial intelligence attributes too much a priori coherence to the project. She claims that the emphasis on mathematics and logic "was the *natural* choice of workers in the field; an example of what is taken to be the highest form of reasoning, something that people find highly abstract and difficult, a masculine standard drawn from their own lives, which was then to form the subject matter of the first significant AI program."[98] They tried to create the ideal "Man of Reason."[99] While it is true that there was some natural gravitation to these problems, AI researchers also argued that mathematics included the type of stripped-down problem that was most amenable to this form of asocial intelligence.

Noting the ambiguities in how AI researchers accepted the hyper-rationality of mathematics is hardly to deny the role of gender in structuring AI. Instead it raises further problems about the connection between the alleged asociality of mathematics and the masculine ideals of universal reason. This tendency to "delete the social" (to use Susan Leigh Star's term) pervades the foundations of artificial intelligence. The problem for understanding the limitations of AI is to recognize the boundary of the small category of problems for which formal descriptions actually define behavior and not to extrapolate from these successes to a general claim about the asociality of reason.

The lesson of anthropological studies such as Suchman's is that science as it is actually performed looks quite different than the ideal science as scientists describe it. Diana Forsythe, an anthropologist and the daughter of George and Alexandra Forsythe, the founders of Stanford's program in computer science, extended these field studies of the laboratory by noting how the professional identity of AI scholars is formed.

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[96] Suchman, 187.

[97] See Bruno Latour, *Science in Action: How to Follow Scientists and Engineers Through Society* (Cambridge, Mass.: Harvard University Press, 1987).

[98] Alison Adam, *Artificial Knowing: Gender and the Thinking Machine* (New York: Routledge, 1998), 36-37.

[99] Alison Adam, "Constructions of Gender in the History of Artificial Intelligence," *IEEE Annals in the History of Computing* 18 (1996): 47-53.

Forsythe focused her attention on the way that professional self-identity circumscribed the problems that AI researchers understood as being part of their work. Particularly interesting to Forsythe were the different approaches that AI designers and anthropologists took to the seemingly shared problem of acquiring information from informants. AI researchers needed to determine what heuristics their expert informants used but believed that the process of interviewing was not a scientifically interesting one. The "knowledge acquisition bottleneck" was one whose existence AI researchers lamented, but they placed the blame for it squarely on the incapacity of human informants to explicitly communicate the full content of their knowledge. To the anthropologist, this approach to interviewing demonstrated a fundamental misunderstanding of the nature of knowledge and the limits of formal articulation.[100] Forsythe's work provided a clear example of how the AI framework for knowledge acquisition ran up against the realities of studying human subjects. Even more fundamentally, the particular form of knowledge that AI researchers employed limited their work to those systems within a very narrow range of topics. The result was the creation of systems with necessarily partial knowledge bases, but which failed to acknowledge their partiality.

Even as critics of AI argued that computers faced fundamental limitations as intelligent beings due to their unsociability, their literal-mindedness, and their lack of physical embodiment, the real successes of computers in certain applications still needed to be explained. Harry Collins asked how such machines could demonstrate any seemingly intelligent behavior at all, given their shortcomings. By building upon Suchman's arguments concerning plans and actions, Collins identified a particular class of problems for which this distinction receded into the background, which he termed "behavior-specific acts," or "machine-like acts," which are "acts that humans always try to instantiate with the same behavior." The classic example is the set of actions amenable to Taylorism.[101] According to Collins, the importance given to plans by cognitive scientists, AI researchers, and others was due to the fact that complex behaviors could be described retrospectively through programs, one of Dreyfus's earlier criticisms. The failures of computers to mimic human intelligence in most fields of behavior were due to the fundamental problem of failing to distinguish activities that can be retrospectively described by plans from those that are actually implemented by executing a fully specified plan.

Significant as these analyses were, actual engagements between computer scientists and their critics have been less than successful. In 1989 the cognitive scientist Peter Slezak provocatively claimed that the successes of artificial intelligence in identifying scientific laws from data sets decisively refuted the "Strong Program" of the sociology of scientific

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[100] Diana E. Forsythe, "Engineering Knowledge: The Construction of Knowledge in Artificial Intelligence," in *Studying Those Who Study Us: An Anthropologist in the World of Artificial Intelligence*, ed. David J. Hess (Stanford, Calif.: Stanford University Press, 2001), 35-58, on 41-42. Also see Forsythe, "Artificial Intelligence Invents Itself: Collective Identity and Boundary Maintenance in an Emergent Scientific Discipline," in *Studying Those Who Study Us*, 75-92.

[101] H. M. Collins, *Artificial Experts: Social Knowledge and Intelligent Machines* (Cambridge, Mass.: MIT Press, 1990), 33. Collins invoked Taylorism differently than Miller, Galanter, and Pribram had done. For Miller et al., Taylorism represented an earlier, pre-computer concept of planned behavior. The very significance of the computer was in expanding the domain of formalized behavior beyond strict Taylorism. The easy invocation of the easily demonized Taylor has long ceased to refer to specific practices, but instead signifies a general form of rule-making.

knowledge.[102] According to Slezak, the fact that computer programs could infer valid scientific laws was an indication that the process of generating these laws could be fully specified and was not causally dependent upon the socialization of their discoverers. Published in the journal *Social Studies of Science*, Slezak's article immediately drew attacks from throughout the world of STS. The debate between Slezak and the defenders of SSK reached no closure. While he attacked a straw man and failed to engage with the substance of contemporary science studies, his interlocutors similarly failed to give an adequate account of the successes of the AI programs described in Slezak's article.[103] But this debate was never really about the implications of technical developments in computer science. The question of whether machines think is both obviously important and, due to its abstraction, infinitely flexible depending on how one defines the terms of the debate. Positions had been staked out well in advance.

Taken together, Slezak and Collins addressed the question of how computerized expertise works. Collins explained how certain problems fit within the scope of computerized expertise, while Slezak provided an argument concerning one particular implementation of this capability. As with the earlier vitriol between Simon and Dreyfus, the animus between Slezak and his critics prevented a thorough parsing of how expertise and autonomy function in the case of computerized theorem production. But this has less to do with the problem at issue and more to do with the uncertain position of STS within the academy—a precariousness exposed again by the Sokal Hoax of the 1990s. This defensiveness is unfortunate, for it reveals one missed opportunity after another for engaging in substantive interdisciplinary analysis.

The critiques of AI provide important examinations of how rational plans are constructed and what assumptions are folded into them. These are important efforts at making the work of formal systems understandable in political terms. Such systems implement particular forms of reason that may or may not seem reasonable to other interested parties. Their approaches to solving problems do not always take into account relevant contextual information. The more self-contained these systems are, the more their architecture needs to be teased apart. The crucial fault line in the politics of rule-governed behavior is essentially about defining limits to authority. The domain marked by the successes of explicit, formal rules is the domain in which absolute control remained possible. The ability to define what counted as legitimate knowledge was an important form of power. The next chapter explores this problem in the context of social science research in the late 1960s and early 1970s.

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[102] Peter Slezak, "Discovery by Computer as Empirical Refutation of the Strong Programme," *Social Studies of Science* 19 (1989): 563-600.

[103] Peter Slezak, "Computers, Contents and Causes: Replies to my Respondents," *Social Studies of Science* 19 (1989): 671-695. For a similar argument concerning the role of "the social" in artificial intelligence, see James Fleck, "Knowing Engineers? A Response to Forsythe," in *Studying Those Who Study Us*, 59-65, and Forsythe, "STS (Re)constructs Anthropology: Reply to Fleck," in *Studying Those Who Study Us*, 66-74.

Part III

The middle section of this dissertation described the developments of time-shared computer systems and the problems inherent in automating intelligence within the context of MIT's Project MAC. This third and final section returns to the problems that motivated the first chapter by studying the applications of computers for the continued development of the social sciences and the application of social management. The state of social science in the mid-1960s was characterized by the turn towards quantification and toward positive as opposed to normative research. Developing better computational tools for the analysis of data was a major component of creating a rigorous foundation for these sciences.

However, this project was entangled with the politics of science patronage in the Vietnam era, caught between a conventionally liberal project of developing social policy and more radical opposition to that view. Recent historiography has emphasized the significance of these critics for the creation of contemporary information technology, a theme that is addressed in the dissertation's conclusion. The analysis of social science computing in chapter 5, however, identifies elements of these later developments in the work of the ARPA-funded Cambridge Project.

This suggests that the state of large-scale computing research was more varied than its critics maintained. Rather than maintaining a contrast between the technocratic architects of large computer systems and the merry pranksters who subverted them, this section instead identifies these tensions as running through the heart of the projects themselves. It therefore continues tracing the many influences on computing identified in the first section.

Chapter 5: Calculating Society, Computing Community

The early development of computers had been structured around ideas of information and organization that reflected a view of social order that was specific to the world of mid-twentieth century America. Theories of social organization and ideas about the importance of manual and mental labor both contributed to the social meanings of computing. Yet the political and social upheavals of the late 1960s and 1970s suggested that this arrangement was coming to an end. The social significance of computing would need to be reinterpreted, and developments in computing contributed to the creation of a post-liberal and post-industrial order.

The cultural significance of computing was felt on several fronts. The most immediate one was on the level of access to computing, where the dream of building large computer systems to democratize computer access foundered on the reality that access to machines would remain differentiated by traditional forms of power. The creation of centralized information systems with varied levels of access created an obvious source of conflict. Yet there were also deeper problems, including what one did with this information and what purposes it served.

Postwar behavioral scientists became interested in computerized databases as part of a project to improve the effectiveness of administration. They were interested in using information about American society to generate predictive power. Critics of these developments resented the expansion of this scientific project, which seemed to transform the American public from active architects of their own destinies into a mass society to be carved up and analyzed. The public perceptions of large computing projects were strongly influenced by the arguments of both the New Left and the counterculture during the late 1960s.

The convergence of these intellectual, technological, and political developments suggested a broader societal transformation. By the middle of the 1970s, the social and political nexus that described the fundamental approach to organization had shifted to a substantially different foundation. The search for a scientific basis for organization, relying on trust in professionals and on top-down models of rationality, was increasingly being challenged from both the right and the left. A growing distrust in institutions during these years dovetailed with a growing interest in protecting individual privacy. The developments of computerized databases and tools for data analysis were at the heart of these controversies.

This history focuses on the reaction against social management. This does not represent the introduction of a new topic, however. It instead marks a return to the larger questions that motivated the early creation of computing technologies that were raised in the first chapter. From these larger questions, the actors' decisions to focus on the industrial management marked an understanding that such topics were simply easier to address. But the broader social concerns never fully receded. The development of more powerful computational tools awakened the possibility of addressing these broader issues, and the same set of computer pioneers who had used industrial management to push the boundaries of computation tried to do the same by using the social sciences.

This had further ramifications for the use of computers in the private sector, which was forced to re-emphasize the distinction between "data" and "information"—acknowledging that the crunching of numbers was, by itself, inadequate to answer the important questions facing corporate management.[1] The quantitative and formalist turn in management was not repudiated, but was rather recontextualized. Scholars of management emphasized that the use of information technologies and of managerial tools was but a small component of the task of the executive.

This chapter traces the history of the Cambridge Project, an effort to create a new way of doing social scientific research through the use of electronic databases and shared computer programs. It begins with the intellectual and institutional origins of the Project in the world of Cold War science funding. The success of this system rested on a very specific understanding of the relationship between social scientific knowledge and the management of organizations. However, this project therefore quickly became bound up in the wider cultural battles of the late 1960s and 1970s concerning the role of the military (which remained a major sponsor of computing research) and the rights of individuals within larger systems. Such conflicts not only reflected the particular conditions of doing technological research circa 1970, but also suggested a much more significant shift in the foundations of legitimacy for managing large-scale systems.

While in later years its critics would embrace the potential of communication technologies to enhance communities, rather than as top-down machines for social management, the great irony of the Cambridge Project was that it involved an early attempt to create this communal foundation within a computer system. As computers stopped being machines to solve narrowly technical problems (if that is indeed what they had been) and became more ubiquitous engines of data processing and symbols of authority, the traditional topics of political economy began to be applied within the new social spaces on computer systems.

The Legacy of Behavioralism for Theories of Social Order

The notion that computers could revolutionize the study of social phenomena was commonplace within academic circles by the late 1960s, as the previous chapters have shown. In particular, social scientists envisioned new methods of large-scale data analysis that could, finally, generate a truly scientific understanding of society. Just as the "New Look" management schools of the 1950s seized upon developments in numerical methods and computer simulations to provide a rigorous foundation for their new discipline, so too would the behavioral social scientists of the late 1960s.

The empirical social sciences described in the first chapter had grown from a vanguard movement to being the heart of the social sciences of the 1960s and beyond. The behavioralist strand of the social sciences, most prominent among political scientists, maintained that individuals acted as "information processors"—meaning simply that they took in information from the world around them, used their mental capabilities to process this information, and then acted. This theory blackboxed the actual work of this information processing, leaving the understanding of cognition to the psychologists (particularly the burgeoning specialty of cognitive science, which itself grew in dialogue with computer science). Its adherents maintained a positivistic orientation toward understanding the connections between environmental

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[1] A basic problem, however, was the looseness in terminology. The terms "data processing" and "information processing" could be used interchangeably, even as the distinction between "data" and "information" became increasingly important.

conditions and political behavior. This was intended as a revolt of scientific Young Turks against a more philosophical tradition of political and social analysis, and focused on asking the questions of "what is" rather than "what ought to be." For all of the novelty of this approach, the turn to a more "scientific" analysis of politics and society had deep roots in American culture, as Dorothy Ross has explained in *The Origins of American Social Science*.[2]

Of course, the scientific understanding of politics was never as divorced from normative concerns as its advocates may have suggested. The intellectual impulse to provide a rigorous foundation for political science was matched by a desire for application—to understand behavior in order to shape it. The behavioralist approach to political science was based, to at least some degree, on the desire to control.[3]

This strand of social science appealed most strongly to those scholars who were already committed to a social vision that built upon Progressive traditions of professionalization and expertise, a New Deal regulatory framework, and the optimistic belief of the Great Society that well-crafted social policy could alleviate social ills. They believed that a scientific understanding of social behavior could make politics less divisive and more technocratic by identifying practical ways of achieving shared goals rather than endlessly arguing about fundamentally irreconcilable values. The fixation on means rather than ends implied the existence of a strong state apparatus for enacting these policies, as well as the political will to do so. The research agenda of behavioralism included pioneering new methods both for collecting data on social trends, and for generating social statistics.

Behavioralism was a compelling intellectual project, and it became one of the predominant forms of social science practiced in the academy during the late 1950s and '60s. But the ambitions of its practitioners outstripped their technical capabilities. Data collected for one study could not easily be compared with data collected for a different study, or by a different scholar. While surveys and questionnaires generated data in standardized categories, scientists still could not ignore the ambiguities inherent in the use of natural language. Many fundamental methodological problems remained.

Meanwhile, the conversation between empiricists and theorists became more strained. American political theory was being strongly influenced by the anti-liberal views of German émigrés such as Leo Strauss. As behavioralism grew into the dominant, confident core of the

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[2] Dorothy Ross, *The Origins of American Social Science* (New York: Cambridge University Press, 1991). The charges of scientism and of positivism obscure certain differences of opinion among the scholars, however. In a letter expressing his ambivalence about Ross's work, Gabriel Almond lamented to Herbert Simon that their version of empirical social science was being squeezed both by critical theoretical works and by heavily quantitative rational choice theories, described below. The differences among "scientific" approaches often were as significant as those between "scientific" and "hermeneutic" ones. See Gabriel Almond letter to Herbert Simon, 4/26/1993, Herbert A. Simon Papers, Carnegie Mellon University Archives, box 90.

[3] On the connection between politics and political theory, see John Gunnell, *The Descent of Political Theory: The Genealogy of an American Vocation* (Chicago: University of Chicago Press, 1993), 82-104. Also see Hunter Crowther-Heyck, *Herbert A. Simon: The Bounds of Reason in Modern America* (Baltimore: Johns Hopkins University Press, 2005), 31-59.

discipline in America, theory was pushed to the margins.[4] These theoreticians challenged the optimism of the behavioralists and drew attention to the *ir*rational elements of political life.

The empirical orientation of behavioralism was not the only way to make social theory more "scientific." An approach that was particularly influential among neoclassical economists focused on constructing mathematical models without reference to the reality or unreality of the assumptions that supported it.[5] A similar approach to political theory developed in the 1960s, centered on William H. Riker at the University of Rochester. Riker's program of positive political science attempted to create formal theories from which hypotheses could be generated and tested. While Riker built the program at Rochester into a formidable and innovative department, it remained on the periphery of the discipline until the 1970s.[6] A characteristic of this political science was its detachment from active political questions. This academic detachment was a significant shift from the reformist impulses that motivated earlier political scientists. And yet, while less motivated by explicitly political agendas, the work of these intellectuals reinforced the ideological foundations of a particularly American form of liberal, capitalist society through its emphasis on individual choice, the operation of markets, and the appeal to individual self-interest with little explicit regard for a discernable "public interest." While the scholarship in rational choice theory was generally hostile to normative speculation, it seemed fully compatible with the work that almost singlehandedly revived American political theory, John Rawls's *Theory of Justice*. Rawls himself seemed supportive of this connection.[7]

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[4] Gunnell, *The Descent of Political Theory*, 199-278. Two volumes reflect the hostility between the camps: Austin Ramney, ed., *Essays on the Behavioral Study of Politics* (Urbana: University of Illinois Press, 1962), representing the behavioralist side, and Herbert J. Storing, ed., *Essays on the Scientific Study of Politics* (New York: Holt, Rinehard and Winston, 1962), representing the Straussian side.

[5] The classic statement remains Milton Friedman, *Essays on Positive Economics* (Chicago: University of Chicago Press, 1953). Deirdre McClosky, recalling her time at Chicago from 1968-1980, observed how "the brilliance of the actual scientific talk in seminar and lunchroom by my fellow Chicago economists—Chicago in the 1970s was the most creative department of economics in the world—contrasted strangely with the simpleton's science recommended by the methodology." From Deirdre McCloskey, *The Rhetoric of Economics* (Madison, Wisc.: University of Wisconsin Press, 1985), xi. She further described how political scientists "have made themselves into departments of third-rate economists," on McCloskey, 190.

[6] S. M. Amadae and Bruce Bueno de Mesquita, "The Rochester School: The Origins of Positive Political Theory," *Annual Review of Political Science* 2 (1999): 269-295. This would ultimately become the default form of political science by the end of the 20th century. Also note that the article concludes with a defense of these developments from the common charge of becoming "economics lite." While Amadae and Bueno de Mesquita convincingly argue that rational choice theory was only fully articulated by political scientists, they also concede the importance of the Chicago school economists on the work done at Rochester. See Amadae and Bueno de Mesquita, 289-291. For an early critique of the orientation toward economics from within the political science profession, see Theodore Lowi, "The State in Political Science: How We Become What We Study," *American Political Science Review* 86 (1992): 1-7, which singles out Herbert Simon as an instigator of this trend. For Simon's defense, see "Lowi and Simon on Political Science, Public Administration, Rationality and Public Choice," *Journal of Public Administration Research and Theory* 2 (1992): 105-112.

[7] Rogers M. Smith, "Still Blowing in the Wind: The American Quest for a Democratic, Scientific Political Science," *Daedalus* 126 (1997): 253-287, on 261-266.

Even as it disavowed explicit politics, positive political science gave a powerful ideological defense of American political economy against the Soviet alternative.[8]

The state of political science in the 1960s was one of diverse methodological approaches, with a renewed interest in rigorously understanding the discipline's foundations. According to MIT political scientist Ithiel de Sola Pool, the next stage of development in the social sciences would involve bringing logical rigor to what had traditionally been loose arguments. For Pool, the computer was a vital factor for determining what social science should become. He cited his MIT colleague Joe Weizenbaum, who had said that "there will come a time when no one will take a social science theory seriously if it cannot be programmed for a computer. This implies a formalization of social science language based on an understanding of what it is social scientists are now saying in their essayistic formulations."[9] Pool observed that the recent literature in social science methodology included several important attempts to formalize the discipline and create a more cohesive sense of community. These were often influenced by the recent meta-scientific literature, including Thomas Kuhn's *Structure of Scientific Revolutions*.[10]

Behavioralism remained the dominant approach to political science at the end of the 1960s, and it continued to have intellectual vitality. However, according to its practitioners, the further development of behavioralism would require both greater rigor and more refined analytical power. They believed that both could be found through the use of computers. The behavioral science community was already closely attached to some pioneering work in computing. Ideas from information theory and cybernetics had already influenced its core concepts. However, the attempt to build a shared computational platform for the behavioral social sciences would face concerns not only about the nature of the social sciences, but also about the political ramifications of this knowledge.

Computers for Social Scientists

The diverse interests and backgrounds of early computing pioneers had suggested many different directions in which the machines could be taken. The gap between the capabilities of existing machines and the power claimed for them was enough to make many of these ambitious goals seem closer to the realm of science fiction than to actual science. Yet by the mid-1960s, a group of social scientists with an interest in technology decided that it was time to implement their vision. The fate of their proposal reveals the tensions within the attempt to unify research

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[8] See S. M. Amadae, *Rationalizing Capitalist Democracy: The Cold War Origins of Rational Choice Liberalism* (Chicago: University of Chicago Press, 2003).

[9] Ithiel de Sola Pool, "Interactive Modeling of Social Science Theory: The General Implicator," Cambridge Project Records, MIT Archives and Special Collections, AC 285, box 2.

[10] For example, see Llewellyn Gross, *Sociological Theory: Inquiries & Paradigms* (New York: Harper and Row, 1967). The theme was also taken up by David B. Truman and Gabriel A. Almond, presidents of the American Political Science Association. See David B. Truman, "Disillusion and Regeneration," *American Political Science Review* 59 (1965): 865-873; and Gabriel A. Almond, "Political Theory and Political Science," *American Political Science Review* 60 (1966): 869-879. In addition to Kuhn, other notable influences include Don K. Price, Derek de Solla Price, Michael Polanyi, and Robert K. Merton.

into the computer as an object of study in its own right with research into the computer as a tool for use.

The Harvard psychologist George Miller and Douwe Yntema, director of the psychology division at MIT's Lincoln Laboratory, had applied for funding from the National Institutes of Health in 1966 in order to find applications for computers in the behavioral sciences. It was a relatively open-ended proposal, driven more by the promise of the technology than by any specific applications. The NIH supported the idea of the project, though it did not supply any funding because the proposal was too expensive and unfocused. The next year, after Miller moved to Rockefeller University, the proposal was taken to the National Science Foundation by Richard Herrnstein from the Harvard psychology department and E. L. Pattullo, director of Harvard's Center for Behavioral Sciences, though they too were unsuccessful. Independently, Ithiel de Sola Pool, of the MIT Center for International Studies, and J. C. R. Licklider, who had just returned to Project MAC from stints at both ARPA and IBM, made an ambitious proposal to ARPA in December 1968 to develop a unified system for data analysis in the social sciences. ARPA encouraged the two teams to join forces, offering them $7.7 million over five years.[11]

J. C. R. Licklider, who had previously run ARPA's behavioral science division as well as its information technology division, was a powerful advocate on behalf of this project. He compared the significance of the computer for social scientists to that of the microscope for biologists, opening up the possibilities of the discipline far beyond what anyone could previously have imagined. Mixing his metaphors, Licklider believed that these new computing technologies would create a "quantum leap" in the behavioral sciences.[12] The behavioral sciences needed better systems for collecting, organizing, and analyzing data, and computers fit the bill. Behavioral scientists could study the properties of individuals or of small groups, but had no easy way to aggregate this to measure social behavior or generate universal laws. It was as if physicists studying thermodynamics could only measure the kinetic energy of individual particles, one at a time, rather than having a simple thermometer. These scientists noted that quantitative social science data were qualitatively different than data in the physical sciences: "They are records of single, highly probabilistic events – a pigeon's peck or a voter's response. Aggregation over a large sample is demanded; still, the identity of the individual items has to be preserved (and so, different from the stochastic processes of the physical sciences)." The character of social science theory was also different, and teasing out the implications of these theories required the raw power of computer models. The end result would be a transformed understanding of social scientific theories, and a greater applicability of these theories to problems of social interest.[13]

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[11] "Proposal for Establishment and Operation of a Program in Computer Analysis and Modeling in the Behavioral Sciences," 11/24/1968, Cambridge Project Records, box 6; "Appendices to the Report of the Subcommittee of the Committee on Research Policy on the Participation of the Faculty of Arts and Sciences in the Cambridge Project," 12/1969, Cambridge Project Records, box 8.

[12] "Appendices to the Report of the Subcommittee of the Committee on Research Policy on the Participation of the Faculty of Arts and Sciences in the Cambridge Project," 12/1969, Cambridge Project Records, box 8.

[13] "1972 NSF," Cambridge Project Records, box 6. Similar language, though less explicit, also existed in the earlier proposals.

This notion that the computer could fundamentally transform the social sciences was common within the circle of empirical social scientists. According to them, the scientific applications of computers for physics and mathematics obscured the fundamental importance of the machine as a tool for social science. The computer was ideal for several reasons. First, the same mathematical capabilities that made it so powerful for the physical sciences could be used to analyze social statistics. Yet this most straightforward of reasons only scratched the surface. Particularly among psychologists and cognitive scientists, the computer's functions as an information processor made it a useful model for understanding the processes of individual human cognition.[14] The continual hope of natural language processing further suggested that the machine could take the fuzziness and ambiguity of natural language (the significance of which even the most committed quantifiers conceded) and parse it with the unerring precision of cold, mechanical reason. The different capabilities could be harnessed together in order to take data collected by different researchers for different purposes and make them universally accessible and readable for scholars working on their own particular projects.[15]

In early January 1969 a group of computer scientists from Project MAC joined social scientists, primarily from MIT's Center for International Studies and from Harvard's Departments of Government, of Psychology, and of Social Relations to inaugurate the ARPA-funded Project for Computer Analysis and Modeling in the Social Sciences, or Project CAM. Though it would take several months to work out the actual organization of the project, the temporary governing committee included Aaron Fleisher of the Harvard/MIT Joint Center for Urban Studies, Myer M. Kessler, Associate Director of MIT Libraries, J. L. McKenney of the Harvard Business School, Philip J. Stone of the Harvard Government Department, with Douwe Yntema of Lincoln Laboratory serving as its chair.[16] Recognizing that many had trouble distinguishing Project CAM from its parent program, Project MAC, its leaders soon changed its name to the Cambridge Project in honor of its inter-university cooperation.[17]

The organizing committee also recommended having student members on the governing board of the Project. The committee recognized that this move would be politically important and would give the board a valuable window into campus opinion during a period of student uprisings. Yet there was an even more practical consideration; few of the "grown-up" members of the Project had extensive experience in working with computers. If the board wanted to have researchers who were deeply familiar with the machines and their uses, they would need to bring students on board.[18]

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[14] On this point, see chapters 2 and 4.

[15] "Proposal for Establishment and Operation of a Program in Computer Analysis and Modeling," 12/1968, J. C. R. Licklider Papers, MIT Archives and Special Collections, MC 499, box 9.

[16] Memo from the Ad Hoc Subcommittee on Organization and Policy of a Governing Board to Frederick Mosteller, 5/26/1969, Cambridge Project Records, box 1.

[17] On the influences of the social sciences and management on Project MAC, see chapter 3.

[18] Memo from the Ad Hoc Subcommittee on Organization and Policy of a Governing Board to Frederick Mosteller, 5/26/1969, Cambridge Project Records, box 1.

The Project's original mission statement included seven primary goals: first, the creation of new tools to handle data files; second, the creation of statistical tools, particularly for time-series analyses; third, techniques for understanding social networks (they observed that "a variety of important social phenomena can be described in terms of networks… Techniques for handling linkages in large networks are beginning to come into being but are still in a primitive state."); fourth, tools for "inferring causal relations from patterns of correlation in data;" fifth, systems for automatically extracting information from natural language; sixth, data reduction tools; and finally, general tools for mathematical modeling of social phenomena and for simulations.[19] Several of these goals were exceedingly ambitious, but collectively they would allow social scientists to better understand their usual forms of data (including survey results, census forms, etc.) and to analyze new forms of data.

The overarching goal was to create an integrated system that would allow social scientists to take advantage of all of these different computational tools. Individual social scientists had developed tools for manipulating large data sets and performing new statistical analyses, but the Cambridge Project was designed to bring order to these disparate efforts. Furthermore, given the falling costs of computing, the Project hoped to make these techniques available to a larger community of social scientists, including those who lacked institutional access to computers.[20]

The Project focused on funding research into new computational methods though individual scholars were also encouraged to test the system using their own research. In practice, given that these scholars were interested in social questions, their research tended to be data-heavy social science projects for which they could plausibly claim to be pushing the boundaries of traditional quantitative social science, but which were primarily about studying particular social topics.[21] The need to create computational tools of general interest while allowing these scholars to pursue their own particular research agendas was a dilemma that was never satisfactorily resolved.

From the beginning the Project stood at the uneasy convergence of two related goals. One, emphasized by Licklider, the psychologists, and most of the Project MAC team, dealt with the design of the "Consistent System," a layer between the user and the operating system, or even between the user and specific programs, which would translate the intuitive commands of a user into the specific commands of the program in question. Time-sharing (Project MAC's bread-and-butter) was an important innovation, but simply expanding access to computers was not enough. Computers needed to be easy to use for those without technical backgrounds, including, but not limited to, the social scientists on the Cambridge Project.

Those approaching the Project from the perspective of social scientific methodology, based at MIT's Center for International Studies, Harvard's Department of Government, and the Harvard Center for Behavioral Science were more interested in using the computer as a logical machine to parse natural language and to understand the implications of social science theories.

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[19] "Proposal for Establishment and Operation of a Program in Computer Analysis and Modeling," 12/1968, Licklider Papers, box 9.

[20] MIT Institute Report, 10/31/1969, Cambridge Project Records, box 6.

[21] Request to the Cambridge Project from Philip Stone et al., Cambridge Project Records, box 5.

For example, Stuart McIntosh and David Griffel of the CIS in 1968 described a scientific model as being "a deduction machine that takes empirical measures as input and deduces, usually over time, the consequences of the model applied to the empirical inputs." From this, they wanted to create a "Computer-Based Information System" that could translate messy data—not necessarily formatted for the model in question—into usable inputs for any scientific model.[22] This information system would contain shared programs and shared data in order to foster collaborations among those scientists who focused on conducting surveys and gathering data, those who developed the computational tools for data analysis, and those who used the computational tools to analyze the shared data sets.

These two agendas overlapped in many ways. They both focused on creating more intuitive or natural methods for working with computer programs and manipulating data, and both stressed the need for consistency. However, they differed in the implementation. The basic question of whether this was primarily about computer systems or about social science methodology was never satisfactorily addressed. The ambivalence between these two approaches shaped the work done on the Project. The work of actually creating unified databases was an application for down the line, an eventual payoff that still lay in the distant future. Yet it was this aspect of the Project that immediately drew the attention of student protesters, who took at face value the Project's claims of creating a unified information system for behavioral science. The Cambridge Project fit into an already troubled relationship between military patrons and university students.

Protests and the Organization of the Cambridge Project

While the counterculture advocated a revolution in values and a dismissal of the instrumentalized rationality of science, the greatest immediate opposition to the Project came from antiwar activists who connected technocratic rationality to the continuation of the war in Vietnam. But opposition to the Cambridge Project was not only about the source of its funding; the protesters recognized that the scientific content of the Project depended upon the organization of the research, which in turn reflected basic ideas about the purposes of such research.

The most striking example of anti-military opposition on a university campus was the protest at MIT held on March 4, 1969. MIT was the third-largest recipient of Defense Department contracts in the country, and the immediate concern of the protesters was ending military patronage at the Institute. Students charged that university scientists were studying narrow technical problems without pausing to consider the social ramifications of their work and the protests succeeded in opening up discussions of the social role of science, even if they failed to end MIT's ties to the military.[23]

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[22] Stuart D. McIntosh and David M. Griffel, "The Requirements for a Computer-Based Information System (CBIS)," 10/30/1968, Cambridge Project Records, box 3.

[23] Bryce Nelson, "MIT's March 4: Scientists Discuss Renouncing Military Research," *Science* 163 (March, 1969): 1175–1178. Also see Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993), 233-241.

The speakers at the March 4 protests pushed for engagement with larger questions beyond the immediate funding regime on campus. Among a roster of distinguished speakers from the left—including Noam Chomsky, Victor Weisskopf, and Howard Zinn—George Wald’s speech received the most energetic response. Wald, a Harvard professor of biology and a Nobel laureate, proposed to diagnose the origins of the revolutionary spirit among the student generation. He drew the obvious connections between student protests and the continuing war in Vietnam, and also to the many social problems facing the world that were not being studied due to the emphasis on military matters. Yet these were mere symptoms of larger problems facing America and the world at the end of the 1960s, in particular the omnipresent threat of nuclear weapons and continued competition between the two Cold War superpowers. In the face of such threats, concerns about industrial efficiency, educational opportunities, and other social goods were understandably secondary.[24]

The suspicions of the protesters were based upon a significantly different understanding of the social sciences than that of the older generation of social scientists. For these protesters, scholarly studies of social movements masked an explicitly political agenda. They saw that the collection of data created a form of power and that the “deduction machine” of social scientific theory was another such form of power. Access to the results of these studies would likewise be structured by existing power relations, as would the ability to act upon them. Insofar as behavioral political science remained an imperfect tool of social control, having a poor track record of predicting the behavior of unpredictable people, it could be tolerated. Yet the claims made by the Cambridge Project scientists that computerized information processing would create a vastly expanded system of control worried activists. The Project made an easy target, given that it was funded almost entirely by ARPA and that many scholars in the Project were based at the CIA-funded Center for International Studies at MIT, where some scholars affiliated with the Project (such as Ithiel Pool) were studying Vietnamese society and communist movements.

According to Pool, claims that the Project was politically motivated were completely off base; on the contrary, he had claimed that “[computers] will change politics itself less than almost any other field because that which it changes is removed thereby from politics.”[25] Any aspects of political science that could be reduced to a form that could then be processed by machines would therefore be subject to rational administration rather than politics. For Pool this was a positive development because administrative problems had definite scientific answers and administrative processes could be managed without recourse to political conflict. The aspects of political science that could not be manipulated in such a manner would be the irreducible essence of political decisions.[26] This was troubling to the student activists who denied that the practice of

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[24] George Wald, “A Generation in Search of a Future,” in Jonathan Allen, ed., *March 4: Scientists, Students, and Society* (Cambridge, Mass.: MIT Press, 1970), 106–115, on 114.

[25] AAAS Commission on the Year 2000: Social Implications of the Computer, 11/22/1968, Robert M. Fano Papers, MIT Archives and Special Collections, MC 413, box 7.

[26] Jürgen Habermas, writing at the same time, succinctly stated the consequence of this development: “Only now has the problem-complex of political decisions been reduced to a core that simply cannot be rationalized any further. Calculation by decision procedures, when carried to extremes, reduces the decision itself to its pure form, purging it of every element that could be made accessible in any way to cogent analysis.” From “The Scientization of Politics and Public Opinion,” in *Toward a Rational Society: Student Protest, Science, and Politics* (Boston: Beacon Press,

rational administration could ever be wholly dissociated from the value systems in which it was embedded.

The opposition to these forms of technocratic administration tied into the radical movements that cut across traditional political distinctions. According to Theodore Roszak, an early analyst of the movement, "[the experts'] stance is that of men who have risen above ideology—and so they have, insofar as the traditional ideologies are concerned. They are simply … the experts. They talk of facts and probabilities and practical solutions. Their politics *is* the technocracy: the relentless quest for efficiency, for order, for ever more extensive rational control."[27] The politics of military patronage was only part of what was at stake in these arguments; the scientists and the students disagreed about the natures of politics and science themselves.

The experience of the protests at MIT on March 4, 1969 led to strict student scrutiny of the Cambridge Project at both MIT and Harvard. A group of MIT students produced a pamphlet entitled "Project CAM Exposed" that accused the Project of facilitating government repression of left-wing movements, both at home and abroad. The crux of the matter was that they believed it would "lead to a further emphasis on computerized people-manipulation. At the same time it [would] increase the blatant prostitution of social science for the aims of the war machine. Until the military-social science complex is eliminated, social scientists will aid the enslavement, rather than the liberation, of mankind." These MIT students lamented their university's position at the heart of military-funded research, and urged that this be brought to an end. Their ideal university would be a place from which to challenge power, rather than to do research on its behalf.[28] Another pamphlet stated bluntly that the project would transform scholarship into military intelligence, making the university a component of the defense apparatus.[29]

As with the earlier protests, some faculty members were supportive of student goals, though most were not. Joseph Weizenbaum (whose influential work in Artificial Intelligence was described in chapter 4) wrote a lengthy article for the MIT student newspaper, *The Tech*, giving his assessment of the strengths of the project, as well as its dangers. He first outlined a typical complaint—that the development of social sciences with such fine-grained segmentations of the population would "lead to techniques for manipulating people as, in effect, abstract objects." Weizenbaum disagreed, however, by arguing that such fears were premature given the rudimentary state of the social sciences. He echoed the assessments of Ithiel Pool and other

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1968), 62-80, on p. 65. Even as Pool sought to bring rationality and rigorous scientific examination farther into the practice of political life, that Weberian wall separating reason and values was being radically rethought. Though Pool maintained his firm position, others associated with the Project (mentioned below) recognized that its foundations needed to be readdressed in the face of the impossibility of maintaining a firm distinction between political judgments and reasoned analysis.

[27] Theodore Roszak, *The Making of a Counter Culture: Reflections on the Technocratic Society and its Youthful Opposition* (Garden City, NY: Doubleday, 1969), 21. Also see Arthur P. Mendel, "Robots and Rebels," *The New Republic* (1/11/1969): 16-19.

[28] "Project CAM Exposed," Spring 1969, Cambridge Project Records, box 2.

[29] "From the People who Brought you Vietnam…," Cambridge Project Records, box 2.

leaders of the Project to the effect that the social sciences were "data rich and theory poor." Weizenbaum strongly supported the Cambridge Project's scientific mission of finding new methods of data analysis in the service of creating a stronger theoretical foundation to these disciplines.

His real complaint was with the system of contract-based science patronage, which he contrasted against an ideal of free inquiry. The Cambridge Project was unlikely to bring to the social sciences a degree of accuracy comparable to that of physics, and Weizenbaum feared the consequences of designing social policy on the basis of such imperfect knowledge. Because the scholars on the Cambridge Project could not honestly guarantee that the results of their research would be useful for the mission of its patron, ARPA, those scientists were guilty of further corrupting science from its pure ideals by overselling it. To the extent that student radicals had seized upon the connection between the Cambridge Project and the mission of the Pentagon, Weizenbaum accused the Project leaders of bringing this situation upon themselves.[30]

The protests were experienced differently on the two campuses, owing to their different experiences with the Cold War science patronage system.[31] At MIT, Ithiel Pool argued that the accumulation of scientific knowledge was an intrinsic good, and that theoretical work, even if supported by ARPA, would eventually yield positive applications in social policy. Furthermore, one of the project leaders, J. C. R. Licklider, had recently returned from a stint as head of ARPA's Information Processing Techniques Office in Washington. MIT already had experience in managing scientific research on contract from defense agencies, and the Provost and other administrators had few qualms about pursuing research in the face of protests.

Meanwhile, a committee within Harvard's Faculty of Arts and Sciences was debating how to organize Harvard's involvement with the Cambridge Project. The schools of business and of education, which were also interested in the Project, formed similar committees. Several members of the FAS faculty strongly believed that Harvard should not formally join the ARPA-funded Project. While they agreed that individual scholars should have the right to do any research they deemed important, this group felt that the institution should not be seen as condoning the military dominance of science funding. This was explicitly tied to the current situation in Vietnam. As Harvard professor Marshall Smith put it, "the key factor at the present time is that there is a military 'establishment' which invades all aspects of our lives. At some point everyone, individuals and organizations, will have to take some action to stop the invasion. Harvard has a unique opportunity to use its prestige and resources to help convince the nation that the best interests of mankind may not be synonymous with the best interests of the Department of Defense."[32] While admitting that the student protesters had "gone off the deep end," Social Relations professor David McClelland acknowledged "a 'kernel' of proper concern," and insisted that the Harvard committee address the privacy issues.[33]

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[30] Joseph Weizenbaum, "Letter to *The Tech,*" 5/12/1969, Cambridge Project Records, box 7.

[31] For a general consideration of the role of universities in shaping a "technological society," see John H. Schaar and Sheldon S. Wolin, "Education and the Technological Society," *New York Review of Books*, 10/9/1969.

[32] Letter from Marshall Smith to Dean Sizer, 9/29/1969, Cambridge Project Records, box 2.

[33] Letter from David C. McClelland to E. L. Pattullo, 9/23/1969, Cambridge Project Records, box 2.

The FAS group, led by Harvey Brooks, Dean of Engineering, concluded in October that the Project's defense funding should not, in itself, rule out collaboration. After all, as the report observed, "the natural sciences have very large amounts of funds from the defense agencies and defense-related agencies than the social sciences. To single out this project just because of defense sponsorship could only be regarded by some as prejudice against the social sciences."[34] Furthermore, the committee report noted that even if certain segments of the Harvard community opposed defense-funded scientific research, their views should not prohibit other scholars from using such funds.

Tensions continued to grow within the Harvard community during the autumn of 1969. A series of articles in *The Crimson* by David Bruck, a former Harvard student, attacked the Project. He first tied it to the fiasco of Project Camelot (in which anthropologists doing field research in Latin America provided information for American counterinsurgency efforts), arguing that the Cambridge Project was a similar demonstration of the applications of social science for defense, chosen because it was less likely to bring controversy than Camelot had been.[35] The article prompted a quick reply from Edwin B. Newman (professor of psychology at Harvard and a founding member of the technology consulting group Bolt Beranek and Newman, mentioned in chapter 3), who acknowledged the sensitivity of having military patronage, but pointed out that the NSF had been the Project's preferred source of support. When it became clear that the NSF faced more constraints in its funding decisions, the choice became ARPA or nothing. Newman added that it was the scientists' responsibility to study these issues, for "technological changes are coming at a dizzy pace. The heat is on. Either the social scientist makes this technology work for him, or, by default, control of this development falls into the hands of technicians and of people who will put it to use entirely for their own limited purposes." These technological capabilities would be explored, regardless of the Project's existence. The question was whether this work would be done by scholars following academic conventions or by private interests.[36]

Bruck's second claim was that the organization of the Cambridge Project (and the association between Harvard and MIT) represented a preliminary stage in the creation of a new form of social science—and one that was being created under the auspices of ARPA. The Project would use the prestige of the Harvard name to legitimize this undertaking, while creating a self-

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[34] "Recommended Policy Concerning Harvard's Relation to MIT's 'Cambridge Project,'" 10/8/69, Cambridge Project Records, box 7.

[35] On Project Camelot, see Mark Solovey, "Project Camelot and the 1960s Epistemological Revolution: Rethinking the Politics-Patronage-Social Science Nexus," *Social Studies of Science* 31 (2001): 171-206. Irving Horowitz, a prominent social scientist, argued that while Camelot had seemed to be a boon for scientists, the rapidity with which it was canceled in the face of political backlash demonstrated the shaky foundations of government-sponsored social science. See Irving Horowitz, "The Life and Death of Project Camelot," in *The Triple Revolution: Social Problems in Depth*, Robert Perrucci and Marc Pilisuk, eds. (Boston: Little, Brown and Company, 1968), 153-169. For background on the involvement of political scientists (many, including Ithiel Pool, involved in the Cambridge Project) in the postwar project of modernization theory, see Nils Gilman, *Mandarins of the Future: Modernization Theory in Cold War America* (Baltimore: Johns Hopkins University Press, 2003).

[36] David Bruck, "'Cambridge Project' Underway Despite Protests of Radicals," *Sunday Herald Traveler*, 10/12/1969; Edwin B. Newman, "Professor Answers Cambridge Project Foes…" *Sunday Herald Traveler*, 10/19/1969. These articles were originally published in the *Harvard Crimson*.

sustaining community of scholars and consolidating ARPA's research interests to save money. Invoking the language of Thomas Kuhn's recent *Structure of Scientific Revolutions*, Bruck noted that this community of scholars could create a new social science based on a new disciplinary paradigm.[37]

Ithiel Pool, one of the major scientists involved in the Cambridge Project, accused Bruck of offering little more than vague insinuations of conspiracy and tarring the scientists with guilt by association. Ultimately, he said, the Cambridge Project was about creating the tools to better understand society. "If [the social sciences] have any social consequences at all it is that they increase the chance that men may come to understand themselves better, and with the aid of that knowledge perhaps make life better," he wrote. "Behind the Cambridge Project and behind all science, lies the act of faith, which McCarthyism in all its forms rejects, that in the end knowledge is a good thing and will help mankind." For the scholars in the Cambridge Project, their work would at worst be a dead end, but had the potential to generate new ways of understanding social theories and improving social management.[38]

Despite these protestations, the radical opposition to the Cambridge Project denied the neutrality of this research. The problem of unequal access to the results of this research meant that it was innately political. Powerful establishment organizations (meaning the students' usual suspects: the Pentagon and large corporations) could more easily purchase access to the machines and gain the necessary technical knowledge than could activists or radical groups on the margins. Bruck went further, denying any easy connection between understanding the world in scientific terms and creating a more just or moral social order. He instead observed that refined knowledge created greater opportunities for social control, but that this made the problem of thinking through the morality of its application more urgent.[39]

The real problem, however, was that when critics of the Cambridge Project objected to the concentration of statistical capabilities and data collection, the scientists in the Project could only offer vague promises that this power was unlikely to be abused. Karl Deutsch, a Harvard professor of government and Cambridge Project member, pushed back against this fear of concentrated statistical power in a letter to the *Harvard Independent* in 1969: "At the present time, I think mankind is more threatened by ignorance or errors of the American or Soviet or Chinese government than it would be by an increase in the social science knowledge of any of these governments. On balance, I believe the world needs more social science knowledge and not less."[40] To the social scientists, the problem was that the state had neither the knowledge nor the administrative capabilities to manage a modern society. To critics on the left, the problem was with the general idea of a society as something to be managed in the first place, and with the potential for databases to greatly expand these administrative capabilities. The social scientists in the Project wanted to construct maps of society within comprehensive databases—entities whose

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[37] David Bruck, "And the Criticism Continues," *Sunday Herald Traveler*, 10/19/1969.

[38] Ithiel de Sola Pool, "MIT Professor Scents McCarthyism in Attack," *Sunday Herald Traveler*, 10/26/1969; Aaron Fleisher, "Nothing Sinister, May Do Some Good," *Sunday Herald Traveler*, 10/26/1969.

[39] David Bruck, "Cambridge Project: The Indictment is Concluded," *Sunday Herald Traveler*, 10/26/1969.

[40] Karl Deutsch, letter to the *Harvard Independent*, 10/9/1969, Cambridge Project Records, box 7.

properties could be studied as needed. The radicals fought for privacy, identifying freedom with the gaps that existed among the possibilities of control.

The controversy in Cambridge attracted the attention of the journal *Science*. Journalist Judith Coburn connected the fate of the Project to the problems facing ARPA's social sciences directorate, which was under attack within Congress, where Senator Fulbright had proposed cutting the Pentagon's social science budget by $48.5 million. The Cambridge Project would create a network of interested parties and a large concentration of research energy, and would therefore be more capable of defending itself against the skeptics. Coburn explicitly compared it to the Manhattan Project, drawing sharp criticism from the members of the Cambridge Project for the hyperbole. They pointed out that their project was nowhere near the scale of that earlier effort.[41] The comparison to the Manhattan Project was telling, as opposition to "Big Science" was becoming an important article of faith among critics on the left.

George Miller, whose ideas had led to the initial proposal, also wrote in to distinguish the goals of the project from their implementation. "It is my impression that no one has questioned the importance of the purposes of Project Cambridge," he wrote. "The central argument against the project seems to be that it is funded by the Department of Defense." He then defended the necessity of continuing to sell the project to the Pentagon.

> Congress, already suspicious that civilians in the Pentagon have been redirecting defense dollars into research that was not sufficiently 'mission oriented,' are likely to listen with special interest while the social scientists of Cambridge try to reassure their local critics that the Project will not really serve the defense agency's operations. By the time Harvard and MIT finish what Miss Coburn calls their 'useful soul searching,' they may find that Congress has resolved their dilemma unilaterally. In that case, everyone will be a loser – the public included.[42]

Miller's pragmatic approach to the question of defense spending put him at odds with Weizenbaum's earlier moral arguments. It extended a familiar argument that university scientists had an obligation to pursue this research following academic conventions, rather than allowing it to be absorbed into a shadowy parallel universe of military research.

Douwe Yntema and other leaders of the Cambridge Project recognized that faculty members had varied opinions concerning the Project's funding, and they circulated a questionnaire to prospective researchers to ascertain their views on its military ties. One such question read: "There is some apprehension that the relation of the Cambridge Project to ARPA, especially through the ARPA network, will give the Department of Defense special access and priorities to the information, methods, and results of this project. It was not the intent of the proposal that this should occur. Is your participation in the project contingent on further clarification of the links between the Cambridge Project and ARPA with regard to this point?"[43]

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[41] Judith Coburn, "Project Cambridge: Another Showdown for Social Sciences?" *Science* 166 (1969): 1250-1253; Draft Letter to the Editor from Participants in the Cambridge Project, 12/9/1969, Cambridge Project Records, box 7.

[42] George Miller, "Letter to the Editor," 12/17/1969, Cambridge Project Records, box 7.

[43] "Questions for Evaluation by Prospective Cambridge Project Participants," 5/12/1969, Cambridge Project Records, box 1.

This questionnaire was accompanied by a set of basic facts about the project designed to dispel concerns about the role of the military or about intrusions on personal privacy. In particular, it stated that the Project's leaders "have put themselves on record that they, too, regard the present funding of science as unbalanced. They would prefer a more diversified funding base for the Project and for the behavioral sciences generally. They will make every effort, in future years, to try to secure extensive civilian contributions to the budget of the Project and to lessen the DoD contribution as other funds become available."[44] While the reliance on military funding had been a large public relations problem for the Project, identifying appropriate funding was about to become an even more complex political calculation.

The passage of the Mansfield Amendment (Section 203 of the 1969 Defense Authorization Bill) in late 1969 had meant that Defense Department patronage could only go to projects that specifically served the mission of the department. At Harvard, Harvey Brooks, still deliberating on the proposal to join the Cambridge Project, wrote his faculty to acknowledge the new uncertainty in its funding. He observed that the GAO had been instructed to interpret its provisions strictly.[45] The fact that the Cambridge project continued to receive ARPA funding even after the passage of this law suggested to observers that the Project was either directly serving military goals, or else that the researchers were lying to make it seem that way. Yntema, the provisional director of the Cambridge Project, wrote to the student newspapers of both universities to explain that the interpretation of the Mansfield Amendment was not so straightforward, and that the long-term vision of the Cambridge Project could be interpreted as falling within the Amendment's provisions if viewed generously. Continued ARPA patronage was a sign of such an interpretation rather than proof of a nefarious militarism.[46]

It was clear to most critics that the Cambridge Project was far from being a "real" military project — though this observation did not prevent critics from continuing to attack its military ties. The Project was caught in a catch-22. It needed to claim a defense mission to satisfy the Mansfield Amendment and receive funding, but it also needed to deny that very same purpose in order to attract researchers. This did little to mollify the critics. Representative of this line of attack was Wells Eddleman, a student at MIT, who wrote in *Strike Daily* in 1971, that "at present, CAM has three basic types of participants — people who are ripping off the government to do computer/social-science research of their own; people who are ripping off the government to accomplish their own capitalist ends; and people who are honest and upstanding and do the war research just like the proposal said."[47]

These attempts to circumvent the Mansfield Amendment by claiming military applications were quite transparent. Yet, when the GAO investigated the Cambridge Project for compliance with the Mansfield Amendment, it quickly agreed that this research was sufficiently oriented toward military purposes.[48] With the GAO seal of approval, continued funding from

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[44] "Facts About the Cambridge Project," 5/12/1969. Cambridge Project Records, box 1.

[45] Memo from Harvey Brooks, 11/25/1969, Cambridge Project Records, box 2.

[46] Memo from Douwe Yntema to Aaron Fleisher et al., 3/6/1970, Cambridge Project Records, box 1.

[47] Wells Eddleman, "CAM-1983?", 1971, Cambridge Project Records, box 7.

[48] Letter from Douwe Yntema to Walter Rosenblith 2/4/1970, Cambridge Project Records, box 7.

ARPA ceased to be a problem. Scientists with the Project did try to find support from beyond the Department of Defense by associating their research with domestic anti-poverty and urban renewal programs, with varying degrees of success. For example, C. Ross Cope, of the MIT Urban Studies program, conducted statistical analyses of gerrymandered congressional districts in Mississippi in order to demonstrate the effect that this had on the voting power of African-Americans.[49] The existence of these efforts did not insulate the larger Project.

The problems facing the Cambridge Project came from two main sources: defense patronage, which ran afoul of Vietnam-era student politics, and the fear that collections of data and analytical tools to parse this data would erode individual privacy. The connections between the Cambridge Project and other projects for both improved communications (such as the nascent ARPANET) and for improved scientific understandings of social movements (such as the maligned Project Camelot) were fodder for the radicals.[50] Both seemed to represent an imposition of control from the top down, centered within the defense establishment. Such systems were, in fact, being created by both public and private organizations that handled large amounts of data. The question was whether academia could also investigate these processes without losing its soul.

Due to these concerns, Harvard's decision to join the Cambridge Project was done in a series of half-measures. Brooks's committee presented two proposals: either joining the Cambridge Project as a full partner of MIT or allowing individual scientists to work as subcontractors. The committee cited two concerns, one based on pragmatism and one based on policy. The pragmatic argument dealt with the uncertainty of the GAO audit for compliance with the Mansfield Amendment. Though the committee members admitted that this penalized the social scientists for being the last ones to embrace military funding, they decided that "in the present political climate participation in Project Cambridge might be thought of as analogous to taking out a second mortgage on a house which had recently been discovered to be infested by termites."[51] The policy argument was a straightforward expression of concern with the state of the military and polarization on campus:

> The policy issue relates to the belief that Harvard participation in the Cambridge Project on an institutional basis, with full partnership in the governance, constitutes in some measure a political endorsement of the Department of Defense, and of its entry into the support of large-scale research in the social and

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[49] C. Ross Cope, "Regionalization Project: Final Report to Cambridge Project on Computer Redistricting Work," Summer 1971, Cambridge Project Records, box 8. Most projects, however, dealt with more concrete technical work. Typical projects included "Computer Programs for Bayesian and Classical Multivariate Analysis of Data" by Arthur Schleifer, Robert Glauber, and Robert Schlaifer, and "The Linking of the PDP-8e Computer to the PDP-9T Computer" by Richard Herrnstein and William Baum, Cambridge Project Records, box 4.

[50] "CAM: A Project to Fill in the Gaps," 12/1969, Cambridge Project Records, box 7. On the history of ARPANET, see Janet Abbate, *Inventing the Internet* (Cambridge, Mass.: MIT Press, 1999). Note that while the left wing protesters at MIT criticized ARPANET as a step toward creating a technological grid of control, the communications potentials of the network were simultaneously being celebrated by many members of the counterculture, such as Stewart Brand and his circle in California.

[51] "Status Report on the Conclusions of the Cambridge Project Subcommittee," 11/23/1969, Cambridge Project Records, box 2.

> behavioral sciences. The opponents of institutional participation point out that the DOD has become a center of major political controversy, and that therefore any institutional stance with respect to the Cambridge Project will be interpreted as a political act on the part of the University as a whole. Since the University is thus forced into taking a political stance, one way of the other, the opponents prefer that this stance be taken against entry of DOD into support of the social sciences.[52]

However, committee members acknowledged that failing to take part in this research meant passing up an opportunity to shape the direction of research using large-scale data banks, leaving such research to those with less commitment to the public interest.[53]

Even this step was provisional because "it was agreed unanimously that if support tended to come exclusively from DOD over a long period, a number of key social scientists would exclude themselves from participation because of DOD sponsorship, even though they were enthusiastic exponents of the substance of the research proposed." However, the nebulous character of the Cambridge Project's research hindered its ability to secure funding. The NSF rejected the Project's initial request for support to create a social science computing center because the creation of this support center seemed uncomfortably close to being an institutional grant, rather than supporting a well-defined project.[54] The committee made two further recommendations: the creation of one panel to investigate the issues of privacy in data banks, and another panel to consider the scholar's responsibilities concerning the applications of his or her research by patrons.[55]

Yet Harvard faculty members also recognized that a failure to engage with the work of the Cambridge Project could harm the school's reputation in research. They noted the tendencies toward Big Science in the physical sciences, and suggested that a refusal to join in Big Social Science would keep Harvard scholars from the cutting edge of contemporary research and would make it harder to recruit graduate students. Douwe Yntema worried that a decision by Harvard not to join the Project as an equal member would consign the university's social science programs to second-tier status.[56] Already, Harvard's reluctance to fully embrace the Cold War science patronage system had put its programs in science and engineering in a secondary position relative to its neighbor down the street. Yet Brooks's committee also sensed that the days of strong military patronage were ending, and were reluctant to make a multi-year commitment to an ARPA-funded program. Ultimately, the Harvard Faculty of Arts and Sciences hedged by deciding not to formally co-sponsor the project, but to allow individual faculty members join the

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[52] Ibid.

[53] Ibid.; "A Prefatory Note," 11/1969, Cambridge Project Records, box 3.

[54] Memo from E. B. Newman, 7/7/1971, Cambridge Project Records, box 11; Letter to Edwin B. Newman from D. D. Aufenkamp, 6/2/1972, Cambridge Project Records, box 11.

[55] "Status Report on the Conclusions of the Cambridge Project Subcommittee," 11/23/1969, Cambridge Project Records, box 2; "Harvard News Release on the Cambridge Project," 12/1/1969, Cambridge Project Records, box 7.

[56] Letter from D. B. Yntema to Dean Harvey Brooks, 11/7/1969, Cambridge Project Records, box 2.

project as subcontractors. The Cambridge Project became an operation run by MIT. In 1970 Douwe Yntema, now on the faculty of the Sloan School of Management, officially became its director, ending a year of uncertainty about the Project's leadership.

The more computer scientists pursued their patrons, the more opposition they created by reinforcing the widespread fears of computerized social science. Said Harvard historian Donald Fleming, "it was, precisely, a scheme for computerizing social data, accompanied by portentous rumblings about the utility of all this to Washington. The previously unpersuasive nightmare of social control through social science became a psychological reality by introducing computers into the picture."[57] Joseph Hanlon, writing for *The New Scientist*, criticized the political naïveté of the Project leadership.[58]

Fleming understood that these radical critiques were informed by recent scholarship explaining the historical contingency of scientific knowledge and the complex relationship between scientific research and the exercise of power. "The transforming element in the situation is the new sense of the equivocality of science as a historically demonstrated fact. Put in these terms, the easy passage that science seemed to be having earlier in the twentieth century was an index to naïveté about it; and the current forebodings are a direct function of greater sophistication about its true nature and potentialities."[59] This observation highlights an important point: while the technological side of the Cambridge Project was designed to create shared resources and an improved sense of intellectual community, the significance of that community for the subsequent development of these disciplines was better understood by the protesters than by the scientists themselves, as discussed below.

Critics of Harvard's decision, such as Donald Fleming, lamented that the result of abandoning such research would be the creation of "a self-contained military research establishment, insulated and isolated from the 'pure' scientists in the universities, and manned on a self-selecting principle by the very investigators who are least fazed by ethical considerations."[60] The radicals, in his opinion, were self-defeating. Perhaps pushing military research projects off the Harvard campus would give the protesters peace of mind, but it certainly would not chasten research sponsors.

The Consistent System and the Creation of a Digital Community

Given the opposition among students and the unfamiliar nature of the technology, the leaders of the Cambridge Project recognized that they needed to make an effort to interest social science faculty. They planned to create "advisory centers" in the major social science departments at both MIT and Harvard, where faculty affiliated with the Project could answer questions from other interested, or merely curious, scholars. They also applied for support from

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[57] Donald Fleming, "Big Science under Fire," *The Atlantic Monthly*, 9/1970, 96-101, on 99.

[58] Joseph Hanlon, "The Implications of Project Cambridge," *The New Scientist*, 2/25/1971, 421-423.

[59] Fleming, 99.

[60] Ibid., 100.

the NSF to train students in computer operations, hoping to create a cadre of trained researchers beyond the community of engineers themselves.[61]

The purpose of the Project was to do social science in a fundamentally different way, and this required proselytizing. Several statistical packages had been developed during the 1960s, such as SPSS (developed at Stanford in 1968). These tools were already proving useful for analyzing quantitative datasets. The power of computational packages allowed social scientists to perform analyses that would otherwise have been prohibitively difficult. The Cambridge Project intended to create a common platform so that these different packages could be integrated by using the same conventions for organizing data. However, the more ambitious goals of the Project involved going beyond such traditional forms of quantitative data. Ithiel Pool, for example, directed his attention to developing the capabilities of understanding verbal propositions.[62] These radical innovations had to be balanced against the more incremental advances in improving such packages as SPSS.

This problem of coordinating the technical work of both the social science community and the computer scientists was difficult because the two groups had very little direct contact in their research. High-level discussions in the Project administration were no substitute for research collaboration. In a proposal to the NSF, the Cambridge Project administrators described the delicate institutional engineering taking place: “while the computer people are tunneling into the mountain from one side, the working behavioral science community is tunneling in from the other side, and great care must be exercised to assure that a successful match takes place.” A usable computer system required a transparent interface and a common library of useful tools and data, as well as standard conventions. Ithiel Pool recognized that there were social scientists interested in learning how to bend the machine to their own purposes and create useful programs on their own—but only if this could be done with minimal training. The Consistent System was created so that the social science community would take the lead in creating computer systems that were useful for their own research. They had the substantive knowledge of what programs

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[61] Proposal for Interim Operating Facilities, 8/28/1969, Cambridge Project Records, box 1; “Teaching Support,” 7/23/1970, Cambridge Project Records, box 1; “Proposal for Support of Educational Activities in Line with Cambridge Project Memo of July 26, 1970,” 9/21/1970, Cambridge Project Records, box 1.

[62] Memo from Ithiel de Sola Pool to the Cambridge Project, 6/7/1971, Cambridge Project Records, box 1. This project drew upon the work of natural language processing that was most common within artificial intelligence research. Pool acknowledged the similarity between his own work and that of the AI community, but described the two as being “poles apart.” The goal of AI was “to produce a program that will draw correct inferences. To do so the logic it uses must be rigorous and elaborate. Furthermore the task of making a computer program do well at inferring is so difficult that it is generally necessary to narrow the task down by making the system work in a very small semantic domain,” while Pool’s work focused on augmenting human capabilities. “We want to accept a very wide range of possible English statements, but rely on the computer only to perform rather dull-witted routine operations on them, and let the human user do the sophisticated job of deciding what inferences are valid … What we have in mind is not a pure machine system for doing proofs, but a man-machine system for handling large structured bodies of text in which the computer provides a crutch to the human thinker.” See Shahriar Ahy and Ithiel de Sola Pool, “Text Representation, Text-Data Management, and Text Modelling With the General Implicator,” 9/1973, Cambridge Project Records, box 9.

were essential that the technical computing experts would not necessarily have. The creation of the Consistent System was an essential step to allow for bottom-up program design.[63]

The design of a stable platform required both technical and social elements. The heart of the "Consistent System" was the "Substrate," a layer of code between the users' programs and the Multics system, so that the user experience could remain consistent even as the Multics designers continually upgraded the system.[64] The idea was that while technical users could keep up with the technical changes to the Multics system, users with less of a technical background (meaning, in this case, social scientists) should not be expected to keep up with these developments. Instead, the substrate would be a computational layer that remained updated with Multics, but that maintained a stable platform for users who expected consistency on time-scales of weeks or months.[65]

Documentation likewise would exist in two forms. Comprehensive manuals would exist, but the fact of having constant upgrades meant that printed documents would either quickly become obsolete, or else would need to be continually reprinted. A solution to this was to have documentation available on-line, which could be easily updated when necessary without needing to publish everything again. The two forms of documentation—printed and electronic—would complement each other. The challenge of making the two compatible, however, involved capturing both the overall structure of the system, as well as the more detailed system of interconnected documentation files. Writing these would "require that the writer have a general picture of the web of documents into which they are linked."[66]

The success of the Consistent System required more than technical solutions to create stability in the system. Yntema also need to create a system of laws and practices that could keep a community of users functioning smoothly. Users needed to be able to do creative and useful work while sharing their results and their methods within the community.[67] This pointed to an important development in Multics, as its developers began to consider the needs of users who were not intimately involved in the system's maintenance.

The users, however, were expected to be developers in a different sense. Yntema distinguished two components of the Consistent System: the "Foundation" and the "Collection." The Foundation included the Substrate and all of the system components that interacted with the machine and the Multics system. The Collection instead included the programs and data for

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[63] "Draft of a Proposal to the NSF," 4/27/1972, Cambridge Project Records, box 1. Of course, here the plans for "bottom-up" programming meant programming by an already elite group of academics.

[64] "1973 ARPA Proposal," Cambridge Project Records, box 1.

[65] "Preface to the Integrated System," Cambridge Project Records, box 2.

[66] D. B. Yntema, "Overview, for discussion, of the documentation on the consistent system," 9/8/1971, Cambridge Project Records, box 2.

[67] "The Social Aspects of Computer Use," 7/23/1971, Cambridge Project Records, box 1. Once again, the development of such a system meant integrating specific technological innovations with the creation of rules and social norms governing its use. Neither the technical nor the social side of the system could support the development of this new form of community alone.

immediate use. The technicians would keep the Foundation running, but users would contribute their own materials to the Collection. Therefore the system required all user-authored files to be compatible, while allowing for the great diversity of applications that this loose community could create.[68] The empowerment of users meant that they would drive the creation of programs, rather than the technical experts maintaining Multics. Decentralizing authority over the system would foster innovation, according to Yntema.[69]

Another important feature of this Consistent System would be the existence of an “agent” within the computer that could handle low-level decisions for the user. The agent would intercept the outputs meant for the user and react accordingly, handing control to the user only when necessary. It was motivated by Licklider’s model of symbiosis, though moving the locus of user control higher up, away from the routine operations of the machine. Yntema noted the similarity between this agent and the work of AI researcher Oliver Selfridge. While this agent was by no means the autonomous intelligence that motivated the most hyped AI research, it grew directly out of the tradition of automating simple decision-making, and of giving machines the ability to direct themselves. Yntema cited what was known at Lincoln Laboratory as Selfridge’s Dictum: “A program that acts as the user’s agent should be permitted to do anything the user can do at the terminal, with the possible exception of pressing the interrupt button.” The task of making computing accessible to non-experts meant handing control over to the machine.[70] According to Philip Stone, “the ideal system should take informal communication as its model and attempt to maximize the strengths and minimize the weaknesses of this system. What is needed, then, is a computer analogue of the available, intelligent, and informed colleague.”[71]

While these goals aligned with those of the Multics system, implementing them required a substantial change from the way that research had traditionally taken place. Though Multics had long been described as a computer utility for eventual public use, in practice it had remained as oriented toward strictly technical research as any other major computing project.[72] The problem, as Yntema recognized early on, was that the process of building this system would not be particularly interesting to scholars interested in computers only as tools to answer social questions, while the project goals were obscure to the technical experts responsible for designing the systems.[73] How could a user-driven system work when the users were not already systems designers?

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[68] “The Consistent System: Substrate and Conventions: Chapter 1,” 2/4/1972, Cambridge Project Records, box 2.

[69] “The Consistent System: Substrate and Conventions: Chapter 4,” 2/4/1972, Cambridge Project Records, box 2.

[70] Memo from D. Yntema to “J. Klensin, J. Markowitz, R. Wiesen, and anyone else who is interested,” 1/12/1971, Cambridge Project Records, box 1.

[71] Letter from Philip Stone to Herbert Simon, 2/27/1967, Simon Papers, box 116.

[72] Memo from Douwe B. Yntema to Robert H. Scott, Director of Information Processing Services, 4/28/1972, Cambridge Project Records, box 1.

[73] Memo from Douwe B. Yntema, 7/9/1969, Cambridge Project Records, box 1.

Individual Privacy in Data Banks

By the late 1960s large, computerized data banks were beginning to be seen as infringements upon individual privacy, a development that limited the appeal of continued large-scale data processing. The awareness that the collection of personal data and the systematic analysis of this data by machine could be understood as a threat to privacy was several years in the making. Bank of America's ERMA system, for example, had been an early pioneer in this form of data management, and yet few of the bank's customers had initially been concerned.

This situation changed in the mid 1960s due to the overreach of both private industry and the government. The issue of privacy was becoming a hot legal topic given the expansion of government record keeping and the development of new surveillance technologies. Alan Westin of Columbia University wrote one of the first significant analyses of this trend in his 1967 study, *Privacy and Freedom*. Westin's basic premise was that "as our industrialized system has grown more complex, as government regulatory functions have increased, as large bureaucratic organizations have become the model in our private sector, and as social science has committed itself heavily to data-collection and analysis, we have become the greatest data-generating society in human history."[74]

Westin understood that the changes in the American approach to privacy had been caused by the use of new technologies that facilitated the collection, analysis, and dissemination of information and by the ideas that justified these practices. The challenge was therefore not only to develop appropriate technological safeguards, but also to confront that rationalizing impulse that motivated such large-scale data analysis. He cited Bob Fano, the director of MIT's Project MAC, to the effect that the continued use of these technologies was inevitable. The challenge for American society was to construct a system of rules and practices to protect privacy while still allowing for the efficiencies of data processing. As Westin put it, "we have moved steadily toward a more behavioral-predictive theory of information, which assumes the need for much psychological and organizational data in order to make the decisions of social science, business, and government. The more computers offer opportunities to simulate behavior, forecast trends, and predict outcomes, the more pressure is generated for personal and organizational information to be collected and processed."[75]

And yet this data collection served an important purpose in guiding administrative decision-making in both private industry and government. The dilemma was captured well by Michael Harrington, who wrote that "bureaucracy is the only way to coordinate the complex functions of a modern economy and society and therefore cannot be dismissed with a curse. Yet it is also an enormous potential source of arbitrary, impersonal power which folds, bends, spindles and mutilates individuals but keeps IBM cards immaculate."[76] Those concerned with the transparency of individual lives before the machine also worried about the psychological effects of living within a system of pervasive data collection and permanent record keeping. Alan

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[74] Alan F. Westin, *Privacy and Freedom* (New York: Atheneum, 1967), 158-159.

[75] Ibid., 322.

[76] Michael Harrington, *Toward a Democratic Left: A Radical Program for a New Majority* (New York: Macmillan, 1968), 144.

Westin saw the beginnings of a deep shift in "the relation between individual spontaneity and social control in our society."[77] Westin recognized that neither the absolutist approach of privacy advocates nor the centralizing impulses of corporations and executive agencies could be maintained. "The fact is that American society wants both better information analysis *and* privacy," he wrote.[78] This conflict pitted large organizations that supported the use of databanks against a weak and disorganized privacy lobby. The challenge facing the application of information technology in the 1960s was to find a reasonable balance.

Such concerns were hardly unique to the age of computerization, having been seen previously in terms of urbanization. According to Ithiel Pool, the recognition that the privacy problem was fundamentally a social one, rather than a technological one, meant that its solution had to be found in the articulation of social norms, rather than in the development of technological quick fixes. "It is important for us to develop a more liberal and tolerant attitude towards deviation," he wrote. "The price of a complex society is the accumulation of vast amounts of knowledge. If this knowledge is to be bearable, perhaps our most important protection will be the development of attitudes which regard it with a good deal of compassion."[79] The technical problem of privacy, however, was not just insufficient compassion. The danger of modern methods of data collection was that it created standardized categories into which individuals were automatically sorted—and that these acts took place without the possibility of human intervention, compassionate or otherwise. These electronic databanks not only handled extensive records and compiled detailed statistics, they also had the drawbacks of automatic systems described in chapters two and four.

Public opinion on the privacy issue was complex, and varied significantly among different demographics. In the early 1970s the data processing industry began to self-consciously reflect upon their position within the culture. The American Federation of Information Processing Societies (AFIPS) and the Association of Data Processing Service Organizations (ADAPSO) were among the leaders in this effort. R. S. Barton of AFIPS organized a conference among computer professionals in October 1971 to address these issues. He was concerned by "a growing anti-computer movement amongst people who are tired of the depersonalization suffered in their contacts with data processing applications" and claimed that "fears of nation-wide data banks which could result in intolerable loss of privacy have put the industry on the defensive." More worrisome still was his contention that the professionals completely failed to

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[77] Alan Westin, "Civil Liberties Issues in Public Databanks [1967]," in Alan F. Westin, ed., *Information Technology in a Democracy* (Cambridge, Mass.: Harvard University Press, 1971), 301-310, on 303. Westin's collection was put together under the auspices of the Harvard Program on Technology and Society. For further information on that program, see Matthew H. Wisnioski, "Engineers and the Intellectual Crisis of Technology, 1957-1973" (Ph.D. dissertation, Princeton University, 2005), 50-65.

[78] Ibid., 310.

[79] "The Impact of Accessible Computing on Society - October 12, 1966," Fano Papers, box 3.

recognize these concerns as valid.[80] A later statement by ADAPSO put the matter more bluntly still: “The data service industry conveys, in general, a very poor image to the buying public.”[81]

An extensive survey done by AFIPS in conjunction with *Time* magazine showed that members of the public had far more sophisticated and nuanced understandings of computers than expected. Few survey respondents believed either the rosy claims of technological boosters or the dystopian fears that preoccupied the radical fringe. Instead, respondents understood the utility of computers for data processing in both business and in government, and, though they did not believe that their civil liberties were at risk, did believe that the potential for misuse existed. While many supported the use of databases to monitor criminals, they were far less willing to allow such tracking in other areas of life. The major reported concern was the use of personal information by advertisers.[82]

The sociologist Irene Taviss believed that the distrust of computerized databanks was a misplaced suspicion of contemporary mass society. Even though “what provokes protest here is the fact that information about persons can be classified and coded into preset categories for computer processing,” she believed that “the real target of this complaint should be the social complexity that results from large populations in advanced technological societies.” The ease of storing individualized data on these systems meant that the computer could, ironically, allow for more individualized treatments of the people coded within its databases.[83]

Describing the symbolism of the IBM punch cards carried by protesters in the Berkeley Free Speech Movement, the prominent philanthropist and activist Wilbur H. Ferry argued that “when the protesters pinned IBM cards to their jackets — an act duplicated on campuses throughout the land — they were declaring against impersonality and standardization; and it cannot be said too often that impersonality and standardization are the very hallmarks of technology.”[84] These analyses pointed to an ambiguity in the notion of privacy, and a contradiction in what was perceived to be the primary problem with computers. Data collection that was geared toward creating broad categories of subjects was claimed to be a dehumanizing process that recognized individuals strictly in terms of their utility as an element of this or that demographic. This type of segmentation represented a deeply anti-human element within modern social sciences, according to the radicals. On the other hand, maintaining records at the level of the individual was a way of making that individual’s identity legible to the creator of that database.

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[80] Letter from R. S. Barton, 9/30/1971, American Federation of Information Processing Societies Records, Charles Babbage Institute, CBI 44, box 14.

[81] “A Statement of ADAPSO’s Role as the Trade Association Serving the Data Services Industry,” 3/12/1973, Association of Data Processing Service Organizations Records, Charles Babbage Institute, CBI 172, box 1.

[82] The only other item that garnered such strong disapproval was the use of databases for matchmaking and dating. “A National Survey of the Public’s Attitudes Towards Computers,” 1971, AFIPS Records, box 16.

[83] Irene Taviss, “Are Computers Dehumanizing?” *Computers and Society* 1 (11/1970): 3-5.

[84] W. H. Ferry, “The Need for New Constitutional Controls [1968],” in Westin, ed., *Information Technology in a Democracy*, 207-213, on 211.

Meanwhile, changes in banking practices and in administration came to the attention of Congress. On the side of private industry, the expansion of credit meant that banks needed systems for assessing the creditworthiness of borrowers. Credit bureaus sprouted like mushrooms, collecting data on borrowers' financial histories and even on their character.[85] While these companies had an interest in collecting accurate information on potential borrowers, they also observed that their purpose depended on identifying some individuals as poor credit risks. The information collected on individuals was not always entirely accurate, nor was it easy for an individual to dispute his or her credit file.[86] Ultimately Congress passed the Fair Credit Reporting Act in 1970. Arthur Miller, a law professor at the University of Michigan, was involved in the project, as was Alan Westin. Miller lamented that pressure from the banking industry meant that the final bill was far weaker than it should have been.[87]

Arthur Miller's interest in these issues was motivated by an earlier conversation with his Michigan colleague James G. Miller. James Miller had approached him in 1966 to ask whether sharing documents within "a national, multi-media information network, that would electronically integrate our colleges and universities," would create any problems with copyright law.[88] Though Arthur Miller had no ready answer, these issues became vitally important in the debate about a National Data Center in 1966 and 1967.

The seed of the idea for the National Data Center was in the 1959 meeting of the American Economic Association, when several economists discussed the challenges of preserving national economic data for analysis. The Social Science Research Council began studying how to better organize data sets in 1960, but after running into problems accessing this data, it handed control of the project to the Bureau of the Budget and the National Archives in 1964. By July 1965, the proposal had made its way to the Committee on Government Operations in the House of Representatives.[89] By that time, the seven federal agencies consulted by the Bureau of the Budget collectively handled over 100 million punch cards and approximately 30,000 magnetic tapes.[90] A year later, in July 1966, the Chairman of the U.S. Civil Service Commission, John W. Macy, Jr., published a short article in *The Saturday Review* describing the use of a similar system for handling personnel files. This anodyne article nevertheless generated an outcry from observers for its claim that "direct tape-to-tape feeding of data from one

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[85] On the historical connection between credit agencies and assessments of character, see Josh Lauer, "The Good Consumer: Credit Reporting and the Invention of Financial Identity in the United States, 1840-1940" (Ph.D. dissertation, University of Pennsylvania, 2008).

[86] Arthur R. Miller, *The Assault on Privacy: Computers, Data Banks, and Dossiers* (Ann Arbor: University of Michigan Press, 1971), 68-69.

[87] Ibid., 86-88.

[88] Ibid., ix-xi.

[89] Jerry Rosenberg, *The Death of Privacy* (New York: Random House, 1969), 27-28.

[90] Ibid., 22.

department to another may become common."[91] As Arthur Miller observed, the decentralized nature of the federal agencies was not only a source of lamentable inefficiency for statisticians, but also a safeguard for basic privacy.[92] As the MIT Committee on the Privacy of Information put it in a 1969 report, "no longer can the inherent inefficiency of unavoidable, dispersed, unmatched data files be relied upon to protect an individual from the dangers that may accompany access to his essentially complete (and possibly unverified) personal history."[93]

The arguments in favor of the proposed center were based on the need for generating more accurate and more diverse social statistics. In a 1967 issue of the journal *American Statistician*, E. Glaser, D. Rosenblatt, and M. K. Wood, from the National Bureau of Standards, explained why the center was necessary. Its goal would be to "provide better understanding of interdependencies within our pluralistic society, leading to better informed choices among alternative policies and programs, and more effective program implementation."[94] In that same issue, Edgar S. Dunn, Jr. of the Bureau of the Budget acknowledged that it been a mistake not to explicitly include privacy protections. He noted, however, that this was not because the Bureau was not attuned to the issue; on the contrary, he argued that the data collection agencies already took privacy very seriously, and therefore the issue was moot.[95] Dunn further explained the difference between an "intelligence" system, such as FBI's National Crime Information Center, that kept detailed information on individuals, and a "statistical" system that merely collected aggregate data. The Congressional committees investigating the center, led by Edward Long in the Senate and Cornelius Gallagher in the House, were skeptical of the sharpness of this distinction.[96]

The opponents of the Data Center succeeded in 1967. Arthur Miller observed the irony of this Pyrrhic victory. The tendency of agencies to collect ever more data would not be stopped by the death of the center. Without a National Data Center to create policies for the accumulation and distribution of this data, each agency would now be free to act on its own. Miller conceded that his arguments in favor of a national, legal response to the privacy question did not jibe with

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[91] John W. Macy, Jr., "Automated Government—How Computers Are Being Used in Washington to Streamline Personnel Administration to the Individual's Benefit," *The Saturday Review*, 7/23/1966, reprinted in Rosenberg, 199-205. Quote on p. 203.

[92] Miller, *Assault on Privacy*, 58.

[93] Committee on Privacy of Information, "Interim Report: The Privacy of the Individual," 6/10/1969, Fano Papers, box 2.

[94] E. Glaser, D. Rosenblatt, M. K. Wood, "The Design of a Federal Statistical Data Center," *The American Statistician* 21 (1967): 12-20, on 20.

[95] Edgar S. Dunn, Jr., "The Idea of a National Data Center and the Issue of Personal Privacy," *The American Statistician* 21 (1967): 21-27, on 23.

[96] Ibid., 24. Gallagher led the Congressional investigations into the practices of credit bureaus in 1968. Following the investigations, he gave a speech to the ACM in which he explained that he did not oppose computerized data collection, but merely wanted "computer professionals [to] realize that there is a body of opinion which questions the very foundations of your work"—a body of opinion that he believed to be important. See Cornelius E. Gallagher, "Computing Power in Real Time [1968]," in Westin, ed., *Information Technology in a Democracy*, 214-221.

the decentralized, anti-statist tendencies of the civil libertarians.[97] Privacy advocates could work with a central agency, but dispersing responsibility for data collection meant multiplying their targets. Ultimately the power of data analysis was too important to be left to individual agencies to figure out.

The significance of these debates was not lost on the creators of the Cambridge Project. Philip Stone circulated a memo to several prominent social scientists in 1967, asking for their input concerning the proposed project. Herb Simon noted that the idea of centralizing data bases was not necessarily the right move; given the growing capacities of computer memories, it was possible that storage on remote computers would be more economical in the long run. He further observed that privacy could grow into a major problem, particularly as electronic records were maintained and multiplied over a period of several years.[98]

E. B. Newman believed that addressing the privacy problem was beyond the scope of the Cambridge Project. The question of balancing individual privacy against the interests of corporations and government was a minefield, and not one in which the Cambridge Project needed to innovate. However, he also added his belief that the expansion of computerized data processing did not represent the intrusion that privacy advocates feared. He cited the traditional form that demographic segmentation had taken, claiming "the whole trend of modern life has been not toward individually tailored responses by the system, but toward general classes of response. More likely what we will see is the aggregation of data about individuals in support of generalized statements that can guide meaningful social action."[99] This view represented the position of the social scientist advocates of such systems, while ignoring the motivations of other interested parties, such as those in law enforcement, who were very interested in monitoring individuals.

Debates about the proper role of computing as it related to governance extended beyond the confines of the Cambridge Project. In 1969 the ACM's Special Interest Committee on the Social Implications of Computing (or "(SIC)^2") had proposed a code of ethics for the association, which included opposition to the creation of large databases.[100] (SIC)^2 was then promptly disbanded.[101] MIT formed a committee in 1968 to identify how issues of privacy affected computer research, reaching no firm conclusions except that the sensitivity of the issue warranted continued study. The question of whether the social sciences and the data processing capabilities of the computer should be united within a centralized information system, or whether decision making should be kept at the individual level remained an open one, though the stakes were now clear.

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[97] Miller, *Assault on Privacy*, 258.

[98] Letter from Philip Stone to Herbert Simon, 2/27/1967, and Letter from Herbert Simon to Philip Stone, 3/8/1967, Herbert Simon Papers, Carnegie Mellon University Archives, box 116.

[99] E. B. Newman, "The problem of privacy in the context of the Cambridge Project," 8/4/1970, Robert M. Fano Papers, box 2.

[100] "Resolution of the disbanded Special Interest Committee on the Social Implications of Computers (SIC^2)," *Computers and Automation,* 4/1969.

[101] Letter to Stuart Lynn, 2/28/1969, Fano Papers, box 6.

Robert Fano, the head of this committee, worried that the growing demand for privacy was an indication of a corrosive distrust of institutions. While hardly unsympathetic to the demands that individual privacy be protected, Fano maintained that a reflexive embrace of privacy and transparency would institutionalize distrust. That distrust, in turn, would lead to new demands for transparency and the protection of individual privacy, creating a vicious cycle.[102]

Jerome Saltzer, another MIT computer scientist, believed that the pace of innovation in data processing was simply too fast for changes in the social framework to catch up. Ordinarily, the task of finding a reasonable balance between privacy and centralized analysis "is developed over a long period with the guidance of cases, the development of legislation, and adjudication by courts. But the rapid changes in technology cause this measured process to fall behind, leaving confusion."[103] Postponing the debate about how to implement reasonable privacy standards in a centralized data processing center meant that this balance would remain elusive.

The Death of a Project

The failure of the Cambridge Project had multiple causes. It was troubled from its beginning by the political situation of military-funded research. Once research began in earnest, the instability of Multics undermined the goal of creating a Consistent System. As Yntema drily put it, "trying to build a complex collection of programs when the underlying system and the compilers are full of 'bugs' and continually changing is extraordinarily difficult." This was a larger problem for Multics itself, which was never fully able to square being a platform for research with being a usable system. An even more fundamental problem for the Cambridge Project was the goal of sharing programs and data files. Even as researchers emphasized the gains that could come from pooling their resources and ideas, in practice they followed their own research agendas. They pushed their programs and data sets in the direction of greater personalization rather than that of greater consistency.[104]

The irony here was that the Cambridge Project had begun by recognizing the promise of integrating several data analysis packages into a coherent whole. A full four years into the project, Yntema continued to implore his team that "we must find a way to make programs written by specialists in different fields work together, and in particular, some way of insuring that they will accept each other's results whenever it is reasonable for them to do so."[105] The Project had claimed that the Consistent System would be more than the sum of its parts. The individual components of the Project were legitimately important research topics; the Project's overall failure was in its inability to generate the synergies that had justified its organization.

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[102] Memo from R. M. Fano to Governor's Commission on Privacy, 5/28/1974, Fano Papers, box 4.

[103] J. H. Saltzer, "Computer Systems and Society," 9/24/1975, Fano Papers, box 2.

[104] "Cambridge Project Final Report," 1973, Cambridge Project Records, box 2.

[105] Douwe B. Yntema, "A General View of the Nature and Purpose of the Consistent System," 4/9/1973, Cambridge Project Records, box 2.

The difficulty of achieving a unified purpose was a major shortcoming of the Project from the beginning. As early as 1970, Licklider had observed that “although much of the research being conducted in Project MAC is good, the air is not full enough of the electricity that pervades a laboratory that is making scientific or technological history and knows it. In short: The situation does not feel right to me; I am fairly sure it does not feel right to some of you…”[106] The individual research topics within the Project were compelling, but its true excitement lay in the promise of creating a new platform for doing research—and this is where its ambitions failed to be realized. The Consistent System rested on a highly *in*consistent computational base, and on the assumption that scholars would find it worthwhile to build a common research platform. The fears of the student radicals had been for naught. The Cambridge Project did not represent a governmental top-down concentration of data processing, but an academic system of individualized research that went in more directions than its professed goal of consistency could accommodate.

At the beginning of the Cambridge Project, E. L. Pattullo, the director of Harvard’s Center for Behavioral Studies, observed that computers were descended from both the physical sciences, through wartime calculations, and from the social sciences, through their massive data processing efforts, such as the census. While the physical sciences had dominated computer research for twenty years, it was time for the behavioral sciences to catch up.[107] Like their predecessors working for the census, these social scientists understood the computer as a powerful administrative tool for organizing and analyzing data, and for solving technical questions.[108] However, this vision was met with a conflicting one, which understood the computer primarily as a tool to empower the individual, an idea expressed by J. C. R. Licklider, among others. These two strands seemed superficially to be compatible, but the debates about privacy and about military patronage in the late 1960s and early 1970s drove them apart.

The Cambridge Project’s troubles demonstrated that the significance of the new computer technologies could not remain confined within the laboratory. Joe Weizenbaum believed that the social problems arising from the new technologies undermined the very justification of schools dedicated to technology. The social consequences of new technologies were now much larger than their purely technical consequences. The job of the technologist had to take these social circumstances into account, and that required training in the humanities and the social sciences. With the social dimensions of science becoming increasingly important, Weizenbaum concluded “that the very concept of a university devoted entirely to science and technology is no longer viable.”[109]

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[106] Memo from J. C. R. Licklider, 6/29/1970, Cambridge Project Records, box 7.

[107] “Appendices to the Report of the Subcommittee of the Committee on Research Policy on the Participation of the Faculty of Arts and Sciences in the Cambridge Project,” 12/1969, Cambridge Project Records, box 8.

[108] Consider the claim by Ithiel Pool, Stuart McIntosh, and David Griffel of the Center for International Studies: “Most information systems may be considered to be exercises in applied social science. The data in them generally concern human beings and their institutions.” From Ithiel de Sola Pool, Stuart McIntosh, and David Griffel, “Information Systems and Social Knowledge [1968],” in Westin, ed., *Information Technology in a Democracy*, 241-249, on p. 241.

[109] AAAS Commission on the Year 2000: Social Implications of the Computer, 11/22/1968, Fano Papers, box 7.

Distinguishing Data from Information

In 1963 Robert M. Gordon, of the consulting firm Arthur D. Little, criticized the prevailing approach taken by managers toward business data and the machines that supported data analysis. In this, he was continuing a critique of such practices that went back to the earliest attempts to create systematic, quantitative approaches to management. The challenge in integrating information technology and management was to understand the difference between raw data and the information that came from thorough analysis. This could be done only if managers recognized that the most important information to come out of systematic analysis was that which ran counter to the expectations or beliefs of managers. Managers, in his view, were loath to let technicians do their proper jobs, both because they did not want to hear the results, and because they saw the necessity of these analyses as proof of a failure to do their own jobs. Gordon disagreed. The most crucial task of management was precisely that part that resisted being programmed into machines or subject to straightforward data analysis. In this respect he was building upon the arguments made by the advocates of office automation.

Successful management required the technical proficiency to understand the administrative work of technicians and other subordinates, but the crucial element was an intellectual curiosity and a mentality directed toward understanding the business environment through systematic research. Gordon lamented that the current generation of managers fundamentally misunderstood the function of information technology and the basic need to look beyond the obvious data to get a full understanding of the business and the challenges that it faced.[110]

The proliferation of data within organizations meant that the experts responsible for analyzing this data and turning it into usable information would play a larger role in any organization. According to administrative theorist Harold Wilensky, this tendency was overturning traditional lines of authority.

> A managerial revolution has taken place but its form is less dramatic than that envisaged by Max Weber and Thorstein Veblen and popularized by James Burnham. … Information is now, as before, a source of power, but it is increasingly a source of confusion. … An increasing share of organizational resources goes to the intelligence function; structural sources of intelligence failures become more prominent; doctrines of intelligence—ideas about how knowledge should be tapped and staff services organized—become more fateful.[111]

Wilensky recognized that this function was vital for the operations of organizations, but pushed back against the tendency to solve information problems by restructuring the information function. It was more important, he said, to improve the executive's attitude toward knowledge

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[110] Robert M. Gordon, "A Commentary on 'The People Problem' as it Affects Computer-Based Management Operations and Data Processing Systems," 2/26/1963, Edmund Berkeley Papers, Charles Babbage Institute, CBI 50, box 23.

[111] Harold Wilensky, "The Road from Information to Knowledge [1967]," in Westin, ed., *Information Technology in a Democracy*, 277-286, on 277.

itself. The influential operations researcher Russell Ackoff echoed this point, claiming that the problem was not a lack of information but an "overabundance of irrelevant information," to which the solutions were better filtering and condensing.[112] Information systems were of limited usefulness on their own, and were no substitute for wise managers and analysts.

The larger category of systems analysis, into which these forms of analysis fell, was also the subject of some critique. Sociologist Ida Hoos (who figured prominently as a critic of the gendered consequences of computerization in chapter 3) observed that the attempt to apply systems theories to social problems involved a category error. The first faulty assumption was that "because the word 'system' can be used for everything from atomic weapons delivery to anthropotomy, the same analytic tools can aid in understanding all of them and the same type of remedies can be applied to their malfunctioning." More worrisome, because it had immediate consequences, was "the related assumption that since large scale, complex systems have been 'managed' by use of certain techniques, then social systems, which are often large and always complex, can be 'managed' in like fashion."[113] The attempt to build these large-scale computer systems was motivated by the turn toward "systems management" that had been popularized by the aerospace industries.[114] Cutbacks in military research in 1970 led to a sudden glut of educated, technologically savvy, and unemployed engineers who could now sell their expertise to business and government.[115]

Hoos denied that this form of systematic analysis was as free from personal biases as its advocates suggested. However, this was not meant to discredit the analyses outright; Hoos had a more subtle point in mind. She instead emphasized that the analyst played a fundamental role in setting up the analysis. "The methodology of systems analysis supplies the form; the analyst, the content. The inputs which he selects become determinative," she wrote. Because the analyst necessarily brought his or her own preconceptions and ideas to the analysis, "the analyst should possess a deep and sensitive understanding of the social matter with which he is engaged. Unfortunately, this is seldom the case."[116] At the same time, Harold Sackman, a computer

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[112] Russell Ackoff, "Management Misinformation Systems," *Management Science* 14 (1967): B147-B156.

[113] Ida Hoos, "Systems Experts: Foxes Guard the Henhouse [1969]," in Westin, ed., *Information Technology in a Democracy*, 444-450, on 446-447.

[114] The primary description of this movement, from the director of NASA during the Apollo program, is James E. Webb, *Space Age Management: The Large-scale Approach* (New York: McGraw-Hill, 1969). For an analysis, see Stephen B. Johnson, *The Secret of Apollo: Systems Management in American and European Space Programs* (Baltimore: Johns Hopkins University Press, 2002). See also David A. Mindell, *Digital Apollo: Human and Machine in Spaceflight* (Cambridge, Mass.: MIT Press, 2008).

[115] John Walsh, "Aerospace: Unemployed Scientists, Engineers Have No Place to Go," *Science* (December, 1970) 170:1384–1387; idem, "Unemployment: What Nixon Is/Isn't Doing to Help Jobless Scientists," *Science* (March, 1971) 171:985–987. On the application of systems engineering to social questions, see Simon Ramo, "The Systems Approach to Social Problems [1968] in Westin, ed., *Information Technology in a Democracy*, 93-98; and Jennifer S. Light, *From Warfare to Welfare: Defense Intellectuals and Urban Problems in Cold War America* (Baltimore: Johns Hopkins University Press, 2003). The state of California had already pioneered the use of techniques from the aerospace industry (concentrated in California) for state government. See Edmund Brown, "Aerospace Studies for the Problems of Men," *State Government* 39 (1966): 2-7.

[116] Hoos, "Systems Experts," 448.

scientist as the Systems Development Corporation (SDC) saw in modern information technology a way of replacing pervasive technocratic control with the possibility of true community. He urged his fellow computer scientists to embrace the tradition of American pragmatism as a check on overconfidence in their analyses.[117] The necessity of interpretation meant that the value of technical analyses depended upon a foundation of practical wisdom and content knowledge. The utility of even the most sophisticated techniques for data analysis would be limited by the intangible soft skills of the analyst.

From Mass Society to Digital Community

The dissolution of the Cambridge Project not only marked the end of a major computing effort designed for a non-technical audience; it also marked the end of a particular vision of how information technology could lead to the betterment of society. The goals of the Cambridge Project articulated a technological vision in which the generation of knowledge would serve public policy, and in which a community of scholars would create the intellectual foundation for a new way of doing social science. Instead, the scholars on the Cambridge Project confronted a public skeptical of their very mission. The problem was not only that their major patron was ARPA at a time when Vietnam tainted the idea of military patronage. Even more fundamental was the suspicion with which these protesters interpreted the logic of the research proposal. A centralized system for storing and processing data could no longer be so easily justified as a way of rationally ordering public policy.

The components of this indictment need to be distinguished. The efficiencies of scale and standardization in a centralized system had to be counterbalanced by the inevitable ways that power would differentiate access to that system. The rhetoric of universality masked a privileging of established power. The concern with privacy extended this argument while advancing the autonomy of the individual against the organization of the state, potentially at the cost of contributing to an atmosphere of systemic distrust. Behind each of these concerns was a generalized opposition to the instrumental rationality that justified the program of massive data collection and analysis. Instead of finding the possibility of liberation through the creation and organization of large technological systems, the critics of the Cambridge Project found their possibilities of liberation through the use of small, personal technologies.[118]

The political opposition to this Project incorporated arguments from both the New Left and from the counterculture. Recent studies of the counterculture have convincingly demonstrated that this was *not* an anti-technological movement. It embraced technologies that allowed for exploration and experimentation (such as synthesizers and LSD) or that allowed for

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[117] Harold Sackman, "Public Philosophy for Real Time Information Systems [1968]," in Westin, ed., *Information Technology in a Democracy*, 222-236.

[118] On the appeal of large-scale technological systems earlier in the century, see Thomas Hughes, *American Genesis: A Century of Invention and Technological Enthusiasm* (Chicago: University of Chicago Press, 2004). At the end of this "century of technological enthusiasm," Hughes described the countercultural backlash of the late '60s and '70s. See Hughes, 443-472.

new forms of communication (such as video recording technology).[119] It was a reaction against an idea of technology as a totalizing system. Even as many in the counterculture rejected the Cambridge Project for its ties to the military, and rejected databases for their associations with banks and with governmental data collection, they enthusiastically welcomed personal computers.[120]

The critique of computer technologies from the left emphasized their potential to be used to reinforce the possibilities of community, as opposed to the top-down applications of information processing for both business and government. The numerous technological prophets of this movement, including Norbert Wiener, Lewis Mumford, and Marshall McLuhan, explicitly drew these analogies between technologies of mass society and technologies of community. The tension between the personalizing and depersonalizing applications of these technologies had been present from the earliest moments of computer research, and this movement sought to redress what it perceived as an imbalance.

The situation was even more complicated because the much-maligned Project was also an experiment in empowering a community of users to share their research and their resources. The goal was to have a self-sustaining system of user-generated innovation. It was, in other words, an early attempt at “crowd sourcing.” Of course, this aspect of the Project’s work was not recognized in its early stages. The Consistent System had a centralizing effect by being a shared platform for research and administration even as the goal of having user-generated content decentralized it in important ways. This dynamic highlights an important point that runs through the dissertation: the language of centralization and decentralization, or of hierarchy and flat organization, is rarely as straightforward as those interpreting the technology would like to claim. The topologies of the networks are complex, and the social effects of technologies are rarely singular. In this case, the tensions between the system’s centralized form and its decentralized content tore it apart. The celebrated possibilities for community within such systems require channeling users into central platforms. The ability to carve demographics into segments of one simultaneously allows for greater individualization even as the impulse to create these technologies comes from the desire to predict individual behavior—or in the language of the radicals, representing individuals as means rather than as ends.

By the middle of the 1970s, however, the backlash against large technological systems created a movement within computing that was ideologically at odds with the powerful number crunching of organized data processing. However, as this dissertation has shown, the two strands were deeply interwoven. Computing had been addressed to questions of industrial and social management from the start, building upon a complex history of social thought and technological innovation (and a long history of reading social meanings into technological development). The counterculture’s re-appropriation of communications technologies added a new dimension to this venerable tradition.

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[119] For example, see Trevor Pinch and Frank Trocco, *Analog Days: The Invention and Impact of the Moog Synthesizer* (Cambridge, Mass.: Harvard University Press, 2004).

[120] This is the major theme of Fred Turner, *From Counterculture to Cyberculture: Stewart Brand, the Whole Earth Network, and the Rise of Digital Utopianism* (Chicago: University of Chicago Press, 2006).

The end of the Cambridge Project did not mean the end of systems for doing quantitative analysis in empirical social science. Nor did it mark the end of the attempts to formalize the logic of the science. The individual research projects that fed into the system continued to be exciting. However, the attempt to organize them into a common purpose was over. The fragmentation of the social sciences into competing (and often conflicting) fields continued apace. This fragmentation was a form of decentralization, albeit one that resulted, as in the case of the National Data Center, in the most questionable elements of the science being hidden away, out of sight and unaccountable. The simplistic rhetoric of centralization and decentralization obscured the more fundamental dynamics of differential power relationships.

Conclusion

In the portentous year of 1984, Claude Shannon, the father of information theory, revisited Herb Simon's infamous 1957 predictions on the future of artificial intelligence. In the wake of the cognitive science revolution, Simon's prediction that psychological theories would be reframed in the language of programming had been fulfilled, though this remained subject to continuing controversy.[1] Shannon updated the predictions to fit the times by added a new fourth prediction in the spirit of the age of Reagan: that a computer would manage an investment portfolio that could beat the market. As if to confirm every stereotype of the mad scientist, Shannon observed how "these goals could mark the beginning of a phase-out of the stupid, entropy-increasing, and militant human race in favor of a more logical, energy-conserving, and friendly species — the computer."[2] This celebration of the perfectability of reason contrasted powerfully with the fears of computer critics. Though the optimism of AI researchers rose and fell over the years, information-centrism remained a constant feature of the period following the history described in this dissertation.[3]

Determining the boundaries of historical periods can devolve into an endless parlor game, but it seems fair to say that the 1970s marked a turning point in the relationship between technology and political economy.[4] The 1970s were a crucial moment in the rise of the American right and the collapse of a certain attitude toward government in the wake of the fall of Saigon and the fall of Richard Nixon. The economic transformations of the 1970s included both the end of the gold standard and the adoption of debit cards—moves toward a virtual basis for currency. This dissertation has focused on the technological story, for which the growth of computers as commonplace technologies was a crucial development in this decade. This conclusion will briefly connect this history to the concept of the postmodern and the rise of the Internet.

Paul Forman has observed that the relationship between science and technology experienced a reversal circa 1980. A characteristically modern ideal of science as determining the shape of technology—and as being a uniquely virtuous pursuit—was replaced by one in which technology occupied the driver's seat and in which the habits of engineers took

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[1] See, for example, Sherry Turkle, *The Second Self: Computers and the Human Spirit* (Cambridge, Mass.: MIT Press, 2005).

[2] Claude Shannon, untitled document, 1984, Claude Shannon Papers, Library of Congress, box 11.

[3] See Paul Edwards's description of "Cold War II" in *The Closed World: Computers and the Politics of Discourse in Cold War America* (Cambridge, Mass.: MIT Press, 1996), 275-301. Céline Lafontaine has argued that postwar French intellectuals were inspired by cybernetics research, and that the warm reception of French theory in American humanities departments owes to this affinity with American cybernetics. See Céline Lafontaine, "The Cybernetic Matrix of 'French Theory,'" *Theory, Culture & Society* 24 (2007): 27-46.

[4] David Harvey marks the transition at 1972, Paul Forman at 1980, but the decade is an important one for both. See David Harvey, *The Condition of Postmodernity: An Enquiry into the Origins of Cultural Change* (London: Blackwell, 1989); and Paul Forman, "The Primacy of Science in Modernity, of Technology in Postmodernity, and of Ideology in the History of Technology," *History and Technology* 23 (2007): 1-152.

precedence over a newly disreputable image of science.[5] The modern world was one in which science, with its methodology of experimentation, was held up as the model of intellectual progress. The idealization of scientific method signified the primacy of means over ends. For Forman, the postmodern moment involved the return to a *pre*-modern elevation of ends over means, marking a retreat from the belief that systematic inquiry could generate true knowledge.[6]

A recurring argument of this dissertation has been that this concern for method lay at the heart of situating computers within their social environments. But the problem of understanding the relationship between logical means and political ends, and "scientific method" in general, is not as straightforward as Forman suggests. Jean-François Lyotard, who stands for Forman as one of the last moderns, observed in the 1970s that "the blossoming of techniques and technologies … has shifted emphasis from the ends of action to its means."[7] The point is not that Lyotard is at odds with Forman here. Rather, this opposition should call the use of these terms into question. Understanding the importance of logical methods in computing requires getting these terms straightened out.

To understand scientific means we must understand Lyotard's larger argument: that the very successes of science have called into question the legitimacy of the scientific world that Forman describes. According to Lyotard this is due to a change in the meaning of knowledge itself. Narrative knowledge and scientific knowledge operate in fundamentally different ways: narrative being validated through transmission, and science being validated through argumentation. While the narrative mode tolerates scientific knowledge as valid, the scientific mode fails to consider narrative as a legitimate form of knowledge at all. The problem is that the legitimacy of scientific argument is built upon a foundation of narrative knowledge; the success of science in diminishing alternative ways of knowing has meant eliminating the principles that legitimate itself.[8] Without this firm foundation, scientific knowledge is left to justify itself through its performance. Legitimacy was no longer to be found in transcendent principles based on narrative; it instead shifted to performance immanent in the workings of a technological society. In other words, without a meta-scientific foundation to legitimate the method of science, its validation came from performance and from achieving specific results.

Now the validity of scientific method had to be derived from its end results. The image of science (and society) driven by the need to achieve ever-greater efficiencies was one path available in the absence of any greater form of legitimacy. But Lyotard offered an alternative: a science that embraced its lack of any fixed foundations and its need for continual self-

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[5] The classic statement of the "modern" view is Vannevar Bush, *Science—The Endless Frontier* (Washington, DC: Government Printing Office, 1945).

[6] Forman, 3.

[7] Jean-François Lyotard, *The Postmodern Condition: A Report on Knowledge* (Minneapolis: University of Minnesota Press, 1984), 37. For Forman's take on Lyotard as modern, see Forman, 51-52.

[8] See Steven Shapin and Simon Schaffer, *Leviathan and Air Pump: Hobbes, Boyle, and the Experimental Life* (Princeton, NJ: Princeton University Press, 1985). The validity of Boyle's experimental method depended on his ability to convince his audiences, making the process of verifying experiments as much "social" as it was "scientific."

legitimation by celebrating difference, paradox, and continual discovery.[9] Down one path lay the subordination of science to the utilitarian/technological impulse that Forman decries, but down the other path is the celebration of science that allows Forman to keep Lyotard in the "modern" camp. The former course, elevating efficiency in the absence of a higher power, suggested parallels for a social world that was similarly bereft of legitimizing foundations, but the unitarity of the efficient systems perspective seemed to Lyotard to be unsupported by contemporary (i.e., post-Kuhnian) science by denying the potential for radical novelty and anomaly in scientific discovery.[10] Lyotard's hope was that "giv[ing] the public free access to the memory and data banks" would generate a multiplicity of analyses and generate emphasize difference rather than control.[11] While his hope was that this would promote freedom, as a political statement it was silent on the question of how one should decide among incommensurable choices. The selection of ends would be subject to some hidden logic that remains by definition extra-scientific.

If, as Forman asserts, the postmodern concern is to select means to achieve a desired end (a sentiment shared by Daniel Bell in his definition of post-industrialism), this reflects the great proliferation of available scientific means rather than a turn away from them. Scientific claims bear upon practical matters, not only upon the laboratory. This has brought the claims of science directly into contact with the messiness of the world. It also introduced the suspicion that scientific method was no royal road to a unitary truth in practical matters. Systematic analyses of practical problems could be done quickly and cheaply to suit the interests of labor, of management, of consumers, or of any other group. Such analyses were no longer reserved for those in positions of power to justify their own authority. Taylorism, for example, had applied a particular method to industrial problems in order to discover "the one best way," empowering technical experts at the expense of workers. The notion that morality is a matter of the ends rather than being a consequence of right reason reflected the disenchantment with the formalist presumptions of Taylorism in a pluralistic world.

But rather than being a rejection of Taylorism, Peter Drucker understood this as a continuation of the industrializing process that had led to Taylorism in the first place. The technological breakthrough of the industrial revolution was the process of turning skilled work into something analyzable—for which Drucker elevated Taylor into the pantheon of history's most significant figures.[12] The trajectory of management has involved the application of

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[9] Lyotard, 67. Recall Donna Haraway's two visions for cybernetics in chapter 2. The image of computers as offering a path to salvation and a path to damnation (with us well on our way down the wrong path) is a recurring motif in the literature. For a more contemporary take, see Jonathan Zittrain, *The Future of the Internet and How to Stop It* (New Haven, Conn.: Yale University Press, 2008).

[10] For the advantages of efficiency, see Lyotard, 62. For Lyotard's reasons why that was incompatible with scientific knowledge, see idem., 61. In brief: the criterion of efficiency "has no relevance for judging what is true or just" while "consensus does violence to the heterogeneity of language games. And invention is always born of dissension." Ibid., xxv.

[11] Ibid., 67. For a criticism, see note 15, below.

[12] "In part, Taylor has suffered because history has proved him right and the intellectuals wrong." Peter F. Drucker, *Post-Capitalist Society* (New York: HarperBusiness, 1993), 34-35. Writing in the 1990s, Drucker is a confirming instance of Forman's claim that post-1980 writers emphasize the primacy of technology over science.

knowledge to tools (in the first stages of the industrial revolution), the application of knowledge to work (through Taylor), and now, crucially, the application of knowledge to the generation of new knowledge (the chief characteristic of the information age).[13] The proliferation of scientific means was now a regular part of the social order.

According to David Harvey, this shift from an industrial system dominated by Fordist and Taylorist practices to one built around "flexible accumulation" was itself made possible by the technological innovations of the age. This system of flexible accumulation involved the geographic distribution of work, rather than keeping it centralized within a factory. It put more responsibility upon the individual worker even as it made that individual's position within the system more precarious. But rather than being a repudiation of the organized world of the Fordist factory, the regime of flexible accumulation in fact required an even more thorough system of organization; decentralization could only happen within a robust, adaptable, and pervasive system.[14] Focusing only on the spatial distinctions between centralized and decentralized organizations neglects one important point: reducing the influence of the center requires paying more attention to the system's connective tissue.

The decentralized system of industrial production was reinforced by social theories that, according to Harvey, "tell us not only to accept but even to revel in the fragmentations and the cacophony of voices through which the dilemmas of the modern world are understood." He admitted that some of this was necessary and proper as a result of the social movements that were empowering women and minorities. Their voices had to be heard and their different experiences had to be acknowledged. And yet this continual division of the world into mutually incomprehensible camps obscured the large-scale forces that affected them all.[15] Finding freedom within a totalizing system required more than acknowledging difference. Harvey's primary concern was the circulation of wealth within global markets, but a similar phenomenon could be discerned within the new space that opened at the close of the millennium: the Internet.

The history set out in this dissertation helps clarify the relationship between humans and machines in the current Internet age. The first point to note is that the vaunted freedom and creativity of individuals on the Internet was something hard won, and not a natural outcome determined by the technology. The supposedly natural course of technological development could either empower users or subordinate them within a larger system, depending on the context. It could support either centralization of control or decentralization. The particular direction in which these technologies led was something continually negotiated among diverse communities of users.

The first and third chapters examined the changing notion of "systems" as either needing to be led by trained experts (who could transcend narrow interests by analyzing the system as a whole) or as being an organic outgrowth of a laissez-faire system (with entrepreneurs serving as

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[13] Ibid., 42.

[14] Harvey, 159.

[15] Harvey mocked Lyotard's suggestion that access to data would set us on the path to justice, both for its ignorance of power and for smuggling in a "universalizing gesture" through his "pristine concept of justice." Harvey, 116-117.

more romantic heroes than bureaucrats). In terms of computer systems, this led to competing models of shared information processing (which promoted system-wide efficiency along the earlier industrial model) or fragmented one-on-one engagement (which promoted certain forms of experimentation and entrepreneurship). The triumph of the latter model in the 1970s was hardly foreordained, and the turn toward "cloud computing" suggests that the problems raised in the 1960s may be more salient than ever.

The second and fourth chapters explored the forms of work done on computers and the connection between this work and the desire for automation. The most optimistic voices in AI believed that the question of setting the boundary between the work done by humans and that done by machines was one of economics rather than of essential characteristics. The analytical problem then was how to divide up complex tasks into simple parts that could be treated as routine and therefore automatable. The tendency today is to identify hard tasks and break them up into "microtasks" for humans to solve through systems like Amazon's Mechanical Turk or the reCaptcha spam filter that simultaneously digitizes printed media. The most perceptive critics of AI recognized that the most important question was not whether machines could mimic human intelligence—for which the performance of increasingly sophisticated systems provided the basis for one answer, and endless argument concerning the essence of thought provided the basis for another—but rather whether the pervasiveness of computers and the need for flexible humans to adapt themselves to inflexible machines would somehow create a change in human sociability.

With computing power becoming cheaper and machine-readable information becoming ubiquitous, the automated practices of data processing extend farther into daily life. This proliferation is self-limiting, for even as more data exists to be processed in more ways, human attention remains a scarce commodity—reinforcing the notion that "ends" are important because logical "means" are so ubiquitous and so cheap. Amidst this swirl of information, we might agree with Lyotard that knowledge is something exteriorized, and the most relevant skill for coping in the information age is one of process—of locating information, of determining relevancy according to some goal, and of executing. An internalized knowledge, what we might call an older notion of expertise, still matters, but only insofar as these experts are "on tap and not on top."

The notion that experts could pool their resources within a knowledge community was explored in the fifth chapter. But unlike our present enthusiasm for organic, democratic online communities, critics circa 1970 feared that more powerful outsiders might exploit their collective knowledge. The open sharing of knowledge could help scholars perform interesting and disinterested analyses, but other parties with very tangible interests could use this information as well. While this data would theoretically be available to groups on the margins as well as those more established, critics recognized that this theoretical equality of access meant de facto greater access for those with means.

The cyber-utopianism of the 1990s has, thankfully, given way to the recognition that a laissez-faire Internet will be no more hospitable than a laissez-faire marketplace. A century after the excesses of the Gilded Age generated a Progressive "search for order," the giddy optimism of John Perry Barlow's "Declaration of the Independence of Cyberspace" is being replaced by a more pragmatic approach.[16] But there remains a sense that the Internet has generated fundamentally new possibilities for social life.[17] Among the most significant changes is the growth of "peer production" and "crowd sourcing," often taking place outside of traditional market operations. The paradigmatic example is Wikipedia, produced by a cadre of volunteers, and generally accurate, if often pedantic and bland.[18] Numerous celebratory books on the subject have been—and continue to be—written.[19]

It is difficult to be opposed to models of production based upon love and organic cooperation rather than upon the almighty dollar and market forces. This is amateurism of the best sort—done out of a genuine desire to do good work, with compensation coming primarily through recognition for having accomplished something significant and having sent it out into the world. The pejorative sense of amateurism typically does not apply; work done on Wikipedia, on Linux, or on other such systems tends to be of a high quality. Yochai Benkler celebrates this nonmarket production, based upon what he calls the "enhanced autonomy" provided by our computerized and networked society. He adds that "it is quite fashionable nowadays to be libertarian, as it has been for a few decades, and more fashionable to be anarchist than it has been in a century."[20] How could anyone object? Has the unholy trinity of the military, MIT, and IBM created the conditions for the withering away of the state?

Alas, no such luck. If the story of the middle of the twentieth century was that government could correct market failures, and the story of the late twentieth century was that the market could correct government failures, the story of our day, according to Lawrence Lessig, will be "libertarian failure," in which "the push to do nothing will produce not no regulation at all, but regulation by the most powerful of special interests. Or in a slogan: When it's wrong to push for regulation, only the wrong will get regulation."[21] In other words, there remains a need for the state to protect the Internet as a sphere of creative exchange.

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[16] John Perry Barlow, "The Declaration of the Independence of Cyberspace," 1996, available at https://projects.eff.org/~barlow/Declaration-Final.html (retrieved 3/17/2011).

[17] There is still a fear that the free-wheeling space of the Internet could lose its vibrancy amidst excessive regulation. See Tim Wu, *The Master Switch: The Rise and Fall of Information Empires* (New York: Alfred A. Knopf, 2010); and Zittrain, *The Future of the Internet*.

[18] See http://wikipedia.org; the author has contributed to a few entries. Wikipedia provides an excellent example of how "writing by committee" erases authorial voice and personality—which is, perhaps, inherent in its encyclopedic mission. For an attack, see Jaron Lanier, "Digital Maoism: The Hazards of the New Online Collectivism," *Edge*, 2006, available at http://www.edge.org/3rd_culture/lanier06/lanier06_index.html (retrieved 3/21/2011).

[19] Clay Shirky, *Here Comes Everybody: The Power of Organizing without Organizations* (New York: Penguin, 2008); Don Tapscott and Anthony D. Williams, *Wikinomics: How Mass Collaboration Changes Everything* (New York: Portfolio, 2008).

[20] Yochai Benkler, *The Wealth of Networks: How Social Production Transforms Markets and Freedom* (New Haven, Conn.: Yale University Press, 2006), 21.

[21] Lawrence Lessig, *Code version 2.0* (New York: Basic Books, 2006), 337-338.

Today's cyber-libertarians ignore the importance of power, which is what makes the theoretical equality of access to information lead to radically different practical capabilities for different users. The problem is one of measuring power within a networked society. Access, while important, is only part of the equation. Recalling the debates about the National Data Center in the late 1960s, critics observed that the easy availability of information eliminated the protection that came from decentralization and disorganization. With data made universally available, and with communication systems pervasive, the protective benefits of distance and decentralization have been reduced. By deciding against establishing an overarching, top-down standard, we encouraged the proliferation of databases with inconsistent policies on data protection.

The relevant question is not whether authority is to be centralized or decentralized, but how authority becomes pervasive within the architecture of the network itself. As the relationship between human and machine becomes one between human and network, the network itself becomes naturalized—something beyond our individual control and part of our institutional ecology. And yet it becomes a controlling part of that ecology. Unlike the natural world, which also maintains a constant influence on our lives, the digital environment has been ordered to achieve certain ends.

The model of networked production and the celebration of localized, bottom-up power on the Internet remains an important development. But it is one to keep in perspective. Enhanced autonomy may have reduced our ambitions to cultivate strong state organs and to empower bureaucrats to run them. This same enhancement has also extended the ability of private agencies to collect information and act upon it. We have traded the Cyclops of state power—unitary, all encompassing—and replaced it with a Hydra of market power.[22] Information may want to be free, but the ability to act upon that information remains closely guarded.

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[22] For the connections between the counterculture and the New Right, see Fred Turner, *From Counterculture to Cyberculture* (Chicago: University of Chicago Press, 2006), 175-262.

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