“Datum for its Own Annihilation:” Feedback, Control, and Computing, 1916-1945

by
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B.S. Electrical Engineering
B.A. Literature
Yale University, 1988

Submitted to the Program in Science, Technology, and Society in Partial Fulfillment of the Requirements for the Degree of

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ABSTRACT

The dissertation is examines how traditional governors and feedback devices became formally integrated into engineering and how modern control theory emerged and contributed to computers and ideas of information. The first half of the study traces four separate threads between 1916 and 1940: — fire control in the Navy Bureau of Ordnance and its contractors (the Ford Instrument Company, Arma, and General Electric); feedback engineering at the Sperry Company; communications engineering and feedback amplifiers at Bell Telephone Laboratories; power system engineering and differential analyzers in the Electrical Engineering department of MIT— each worked with a distinct concept of "system," depending on their technical and institutional goals. From these distinct ideas of systems flowed separate conceptions of feedback, stability, control, and the human role in operating technical systems.

The second part of the study begins in 1940 and covers World War II. The National Defense Research Committee (NDRC), founded by Vannevar Bush in 1940, included a division devoted to fire control, Section D-2, later called Division 7. This committee subsumed much of the pre-war work in control systems and let contracts which developed a broad array of automatic controls, systems, and theory. These included directors, predictors, radar-controlled devices, and psychological models of human operators. The NDRC's fire control work was supervised and coordinated by representatives from the four threads discussed above: the Navy, Bell Labs, Sperry, MIT, among others. Diverse notions of systems and control conflicted and fused amid the frenetic and creative atmosphere of wartime technology.

Several important contributors to early computing, including Jay Forrester, Norbert Wiener, Claude Shannon, and George Stibitz, participated in wartime work on control systems. Their ideas and experiences gave rise, through varying routes, to the large command, control, and information systems which characterized the era of nuclear standoff and remain in place today. The world these systems created, and their technological politics, contributed to the sense of alienation and powerlessness from which gave rise to critiques of technological society. Thus the cultural dilemma of *technology out of control* responded to the pervasiveness of *technologies of control*.

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PREFACE AND ACKNOWLEDGEMENTS:

As the child of a writer and an engineer, I came to this topic through a number of routes. In my work at Woods Hole Oceanographic Institution with manned submersibles with remote and autonomous robots, I began to see that "automation" was never as simple as replacing a human function with a machine. Unexpected benefits and disadvantages always became apparent after the fact. I've also always been fascinated by the animism of things, especially machines. For me, the thrill of engineering involved breathing spirit into dead matter, usually with feedback loops or computers. Literature and mythology, from Pinocchio to Robocop, Joyce to Pynchon, often articulates the issues at stake. I want to show similar forces at work in engineering practice. My study of literature sparked an interest in cultural criticism, but I've always found it frustrating: when cultural critics address technology, even military technology, they seem hesitant to go below the surface, to study the creation of machines in concert with their representations. In technological practice, however, I find a rich interplay of perception, language, and autonomy. The reader will note my deep debt to Thomas Pynchon and Gravity's Rainbow, the subject of my undergraduate thesis. While I do not discuss the book here, it lurks below the surface, and certainly helps frame my questions. Much of the control technology I discuss deals with ballistic trajectories; at one point even to counter the V-1 and V-2 missiles. Hence, one might consider the subject "gravity's other rainbow."

As Bruno Latour points out, any seemingly pure space of knowledge is always defined and supported by an extensive social network. The years I devoted to this study owe to a number of individuals and institutions. Three years of graduate school were supported by a graduate fellowship from the National Science Foundation. I conceptualized the dissertation on an extended teaching visit to the History of Science and Technology Department at the Royal Institute of Technology in Sweden. The trip gave me ample time to think and read, as well as many valuable conversations with Svante Linquist and Mats Fridlund. Several archivists were particularly helpful in uncovering material: Helen Samuels, Elizabeth Hutchins and the staff at the MIT Archives, Sheldon Hochheiser at the AT&T Archives, Michael Nash and Barbara Hall at the Hagley Museum and Library, and Marjory Ciarlante at the National Archives. The Dibner Fund and the Kelley Fund generously supported travel for archival research. John Sumida allowed me to copy material from his personal library of fire control documents for Chapter 2. Ron Kline generously loaned me copies of archival material from AT&T. Paul Ceruzzi entrusted me with his rare copy of George Stibitz's memoir. The final year of writing was greatly enhanced by a graduate fellowship at the Dibner Institute for the History of Science and Technology, which provided financial support but much more: beautiful facilities, interested colleagues, and an environment most conducive to thinking and writing. I must also thank the STS Department at MIT; its staff, especially Judith Stein, Graham Rumsay, Debbie Meinbresse, and Sarah Trautman, made five years of graduate school a daily pleasure. No small number of colleagues have endured my ravings, read pieces of the draft, and pushed me to be clear. They include: Atsushi Akera, Ed Eigen, Robert Friedel, Rebecca Herzig, Michael Mahoney, Jennifer Mnookin, Bob Post, and John Sumida. My mentors, in the form of my thesis committee, Tom Hughes, Leo Marx, Tom Sheridan, Roe Smith, and who, each in their own way, through their work, their teaching, and their examples, shaped my formulation of this study and my identity as a scholar.

I must also acknowledge the memory of James Snead, my undergraduate mentor in literature, who first suggested I might be a humanist as well as an engineer, and who directed me to MIT. His tragic early death pains me often, but his voice echoes in my work. This study is partly in conversation with him.

My brother, Joe Mindell, has been a confidante and colleague for many, many years. In a sense, he will understand this work better than anyone. He and his wife, Ossie Borrosh, saw me through the many stressful months of this work. Their wedding and subsequent move to Boston kept the absorption of dissertation writing from becoming isolating. They have tolerated my sometimes-dull single-mindedness on control systems with grace and humor.

This dissertation coincides with a happy period for my family; my brother's maturing as a doctor and a scientist, his wedding; the publication of my mother's books and broad recognition of the fruits of her many years' work; my father's moral guidance and his continued health. These are not coincidental: we owe much to mutual intellectual and emotional support (and a fair degree silliness). In that sense, this is a shared accomplishment.

Few graduate students have the luxury of a skilled and experienced engineer and an internationally-renowned writer, editor, and teacher on the other end of a fax machine at 1 A.M. For that I thank my father, who nurtured my love of machines, historical, contemporary, and imagined. What I hope is the clarity of writing in this work owes more to my mother, Dr. Phyllis Mindell, than to anyone else.

I dedicate this work to my parents, Phyllis and Marvin Mindell.
My first teachers, my first students, and my first colleagues.

SOURCE ABBREVIATIONS:

Abbreviations used in notes for archival material as follows:

ATT	AT&T Archives, Warren, New Jersey
EAS	Elmer Ambrose Sperry Papers, Hagley Museum and Library, Wilmington, Delaware
NWC	Naval War College Library, Newport Rhode Island
NWP	Norbert Wiener Papers, MIT Archives
OSRD	Record Group 227, Office of Scientific Research and Development, National Archives and Records Administration, College Park, Maryland
OSRD7	Record Group 227, Division 7 Records
OSRD7 GP	Record Group 227, Division 7 General Project Files (E-86)
RG-74	Record Group 74, U.S. Navy Bureau of Ordnance Records, National Archives and Records Administration, Suitland Maryland
SGC	Sperry Gyroscope Company Papers, Hagley Museum and Library, Wilmington, Delaware

Journals Abbreviated as Follows:

JFI	Journal of the Franklin Institute
BLR	Bell Laboratories Record
BSTJ	Bell System Technical Journal

In notes for Chapters 6 through 9, names of D-2 and Division 7 members are be abbreviated as in the original memos:

CSD	Charles Stark Draper
DJS	Duncan J. Stewart
EJP	Edward J. Poitras
GAP	George A. Philbrick
GSB	Gordon S. Brown
HLH	Harold L. Hazen
IAG	Ivan A. Getting
KTC	Karl Taylor Compton
PRB	Preston R. Bassett
SHC	Samuel H. Caldwell
TCF	Thornton C. Fry
WW	Warren Weaver
NW	Norbert Wiener

David A. Mindell grew up outside of Rochester, New York. He has undergraduate degrees in Literature and in Electrical Engineering, both from Yale University. Before coming to MIT he worked as a staff engineer in the Deep Submergence Laboratory of the Woods Hole Oceanographic Institution. There he conducted research in distributed control systems for remotely-operated and autonomous underwater vehicles for exploring the deepest parts of the ocean, and participated in more than a dozen oceanographic cruises. He developed the control system and pilot interface for Woods Hole's JASON vehicle, and has also consulted on engineering and policy for a number of industrial and research organizations (including the National Research Council). He invented a high-precision sonar navigation systems for closed-loop control of undersea robots in very deep water, called EXACT and licensed it to a company for sale and manufacture. In 1991 he entered the doctoral program in the Program in Science, Technology, and Society at MIT. From 1992-95 he was a National Science Foundation Graduate Fellow, from 1995-96 he was a fellow at the Dibner Institute for the History of Science and Technology at MIT. His research interests include technology policy (historical and current), the history of automation in the military, the history of electronics and computing, and cultural studies of technology. On July 1, 1996, becomes the Frances and David Dibner Assistant Professor of the History of Engineering and Manufacturing in the Program in Science, Technology, and Society at MIT.

If you want the truth — I know I presume — you must look into the technology of these matters. Even into the hearts of certain molecules — it is they after all which dictate temperatures, pressures, rates of flow, costs, profits, the shapes of towers...

You must ask two questions. First what is the real nature of synthesis? And then: What is the real nature of control?

Ghost of Walter Rathenau to the Nazi elite,
Thomas Pynchon, Gravity's Rainbow

People 'track' during every conscious moment...alignment processes, in which the alignment error serves as datum for its own annihilation, are forever being carried out in the familiar operations of living...The needs and nature of the interpretive and computing equipment cannot finally be separated from those of tracking controls.

George Philbrick, 1945

Chapter 1

Introduction

Sign of the Machine and Metaphor of Technology

Lewis Mumford erred when he rejected the steam engine in favor of the clock as the "outstanding fact and typical symbol of the modern industrial age."[1] The governor integrates the two, making the steam engine a powerful clock, harnessing power with precision. It works like a coxswain, directing both the crew and the boat, commanding the rowers to stroke in synchrony and the vessel to hold its course. Without synchronization each oar pushes alone, its power wandering aimless. The coxswain observes the course and makes corrections, shouts commands, integrates individual rowers, and links them, united, to the vessel. Through perception and speech the coxswain makes a machine. The Greeks called the action *kuberman*, which in Latin became *gubernator* and came to English as *governor.*[2]

Like a superego of the machine, the governor coxswains the steam engine. [*Figure 1-1: Steam Engine Governor] Two rotating balls monitor the speed, spinning faster and moving outward with centrifugal force. If the engine goes too fast, the balls swing out, and through a linkage (a channel of communication) operate a valve which reduces the steam into the cylinder. The faster the machine goes, the more the governor slows it down — *negative feedback*. If the engine runs too slow, the balls drop in and allow more steam into the cylinder, speeding it up. Ideally, the machine and the governor reach equilibrium, balance, stability. Unregulated, the engine loses speed with an increase in load. Regulated, it maintains a constant speed, regardless of load (or variations in steam pressure). Through feedback the governor speaks, transmitting low-power information to enlist the high-power machine in its own regulation, rendering it automatic.

This study examines the governor and its transformations in the twentieth century. Like the coxswain, the governor works as an observer, as a speaker, as an integrator. It integrates

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[1] Lewis Mumford, Technics and Civilization (New York: Harcourt, Brace & Jovanovich, 1934), 14. Mumford's book makes no mention of governors or regulators. His "neotechnic" phase of technology is one of electricity and light, not of control or information.

[2] The Oxford English Dictionary lists the first definition of "governor" as "A steersman, pilot, captain of a vessel." Definition number eight reads "a self-acting contrivance for regulating the supply of gas, steam, water, etc...to a machine to ensure an even and regular motion."

disparate elements, and also integrates mathematically, adding and averaging over time (frequently a human operator, like the coxswain, performs this function). It translates perception to articulation. Perception refers to how the governor apprehends and absorbs the world, from telescopes to radars. Articulation refers to speech or any complex, jointed output, particularly moving machinery (an articulated crane, for example, concatenates segments like words concatenate syllables). Technologies of control aid and automate each of these functions, enhancing perception, amplifying articulation, tightening integration.

Norbert Wiener derived *cybernetics* from the Greek word for steersman.[3] The astonishing resonance of the prefix *cyber-* in today's technological vernacular reminds us that governance remains a central issue in the public imagination of technology, both as a sign and as a metaphor. As a sign, it stands for harnessing machines to individual intentions. For example, in recent decades most increases in computing speed have gone to serve the "user interface" in a concerted effort, still only partially successful, to couple the power of the machine to human intentions. As a metaphor, governance symbolizes technology as a force that itself needs harnessing. One question continues to dog our seemingly endless progress: is technology out of control?

I use "control" to refer simultaneously to these two senses of governance: the sign of the machine and the metaphor of technology.[4] The trouble is, we have no map of this varied and complicated representation. Controls are things (rudders, buttons, keyboards, steering wheels) and we each experience the complexities of machine control (training, skill, augmentation, automation, loss of control). Technology is an idea, and we share notions of its dynamics (construction, autonomy, conspiracy, systems). Yet we lack a conceptual chain to link *technologies of control* to *the control of technology*.[5] Until now, the jump from a machine to The Machine has largely been a leap of faith. This study remakes that leap as a trajectory, a history.

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[3] Norbert Wiener, Cybernetics: Or Control and Communication in the Animal and the Machine (Cambridge: MIT Press, 1948), 11-12.

[4] *Control* is preferable to *governance* because the latter has become slightly archaic. Control, in addition to its political and psychological dimensions, finds broad application in technology and represents a genuine subspecialty in engineering (in fact, part of this study traces a conceptual shift from *regulation* to *control* in feedback technology).

[5] One might argue that these connections are merely semantic, a linguistic coincidence that the same word, *control*, finds currency in both engineering and social discourse. *Control* is no isolated case; it actually represents a broad convergence. The technical language of control systems is full of words laden with political meaning, including *governor, stability, servo* (meaning slave), and *system*, suggesting the connections are more than coincidental. The most striking aspect of the discourse of control is not that those discussing *the control of*

In this century, that history concerns not only control engineering, but also the stability of large technical systems, the secrets of military control, and the profound and gradual shifts from mechanical to electrical computers, from continuous to symbolic representation, from analog to digital electronics. R&D superseded invention, systems eclipsed apparatus, perception replaced force. The governor transformed from the simple regulator to the general information processing system, the computer.

Historical Work on Control Systems

For pre-twentieth century technology, Otto Mayr's work on feedback devices uniquely attempts to link governors to governance. He approaches the subject as "a case study in the intellectual history of technology," and explores the technological background and cultural resonance of the feedback loop before the nineteenth century. He aims "to reconstruct an instance of interaction between and society's practical technology and its intellectual and spiritual culture."[6]

Beginning with the preoccupation with clocks and automata in the sixteenth and seventeenth centuries, Mayr traces mechanical trends parallel with the scientific revolution and mechanistic philosophy of the "world machine." Leibniz, Descartes, and Boyle used clocks to demonstrate analogy between machines and the cosmos; the clockmaker's craft contributed to the production of scientific instruments. According to Mayr, the clock epitomized mechanical philosophy because "the system has a centralistic command structure; the original design, continuing functioning, and ultimate survival of the whole system depend ultimately upon a single authority... No dialogue was possible between the center and the lower branches; the flow of communication was one way — downward." Thus the clock had no feedback, it was fully deterministic (in modern parlance, open-loop) in what Mayr calls "the authoritarian conception of order." It is as though the coxswain were replaced by a phonograph, an automaton which shouted orders to the crew and manipulated the rudder on a preset mechanized schedule (programmed by a geographical map). Automata, which derived both from clocks and from mechanical astronomical simulacra, embodied this top-down direction: observation and perception did not

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technology speak in technical terms, but rather that those who design *technologies of control* speak in language so overtly political.

[6] Otto Mayr, Liberty, Authority, and Machinery in Early Modern Europe (Baltimore: Johns Hopkins University Press, 1986), xvii, 1.

contribute to the system, which integrated its elements solely by mechanical structure and not by feedback or communication.[7]

Mayr argues the British rejected the clockwork universe in the eighteenth century in favor of a liberal vision of balance and self-regulation.[8] In 1776, Adam Smith's Wealth of Nations built on David Hume's critique of mercantilism to elaborate the economic implications of self-regulation. Smith applied it three phenomena: the distribution of compensation for various occupations, the size of a nation's working population, and supply and demand. The substance of Smith's arguments were not new. His predecessors had explored all three, but not as self-regulating systems; Smith explored the idea in detail, speculating about both static and dynamic behavior.[9] Mayr attributes Smith's vision to a "liberal conception of order" which flows from balance and self-regulation. Liberal order still involves hierarchy, but a structure built on balance and not centralized control. "Thus it is possible to envision the entire universe as a network of superimposed and interacting self-regulating systems, maintaining themselves and the world permanently — despite occasional lapses — in some sort of dynamic equilibrium."[10]

Similarly self-regulating (although in Mayr's scheme, more authoritarian), Foucault's "disciplinary society" also emerged in the late eighteenth century. The icon here is no machine but Jeremy Bentham's panoptic prison. Inmates, with the omnipresent possibility of the guards' vision upon them, became self-regulating like machines; they behaved like proper prisoners because they knew they were under surveillance. Foucault sees the panopticon not just as a building but as "a type of location of bodies in space, of distribution of individuals in relation to one another." This "political technology" made the hierarchy self-governing, it allowed traditional methods of control to throw off the limitations of the physical world and vastly increase their potency. Technical advances frequently address the "weight" of the governor's functions, how much force, mass, and

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[7] Mayr, Liberty, Authority, and Machinery, 39, 118, 69, 120. Also see Silvio A. Bedini, "The Role of Automata in the History of Technology," Technology and Culture 5 (no. 1, Winter, 1964), 24-42 and Derek J. De Solla Price, "Automata and the Origins of Mechanism and the Mechanistic Philosophy," Technology and Culture 5 (no. 1, Winter, 1964), 9-23. Price (22) writes, "By the time of Shakespeare, man's ancient dream of simulating the cosmos, celestial and mundane, had been vividly recaptured and realized through the fruition of many technological crafts, including that of the clockmaker, called into being in the first place by this lust for automata." Also see Bruce Mazlish, The Fourth Discontinuity (New Haven: Yale University Press, 1993), Chapter 3.

[8] Otto Mayr, "Adam Smith and the Concept of the Feedback System." Technology and Culture 11 (no.1, 1971), 3.

[9] Mayr, "Adam Smith," 11-12.

[10] Mayr, Liberty, Authority, and Machinery, 187.

energy they require. This study analyzes how control systems connect people, machines, systems. "Speed," expresses the varying weights of those connections.[11]

I base my reading of Foucault, especially the emphasis on the visible, the articulable, and their integration, on Gille Deleuze's essay "A New Cartographer." For Deleuze, the disciplinary society emerged when vision and articulation separated as two discrete forms of the realization of power. His distillation of Foucault, that "All knowledge runs from a visible element to an articulable one, and vice versa" echoes the translation performed by the governor. At the core of this translation lurks control, which makes the system more than the sum of its parts. Extend this assemblage to a broad geography, connected by wires or networks, and it resembles a technological system, "the *diagram* is no longer an auditory or visual archive but a map, a cartography that is coextensive with the whole social field. It is an abstract machine."[12]

The mechanical governor appeared simultaneously with Smith's liberal balance and the disciplinary society. Feedback devices had been invented at least as far back as ancient Greece, including water level cut-offs (as in a modern toilet tank), pressure valves, and constant-temperature furnaces.[13] The centrifugal flyball governor for steam engines, however, became the first feedback mechanism to be widely employed by technologists and to enter the popular imagination. That device appeared in 1788, only twelve years after The Wealth of Nations, and it was invented by a friend of Adam Smith, James Watt. Well into the twentieth century, one expert estimated that ninety percent of the governors in existence were of the centrifugal type derived from Watt's invention.[14] Mayr attempts to connect Smith's model of the economy as a feedback system to Watt's governor, but Watt himself did not conceptualize his device as a feedback mechanism or a self-regulating system (although those who later improved the device did). Moreover, since only circumstantial evidence connects the governor to Smith's work on supply

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[11] Michel Foucault, Discipline and Punish, trans. Alan Sheridan (New York: Pantheon Books, 1977), 205, also see Idem., The Birth of The Clinic, trans. A.M. Sheridan Smith (New York: Vintage, 1973), Chapter 7, "Seeing and Knowing."

[12] Gilles Deleuze, "A New Cartographer," in, Gille Deleuze, Foucault, trans. Sean Hand (Minneapolis: University of Minnesota Press, 1988), 23-44. Also see "Micropolitics and Segmentarity," in Gilles Deleuze and Felix Guattari, A Thousand Plateaus, trans. Paul Bove (Minneapolis: University of Minnesota Press, 1983), 208-31, which explicitly incorporates Foucault's disciplinary society into a discussion of connections and nodes in political systems. Deleuze wrote of the transition from a disciplinary society to "societies on control," and the erosion of institutional pillars of the former (schools, churches, prisons) in favor of ubiquitous coding and the corporation, "Postscript on the Societies of Control," *October* 59 (1992), 3-7.

[13] Otto Mayr, The Origins of Feedback Control (Cambridge: MIT Press, 1970).

[14] W. Trinks, Governors and the Governing of Prime Movers (New York: Van Nostrand and Co., 1919), 3.

and demand, Mayr is content to outline the affinity of their tracks. Still, he lays out early parallels between the trajectories of the steam regulator and the self-regulating economy, governors and governance.

At least one other British philosopher made similar connections: Charles Babbage. Historians usually refer to Babbage only as the inventor of the "Difference Engine," and "Analytical Engine," unrealized early computers. Simon Schaffer, in contrast, argues Babbage's industrial philosophy (expressed in his 1832 *Report on Machinery and Manufactures*) intimately related to his calculating machines (just as Shapin and Shaffer argued for attention to Hobbes's science and Boyle's politics). Like Bentham, Schaffer argues, Babbage saw his technology as a miniature field of visibility and control, "a manufactory of numbers." "The replacement of individual human intelligence by machine intelligence," writes Shaffer, "was as apparent in the workshop as in the engines." But there was a catch. Machines deskilled workers while defining human operators as intelligent and non-mechanical, "an unresolved contradiction between stress on the subordination and thus mechanization of workers' intelligence and on the coordination and thus cerebration of their labor."[15] Put another way, in a system, did people form the unreliable "weak links," or thinking, judging "strong links"?

For the twentieth century, few have connected governors to governance. Stuart Bennett's two volume work, addressing 1800-1930 and 1930-1955, examines the history of control engineering.[16] His internal accounts leave it "to others to delve into the complex relationships between the technology and its social and economic consequences" by which he means "unemployment, economic growth, removal of degrading and onerous work, and de-skilling."[17] His second volume follows three "areas" of control technology between the world wars. These areas, process control, electronic negative feedback amplifiers, and servomechanisms, formed the basis of "classical" control theory, the set of techniques that dominated control engineering until the 1960s.

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[15] Simon Schaffer, "Babbage's Intelligence: Caluclating Engines and the Factory System," Critical Inquiry 21 (Autumn, 1994), 222. For Boyle and Hobbes, see Steven Shapin and Simon Schaffer, Leviathan and the Air Pump: Hobbes, Boyle, and the Experimental Life (Cambridge: Harvard University Press, 1985).

[16] Stuart Bennett, A History of Control Engineering, 1800-1930 (London: Peter Peregrinus, 1979). Stuart Bennett, A History of Control Engineering, 1930-1955 (London: Peter Peregrinus, 1993).

[17] Bennett, A History of Control Engineering, 1930-1960, viii.

Like Bennett, I follow separate threads between the wars, showing how they developed separately and grew together during the early 1940s. In contrast to Bennett, however, I examine control technology along axes defined by Mayr, Foucault, and Shaffer through their readings of Smith, Bentham, and Babbage: as "a conception of order," as a discipline (both epistemological and professional), and as a technology of visibility and articulation. Hence my threads correspond not to technical fields but to institutions. Laboratories, committees, and military-industrial alliances represented innovations in the conduct of technical work; they shaped approaches to problems (indeed defined the problems) and established conditions of knowledge production and authority. The organizational shifts are hardly separable from technical inventions. Where Bennett shows a unified methodology of control engineering emerging from the pressure of war, I look critically at the emergence of computers, information systems, and system engineering, for how they carried the legacies of earlier threads and the scars of their collisions. Throughout these multiple paths, however, runs the common theme of systems.

Systems

> Systems in many respects resemble machines. *A machine is a little system*, created to perform, as well as to connect together, in reality, those different movements and effects which the artist has occasion for. *A system is an imaginary machine* invented to connect together in the fancy those different movements and effects which are already in reality performed.
>
> Adam Smith.[18]

Alfred Chandler brings the coxswain and governor into the world of industrial systems when he observes "the railroad and the telegraph marched across the continent in unison." The low-power telegraph regulates and coordinates the high-power railroad. Power means moving trains along the rails, but control means moving them where you want them and when: power with precision. Chandler persuasively argues the alliance between information transmission and physical power, his oft-repeated "coordination and control," lay at the heart of industrialization in America. Managerial control marched in unison with industrial capitalism. Management techniques, organizational forms, and data processing machines steered and synchronized the

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[18] Adam Smith, "Principles Which Lead and Direct Philosophical Enquiries, Illustrated by the History of Astronomy," in Adam Smith, Works, ed. Dugald Stewart (London, 1811), 5:55-90. Quoted from Mayr, "Adam Smith," 17.

economic vessel as the coxswain steered and synchronized his rowers.[19] Professional managers acted to ensure stability, favoring long-term expansion over short-term profits. They tried also to make their systems "self sustaining," able to survive and operate independent of outside connections. They observed the system, collected data on performance, then tweaked the parameters accordingly, "for the middle and top managers, control though statistics quickly became a science and an art."[20] Feedback about the performance of an industrial system became essential for making it run efficiently, indeed for making it run at all. An ideology developed that managers could make human organizations as precisely as engineers made machines, echoing Smith's observation that a system is an imaginary machine.

Imaginary machines became real through writing. Orders, procedures, documents, and policies became the linguistic instruments of an increasingly rationalized management structure, relying heavily on internal communications. "Oral exchanges, whether face-to-face or by telephone, were idiosyncratic, often inexact, and undocumented. The ideology of systematic management demanded increasing written communication to provide consistency, exactness, and documentation" writes JoAnne Yates. Managers employed writing for perception, sending feedback up the hierarchy in the form of charts, tables, forms and reports. Down the ladder went articulation: announcements, circular letters, manuals, and company magazines. Gradually, managers mechanized and then automated these activities. Vertical filing, carbon paper, mimeographs, and the typewriter carried linguistic traffic while adding machines, punched card tabulators, and cash registers ran the numbers.[21]

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[19] In Chandler's view, management in American business arose from a problem of machine control. Railroad lines got so long (150 miles) that they grew beyond the power of an individual to keep the trains from colliding, and cooperative management procedures were created to coordinate rail traffic. In effect, Chandler's managerial controls arose to head off instability (characterized by accidents) in the rail network. Alfred D. Chandler, The Visible Hand: The Managerial Resolution in American Business (Cambridge, Mass.: Bellknap Press, 1977). Also see JoAnne Yates, Control Through Communication: The Rise of System in American Management (Baltimore, Johns Hopkins University Press, 1989) for connection between data processing technology and management technique. James W. Cortada, Before the Computer: IBM, NCR, Burroughs and Remington Rand and the Industry They Created, 1865-1956. (Princeton: Princeton Unversity Press, 1993). James Beniger, The Control Revolution (Cambrdige: Harvard University Press, 1986). See William Cronon, Nature's Metropolis (New York: W.W. Norton and Co., 1991) for an account of nineteenth-century industrialization which discusses the relationships between technical systems and natural geography.

[20] Chandler, The Visible Hand, 10, 159, 109.

[21] JoAnne Yates, Control Through Communication: The Rise of System in American Management (Baltimore, Johns Hopkins University Press, 1989) 65-94. James W. Cortada, Before the Computer: IBM, NCR, Burroughs and Remington Rand and the Industry They Created, 1865-1956. (Princeton: Princeton Unversity Press, 1993). James Beniger, The Control Revolution (Cambridge: Harvard University Press, 1986).

These methods were not limited to commercial organizations or imaginary machines. Technical systems, especially electrical ones (but also railroads, steamships, weapons) continued to require harnessing as well. Like Mayr's notion of liberal order or Foucault's disciplinary society, each contained numerous small governors, themselves feedback devices regulating a local parameter and transmitting up the hierarchy. For example, governors in electric power systems maintained the speed of turbines and generators, critical to maintaining the consistent frequency of alternating current, and voltage regulators maintained stable power levels. The system did not distinguish between "imaginary machines" and metal machines. Linking technical governance to Chandler's managerial control, Thomas P. Hughes has shown how technical managers ("systems builders") conceived their systems as seamless webs which included social, political, and economic factors in their construction and operation.[22]

This study, in a similar vein, uses the idea of system to link the machine to The Machine. This is a history of *control systems,* with all the complexity and diversity that follows from the idea of "system." While including aspects of *control theory* and *control engineering*, the term "control systems" also suggests a concrete, artifactual approach, encompassing the development of particular technologies.[23] This strategy, in effect, connects Hughes's work on Elmer Sperry with his work on electric power: I explore the confluence of feedback control with large technical systems. The engine and the clock survive in the dual imperatives of stability and synchronization.

Military Command

One further ingredient completes this frame: military command. Military organizations have always stressed order, discipline, and hierarchy. The words "command and control" became linked in the 1950s to describe the military's simultaneous direction of people and machinery. In fact, the work of the governor — observation, communication, and integration — also describes the work of the commander. Like managerial control in industry, modern military command emerged in the nineteenth century when general staffs arose to administer armies, driven in part by

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[22] Thomas P. Hughes, "The Evolution of Large Technological Systems" in Wiebe E. Biijker, Thomas P. Hughes, and Trevor Pinch eds., *The Social Construction of Technological Systems* (Cambridge: MIT Press, 1987), 54. For the connections between Hughes and Chandler, see David Houndshell, "Hughesian History of Technology and Chandlerian Business History: Parallels, Departures, and Critics," History and Technology 12 (1995) 205-224.
[23] *Control theory* refers to a body of mathematical concepts that quantitatively describe the behavior of dynamic systems. *Control engineering* is practice and technique that employs control theory to design such systems,

telegraphs and railroads. This match could be problematic: coupling command to systems imposed the constraints of machinery on otherwise flexible human organizations with potentially disastrous results. In 1914, for example, the German military, designed to march into action with clock-like precision, proved unstoppable and inflexible once set in motion, an early example of "technology out of control."[24]

Military systems, when they work well, resemble Mayr's liberal order more than the strict authoritarian order; military command does not necessarily imply rigid mechanical hierarchy. The chaos of battle always threatens communicating links. Ideally, commanders coordinate independent units capable of autonomous operation.[25] Historically, military control did not proceed by ever increasing automation, building ever higher degrees of rigidly centralized authority. Rather, it maintained a delicate balance of independence and autonomy in operating forces, from the level of systems down to individual soldiers. The militarist may dream of total control, but experience suggests flexibility.

"Seamless webs" always have points of friction, vulnerabilities, and margins. At its furthest extension, the system faces its limitations and its impotence. A military-logistics system reaches them at the front, the point of contact with the enemy, what John Keegan calls "the face of battle."[26] Technical systems have another margin in the command center, at the human operator. Here machines meet people across an anxious and unsteady boundary, the control system, the face of technology. The history of control shows systems constantly trying to extend and envelop these margins, to bring the outside inside.[27] They fail by definition.

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including the professional development of a discipline of control engineering, with its own journals, professional societies, and career tracks.

[24] Arden Bucholz, "Armies, Railroads, and Information, the Birth of Industrial Mass War," in Jane Summerton ed., Changing Large Technical Systems (Boulder, Colo.: Westview Press, 1994). Also see Stephen Kern, The Culture of Time and Space (Cambridge: Harvard University Press, 1983), Chap. 10, "Temporality of the July Crisis."

[25] Martin van Creveld, Command in War (Harvard University Press: 1985).

[26] John Keegan, The Face of Battle. For Keegan, the "face" has two meanings: the point at which an army faces its enemy, and the individual human experience of warfare.

[27] Deleuze and Guattari call this outside "the war machine," the figure which constantly escapes inclusion by proliferating networks. See "Treatise on Nomadology — the War Machine," in A Thousand Plateaus, 351-423. "The war machine's form of exteriority is such that it exists only in its own metamorphoses; it exists in an industrial innovation as well as in a technological invention, in a commercial circuit as well as in a religious creation, in all flows and currents that only secondarily allow themselves to be appropriated by the state." Not to be confused with military technology in general, or machines of war, "war machines" appear in this study in several forms: Kamikazes, parasitic noise, unstable human behavior in machine operations. Each threaten technologies of control, which respond by increasing their extension and complexity. See Michel Serres, The Parasite, trans. Lawrence R. Schehr (Baltimore: Johns Hopkins University Press, 1982) for a discussion of noise, anti-information,

After World War II, a constellation of techniques — Systems Engineering, Systems Analysis, Systems Dynamics, Operations Research and Cybernetics — sought to extend systems around various margins and to apply the war's engineering rationality to a broad range of problems. Historians are only beginning to chronicle the colonization of other disciplines by the systems constellation during the last fifty years.[28] In that story, the discipline of the history of technology may itself be the last chapter. Hughes himself avoids the pitfall, insisting only on consideration of technical and non technical components as part of the history of systems. Some, however, take up his "systems approach" as a means for making "systematic" the history of technology, "developing a systems approach to the social and historical study of technology as a strategy for integrating the history of technology into the social sciences."[29] I reject such attempts at rationalization; history flattens when designed like a machine; "system" has no stable, ahistoric essence. Most important, I avoid a systematic history because this study chronicles the very growth of the systems constellation in engineering, and its accompanying abstraction of technologies and people. Within engineering the concept of "system" has various meanings, and this study examines how it developed differently in a number of discrete environments. To attempt such a project from within the critical framework of a systematized historical approach would be merely self-justifying, and would choke on its own tail.

The brightest star (or at least the loudest) in the systems constellation was Norbert Wiener's cybernetics, a vision whose impact was exceeded only by its ambition. Despite its currency, we have little historical understanding of cybernetics. Wiener's seminal 1948 book *Cybernetics* suggests that the engineering of human/machine boundaries emerged whole, Athena-like, from the heads of Wiener and his colleagues. "I think that I can claim credit," he wrote in his memoirs, "for transferring the whole theory of the servomechanism bodily to communication

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as a kind of war machine. The state appropriates the "war machine," in the form of a military; discipline and hierarchy harness and direct it outward, across geography and toward the enemy, anxiously preventing the war machine from turning in on the state. See William McNeill, The Pursuit of Power: Technology, Armed Force, and Society Since A.D. 1000 (Chicago: University of Chicago Press, 1982) for an account of the tense relationship between states and military force, and technology's role as a mediator between the two. Also see Manuel DeLanda, War in the Age of Intelligent Machines (New York: Zone Books, 1991) for a "Deleuzian," reading of military technology. DeLanda primarily translates existing histories (including McNeill's) into Deleuzian terms. Ideas like the war machine, however, prove most valuable when they point to new and unexplored areas of research.

[28] See, for example, Lily Kay, Who Wrote the Book of Life? (forthcoming).

[29] Svante Beckman, "On Systemic Technology," in Jane Summerton ed., Changing Large Technical Systems (Boulder, Colo.: Westview Press, 1994), 311.

engineering."[30] Wiener rarely cited any work on feedback between James Clerk Maxwell's 1867 paper "On Governors" and the end of World War II, despite the maturing of multiple, layered traditions of control engineering during the period.

What was genuinely new about the human/machine relationship articulated by cybernetics? How did cybernetics affect engineering practice? What was the legacy of cybernetics? How did it relate to the other stars in the constellation? Answering these questions, or even posing them, requires a historical understanding of cybernetics and the entire systems constellation, including their relationship to automation, to military command, and to the history of computing.

These topics have been obscured, at least in part, behind the thick veil of military secrecy. It is no coincidence that the man who went down as the founding father of cybernetics, Norbert Wiener, renounced secret work after (and even during) World War II. Others did not have the freedom to appeal to popular imagination. The technology of "fire control," which led Wiener to his insights, was among the most secret technologies in the American arsenal. The records of Wiener's sponsor, the National Defense Research Committee (NDRC), were declassified only in the 1970s. By then, many participants had written their memoirs and several historians had produced authoritative accounts. Wiener worked on two of eighty projects in control funded by this group. Others addressed information theory, classical feedback control, human factors engineering, and digital computing. Similarly, the Naval Bureau of Ordnance, which oversaw fire control between the World Wars, released its pre-1925 records to the National Archives only two years ago. Later records remain in navy hands. In addition to the sources, secrecy materially affected the history as it unfolded, sometimes providing engineers extraordinary creative freedom behind its walls, other times breeding isolation and stagnation. Once removed, these walls prove a boon for the historian. Correspondents spoke frankly when federal law protected their confidentiality. Classified documents were tracked with precision as they proliferated, allowing a detailed reconstruction of diffusing ideas and technology.

World War II transformed the governor: radar automated perception, servomechanisms amplified articulation, and computers integrated systems. Seeing these technologies in this light begins to answer further questions: How did control and communication come together, and what

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[30] Norbert Wiener, I Am a Mathematician: The Later Life of a Prodigy, (Cambridge: MIT Press, 1956), 265. Also see Cybernetics: or Control and Communication in the Animal and the Machine, (Cambridge: MIT Press, 1948), 8 for a similar account and a similar claim.

was Wiener's role in the match? What drove the growth of engineering systems? How did automation change in World War II? What initiated the change? What role did feedback control play in the emergence of computers and information systems? What became of the common threads of perception, articulation, and integration?

Four Threads and Previous Science

> The social context of a science is rarely made up of a context; it is most of the time made up of a *previous* science.
>
> Bruno Latour[31]

To answer these questions, this dissertation chronicles a period of both technical and institutional change, the history of control systems in the United States from 1916-1945. The complex and continuous nature of the process makes the choice of beginning and ending somewhat arbitrary. Starting with the battle of Jutland, which in 1916 demonstrated the inadequacy of British fire control systems, control engineering became part of formal engineering, and produced control systems of increasing performance and delicacy. This period culminates in 1945, with the end of World War II and the emergence of the general-purpose digital computer. This ending, however, was itself the start of yet another period in American technology which saw dramatic developments in control systems, computers, and the role of technology in political and cultural life in America.

The first part of this study follows four discrete traditions, or threads, of technological practice during the interwar period. These traditions consist of different types of institutions, each with its own culture and technical environment, each with different controls of technology. Each worked with a distinct concept of "system," depending on technology and institutional goals: fire control in the Navy Bureau of Ordnance and its contractors (the Ford Instrument Company, General Electric, and the Arma Corporation), feedback and manufacturing at the Sperry Company; communications engineering at Bell Telephone Laboratories; and power system engineering in the Electrical Engineering Department of the Massachusetts Institute of Technology — from these distinct ideas of systems flowed distinct concepts of feedback, stability,

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[31] Bruno Latour, The Pasteurization of France, trans. Alan Sheridan and John Law (Cambridge: Harvard University Press, 1988), 19. Emphasis original.

control, and the role of the human operator. At various points, the study analyzes specific systems which either typified practice or marked significant advances.

Of course, these four traditions do not cover the entire field of control systems during the period in question. Other technical communities, in other industries, companies, universities, and government institutions contributed to ever broadening fields of control. I pay little attention, for example, to industrial process control, because it played a minor role in wartime development projects, although it was arguably more common, if less sophisticated, in industry between the wars than the forms of control I trace. Also, I discuss only briefly developments outside the United States. During the world wars, secrecy made military controls truly national, although the United States and Britain shared significant technology in wartime. Engineers in Germany and Russia also made significant contributions, although neither country defined control as a discrete category until after 1945. While the four traditions I have selected do represent the field, they were more than typical: they were central. Their people, ideas, and devices played major, determining roles in control systems during the war and after.

The four threads do more than span the field, however, they also serve as a comparison. Each had different imperatives, different organizational structures, and different relations to the broader world of technology. Individual careers proceeded differently in each case. These factors comprise what I call the "engineering culture" of each organization. The Navy Bureau of Ordnance, for example, rotated officers through technical supervision every few years, and thus had less continuity but more field experience than universities. Different perspectives also arise from differences in source material. Academic engineers progress through publication, so the published record reflects their work more than that of industrial researchers. Little contemporary documentation exists, however, for the laboratory culture of engineering students at MIT in the 1930s, so instead I rely on theses, published papers, and memoirs. In contrast, the navy installed a Naval Inspector in the factories of both Sperry Gyroscope and the Ford Instrument companies, and the inspector's reports to his superior in Washington lend a unique window into the culture of the manufacturers, but these companies did not publish their research. More such comparisons emerge in the course of the text; a balanced picture of the complex enterprise of control entails examining several worlds simultaneously.

The first tradition involves the mechanization of command in the navy. To hit distant moving targets, heavy naval guns and antiaircraft artillery required mechanical computing devices built into complex "fire control" systems. These systems not only integrated diverse perceptions, but they also centralized information and replaced human operators with ever higher degrees of automation. This tradition developed primarily in the Navy Bureau of Ordnance and its contractors — the Ford Instrument Company, the Arma Corporation, and General Electric. The Bureau of Ordnance had particular requirements for systems at sea based on tradition, training, and combat conditions, as well as their desire to control the space of battle. Control systems thus formed part of a much longer history of the military's drive to order its world. Only private industry, however, had the skills to build the demanding machines. The Bureau of Ordnance built a closed and highly-secret community of fire control contractors. While the technology grew quickly from about 1915 through the twenties, it stagnated in the decade before World War II. When, in World War II, the airplane seriously threatened the survival of the capital ship, the navy responded with a crash program in antiaircraft fire control and integrated radar into feedback loops. The unique conceptual, operational, and production demands (as well as the funding) of these closed-loop systems demonstrated the difficulty of matching technical systems to command structure.

The next tradition arose more directly from the "feedback culture" which developed out of a long series of governors and regulating devices. By "feedback culture" I mean a set of techniques, tools, knowledge, and, above all, a group of people who were skilled in applying traditional governors.[32] In the early twentieth century, the technologies of the machine age, especially steamships and airplanes, became so powerful they could slip out of human control, risking wildness and instability. The Sperry Gyroscope Company manufactured devices that domesticated these wild machines. In the 1920s and 30s, it developed an array of control and feedback devices, from autopilots for airplanes to antiaircraft systems for the army. These controls included sensors, data transmitters, centralized processors, and varying degrees of automation — corresponding to the observation, articulation, and integration of the original governor. A system was usually part of an airplane or a ship, and stability meant flying level or

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[32] The concept of "feedback culture" expands on Donald MacKenzie's idea of "gyro culture" in *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance* (Cambridge: MIT Press, 1990), 31.

sailing on course. Furthermore, Sperry developed an industrial infrastructure which could apply and manufacture advanced control technologies as they emerged from research. Through military and commercial projects, the company came to see operators as "human servomechanisms" who, like the machine itself, required regulation and taming by the system. Sperry's controls had to respond not only to the needs of the operator but also to the demands of the production line, hence the company's products tended to eschew the large, distributed systems typical of naval fire control in favor of smaller-scale automation tightly coupled to the human operator's body. By World War II, Sperry engineers could articulate a coherent vision which connected a human to a machine "to extend the functions and skill of the operator far beyond his own strength, endurance, and abilities."[33] Before World War II, Sperry spent more than two decades grappling with the complexities of what is now called the cyborg.

Tradition three, communications engineering, arose from the large system of the telephone network and industrial research at Bell Telephone Laboratories. Here telephone engineers developed not only feedback theory but expertise in signals and electronics. Bell engineers, especially Harold Black, Henrik Bode, and Harry Nyquist, conceived and formalized a theory of negative feedback to solve the practical problems of long distance transmission of voice signals. This theory contributed to a larger project of running a large network and connecting it to people. Engineers studied the shape of the telephone handset, the physics of hearing, the wave nature of transmission, and even developed nascent theories of information. Within the telephone company, "The System," as it was commonly called, meant the telephone network, and stability meant electronic amplifiers that did not oscillate. The tradition of telephone engineering allowed Bell engineers to conceptualize control systems in terms of information, signals, and noise, ideas that were critical for the rigorous understanding of computers. In 1940, because of its sophistication in electronics and in coupling humans to communications systems, Bell Labs took the lead in designing fire control systems for the armed services.

Feedback theory at Bell Labs developed in parallel with the fourth tradition, work on simulation, calculation, and servomechanisms at MIT. This academic setting had ties to the other large system of the day, electric power. The 1920s saw the connection of local power systems

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[33] "Introduction," to Sperry Company History, n.d., probably 1942. Sperry Gyroscope Company records, Hagley Museum and Library, Wilmington Delaware, Box 40.

into ever larger regional and then national grids. These networks had the potential to get out of control, becoming unstable in response to transient events such as lightning strikes or short circuits. Researchers at MIT studied relationships of the regulators and governors on individual machines to the characteristics and stability of the overall system. Vannevar Bush and his students at first built models, and then conceived more abstract "simulations:" machines as general, programmable representations of physical phenomena. The Network Analyzer, a simulation machine, and the Differential Analyzer, a mechanical calculator, had features that would appear later in real-time control systems including programmability, graphical user input, and digital switching. Both spurred important advances in control. Harold Hazen's "Theory of Servomechanisms," provided a taxonomy of feedback devices and shifted the emphasis of the feedback culture from static, steady-state performance, to dynamic, transient phenomena: that is, from *regulation* to *control*. In this context, a system was an electric power network, and stability meant that it would not fail when struck by transient events. The atmosphere of simulation and calculation that prevailed at MIT in the 1920s and 30s prepared a generation of engineers to innovate, manage, and organize the complex control and computing devices required by the second world war.

While each of these four traditions corresponds to one or more institution, they by no means proceeded in isolation. The borders between technical communities were porous and shifting, with individuals, information, and even hardware constantly moving between them. Sperry Gyroscope hired MIT professors as consultants. MIT taught a special course in control engineering for naval fire control officers. The Naval Bureau of Ordnance directed computer development at Sperry. Bell Labs had close intellectual exchange with MIT. Other factors inhibited these flows, including military secrecy, industrial concerns with patents and proprietary development, and plain narrow-mindedness. Still, the constant crossings and exchanges played a critical role as mindset and technique flowed from one institution to another. The technology of control systems developed as ongoing conversation and competition between organizations.

World War II

These connections greatly accelerated in 1940 when Vannevar Bush organized the country's research and development for war, bringing the four traditions together. The second half of the thesis covers the merging of the four traditions between 1940 and 1945. Problems of defending ships (especially battleships) against new high-speed airplanes became critical early in the European war in 1939, and the Battle of Britain underscored the difficulty of defending cities against attacking bombers. Bush's worry about the antiaircraft problem drove his attempts to form a new research organization dedicated to defense. When, at Bush's request, President Roosevelt established the National Defense Research Committee (NDRC) in 1940, it included a division devoted to fire control, section D-2, headed by Warren Weaver. Projects led by the NDRC developed a broad array of automatic controls, systems, and theory, including directors, predictors, and radar-controlled devices. Section D-2, and its successor, Division 7 (headed by Harold Hazen) were run primarily by representatives from Sperry, MIT, Bell Labs, and the navy. Again, the sources reflect the institution; fortunately, fairly complete correspondence between the individuals in this group survives. Geographically dispersed committee members described their work to each other in secret and frank memos. These sources depict how diverse notions of systems and control conflicted and fused amid the frenetic and creative atmosphere of wartime technology.

World War II marked a watershed in the history of science and technology in the United States. Initiated by what Hunter Dupree has called "the great instauration of 1940," etched into the public imagination by the atomic bomb, and codified by Bush's famous 1945 report Science the Endless Frontier, the transformations of World War II ushered in a new era of government relations with science and technology. It would last for several decades, and its effects will continue indefinitely.[34] This era included government and military sponsorship of basic research, huge sums for technology development, reliance on technical experts and their advice at the highest levels of government, and an unprecedented coupling of political decision making to large technical systems.

Historians have written much on the profound organizational changes in science during the Second World War, but they have attended less to the equally profound organizational changes in technology. Most discussions of the NDRC revolve around the atomic bomb, which transferred to the Army when it became the Manhattan Project. Even the MIT Radiation Lab, known for its work in short-wave radar, was atypical because it consisted mostly of physicists thrust into an

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[34] A. Hunter Dupree, "The *Great Instauration* of 1940: The Organization of Scientific Research for War" in Gerald Holton, ed., The Twentieth Century Sciences: Studies in the Biography of Ideas (New York: W.W. Norton & Co., 197).Vannevar Bush Science: The Endless Frontier (Washington: U.S. Government Printing Office, 1945).

engineering environment. In contrast, work in control and systems tended to be the domain of engineers. Control system engineering was a sweet problem, appealing to engineers' sense of balance and precision. Still, in comparison to the big physics problems of the day, control, concrete and unglamorous, lacked the wartime cachet of atomic physics. Historians' view of wartime research is weighted accordingly. To redress this imbalance this study emphasizes the technology of the NDRC, examining how engineers, as opposed to scientists, created their new relationship with government.

The internal workings of the fire control division of the NDRC reveal the dynamics of the wartime transformations as they occurred. The NDRC fostered control not only by letting research contracts but also by serving as a central clearinghouse for information, a medium for technology diffusion. Several of the post-war and Cold War command and control systems, as well as the epistemologies comprising the systems constellation, inherited the organizational, intellectual, and personal infrastructure of the NDRC control systems projects.

Wartime work produced not only a new role for science and technology in America, but a new conception of system and information. These ideas formed the core of possibly the most important invention of the century: the digital computer. The historiography of computers has been dominated by priority disputes and sequences of hardware. Instead I show how wartime experiences, as well as the war's demands, shaped the turn to digital techniques and the construction of digital control systems as information processors. In 1948, for example, Claude Shannon published the "Mathematical Theory of Communication" which defined the modern conception of information. Shannon worked at MIT for Bush in the late 1930s, performed contract work for the NDRC, and eventually moved to Bell Labs. Similarly, the Whirlwind computer, the first real-time control computer and progenitor Cold War command and control systems, emerged from the MIT Servomechanisms Lab, founded by a contract with the NDRC. Through the NDRC's fire control projects, methods of feedback devices, electrical power, and telephone engineering contributed to the conception of computing and information that arose after the war. Perception, articulation and integration, the legacy of the governor, shaped the rise of digital information processing as a discrete activity.

Automation and the Myth of Autonomous Technology

Technology in general has always been susceptible to mystification. Control systems, because they create "automatic" machinery, are particularly vulnerable to the myth of autonomous technology. The development of automata resembles the search for artificial life, the modern robot the autonomous mechanical human. The same holds true for the computer: observers often present its history as an intellectual search for a thinking machine. Recent scholarship that criticizes the myth of autonomous progress, however, argues instead for a vision of technology based on human choice and decision.[35] This lesson applies particularly to the history of automatic control, with its special pretension to autonomy. As we shall see, automatic control does not set machines free as autonomous agents, but rather brings them under the purview of human intention. While the autonomous vision has an undeniable metaphoric appeal and mythic significance, it coexists with another venture: the search for technological aids to human capabilities, for mechanical extensions of the body, the mind, and the social structure. Whether the operation of an individual device, the piloting of a vehicle, or the command of a large system, control involves a complex exchange of function and responsibility between operator and machine, traversing the boundary between human and artificial. The technologies that traverse these boundaries link not only machines and people, not only social and technical systems, but political power and manufactured force as well.

> Gentlemen! You can't fight in here, this is the war room!
>
> President Merkin Muffley, Dr. Strangelove

Stanley Kubrick's 1964 film captures the Cold War icon of control systems: air defenses, bomber forces, and ICBMs run from centralized locations in technological environments. Images of "war rooms" or "command and control centers" with their banks of computers, animated maps, and clean sense of order, represented the ultimate in technological progress (hence President Muffley's ironic injunction against fighting). [*Figure 1-2: SAC Command] The "control room" came to stand for the increasingly abstract nature of technical systems and the technological military. World War II produced these controls, defined the relationship between technology and government which they embody, and brought technologically mediated warfare to the popular

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[35] Merritt Roe Smith and Leo Marx, eds., Does Technology Drive History? The Dilemma of Technological Determinism (Cambridge: MIT Press, 1994).

imagination. The systems the war spawned, the command and control networks that characterized the era of nuclear standoff, remain in place today. The images these systems created, and their technological politics, contributed to the sense of alienation and powerlessness from which the critiques of technological society of Mumford, Ellul, Marcuse, and others arose. The cultural dilemma of *technology out of control* responded to the pervasiveness of *technologies of control*.

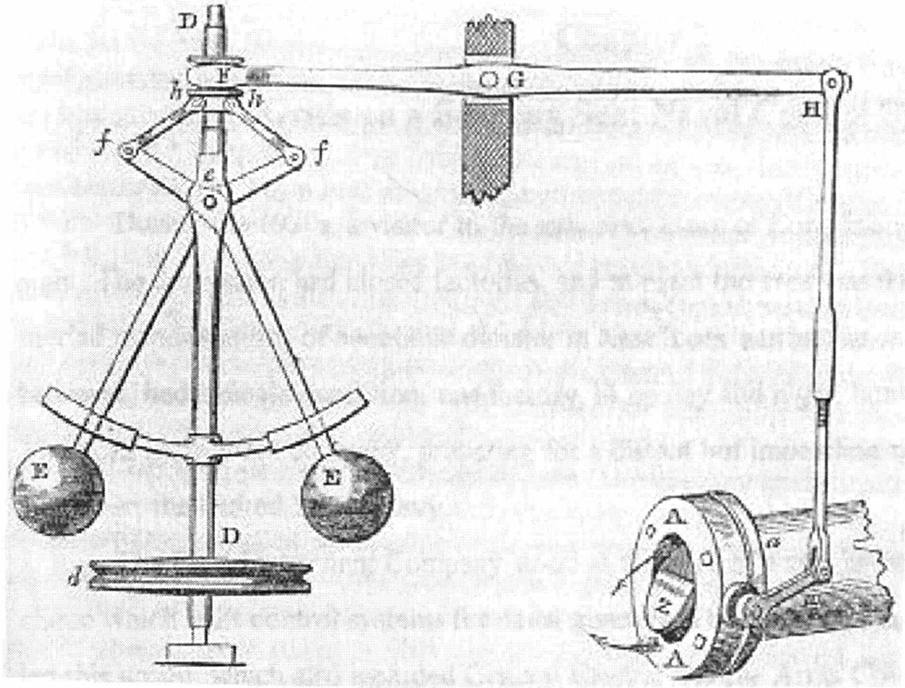

Figure 1-1: Flyball steam-engine governor. A cord from the engine crankshaft rotates pulley d. Balls E "observe," the speed, swinging outward, pulling down bar FGH and closing steam valve Z. (John Farey, Treatise on the Steam Engine (London: 1827), reprinted in Louis Hunter, A History of Industrial power in the United States, 1780-1930 Volume Two: Steam Power (Charlottesville, Virginia: University Press of Virginia, 1985), 123).

Figure 1-2: Perception and articulation: Strategic Air Command command and control system, underground Offutt Air Force Base, Nebraska. Note the prominent place of the telephone. (Claude Baum, The System Builders: The Story of SDC (Stanta Monica, California: System Development Corporation, 1981).

Chapter 2

Grids on a Swirling Sea: Naval Control Systems

During the 1930s, a visitor to the industrial areas of Long Island City would find a curious sight. The depression had closed factories, and at night the area was dark and deserted, one of the myriad manifestations of economic disaster in New York's urban landscape. Long Island city, however, had a single exception: one factory, lit up day and night, humming with production.[1] The Ford Instrument company, preparing for a distant but impending war, busily served its single customer, the United States Navy.

The Ford Instrument Company stood at the core of a specialized and secretive technical clique which built control systems for naval gunnery. The Naval Bureau of Ordnance founded and led this group, which also included General Electric and the Arma Company. The fire control clique spent the years between the two world wars advancing and perfecting the technology of aiming naval guns at surface targets and attacking aircraft. The navy had been instrumental in establishing both Ford and Arma specifically for this purpose; both companies had a single customer and a single product line. General Electric, though a vast and diversified firm, had a secret division for fire control set up at navy request.

From this closed community grew a distinct engineering culture of control systems. In this culture, a system was a set of interconnected instruments aboard a warship. Feedback was accomplished by spotting shell splashes and adjusting gunfire accordingly. Stability meant freedom from the pitching and rolling of a ship. The machines this culture produced achieved a level of automation, feedback, and human integration that dwarfed those in other fields in scope and complexity. Together, Ford, Arma and G.E., in close cooperation with the Bureau of Ordnance, built technologies of control separate from the theoretical work in feedback at other institutions, a separation enforced by military secrecy. They responded to a single customer, received handsome premiums for staying out of other fields and developed significant techniques in mechanics, feedback, and computing. The navy's strict control bread conservatism, however, and by World War II naval fire control reached the limits of its creative and technical resources.

Control systems for naval guns, or "fire control systems," aim naval guns. They incorporate a host of factors, including the range and bearing of the target, the pitch and roll of the firing vessel, wind speed, air temperature, and ballistics, they calculate the proper angle and elevation for the guns and transmit data to the gun turrets, along with orders to fire. Until about 1930, the leading edge of this technology concerned "surface fire," getting the main guns on destroyers, cruisers and battleships to hit distant targets, usually other ships. During the 1920s, and 30s, however, the airplane emerged as an offensive threat to ships of all kinds; antiaircraft became the critical problem which drove fire control in the decade prior to Pearl Harbor. On the eve of the war, the typical fire control system included a set of diverse machines: A "computer" built by the Ford Instrument company calculated the course and speed of the target based on data from rangefinders, telescopes, and, eventually, radar. A gyroscopically-controlled "stable element" built by Arma corrected the solution for the pitch, roll, and yaw of the firing ship, and ordered the guns to fire when the ship rolled to a specified point. General Electric built the data transmitters that tied these elements together, along with electric motors to turn the turrets automatically, and switchboards to program the system for different configurations.

Between 1916 and 1940 the Bureau of Ordnance worked with its control systems contractors in roughly three phases. First, from about 1916 to 1920, the US Navy struggled to catch up with the British and develop domestic engineering resources for fire control. Rapid organizational and technical change, intense effort, and the urgency of war characterized this phase, when American naval control systems went from virtually nothing to major innovations. Second, during the 1920s, the Bureau brought in, at the expense of its first supplier Sperry Gyroscope, an established contractor, General Electric. The bureau consolidated its pool of control engineering in two smaller contractors, Ford Instrument and Arma, both of which supplied exclusively to the navy. The basic system these three companies produced in 1930 would remain largely unchanged throughout World War II. The third phase, then, during the 1930s, saw numerous incremental innovations in this setup, but none that altered basic system structure. This period also saw the emergence of, and response to, a new challenge: antiaircraft. The Bureau of Ordnance and its contractors tried numerous solutions to the problem, mostly with limited

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[1] This image is drawn from the author's interview with William Newell, former Ford Instrument Company Chief Engineer, on May 12, 1995.

success. At the end of this third phase, the closed and secret community had reached its technical limits. Naval fire control could go no further without incorporating the latest work in other fields: servomechanisms, electronics, and radar.

Fire Control: Historical Background

Ever striving to measure, rationalize, and order their world, military technologists were children of the "low enlightenment," to use Ken Alder's phrase. Drill, discipline, uniformity, measurement, standardization, and above all, control, characterized the military's special brand of modernity.[2] Naval control systems evolved in harmony with this worldview; the ability to control machinery meshed with the desire to control personnel, production, logistics and ultimately the field of battle. In naval warfare the field has no landmarks, no terrain, no features. It is more akin to a magnetic field than a field of wheat: characterized only by imaginary lines of force imposed upon an otherwise smooth space. In the words of Paul Virilio, "the history of battle is primarily the history of radically changing fields of perception."[3] Naval fire control systems standardize the perception of these spaces and bring them under quantitative control. They establish a solid reference (heading, horizon) from which to encounter the enemy, known relatively as range and bearing. Polar coordinates track the enemy and bring it into the machine, where it can be manipulated. On the turbulent ocean, fire control structures the space of war and fuses the "low enlightenment" with modern technology.

In the first decades of the twentieth century increasing size and speed of turbine-powered warships, combined with advances in gun and powder technology, created a "revolution in naval gunnery." At the turn of the century, typical naval engagements took place between 2,000 and 4,000 yards. World War I battleship main batteries could shoot 20,000 yards, which increased to 34,000 yards by World War II and to 40,000 with wartime advances.[4] Firing shells and military

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[2] Ken Alder, "Forging the New Order: French Mass Production and the Language of the Machine Age," (Ph.D. Dissertation, Harvard University, 1991). Lewis Mumford argues the military is the prototypical industrial production process in Technics and Civilization (New York: Harcourt Brace Javonovitch, 1934). For the historical origins of military discipline, see William McNeill The Pursuit of Power: Technology, Armed Force, and Society Since A.D. 1000 (Chicago: University of Chicago Press, 1982), esp. Chapter 4. For a recent, synthetic view, see Merritt Roe Smith, "Introduction" in Military Enterprise and Technological Change (Cambridge: MIT Press, 1987).

[3] Paul Virilio, War and Cinema: The Logistics of Perception, trans. Patrick Camiller (London: Verso, 1989),7

[4] Estimates of gun ranges vary depending on whether one measures the distances at which fleets conducted battle practice, the distance of historical engagements, or the distance theoretically possible under ideal conditions. Elting Morison reports that Admiral Sims trained gunners to fire at 1600 yards, to prepare for battle conditions at 6,000

utility, however, are not the same thing, and the revolution precipitated an attending crisis. Shooting to great distances exceeded the ability to hit anything that far away. As ranges grew, accuracy in aiming became critical; errors of fractions of a degree, difficult to eliminate from guns mounted on a moving, pitching platform, caused shells to miss their targets altogether. Hitting at extreme ranges had important implications for naval tactics and ship design, for it allowed one's own ship to fire while safely out of range of the enemy — thus permanently supplanting the time-honored naval tradition of closing on the enemy.[5] Naval gunfire was useless if it could not be governed; It had power but not precision.

To address this problem, between 1900 and World War I the British navy built up the most sophisticated fire control technology in the world, the result of a difficult, often contentious development program and the Dreadnought-era naval arms race.[6] Only around 1915 did the Americans pay serious attention to the technology. Before examining the history of American fire control from 1916 until 1940, then, it is necessary to assess prior work in both the U.S. and Britain.

Naval Gunnery and Continuous Aim

Until the 19th century, naval gunnery had changed little for centuries. Ships fought at close ranges, firing straight into the enemy required little aiming. As ranges grew, this approach proved disappointingly inaccurate: as a ship rolls, the elevation of its guns rolls as well, thus changing the aim. Traditionally, gunners adjusted to this problem by setting their sights relative to the deck, waiting until the roll of the ship brought the target into sight, and then firing. This method introduced a time delay, called "firing interval," as the gunner anticipated the proper moment to fire to compensate for his own reaction time. The firing interval varied widely, a major cause of inaccuracy.

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yards in Admiral Sims and the Modern American Navy (Boston: Houghton Mifflin, 1942), 142. For other estimates, see Administrative History of the U.S. Navy in World War II, Volume 79, Fire Control (Washington: United States Navy, 1946), 2-3. Rodrigo Garcia Y Robertson, "Failure of the Heavy Gun at Sea, 1898-1922," Technology and Culture 28 (no. 3, 1987) 539-557. For a detailed assesment of accuracy at long ranges, see W.J. Jurens, "The Evolution of Battleship Gunnery in the U.S. Navy, 1920-1945," *Warship International* (no.3, 1991), 240-71.

[5] John Testuro Sumida, "British Capital Ship Design and Fire Control in the Dreadnought Era: Sir John Fisher, Arthur Hungerford Pollen, and the Battle Cruiser," Journal of Modern History 51 (June 1979): 205-230.

[6] For an account of the politics of technology during this arms race, see McNeill, The Pursuit of Power, Chapter 8.

Elting Morison chronicled the advent of "continuous aim" firing, which vastly improved gunnery accuracy. In 1898, Captain Percy Scott of the Royal Navy introduced two technical changes which allowed gunners to keep their target constantly in sight as the ship rolled. The dynamics of "tracking" the target depended on the quality of sighting, human reaction times, and the sensitivity of controls: in short, the matching of human to machine (similar "tracking" problems often characterize military controls). Scott increased the gear ratio of the gunner's handwheel to make a more sensitive control, allowing him to move the gun more quickly. Second, Scott added an improved telescope to the gunsight.[7] Now gunners could practice "continuous aim," which not only improved their individual accuracy, but reduced variability across human operators. In Morison's words, "where before the good pointer was an individual artist, pointers now became trained technicians, fairly uniform in their capacity to shoot."[8] Governance introduced not only precision, but regularity

Continuous aim firing greatly improved gunnery, and it was brought to the U.S. Navy in 1902 by William S. Sims, who learned it from Scott. Sims's own measure, probably exaggerated, estimated gunnery performance improved by a factor of three thousand percent by the 1905 target practice. Morison's work on continuous aim concentrates on resistance to innovation in institutional culture, but the technique was also an innovation in control. Continuous aim constructed an assemblage of man and machine with the essential features of succeeding generations of control systems. Scott's innovation made the gunner into a governor: he integrated an instrument of perception (the telescope), and a means of articulation (the handwheel).

Director Firing

Continuous aim firing implied "pointer fire," where a gun captain at each turret or gun sighted and fired his gun independently. A new method (also introduced by Perky Scott), "director fire," removed that responsibility to a centralized location or "director."[*] The director,

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[7] Naval guns already had such sights, but gunners avoided them because the guns' recoil jammed the sights against their eyes. Scott added a flexible mount which decoupled the sights from the recoil of the gun and allowed the gunners to keep their eyes constantly in the scope and on target. Morison, Admiral Sims, 83. Also see Elting Morison, "Gunfire at Sea," in Men, Machines, and Modern Times (Cambridge: MIT Press, 1966).

[8] Morison, "Gunfire at Sea." Morison, Admiral Sims, 178, 145.

[*] A note on the term *director*. In this context, *director* can mean the actual mechanism whereby the main gunnery officer aims his telescopes and transmits orders to the turrets. *Director* can also refer, however, to that officer himself. *Director fire* tends to refer to the whole system as set up with a main officer in the foretop controlling fire. Later, in U.S. Navy terminology, the mark series assigned to *directors* had a variety of meanings; the Mark I, for

aloft or on an upper deck, sighted the target with a precision telescope and calculated settings for each gun which would make their fire converge on a single point. He then communicated these sightings to the individual guns.[9] The director could fire all guns simultaneously with an electrical trigger, termed "single key" or "master key" firing. The main advantage of this system derived from "spotting." When shells exploded near the target, an officer with a telescope "spotted" the splashes, and called corrections to the gunners, adjusting up, down, right, or left to bring the next shots closer to the target. All guns firing at the same time made spotting easier and quicker. The ideal would be "straddling," wherein some splashes would be spotted short of, and some beyond, the target — thus indicating that others had hit in between. The US Navy first installed director firing in a tower ("the tops") on a battleship in 1916. [10]

Unlike in continuous aim, in director firing the guns no longer followed the roll of the ship. Now the gunnery officer in the director tower (himself the "director") waited for the ship to come to a particular point in the roll, and pressed a button which sounded a buzzer in the turrets, commanding the turret operators to fire (soon the director's trigger actually fired the guns remotely). The operators in the turrets, the "pointers," concentrated on indicators which transmitted the gun orders to them from the director; the pointers brought their equipment into line accordingly. Thus director firing achieved, in Peter Padfield's words, "a complete reversal of the 'continuous aim' Percy Scott himself had pioneered."[11] The capability for local pointer fire remained in place, however, as a backup in case the director system failed during battle.

Director fire introduced new instruments of perception. Telescopes in the director tower ("directroscopes") measured the elevation and bearing of the target to a fraction of a degree. A optical rangefinder, a device like a giant set of binoculars, determined the target's range. The bearing of the target could also be read off of a separate reading on the rangefinder or on another

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example, was really a computer, whereas the Mark VI was a gyroscopically controlled stable-element. A given fire control system could be composed of many different elements called *directors*, but each with a distinct function.

[9] Peter Padfield, Guns at Sea (London: Hugh Evelyn, 1973), 245.

[10] For an excellent first-hand summary of fire control development in the U.S. Navy 1915-1920, see William R. Furlong "Development of Fire Control," undated memo (probably 1920), William R. Furlong Papers, Library of Congress, Box 6, Folder Ordnance — American. For Percy Scott and director firing see Padfield, Guns at Sea, 246. Director firing, which allowed all guns to fire simultaneously in a complete salvo, made shell splashes easier to see at extreme ranges. Scott remained a firm believer in spotting and, while he favored director fire, he was not as sanguine about the new "fire control" technologies. Jon Testuro Sumida, "The Quest for Reach: The Development of Long-Range Gunnery in the Royal Navy, 1901-1912," unpublished manuscript, March, 1995, 8, 13.

[11] Padfield, Guns at Sea, 247. Furlong, "Development of Fire Control." Norman Friedman, US Naval Weapons, (London: Conway Maritime Press, London, 1983), 26.

rotating telescope called a "Target Bearing Indicator." Ideally, these instruments would be gyroscopically stabilized to remain on target while the ship rolled (introduced about 1920). All were connected by electrical data transmitters, which had to be accurate to fractions of a degree and rugged enough to withstand the shock of firing. With the introduction of director firing, fire control became a system —instruments of perception and articulation distributed around the ship and connected by electrical communications.

The Pollen System

Gunnery officers in a "plotting room" integrated perception and articulation. In this armored room below decks, they plotted data, calculated firing solutions, and sent orders to the guns. This process required combining two primary operations, prediction and ballistics. The time of flight of a shell could easily exceed a minute, during which time a target ship moving at twenty-five knots would move more than seven hundred yards. Officers thus had to predict (i.e. lead) the position of the target, which required knowing not only the range and bearing of the target, but its course and speed as well. They looked up the ballistics of the gun in tables, to determine what elevation would send the shell to the proper range.

Originally officers in the plotting room did this work by hand in classic naval fashion — plotting successive ranges and bearings on a chart and measuring rates and courses with a compass or protractor. The British began automating the process with a simple mechanical slide rule (a "dumaresq") and a "range clock," or "Vickers Clock." The dumaresq calculated the rate of change of range and the rate of change of bearing to the target, from which prediction could be calculated. An officer set the dials on the Vickers Clock with an initial range and set the clock's speed with the rate of change of range as calculated by the dumaresq. The clock then indicated on its dial how the target range changed as time progressed. For example, if one determined a target was five thousand meters away, and its range was changing five hundred meters per minute, the clock would read 5,500 after one minute, 6,000 after two, then 6,500 and so on into the future. If, over time, ranges indicated on the clock did not match the rangefinder's actual observations of the target, then the estimated course and speed of the enemy were incorrect and the estimates needed to be adjusted or "tuned."[12] This tuning, a feedback process, would be repeated until it converged on a correct solution for course and speed, which would then produce an accurate prediction.

This setup, however, had a critical flaw.[13] The rate of change of range was itself rarely constant, even if the target remained on a constant heading. If two ships headed straight toward each other, then obviously the change of range would be constant as the sum of their speeds, or the difference of their speeds if they headed directly away from each other. If the two were offset by any distance, or on different headings — a far more likely scenario — the rate of change of range would vary in time, as shown in Figure 2 [*Figure 2-1: Rate of change of range] The Vickers Clock ran at a fixed speed, so it could not track continuously. The clock would have to be constantly adjusted, also by hand, to read the proper values.

Arthur Hungerford Pollen, an English entrepreneur and managing director of a newspaper equipment manufacturer, understood the problem of continuously changing range rate. He invented a fire control system to solve it and struggled to bring "scientific" fire control to the Royal Navy.[14] His system predicted the future position of the target based on an accurate and continuously updated derivation of the rate of change of target range. An automatic printer plotted the data on paper.

The Admiralty, and especially its gunnery officers, were by no means predisposed to such radical automation of their plotting and fire control procedures.[15] After extensive trials and much debate, in 1913 the Admiralty officially adopted a less-automated system, the Dreyer Table, designed by a gunnery officer, over Pollen's. John Sumida explains the Admiralty's rejection of the Pollen system in part by the threat the highly-automated Pollen system posed to traditional gunnery officers: "Fire control was regarded as the special preserve of gunnery officers, who had been able to establish themselves as an elite in the late 19th and early 20th century with the onset of the gunnery revolution. Their high standing had been derived in large part from the importance

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[12] Padfield, Guns at Sea, 225. Also see John Testuro Sumida, In Defence of Naval Supremacy: Finance, Technology, and British Naval Policy 1889-1914 (London: Routledge 1989), 74-75.

[13] Sumida lists three major weaknesses in the dumaresq/Vickers clock combination: poor visibility could obstruct the necessary range readings, the range rate itself was inaccurate, and data had to be transferred manually out of the Vickers clock, because its output was too weak to drive a data transmitter, "The Quest for Reach," 15.

[14] Sumida, In Defence of Naval Supremacy. It is worth noting that Sumida follows Pollen's terminology and uses "change of range rate," to mean "the rate of change of the range" (velocity). For clarity, I avoid "change of range rate" because to the modern reader it might also suggest "the rate at which the change of range is changing" (acceleration), thereby causing confusion.

[15] Sumida, In Defence of Naval Supremacy, 133, *idem.*, "The Quest For Reach," 28.

of human marksmanship. Pollen's work...reduced the significance of human intelligence, training, skill, and the courage that was required to perform complicated tasks while under fire."[16]

Comparing Pollen and Dreyer's equipment is complex and contentious, especially because Dreyer eventually incorporated several Pollen innovations. Still, both established a fundamental reliance on feedback: a cycle of correction and recorrection through the machine, aimed at estimating the critical values of target course and speed. These values themselves formed the foundation of another feedback loop, that of firing and spotting. In fact — and this point was much misunderstood during the debate on these systems — the computing mechanisms did not seek to provide a complete solution, but rather to factor out the relative motion of firing ship and target.[17] Automatic control compensated for the relative motion of the ships to cancel it out of the gunnery spotting feedback loop. Thus, "the target could be regarded as motionless," in Pollen's words, "exactly as if the firing ship and target were standing still."[18]

The dominance of British fire control began to erode in May, 1916 at the battle of Jutland. In this, the largest Naval battle of World War I, British and German main battle fleets engaged at ranges from 14,000 to 18,000 yards. The British, with all their equipment, achieved less than three per cent hits — an embarrassing and potentially scandalous performance. The conditions of the battle exposed the fatal weakness of the Dreyer table: its inability to account for changes in the rate of change of range. The single British ship equipped with a Pollen system turned in the best shooting accuracy. "Never has the potential power of naval force" Pollen wrote of Jutland, "stood in so sharp a contrast with its actual efficiency in war." The Admiralty recognized their mistake; a subsequent investigation privately criticized the Dreyer table as inadequate.[19] By this time the Royal Navy had a great deal of practical experience with fire control equipment but an elegant, workable solution would come only in the 1920s. By then, however, Americans had drawn their own lessons from British technology and from Jutland and developed competing technologies.

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[16] Sumida, <u>In Defence of Naval Supremacy</u>, 333, 217-219. Padfield, <u>Guns at Sea</u>, 226

[17] "Target range," i.e. that calculated by the system, was quite different from "gun range," which included corrections for various ballistics factors (as well as wind) and actually set the guns. Pollen himself believed that only spotting shell splashes, and not rangefinding equipment, could determine gun range. Without fire control, however, spotting would have to find both gun range and the target's motion simultaneously — literally shooting at a moving target.

[18] Arthur Hungerford Pollen to Lieut. Reginald E. Gillmor, April 14, 1916. Pollen Papers, courtesy John Sumida. Padfield, <u>Guns at Sea</u>, 226.

[19] Arthur Hungerford Pollen, in <u>Land and Water</u>, August 17, 1916. Sumida, "The Quest For Reach," 36.

American Fire Control Before World War I

Despite its problems, in World War I the Royal Navy's fire control was more sophisticated than United States Navy. The Americans were beginning to make their own progress, however, and Jutland provided a window for new ideas. In 1905, a Fire Control Board within the US Navy, which included Admiral Sims, had formally advocated director firing. The board recommended "a system of fire control and a system of information," where "system of information" meant data transmission between fire control operators. The board's report laid special emphasis on communications and on operators' interaction with machinery:

> It is considered as an essential that the system of communicating ranges and deflections to the guns be *rapid* and *continuous* and that the ranges and deflections be made to appear on an indicator directly *in front of* the eyes of the sight setter when he is at his station...Means are essential for communicating regular battle-orders direct from the fire control stations to each gun controlled therefrom.

In the years before World War I, with a boost from another Fire Control Board in 1910, fire control in the US Navy began to take shape. The Naval Academy began teaching fire control in 1911.[20]

American plotting rooms contained a plotting board, instruments for reading wind, speed, and course corrections, and means for sending data to the turrets (often voice tubes or telephones). Officers manually plotted data on a chart as it came from the aloft director. As data points accumulated, the officers read off target bearing and speed. They visually averaged the data, eliminating spurious readings and errors and carefully observing trends. Then target bearing and speed were entered into instruments essentially similar to the British dumaresq plus Vickers clock arrangement.[21] From the output of these calculators, the officers in the plotting room figured firing solutions and sent them to the guns. In contrast, the more automated British system conveyed orders from the director in the foretop to the guns, with the plotting room playing a supporting role.[22]

Officers in the plotting room on American vessels served as "integrators" in both senses of the word: (1) gathering data from distributed sources and integrating it into a center of calculation and (2) integrating mathematically, averaging and smoothing the data with their eyes to eliminate

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[20] Fire Control Board quoted in Friedman, US Naval Weapons, 28. Wilbur R. Van Auken, Notes on a Half Century of United States Naval Ordnance 1880-1930 (Washington: George Banta Publishing, 1939), 20.
[21] Norman Friedman, U.S. Naval Weapons: Every Gun, Missile, Mine and Torpedo Used by the U.S. Navy from 1883 to the Present Day (London: Conway Maritime Press, 1983), 27.

error. This role reflects a "strong link" role of humans in automation: they make subtle and high-level judgments beyond the capability of the machine. Where a "weak link" vision seeks to replace people with automation, a "strong link" approach seeks to improve communications, between operators and between operators and machines. As the 1905 Fire Control Board had reported, "a method which would permit orders being received by the sense of sight rather than hearing would be preferable," but only telephones and voice tubes were available at the time.[23] Voice was an imperfect medium, especially compared to instruments which made information visible. To link its strong-link operators, the board chose writing over speech.

With several years of neutrality before entering World War I, the American navy had time to catch up in technology. Between 1914 and 1917 it modernized its fleet, brought its fire control closer to British standards, and sowed seeds for its own control technologies. Before 1916, the US Navy did not generally recognize director fire as a significant improvement over individually controlled pointer fire. Intelligence from the war in Europe, however, began to change that view, and the battle of Jutland drove the point home.[24] While the British still had superior equipment, they also a more rigid hierarchy and a more traditional institutional culture whose resistance to outsiders (like Pollen) hindered innovation. The Americans' relative backwardness made them more receptive to new solutions. A small group of line officers within the Bureau of Ordnance became the driving force behind fire control in the United States. With no expertise internal to the Navy, they had to import technical skill from outside the service (and outside the country), and maintain control of that expertise. To establish independence and leadership in fire control, BuOrd fused naval experience with American manufacturing.

The Bureau of Ordnance

In the US Navy, the Bureau of Ordnance had primary responsibility for fire control systems, with its Fire Control Section. This group, led by naval line officers who were not necessarily engineers, specified equipment, let contracts, supervised manufacturers, and oversaw the installation and operation of fire control in the fleet. In effect, these officers formed a technology agency for naval control systems. Before considering the navy's efforts to control and direct this technology, however, we must first understand something of the organization of the

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[22] Furlong "Development of Fire Control." Friedman, U.S. Naval Weapons, 27.

[23] Quoted in Friedman, US Naval Weapons, 28.

navy and how technical responsibility divided up among its bureaus. Who were the officers in charge of fire control? What was their background and experience? What were their goals?

A modern navy consists two units: operating forces, "the fleet," which sail the seas and fight battles, and a logistics or support structure, which builds, staffs and supplies the fleet. By the mid-nineteenth century, naval technology had advanced to the point where the skills (both personal and institutional) required for logistics largely differed from those required for operations. In the words of Matthew Fontaine Maury "shipbuilding and ship sailing are entirely distinct and separate professions."[25] In 1842 Congress established "the Bureau system," of naval organization, dividing the support functions of the navy among five bureaus: Yards and Docks; Construction, Equipment, and Repair; Provisions and Clothing, Ordnance and Hydgrography; Medicine and Surgery. In 1862, the structure adjusted to eight Bureaus from the original five adding, for example, the Bureau of Steam Engineering. Hydrography moved into the Bureau of Navigation, thus creating the Bureau of Ordnance. With few changes (e.g. the addition of the bureau of aeronautics), the bureau system remained largely the same until 1947 — minor alterations considering the extent of technological change during those hundred years.

The responsibilities and authority of each bureau, known officially as "cognizance," were hotly contested. Originally, the Secretary defined cognizance, but in 1909 the Attorney General ruled that only congressional appropriation could define the bureaus' assignments. In theory, the bureaus reported to the Secretary of the Navy, but in practice they lobbied congress directly for their money, bypassing the Secretary. The Bureaus themselves had a great deal of discretion allocating funding — a significant source of power. Operating forces of the navy then, had very little say over their ships, weapons, and supplies. The bureau system was never popular with the navy's front-line forces.[26]

The Bureau of Ordnance (BuOrd for short), however, lived in a slightly more rational world. The other Bureaus (save one) consisted of officers from the navy Staff Corps, often specialized engineering professionals. The Bureau of Ordnance, however, employed line officers, men from the fleet with gunnery credentials. This arrangement brought field experience into

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[24] Van Auken, Notes on Half a Century of Naval Ordnance, 23, 27.

[25] Quoted in Julius Augustus Furer, Administration of the Navy Department in World War II (Washington: United States Navy, 1959), 196.

[26] Furer, Administration of the Navy Department, 205-6.

harmony with weapons development and procurement. It also had a disadvantage: the officers of BuOrd, who rotated between sea duty and staff jobs, did not have engineering or management expertise in the design and construction of weapons as did Staff officers in other bureaus (in, for example, shipbuilding). The officers of BuOrd, then, while knowledgeable of operations, had to rely on outside sources of technical skill to implement their ideas.

BuOrd had cognizance over developing and building the navy's guns, as well as production and procurement, including the massive amount of ammunition required in wartime. Internally, the bureau had a number of divisions, including technical, research, industrial, maintenance and operating, and civil. These divided further into sections, including guns, turrets, torpedoes, armor, gun mounts, mines, powder, contracts, patents, and personnel. BuOrd had its own facilities, including the Naval Ordnance Laboratory, the Naval Gun Factory (at the Washington Navy Yard), the Naval Proving Ground (in Dahlgren, Virginia) and a number of other factories (many run by private contractors). These assets contained significant expertise in traditional naval crafts; for new technologies the Bureau depended on private industry for much of its design, development, and production.

To incorporate private technology into the fleet, a Naval Inspector of Ordnance (NIO), usually a gunnery officer on rotation, monitored production at each manufacturer and served as a liaison between the Bureau and the contractor. In the words of a BuOrd Chief, "A good inspector must combine the functions of confessor, advisor, stimulator, and if need be, a spur. In simple words, it is up to the inspector to put the Prod into Production."[27] The naval inspectors embodied the technical exchange between the government and its contractors: they brought requirements and specifications from the navy to the company, and sent design and production data in the other direction. Thus the naval inspectors provide a rich resource for the historian; their correspondence with the bureau paint a detailed picture of industry's role in naval technology. The NIO was by no means a neutral observer; he directly represented the all-powerful customer and could halt payments for unacceptable work. Still, the NIO, stationed in the factory, participating in its daily life, and working with the problems of production, often articulated a more honest appraisal of working conditions than the company staff, who always tried to represent themselves in the best possible light to the navy.

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[27] Admiral William H. P. Blandy, quoted in Furer, Administration of the Navy Department, 326.

During World War I, the U.S. Navy grew from 50,000 people to more than 500,000, and the Bureau of Ordnance grew by a similar factor of ten. The fire control section grew from one officer and one clerk to seven officers and eight support staff. Commander F.C. Martin headed the section until July 1917, when he was replaced by Commander Wilbur R. Van Auken, who remained until nearly the end of the war when Commander William R. Furlong took over.[28] Because of these very rotations, the bureau had to rely on stable companies to provide continuous expertise in the new technology.

In the early twentieth century, steam and steel technologies matured and the reverse salient of naval warfare shifted to gunnery. It also saw the golden age of the battleship, and naval strategy and tactics revolved around its striking power. Thus the Bureau of Ordnance and its professional "gunnery officers," stood on the forefront of the Navy's mission. Despite the rise of submarines and aircraft, until World War II gunnery remained an elite occupation, colloquially known as the "gun club," both highly technical and intimately involved with fighting the enemy.[29] To an increasing degree, gunnery officers in the plotting room actually fought naval battles — they aimed the big guns and carried the prestige of the marksman. Within gunnery, powder, shell, and gun technology matured, and the reverse salient shifted to fire control.

Sperry Enters the Field

The Sperry Gyrocompass

In 1909 and 1910, inventor Elmer Sperry built a gyrocompass which eliminated problems of the magnetic compass by pointing to "true north" instead of magnetic north.[30] The spinning gyro sensed the rotation of the earth and aligned itself to the earth's rotational axis without regard to magnetic fields. By 1911, Elmer Sperry completed a gyrocompass and tested it for the U.S. Navy aboard one of its first dreadnought battleships, the <u>Delaware</u> (also the first US ship to

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[28] U.S. Bureau of Ordnance, Department of the Navy, Bureau of Ordnance, <u>Navy Ordnance Activities: World War 1917-1918</u> (Washington, 1920), 151.

[29] Arleigh Burke, for example, a World War II hero and post-war Chief of Naval Operations, served as gunnery officer aboard the <u>Arizona</u> (BuOrd's gunnery officers first took charge of nuclear weapons when they entered the navy in the late 1940s). David Alan Rosenberg, "Officer Development in the Interwar Navy: Arleigh Burke —The Making of a Naval Professional, 1919-1940," <u>Pacific Historical Review</u> (1975).

[30] For problems of the magnetic compass, see British Admiralty, Technical History Section, "The Development of the Gyrocompass Prior to and During the War," October, 1919, Pamphlet TH 20, 3. Courtesy Jon Sumida. For early gyrocompass history in Germany, see Donald MacKenzie, <u>Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance</u> (Cambridge: MIT Press, 1990), 34-35.

implement director firing). The Delaware trial defined the personal core of the Sperry Gyroscope Company and its close ties to the navy. Hannibal Ford, Sperry's Chief Engineer, supervised the installation. At the Boston Navy Yard, Ford met two enthusiastic young men who had responsibility for the trials. Ensign Reginald E. "Foxy" Gillmor, originally from Menominee, Wisconsin, had graduated from the Naval Academy in 1907. Electrician petty officer Thomas Morgan, "a square hewn-country boy from North Carolina," joined the navy after high school.[31] Both had good electrical skills and they backed Sperry's new device. In 1912, Gillmor and Morgan left the navy and joined Sperry Gyroscope; both would eventually become president of the company. The gyrocompass performed well on the Delaware, and the navy immediately ordered six units from Sperry Gyroscope for dreadnoughts and submarines.

The Sperry gyrocompass was an observer: it sensed the heading of the ship and displayed it to the crew, but articulated no further motion. It corrected errors arising from the course, speed, and latitude of the ship — operations similar to those involved in fire control calculations. A "follow up" or "phantom," a feedback device, which automatically tracked the movement of the sensitive element of the gyro, amplifying its signal without drawing power and affecting its accuracy. The follow-up servo could drive any number of "repeater compasses," located anywhere around a ship and wired to a single central gyrocompass, exactly replicating its reading.[32]

With the navy's help, Sperry developed his invention into a practical system and a viable commercial product, the Sperry Gyrocompass for ships, installing it on more than 700 ships worldwide by 1920.[33] The product brought Sperry Gyroscope not only commercial success, but also valuable marine engineering experience and a pattern of contact and technical exchange with Naval officers. The Sperry Gyrocompass, with its ability to track "true north" as opposed to the unreliable "magnetic north," had obvious advantages for piloting and navigation at sea. Both the merchant marine and the navy recognized these benefits, but the latter had an additional, unique interest in the device. Repeater compasses, because they transmitted a solid heading reference to various points in the ship, could aid in gunlaying. The gyrocompass, with its stable heading and

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[31] "Sperry: The Corporation" Fortune, May, 1940.
[32] Hughes, Elmer Sperry, 146. Patent Nos. 1,255,480; 1,296,440.
[33] Hughes, Elmer Sperry, 241.

connected repeaters, formed the reference point for a fire control system. Repeater technology could do even more.

Ever since the Delaware trial, Elmer Sperry had been thinking about fire control. The same system which transmitted compass readings from the master gyro to its repeaters could also transmit fire control information from the director to the plotting room and the turrets. By 1914 Sperry introduced a system of repeaters, based on his gyrocompass repeaters, to transmit data for fire control, communicating target bearing and turret train to and from the plotting room.[34] Gyro repeaters replaced unreliable and error-prone voice-tube communications with a system of visual data.

Technology Transfer from England

Sperry's early fire control work did not compare to British technology in sophistication. Fire control novice Sperry Gyroscope, however, had a valuable source of intelligence in England which allowed it to incorporate the Royal Navy's experience. In 1913, Reginald Gillmor and Tom Morgan went to London, were they founded Sperry Gyroscope's British subsidiary, the Sperry Gyroscope Company Ltd. From England, Gillmor corresponded regularly with Elmer Sperry. The inventor frequently asked Gillmor's opinions, and he would write detailed reports which Sperry distributed to his naval contacts. In 1916, Gillmor reported about Jutland and the role of the Sperry Gyrocompass in the engagement.[35] Gillmor proved so knowledgeable in these matters, and had developed so many contacts within the Admiralty, that in April 1917 he returned to the navy as Flag Secretary on the staff of Admiral Sims, who served as the US Navy liaison in London.

Gillmor conducted a technical survey of European fire control systems and sent it to Sperry. Gillmor was also closely in touch with Arthur Pollen and sent Sperry details of the Englishman's work and opinions on naval strategy and tactics. Gillmor felt Sperry Gyroscope should license and manufacture Pollen's system. A twenty-four page memo from Gillmor, dated August 1, 1916, compared the tactical and technical issues of British versus American fire control with a clarity unsurpassed in any BuOrd documents of the time. Sperry passed the memo on to

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[34] Elmer A. Sperry (hereafter referred to as EAS) to BuOrd, March 14, 1914. RG 74 National Archives and Records Administration, Suitland, Maryland (hereafter referred to as RG 74) E-30 Box 587 Subject 29758 Folder 1-50.

[35] Gillmor to EAS, June 16, 1916, and EAS to J. Strauss, Chief of BuOrd, July 17, 1916, including excerpts from Gillmor's letter. RG 74 E-30 Box 586 Subject 29758 Folder 101-150.

the bureau.[36] Through what Elmer Sperry called "channels which insured their freedom from censorship" Gillmor transferred fire control technology from the Royal Navy to Sperry Gyroscope.[37]

The Sperry Fire Control System

Gillmor's communications, combined with Sperry's own contacts, gave Sperry Gyroscope an initial lead among American companies adapting the technology to the U.S. Navy.[38] In 1916 the company introduced a complete system, the "Sperry Fire Control System," with a central plotting machine, the "Battle Tracer." The Sperry Battle Tracer plotted on paper the observed and predicted course of both the "own ship" and the target, allowing a gunnery officer to read the bearing for his guns to fire [*Figure 2-2: Battle tracer]. Although less sophisticated than the Pollen system in its firing solution, the Battle Tracer used more advanced electrical transmission. The mathematics were not complicated, but the inputs came from varying sources and in varying forms. The Sperry Battle Tracer and Fire Control system integrated a diverse array of factors into a single graphical representation of the field of battle. The U.S. Navy liked the Sperry system because it was lighter, less bulky, and simpler than Pollen's, and also of domestic origin.[39]

A set of 1916 Sperry Gyroscope bulletins describes the fire control system in detail. The Battle Tracer itself formed but one part of a larger set of equipment "designed to concentrate the control of all gunfire at one point, causing the entire battery of the ship to operate as a single unit." The system included rangefinders and target bearing telescopes to transmit range and bearing of the target from the foretop to the plotting room. [*Figure 2-3: Battle Tracer system diagram]. The ship's gyrocompass provided own ship's heading as a reference to resolve other readings into "true course" bearings. Two "Revolution Transmitters" logged the rate at which the ship's propellers turned; a "Revolution Converter" averaged these rates, corrected them for tides and currents, and sent an estimate of own ship's speed to the Battle Tracer. These instruments of

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[36] "Mr. Gillmor's Report — Result of his Investigations," RG 74, E-25, Box 586, 29758/110.
[37] EAS to Lieut. Comdr. F.C. Martin, August 31, 1916. EAS Papers, Hagley Museum and Library (hereafter referred to as EAS Papers), Box 32, Lt. Logan Cresap Fire Control Correspondence folder.
[38] Sperry was also a friend of Admiral Bradley Fiske, credited with an early invention of director firing. The two men had patented a device for "automatic gun pointing," in 1914. Patent no. 1,238.503.
[39] W.R. Auken to EAS, March 4, 1917. EAS Papers , Box 32, W.R. Auken folder.

perception brought data into the plotting room; other devices allowed the gunnery officers to articulate commands to the gun crews in the turrets.[40]

In the plotting room, four motors moved the Battle Tracer across a plotting table to draw own ship and target ship courses over time. These motors corresponded to four variables: own ship's heading, own ship's speed, target bearing, and target range. A "Contact Clock" sent a signal to the Battle Tracer once a minute to depress its pens to paper and plot the current data (this clock provided only the time baseline for the system to mark on the plot; it did not perform any of the ranging or prediction that clocks performed in the Pollen system). A manually operated protractor-like device attached to the tracer to extrapolate target course into the future to calculate the lead necessary to compensate for the projectile's time of flight. The gunnery officer in the plotting room read the firing solution off the instrument and manually entered turret train (i.e. turret rotation) into the Target Turret Transmitter by a hand crank. This value then appeared on the Turret Train Receiver in the turret itself, where the operator saw desired position in the form of an arrow, and actual turret position as a cartoon of the turret on the dial, "a visual means of comparing the designated train with the actual train of the turret." His only job, then, was to "follow the pointer" and bring the two dials into coincidence — the human operator thus closed a feedback loop, moving the turret with visual feedback. A Turret Train Transmitter, which meshed with the gear teeth on the rack that turned the turret, sensed actual turret train. This data went back to the gunnery officers in the plotting room, as a "return check" to visually ensure the turrets were properly trained before firing — another feedback loop.[41]

The Battle Tracer was a literary technology — it not only calculated and integrated on paper, but also recorded the battle in writing, "to make a permanent chart record of the conditions throughout an entire maneuver." Recording furnished "practically a bird's eye view of the engagement and maneuvers involved," and it monitored the performance of subordinates, "carelessness on the part of any of the operators of the fire instruments...are immediately shown."[42] Sperry's pamphlet emphasized the units' integration:

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[40] See also Patent nos. 1,356,505, "System of Gunfire Control;" 1,215,425, "Plotting-Indicator;" 1,296,439, "Multiple-Turret Target-Indicator," (all Elmer A. Sperry).

[41] "Turret Control Equipment," Bulletin 303, 1916. Sperry Gyroscope Company Papers, Box 33.

[42] "The Sperry Fire Control System," Bulletin 304, 1916. SGC, Box 33. See also Patent no. 1,293,747, "Battle Tracer."

> Certain of these units or subdivisions of this Fire Control System may be installed to advantage without the rest of the system, but to obtain the maximum operating efficiency it is desirable to install the entire equipment, as the units are closely related and the results obtained from any one unit become more valuable when combined with the results that are contributed by other units.[43]

This passage presents an early example of what would later be called systems engineering, emphasizing the interconnection of the components and their synergistic efficiency when operated in concert. Still, Sperry's sense of competition rings in this statement. In the development of "systems thinking," integrating systems could be as much a marketing strategy as a technical one. "The individual components will work," the company seemed to tell its customers, "but not nearly as well as if you buy the whole thing." In 1916 this threat became real, as competitors built equipment to fit into and replace the Sperry system.

In early 1915, Sperry Gyroscope installed a Battle Tracer aboard the battleships Utah, New York, and Arkansas for testing at no cost to the government and they employed the tracer during maneuvers in 1915-16. Since the device primarily tracked the course of its own ship, with only limited ability to track a target, the fleet found it more useful for navigation than for fire control. In general, they saw it as a "dead reckoning instrument...of the greatest assistance as an aid to navigation."[44] The commander of the New York noted "the apparatus requires an attendant to keep it from running off the table under some circumstances," and that it was unlikely to remain in operation during a battle due to the exposed nature of its optical instruments. Still, the New York did use the device for fire control exercises, and was able to obtain from it the course and speed of a target ship.[45] The general consensus among the ships employing the Battle Tracer was that it had potential but needed more engineering. The navy was less interested in the Battle Tracer itself than in the transmission instruments, put together into a "follow-the-pointer in train" system. By the end of the war, the navy bought only 20 Battle Tracers for its battleships, but several hundred Target Bearing Transmitters, Target Turret Indicators and transmitters, and Turret Train Transmitters.[46]

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[43] "The Sperry Fire Control System," Bulletin 304, 1916. SGC, Box 33.

[44] Capt. Albert Gleaves (USS Utah) to Secretary of the Navy, and BuOrd, June 1, 1915. RG 74 Box 587 Subject 29758, Folder 1-50.

[45] Lieut. Palmer (Commanding Officer, USS New York) to BuOrd, July 14, 1915. Commander, New York to Commander, Battleship Division 6, August 2, 1916. RG 74 Box 587 Subject 29758, Folder 101-150.

[46] U.S. Bureau of Ordnance, Naval Ordnance Activities, 152.

Sperry system dealt only with target bearing and guns' train (or rotation), not with target range or gun elevation. In 1915 the commander of the New York suggested developing "a range keeper which would utilize the course and speed of the enemy," and the following year again recommended "fitting to the tracer an automatic range projection feature which will show the components of motion in the line of fire, or of ship and target, also the components at right angles to the line of fire for deflection." Gillmor suggested a similar device in his 1916 memo to Elmer Sperry.[47] The bureau not only agreed with these suggestions, it had already started a program to build this new "range keeper," in a program at a spin-off company of Sperry Gyroscope that would grow to dwarf the company's efforts in fire control. This device emerged directly from Sperry's Gyroscope's fire control work when, in 1914, the company's chief engineer, Hannibal Ford, left to start his own firm. Ford had been the company's first employee; he had played an instrumental role designing Sperry's gyrocompass; he had invented the Battle Tracer.[48]

The Ford Instrument Company

Hannibal Choate Ford (no relation to Henry Ford), whom Vannevar Bush called "about as ingenious an individual as I ever heard of," was born in Dryden, New York, in 1887. He later lived in Cortland, New York, Elmer Sperry's home town, and his older brother had been a friend of the young Sperry. Ford's father published the local newspaper, and Hannibal got his introduction to machinery in the press room and on the precision lathe of the local jeweler. He attended Cornell University, studied electrical and mechanical engineering, and met Sperry for the first time at a gathering of the Institute for Electrical Engineers in Niagara Falls in 1903. The two got on well and kept in touch during the following years while Ford held a number of different engineering jobs. In a series of posts before and after college, Ford patented speed control devices for the New York City subways, worked for several different companies designing typewriter mechanisms, and at Westinghouse as a tool maker. Ford possessed a special mechanical talent augmented by an enthusiasm for machinery and fine shop skills.[49]

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[47] Lieut. Palmer (Commanding Officer, USS New York) to BuOrd, July 14, 1915. RG 74 Box 587 Subject 29758, Folder 51-100. Commander, New York to Commander, Battleship Division 6, August 2, 1916. RG 74 Box 587 Subject 29758, Folder 101-150. "Mr. Gillmor's Report — Result of his Investigations."

[48] Patent no. 1,293,747, "Battle Tracer," Hannibal Choate Ford assignor to the Sperry Gyroscope Company.

[49] Vannevar Bush, Pieces of the Action (New York: Morrow, 1970) 183. R.F. Jahn, "Employees Honor Hannibal C. Ford" Sperryscope12 (no. 2), Summer 1950. pp. 11-12. Roswell Ward, "Hannibal Ford, Sperry Pioneer" Sperryscope 9 (no. 11), March, 1943. pp. 12-13. Also see R.F. Jahn, "Employees Honor Hannibal Ford,"

Unsatisfied in his early engineering jobs, Ford corresponded with Elmer Sperry on matters of mechanical design for gyroscopes. In 1909, Sperry hired Ford as a design engineer. While at Sperry Gyroscope, Ford worked closely with Elmer Sperry, helping him perfect the gyrocompass and designing much of the company's fire control instrumentation. Yet Ford had higher ambitions and left Sperry in 1914. The following year, with investors Jules Breuchaud and J.B. Goldsboro and $50,000 in capital, he organized the Ford Marine Appliance Corporation. Later accounts describe Ford's aims for the new company as the exclusive design of fire control instruments, but documents from the company itself suggest it had originally intended to manufacture the Carrie Gyro Compass, a British device, to compete with Sperry Gyroscope in its core business (although a gyrocompass could be considered a fire control instrument).[50] The navy's needs, however, and their confidence in Ford's skills, soon changed these plans.

By 1915, the navy was installing Sperry Gyroscope's follow-the-pointer in train instruments throughout the fleet, and employing the Vickers clock as a means to determine and follow range.[51] To solve the clock's problem of continuous change of range rate, and at the urging of the fleet officers who had tested the Battle Tracer, the navy requested proposals from both Ford Marine Appliance and Sperry Gyroscope, "to develop a more efficient method of maintaining the Range in action than at the present time."[52] In May of 1915, both companies

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Sperryscope 12 (no. 2), Summer, 1950, Hannibal Ford, Obituary, New York Times March 19, 1955, Ford Instrument Company, Division of the Sperry Corporation, "News Releaase: For Release in the Event of Mr. Ford's Death." The connection between printing and fire control may be more than coincidence: Arthur Pollen's company had originally manufactured newspaper equipment; see Sumida, In Defence of Naval Supremacy: Finance, Technology, and British Naval Policy, 1889-1914 (London: Routledge, 1989) 77. After Wolrd War II, to convert its wartime production capacity to civilian use, the Ford Instrument Company began producing printing machines, largely based on its fire control technology. William Newell, interview with author, on May 12, 1995. For an anectodal account of the Ford Instrument Company, see Quentin Reynolds and Wilfrid S. Rowe, Operation Success (New York: Duell, Sloan, and Pearce, 1957) Chapter 15, "The Father of Weapons Control," 153-60.

[50] Ford Instrument Company, "Report on Organziation and War Activities of the Ford Instrument Company, Inc.," June, 1919. RG 74 E-25 Box 2740, Subject File 36276/110, mentions that Navy interest "necessitated the abandonment of commercial projects then underway, including the manufacture of the Carrie Gyro Compass, and other work in which the company was engaged." This document was the source for the information on Ford Instrument in Bureau of Ordnance, Navy Ordnance Activities, 159. The book does not mention the Carrie compass, but has been the source for the majority of the scant historical material on Ford Instrument. For a description of the Carrie Gyro Compass vs. the Sperry model, see British Admiralty, "The Development of the Gyrocompass Prior to and During the War," 7.

[51] The Navy purchased more than 400 Vickers Clock Mark IIs during World War I. U.S. Bureau of Ordnance, Naval Ordnance Activities, 152.

[52] BuOrd to Secretary of the Navy, June 3, 1915. RG 74, E-30 Box 587, Subject File 29758, Folder 1-50.

submitted proposals, Sperry's for a "Range Clock," and Ford's for a "Range and Deflection Predictor."

Documents in the Sperry archives reveal a curious collusion between the two companies. The week before submitting its proposal, Ford Marine Appliance entered into a licensing agreement with Sperry Gyroscope to make Battle Tracers and sell them to the navy in conjunction with its new machine. Ford, even though he assigned the Battle Tracer patent to Sperry Gyroscope, may have had an agreement to receive royalties on the device from his old employer. Perhaps as insurance against losing the competition, Ford licensed the Sperry Range Clock as well. Two years later, Ford released his interest in the Battle Tracer, and licensed his own Rangekeeper to Sperry Gyroscope (which never produced them). These arrangements suggest a spirit of healthy competition between Sperry and Ford, rather than resentment at the split. Elmer Sperry loudly voiced his opinions when he thought his patents were violated, but no documents betray a bad word about Hannibal Ford, other than a mild jealousy of his success.[53]

The Bureau chose to purchase one each of the Sperry and Ford machines, "to encourage competition in working out future development along this line." This decision, coming less than six months after Ford founded his company, implies the navy had encouraged him from the start to compete with Sperry Gyroscope because of the service's aversion to single-supplier technologies.[54] They probably became nervous about depending on Sperry Gyroscope's monopoly in the American gyrocompass market and had originally intended Ford to compete in that arena. In any case, Ford Marine Appliance quickly abandoned its gyrocompass plans and regrouped in response to the navy's interest in range predictors. In late 1915, a new firm, the Ford Instrument Company, with $250,000 capital, absorbed Ford Marine Appliance. In its official announcement, the new company dedicated itself to "the inventions of Mr. Ford, in the line of scientific instruments and automatic machines involving mathematical and technical problems, intricate mechanism, epicyclic gearing, electrical devices, etc." — technical euphemisms for fire

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[53] License agreement between Ford and Sperry for Battle Tracer, May 4, 1915. Licence agreement between Ford and Sperry for Range Clock , July 7, 1915. SGC, AC #1915, Box 67, "Ford Instrument Company Patents." Thompson to EAS, February 27, 1919 and December 1, 1919. SGC Box 32 Folder Fire Control Patents and Hannibal Ford Interference. Contrast this situation with that of Carl Norden, who quit Sperry Gyroscope in 1913 to manufacture bombsights for the Navy. "Sperry took Norden's resignation as a personal affront, beginning a half century of conflict between the Sperry Company and Norden. Sperry felt he had taught Norden everything he knew about gyroscopes and therefore should share in any of Norden's future patents." Stephen L. McFarland, America's Pursuit of Precision Bombing 1910-1945 (Washington: Smithsonian Instiution Press, 1995), 50.

control. The new company's logo was built around a differential gear. Breuchaud remained as president, and Goldsboro as treasurer, with additional management, "men of experience in large financial and engineering operations." Hannibal Ford became Vice President and General Manager, reflecting his interest in the daily engineering and production of the company rather than managing the business. The company established its factory and headquarters at 80 Lafayette street in New York City.[55]

In May of 1916, Sperry Gyroscope and Ford Instrument each demonstrated their new range predictors to the navy. The Ford device, called the "Rangekeeper," was "entirely successful," whereas, the inspector noted, the Sperry machine required more work.[56] Tests showed the Ford Rangekeeper, "affords a means of rangekeeping far superior to present methods."[57] By July, a Ford delivered his rangekeeper to the fleet for testing. This was the same spring as the Battle of Jutland, and reports trickling in from England (largely through Sperry's Gillmor) underscored the need for a device which could continuously track the changing rate of change of range.

The Ford Rangekeeper

Like the Dreyer Table, the Ford Rangekeeper struck the right combination of automation, operator control, and credibility with the service. [*Figure 2-4: Ford Rangekeeper] In July of 1916 BuOrd forwarded the prototype to the battleship Texas, with a request for the fleet to appoint a board of gunnery officers to conduct sea trials, and report "as to its merits as compared to the present methods of rangekeeping."[58] Hannibal Ford personally installed and tested the device. In early August, the Texas conducted trials, and the evaluation board, made up of gunnery

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[54] BuOrd to Secretary of the Navy, June 3, 1915.

[55] The Ford Instrument Company, Inc., "Announcement," n.d., received by BuOrd December 4, 1915. see R.F. Jahn, "Employees Honor Hannibal Ford," incorrectly lists the date of reorganization as November, 1916, probably intending the previous year. Ford Instrument Company, "Report on Organziation and War Activities," erroneously lists the birth of Ford Instrument out of Ford Marine Appliance as January, 1915, which was actually the founding of the latter.

[56] Elmer Sperry to F.C. Martin, BuOrd, May 8, 1916. Naval Inspector of Ordnance to BuOrd, May 11, 1916 (witnessed Sperry Gyroscope rangekeeper). Naval Inspector of Ordnance to BuOrd, May 11, 1916 (witnessed Ford Instrument Co. rangekeeper). RG 74 E-30 Box 587, Subject 29758, Folder 51-100. The Naval Inspector for these trials was assigned to E.W. Bliss, Company, a manufacturer of torpedoes in New York. At this point neither Sperry nor Ford had enough Navy work to justify their own inspector, but soon thereafter a permanent inspector was assigned to both companies.

[57] BuOrd to Commander-In-Chief, Atlantic Fleet, July 11, 1916. RG 74 E-30 Box 747 Subject 30309, Folder Without Line Numbers, 1915-1917.

lieutenants, filed a favorable report. Because the rangekeeper had manual inputs, it could act as a calculator to simulate typical problems, "the Board worked a problem out on the machine giving both firing and target ships' speeds of thirty knots and causing them to pass in opposite and parallel courses...due to the constantly varying rate of change and change in deflection, this problem could not be handled by any of the present methods." Hannibal Ford 's hand is evident here, because the problem the evaluation board worked out on the machine matches exactly, down to the speed of the ships, with the one Ford included in his original proposal for the device. The board further noted that, "The rangekeeper works out problems with mathematical accuracy," and emphasized the importance of the human operator, "Its value therefore depends considerably upon the expertness of the operator and his skill in utilizing the data supported." Unlike Pollen's system, which sought to eliminate this operator from the equation, Ford's system left an important place for the experts. Who were the operators who would run this machine in practice? The very gunnery lieutenants who comprised the evaluation board — pleased, no doubt, that the success of the device depended on "an experienced operator."[59] The board recommended six or eight of the rangekeepers be supplied to the fleet as soon as possible, and that the machines be employed continuously alongside existing manual methods of range plotting.

The Ford Rangekeeper quickly won the favor of the naval establishment. The Commander in Chief of the Atlantic Fleet supported the evaluation board's recommendations to the Chief of Naval Operations. The following month, Ford Instrument quoted a price of $100,000 to the navy for delivery of nine Ford Rangekeepers within eight to ten months.[60] Sperry Gyroscope enjoyed no such luck. Its device did not become ready for testing until August of 1916, when the board's report on the Ford machine had already been issued. Sperry did not demonstrate the device to battleship officers until December, by which time contracts and production design for Ford's machine were well underway.[61] No further mention of the device exists in the bureau's records.

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[58] BuOrd to Commander-In-Chief, Atlantic Fleet, July 11, 1916.

[59] Board to Test Ford Rangekeeper to Commander, Battleship Fleet, "Test of Ford Rangekeeper," August 3, 1916. See also the Official Report of the board to Commander, Battleship Fleet August 28, 1916. RG 74 E-30 Box 743, Subject 30309, Folder 1-50.

[60] Commander-In-Chief, Atlantic Fleet to Chief of Naval Operations, August 11, 1916. Ford Instrument Company to Chief of BuOrd, September 20, 1916. RG 74 E-30 Box 743, Subject 30309, Folder 1-50.

[61] BuOrd to Battleship New York, August 2, 1916. EAS to Lieut. Comdr. F.C. Martin, December 2, 1916. RG 74 E-30 Box 586, Subject 29758, Folder 101-150. Sperry may have missed the yard period of the New York in August, which was why it had to wait until December to demonstrate the device. battleship captains were willing to help the bureau evaluate new technologies, but not if it meant holding their ships in port.

1916 was a busy year for Sperry, as the Battle Tracer was just maturing and, as we shall see, the company's data transmission instruments began to have significant problems.

After its summer trials, Ford instrument spent the winter of 1916-17 putting the rangekeeper into production [*Figure 2-5: Ford Rangekeeper production]. The device itself incorporated much of the experience of British fire control, but with a mathematical precision and mechanical elegance unmatched by anything of the time. For reliability, the rangekeeper's calculations were almost entirely mechanical; the only electrical input came from the ship's gyrocompass. Ford also made a point of the rangkeeper's only semi-automated calculation, "All of the automatic features are under supervision of an attendant who is enabled to exercise a certain amount of discretion in acceptance of incoming information and, in case of emergency or any electrical trouble with any part of the instrument, to operate the same manually." Ford astutely perceived the technical core of the problem and automated it only to the degree necessary. Perhaps he chose not to build a highly automated device in order to stay within his young company's infant capability.

In 1917 a Royal Navy fire control expert visited the US and noted the Ford device "is very similar to Pollen's clock and possesses about the same merits and demerits when compared with our system."[62] In 1918, on the Louisiana, the US Navy compared the Pollen and Ford sytems head-to-head and found the two instruments equally accurate. The smaller and lighter Ford machine was more difficult to maintain and much harder to use, having little of the automation of the Pollen, "Any good man may be allowed to operate the Pollen even without an officer present whereas no one should be allowed to touch the Ford excepting in the presence of an officer." While the Ford was new and "the ship's force have not sufficiently operated the Ford to feel fully qualified to draw a comparison...at the present time they have considerably more confidence in the Pollen than in the Ford."[63] Clearly the Ford's success in the fleet did not derive from a radical technical superiority, although the machine did hold its own in the comparison. The Pollen's automation broadened the pool of potential users, just as the Ford's manual nature restricted users to officers. Nonetheless, looking only at the artifact misses a key feature of the Ford Rangekeeper

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[62] Quoted in Friedman, US Naval Weapons, 33.

[63] Commanding Officer, Louisiana to Commander, Battleship Force One, "Pollen Fire Control Instruments," March 31, 1919. RG 74 Box 345 Subject 28499.

that made it attractive to BuOrd: it was domestic technology, produced by a supplier under total control of the navy.

It is unclear where Ford got his understanding of the range problem, which closely matched Pollen's. The navy's request for proposal does not survive in the bureau records. Much of the inspiration for the Ford Rangekeeper's design undoubtedly grew from the specifications in this request, as the Sperry Range Clock shared some similar features. The device embodied a more sophisticated understanding of the fire control problem than Ford's previous fire control project at Sperry Gyroscope. Somewhere, Hannibal Ford learned about British fire control. He built a company selling that knowledge to the navy.

Feedback and Integration in The Ford Rangekeeper

To understand the basic mathematical problem of the rangekeeper, consider Figure 7 [*Figure 2-6: FRK Angles] (Also see Appendix 2-A for a detailed explanation). The *own ship* does the firing (and contains the rangekeeper), shooting its guns at the *target ship*. The basic problem is to use a series of range and bearing measurements to solve for the course and speed of the target, which then can be extrapolated into the future to track the target's position as it changes and to predict the position of the target. If a shell fired from the own ship takes a minute to reach the target, then the target's course must be estimated for a minute into the future when the gun is fired. Similarly, if the target becomes obscured behind fog or smoke and its course and speed are known, it can be continually tracked while not under visual observation.

The rangekeeper solves a series of equations to determine the course and speed of the target, from which it calculates *present range*, i.e. where the target is now, and *advance range*, where the target will be at some time in the future. It takes as input the own ship's course, which comes automatically from a repeater compass, the own ship's speed, which can be entered manually or from a *revolution transmitter* connected to the ship's propellers. A *target bearing instrument*, on the foretop measures the target bearing and sends it to the plotting room, where it is entered into the rangekeeper either automatically or manually. In addition to these data, the gunnery officer enters initial guesses for the factors the rangekeeper: target course and speed, and target range, from "any source whatever, such as plotting board or Battle Tracer." The basic operation of the machine, then, consists of "tuning" these guesses, based on observed data, until converging on a stable solution.

Consider, for example, if the target ship's course speed as entered are correct. Then the rangekeeper calculates, by integration, the expected range and bearing of the target as it changes with time. If this calculated range and bearing does not match future observations of the target's range and bearing (from the rangefinder, spotting, and the target bearing instrument), then the estimated course and speed of the enemy are in error. The same holds true for deflection, and "we can compare this range and bearing produced by integration with the range finder range and the exact bearing from the target bearing instrument."[64] In this way the estimated course and speed of the enemy can be corrected until the calculated range and bearing matches observed data. The result of this feedback process, then, is to produce an accurate course and speed of the enemy, which may be used to predict its future position to set the guns.

This feedback cycle, of course, assumes the target's course and speed remain constant long enough to converge on a solution. The assumption will hold for short periods of time during a naval engagement. The Ford Rangekeeper, however, really comes into its own when the target changes course. Assume that the system has been corrected and settled on a proper course and speed. The operator overlaid dials, which indicate the calculated and observed speed and bearing indicate the same value. [*Figure 2-7: Calculated and observed speed and bearing] When the target changes course, those dials will diverge, immediately signaling the operator that a change has occurred. If the system has already settled on a solution, the old course and speed serve as the initial guesses for the new solution. Since, of course, the target cannot change its course and speed instantaneously — in fact they change rather slowly — the old solution will likely be a pretty good estimate for the new values, and the system will converge on a new solution fairly quickly. Thus the rangekeeper and the operator can "track" the target as it changes direction, and can solve for its new parameters continuously as they change.

In a sense, the Ford Rangekeeper combines into one instrument the British dumaresq and Vickers clock, a fact not lost on the gunnery officers of the time.[65] The Ford adds a crucial ingredient: mathematically accurate integration. The "Ford Integrator," as the mathematical component became known, improved on Pollen's integrator. Consider the simplest type of

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[64] Lieut. H.M. Terril, "Notes on the Theory of the Ford Range Keeper," print issued by the U.S.S. New Mexico Gunnery Department, c. 1919, National Archives, RG 38, Entry 178, Box 3, File Folder Conf. 59 (65), courtesy Jon Tesuro Sumida, Christopher Wright.

[65] Terril, "Notes on the Theory of the Ford Range Keeper," 2.

integrator, the wheel and disk type. [*Figure 2-8 : 3 types of integrators] Here the disk rotates at a constant speed, providing the time variable for integration. The wheel contacts the disk at a right angle and rotates at a speed proportional to its distance from the center. If the wheel is close to the center, it rotates relatively slowly, and ever faster as it moves toward the outside. The function to be integrated, then, actuates the distance of the wheel from the center, and the rotation of the wheel reflects the integral of that function. This setup works like a continuously variable transmission, and is often referred to as a *variable speed drive.*

The wheel and disk integrator, however, has a problem: the wheel slips while in contact with the disk. Hence this mechanism is relatively inaccurate, especially if the wheel drives a load; to make the device useful in a calculating machine, it needs to drive other mechanisms. Lord Kelvin created the "ball and cylinder" integrator in the 1870s on inspiration from his brother, James Thompson. Kelvin probably suggested the device to Pollen personally in 1904, as Kelvin served on the board of Pollen's Linotype Company and as a scientific mentor to Pollen.[66] Here, a ball contacts the disk and transmits its rotation to a cylinder which lies across it. Pollen incorporated this device into his own machine, but it too has serious faults, most notably that the ball is pressed to the disk only by gravity, and its own weight is not enough to prevent slipping.[67] Hannibal Ford improved this device to eliminate this shortcoming and made it useful for numerous computing tasks. He added another ball, which further reduced friction, and tight springs on the cylinder bracket to hold the two balls firmly in place. This device could perform highly accurate mechanical integration with sufficient force to drive mechanisms connected to its output.[68] The integrator formed the central component in analog computers through World War II, and even early digital computers were often referred to as "electronic integrators."

"The Secret Fire Control Design Section of the U.S. Navy"

After the initial tests in the summer of 1916, Ford Instrument geared up for production while the prototype rangekeeper remained on the Texas for further testing. The navy ordered

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[66] Sumida, In Defence of Naval Supremacy, 78. Sumida (210) speculates that Pollen might also have learned of the mechanism from another British physicist, Charles Vernon Boys, who also advised him on fire control.

[67] A.B. Clymer, "Mechanical Integrators," M.S. Thesis, Ohio State University, 1946, 20-22. Clymer was an engineer at Ford Instrument. See also A. Ben Clymer, "The Mechanical Analog Computers of Hannibal Ford and William Newell," *IEEE Annals of the History of Computing* 15 (no. 2, 1993) 19-22 and Allan G. Bromley, "Analog Computing Devices," in William Aspray ed. Computing Before Computers (Ames, Iowa: Iowa State University Press, 1990).

more than twenty five Ford Rangekeepers at a cost of $8,000 each, intending to install four to six units in each of its battleships. In July, 1917 installation began on first-line battleships, including New York, Wyoming, North Dakota, Pennsylvania, and Arizona. A new machine replaced the prototype on the Texas (the prototype is likely now in the collection of the Naval Academy museum). That same summer of 1917, Ford Instrument introduced a low-cost version, the Rangekeeper Mark II, for $800. This device, nicknamed the "Baby Ford," included only the initial stages of the calculation, where components of the own and target ship's motions are resolved and added. From the course and speed of the own ship and that of the target ship, the Rangekeeper Mark II solves for present range, range rate, and bearing rate only, it included no integration, no prediction, and no feedback or correction. The Mark II began production in August, 1917 and because of their simplicity and low cost the navy ordered Baby Fords for all its battleships, cruisers, destroyers, and gunboats. The Bureau found both models of Ford Rangekeepers "very reliable and [they] require little or no repair."[69]

From its introduction in 1916 until World War II, the Ford rangekeeper underwent a number of different variations and mark numbers, although its core function remained essentially the same: assimilating diverse data, eliminating contradictions and discrepancies, calculating sighting for the guns, and maintaining a plotted record.[70] The Mark 8, introduced in the early thirties, served as the primary rangekeeper for both battleships and cruisers up to and during World War II. The Mark 8 differed little from the old Mark I machine in basic structure: it took initial estimates of enemy course and speed, matched them with observed data, and allowed the human operator to make corrections accordingly. By tracking the target, the rangekeeper

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[68] Patent no. 1,317,915.

[69] The production device became the Rangekeeper Mark I mod 1, the mod number indicating minor modifications made after the Texas tests.These modifications mostly entailed removing the automated input from the target bearing transmitter and replacing it with a follow-the-pointer operation, thus making the machine "entirely independent of all other apparatus...In other words, the machine is self-contained." F.C. Martin, BuOrd, to D.C. Bingham (Fleet Gunnery Officer on the staff of Admrial Mayo), May 14, 1917. For the navy's order, see F.C. Martin, BuOrd, to D.C. Bingham, May 14, 1917 RG 74 E-30 Box 743 Subject 30309, Folder No Line Numbers 1918. For the Mark II "Baby Ford" Rangekeeper, see Patent no. 1,370,204. This patent covers the Mark II machine, no individual patent covers the first rangekeeper. Also see United States Naval Academy, Notes on Fire Control 1940, Chapter 6, "Secondary Battery Rangekeepers," (Washington, 1941) for the Baby Ford, and Martin's comments on the Baby Ford, F.C. Martin, BuOrd, to D.C. Bingham, May 14, 1917. For BuOrd's comments on reliability, see BuOrd to Ford Instrument Co., August, 1917. BuOrd to Commander In Chief, Atlantic Fleet, August 3, 1917. RG 74 E-30 Box 747 Subject 30309, Folder Without Line Numbers 1915-17.

[70] The Mark II, as we have seen, was the "Baby Ford," attached to the gun directors themselves. Mark III was another main battery director. Mark IV attached to an antiaircraft director. Mark VII was used in cruisers.

extrapolated "advance range," and generated orders for the guns to fire.[71] The Mark 8, however, included many more variables and corrections than the Mark I.

The original Mark I rangekeeper was a box on a pedestal easily operated by one man who turned cranks. None of the variables were automatically entered, except for the own ship's course, which came in via a gyrocompass repeater (but could be bypassed and entered manually). The Mark 8, in contrast, achieved much greater automation, reflecting the navy's increasing level of comfort with electrical machinery, and especially with electric data transmission. Most data came into the unit automatically, although a number of manual inputs were available as backups [*Figure 2-9: Course and Speed]. It also looked much more like a "computer" in the modern sense than the older Mark I. The Mark 8 consisted of five separate boxes (divided as functional units) bolted together into a single console. [*Figure 2-10: Mark 8 rangekeeper]

To support these delicate and intricate devices, BuOrd and Ford Instrument forged a tight and complicated relationship. Ford had a privileged position in fire control, but at the expense of a wider business. Bureau chief Ralph Earle ordered, "that you do not disclose to any one even the fact that you are making rangekeepers, and that you do not dispose of similar instruments to other governments."[72] Ford Instrument provided space in its factory for the Naval Inspector of Ordnance, Benjamin B. McCormick, who was also the naval inspector at Sperry Gyroscope. The navy began sending its gunnery officers and machinists to Ford Instrument to learn the principles, operation, and repair of the Ford Rangekeepers, and before long Ford Instrument set up formal courses of instruction on its products. These courses involved "solving problems" on the machine (as though it was a calculator), receiving lectures on the mechanisms, taking tours of the assembly rooms, and learning proper adjustment of the rangekeepers.[73] Eventually, the navy took over instruction of the courses, but still within the Ford Instrument facility. For new development work, the navy formulated requirements, and Ford Instrument responded with a design, which, if accepted, the company would manufacture only for the navy. The Ford Instrument Company

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[71] "Rangekeeper, Mark 8" in U.S. Naval Academy, Postgraduate School, Fire Control Installations, 7.

[72] Breuchaud to Earle, May 28, 1917. RG 74 E-30 Box 743 Subject 30309 Folder 1-50. Earle to Breuchaud, July 1, 1917. RG 74 E-30 Box 747 Subject 30309, Folder Without Line Numbers 1915-17. Elmer Sperry, as a member of the Naval Consulting Board, however, did have access to the Ford machine and when the board visited BuOrd late in 1916, Ford wrote that "[I] have no doubt that Mr. S. was all eyes when examining the instrument." Ford to Martin, October 4, 1916. RG 74 E-30 Box 743 Subject 30309 Folder 1-50.

[73] Ford Instrument Company, "Report on Organziation and War Activities of the Ford Instrument Company, Inc.."

evolved, in the words of a British observer, into "the secret Fire Control Design Section of the U.S. Navy."[74]

To make this special institutional coupling work, BuOrd had to carefully define and control the nature of technical knowledge. Where did fire control technology originate and reside? In the tactical and strategic requirements specified by line officers? Or in the ability of engineers to turn those requirements into practical mechanisms? At first, the navy and its contractors had the same answer: knowledge was imported from outside the country. The navy virtually stole technology from abroad and protected the companies which produced it from legal action. In 1918 Ford Instrument expressed its concern to the navy that its rangekeepers might violate patents for Pollen's system (both Sperry and Pollen believed they did). Assistant Secretary Franklin Roosevelt responded by guaranteeing "to hold and save you harmless against any and all suits" brought for infringing patents on British fire control.[75]

Other times the location of technique proved more contentions. "Research and development," was not understood as a specific activity; when the navy wanted a new machine, it simply specified one and ordered it from a company. BuOrd then did what it pleased with the technology, including awarding production to another company. In contrast, the contractors saw navy specifications as broad outline for work which often required engineering talent to implment in a working device. Contractors thus claimed ownership of technology built under navy specifications and challenged the navy's right to let production contracts to other manufacturers. Sperry, G.E., Ford, and Arma had numerous disputes over ownership, especially when one company went through a long development process only to see BuOrd award production contracts to other vendors. In the late thirties BuOrd instituted a policy whereby the government could license inventions, royalty-free, but only within the sphere of its clique of contractors (Ford, G.E., and Arma).[76]

Secrecy further defined the sphere, hampering the contractors' ability to profit from their inventions beyond the confines of BuOrd. Handling patents for secret technology raised difficult

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[74] Captain H.J.S Brownrigg, RN, "'Ford,' Fire Control System: Interviews with representatives of Ford Instrument Coy. of New York," IQ/DNO (January-June, 1919) Naval Library, Ministry of Defence, London. Courtesy Jon Testuro Sumida. See Also Sumida, In Defence of Naval Supremacy, 314-315.

[75] Roosevelt to Ford Instrument Company, April 30, 1918. RG 74 E-30 Box 747 Subject 30309 Folder 201-250.

[76] Department of the Navy, Office of the Judge Advocate General to Admrial Furlong, April 6, 1938. RG 74 Box 1740, Ford Instrument Company Folder.

contradictions. Initially the bureau insisted that no fire control devices could be patented, owing to their essential origins within the navy and the need for secrecy. Hannibal Ford, in fact, never patented the first rangekeeper, but only discrete parts of its computing mechanisms, usually under nondescript names like "mechanical movement," "calculating instrument," or "control system." One strategy for maintaining secrecy imposed a long delay from application to granting the patent, since that amounted to declassifying the invention. Instead of the usual three years, Hannibal Ford's "Range Predicting Apparatus" took six years to be approved; his "Calculating Instrument" took five. Elmer Sperry submitted an application for a "Director Firing System," in 1917 and did not receive a patent until after his death in 1930.[77] Delays of even twenty years on fire control patents were not unusual — in fact, long delays between patent and issue often signals an innocuous-sounding invention had military applications. Through the fire control clique, and its sometimes contradictory conditions of knowledge, BuOrd appropriated technical expertise from private companies into its unique mission.

Fire control was not the only arena BuOrd built these special ties. BuOrd handled bombsight development and manufacture much the same as fire control. The primary contractors were Sperry Gyroscope and another captive contractor formed by a former employee, Carl Norden. Sperry competed with Norden for Navy bombsights, which, like fire control, were delicate and precise mechanical calculators which had to work under demanding conditions. They became the paradoxical "famous secret weapon" of World War II. BuOrd's relationship with Carl Norden Inc. and Ford Instrument (and, as we shall see, with Arma) represented a concerted effort to found and foster captive contractors to make new technology in what amounted to private arsenals.[78]

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[77] Patent nos. 1,387,551; 1,450,585;1,755,340.

[78] Like Hannibal Ford, Norden had been an early Sperry employee, and he left about the same time as Ford to consult for the navy. Unlike Ford, however, Norden fell out with Elmer Sperry, as the two disagreed about Norden's patent obligations to Sperry for the ship stabilizers on which Norden had worked as a Sperry employee. Norden did design work for the navy, until 1927 when he and a partner set up a company, Carl Norden Inc., to manufacture his devices (in 80 Lafayette St., the same building in which Ford Instrument had started). Historian Stephen McFarland writes "Norden's relationship to BuOrd was irregular from the beginning" because the navy illegally gave production contracts to Norden without competitive bidding. He calls Norden's facility the navy's "private bombsight factory;" Ted Barth, Norden's partner wrote "Our business policy was to be controlled by the Bureau [BuOrd]...and we were to function as a sort of subdivision of the Bureau as far as the bombsight problem was concerned." McFarland, America's Pursuit of Precision Bombing, 50-60. Barth quoted in McFarland, 58-9. I borrow the idea of a "captive supplier" from the semiconductor industry, which uses the term to describe suppliers,

Sperry's Fate in Fire Control

Sperry Gyroscope, though it started out as BuOrd's main fire control contractor, could not fit this constricting, if profitable, role. By 1920, the US Navy had installed Sperry fire control systems on nineteen dreadnoughts, eleven second line battle ships, and nine armored cruisers, including many hundreds of data transmitters, receivers, and indicators.[79] Most of these instruments supplied data to Ford Rangekeepers. Despite Sperry Gyroscope's strong initial position, Ford and his company eclipsed Sperry's dominance. The rise of Ford Instrument as BuOrd's favored fire control manufacturer, technical troubles with Sperry instruments, Sperry's own organizational problems, and its unwillingness to be confined to serve BuOrd exclusively ended the company's status as a member of the clique.

When Sperry Gyroscope's range clock had lost out to Ford's machine, Elmer Sperry recognized the importance of the rangekeeper and still wished to compete. "The navy are obsessed by the [Ford] range clocks," he wrote to Gillmor in England as he considered licensing Pollen's machine. Sperry believed Ford's rangekeeper overlapped with both his and Pollen's systems, "However he [Ford] is very cute in this regard and probably has worked out some other method. This, of course, is a direct attempt on his part to short circuit our further sales of the Battle Tracer." Gillmor promoted Pollen's system to Elmer Sperry, but the inventor was more cautious of the assertive Pollen, thinking "P's belief in his own system borders either on bigotry or fanaticism." Sperry also saw a "vital weakness of P's patent situation," and lamented, "I do not now see very much hope of being able to stop the other fellow [Ford]." Pollen argued that Ford had stolen his rangekeeper "bodily" from him, but Sperry Gyroscope never licensed Pollen's Argo Clock.[80]

Sperry's own devices also had crippling technical problems. Naval officers who worked with the Sperry system complained of light construction, difficult maintenance, and unsuitable data transmission. Myriad correspondence from the fleet to the bureau, as well as directly to the

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such as the chip-making capacity of IBM, which (until the 1990s) provided chips only to the parent company for its products. I use "captive supplier" slightly differently, to mean a company that sells only to the navy.

[79] Sperry Company Memorandum, June 5, 1920. EAS Papers , Box 32, Capt. William McEntree, Naval Constructor Folder. U.S. Bureau of Ordnance, Naval Ordnance Activities, 152.

[80] EAS to Lieut. Comdr. F.C. Martin, August 31, 1916. EAS Papers , Box 32, Lt. Logan Cresap Fire Control Correspondence folder. EAS to Gillmor, October 20, 1916. EAS Papers , Box 32, Capt. William McEntree, Naval Constructor folder. EAS to Gillmor, February 13, 1917. EAS to Gillmor, July 25, 1917. EAS, Box 32, Comdr. F.C. Martin folder.

company, displayed consistent frustration and disappointment with Sperry Gyroscope's fire control equipment. Through 1917 and into 1918, Sperry Gyroscope experienced delays in getting its equipment installed in the fleet; much of it was returned for repair due to errors in construction and installation.[81]

As for the Battle Tracer, the fleet tested the device and deemed it potentially useful and worthy of further work, but Sperry Gyroscope did not respond. The Commander of the Arkansas, for example, made detailed recommendations for improvements, but he complained, "none of these suggestions have been acted upon [by Sperry Gyroscope] and both ships [New York and Utah] have been left largely to their own devices to develop on board the ways and means for solving the problem...the battle tracer, in its present form, leaves something to be desired when considered either as a fire control instrument or as a navigational instrument." He reported a scathing opinion of Sperry Gyroscope's approach to naval engineering and suggested naval officers contributed more to the technology than the company:

> It should be remembered in this connection that the designers of the apparatus turned out by the Sperry Gyroscope Company are trial and error men. So far as is known not a single instrument or appliance that has so far been turned out by this company has been thoroughly satisfactory in its original form. The gyrocompass itself, manufactured by this company, was at first unsuccessful and has been subject to repeated modifications leading to its improvement as suggested by naval officers as a result of their experience with this instrument. The same is true of the target bearing transmitters, the turret target indicators, the optical range transmitters, the multiple turret indicators, the plotting indicators, and finally the battle tracer.

This missive concluded with a suggestion for choosing an experienced officer to "take up with the Sperry Corporation the incorporation of the necessary features."[82] Naval officers did not expect technical perfection straight off, and they expressed desire to work with the company to improve their instruments. When reporting problems with Sperry equipment, officers frequently stated their belief that it could be made to work, and that its potential usefulness was worth the effort. The attitude, however, was predicated on the officers' perception of the company's willingness to cooperate, and confidence that the technical problems were tractable. Not only did Sperry

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[81] Chafee to BuOrd, January 26, 1915. Commander, Battelship Force to Commander in Chief, January 25, 1918. RG 74 E-30 Box 585 Subject 29758 Folder 1-50.

[82] Commander, Battleship Force to Commander In Chief, October 5, 1916. See also Commander in Chief, to Commander, Battleship Force, September 22, 1916, Commanding Officer, Utah to Commander, Division 7 Battleship Force, August 25, 1916, Commander, New York, to Commander, Battleship Force, August 6, 1916, and Commander, New York, to BuOrd, August 14, 1915. RG 74 Box 586 Subject 29758 Folder 101-150.

Gyroscope's reputation for cooperation erode, but sea experience increasingly showed that Sperry equipment had an insoluble flaw: the problem of synchronization.

The Sperry system transmitted data in a DC "step-by-step" mode, derived from the original compass repeaters. An on-off relay servo sensed when the receiver was in a different position from the master transmitter, and moved the receiver dial one way or the other to bring it into line. This method transmitted relative and not absolute position, requiring that that "before the equipment is ready for use the pointers in the Turret Indicators are synchronized with the Target Turret Transmitter by pushing a synchronizing button."[83] These on-off elements could not withstand nearby firing of a battery of sixteen-inch guns, however; the shock of a salvo usually knocked the system out of synchronization. Gunners had to continually reset the system to get accurate readings, an operation annoying, if not impossible, under battle conditions. Similarly, the navy was gradually incorporating the ability to swap components between multiple subsystems. With the Sperry equipment, switching a data transmitter to a new set of receivers (or vice versa) would require resynchronization.

Sperry Gyroscope stonewalled, then submitted a number of stopgap solutions, including an additional "Turret synchronizing system" which provided a central button to synchronize the entire system. Elmer Sperry himself patented no less than 3 "synchronous data transmitters" in 1919 and 1920, seven in the decade before his death in 1930.[84] The complaints continued. The commander of the Pennsylvania reported, "every day at General Quarters the synchronizing of the instruments is checked which takes considerable time." BuOrd Chief Ralph Earle summarized the situation to Sperry's Naval Inspector in 1918: "the instruments, being of the step-by-step type, lead to errors...there is an opinion current in the fleet that these instruments may prove to be unreliable." The inspector echoed to the company, "As instruments frequently get out of step, this fault appears to be a most serious one...They do not appear sufficiently rugged to stand up under weather." A Bureau of Standards engineer asked to examine the situation wrote to Ralph Earle "it will be impossible to alter Sperry's system to be self-synchronizing... We can make

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[83] Turret Control Equipment," Bulletin 303, 1916. Sperry Gyroscope Company Papers, Box, 33
[84] EAS to F.C. Martin, June 6, 1917. Sperry Gyroscope Company to BuOrd, December 1, 1917. RG 74 E-30 Box 586 Subject 29758 Folder 151-200. Patents 1,468,330; 1,850,640; 1,656,962.

synchronizing easier but we cannot make the Sperry system self-synchronizing." [85] While the bureau continued to order Sperry instruments, the complaints did not cease; by the end of 1918 confidence in the Sperry apparatus had evaporated.

The navy's disenchantment with Sperry Gyroscope was well founded: the company was in crisis. In March of 1918, Van Auken visited the company and grew concerned about its lack of organization.[86] Gillmor was in the midst of a radical restructuring; he took the bold, and perhaps desperate step of exposing his company's internal problems to the navy, "in an endeavor by the Company to reinstate itself in the confidence of the department." In a letter to the Assistant Secretary of the navy attached to a "Statement respecting the situation now existing in The Sperry Gyroscope Company and the policies now pursued by that company," Gillmor admitted that "during the past year it has been evident to anyone who has come in contact with the Sperry Gyroscope Company that its efficiency and its policies have not been such as to create confidence."

Gillmor attributed this situation to corrupt management by the Secretary and Treasurer of the company. In December, 1917 at Elmer Sperry's request, Gillmor had left the navy and his post in England to take control of the company in New York.[87] He fired a number of workers and managers, including the Secretary and Treasurer, F.R. Allen, imposed a more strict, rational structure on the place, and brought production under control. Allen, Gillmor warned, "has made violent threats against Mr. Sperry personally and has attempted to blackmail him," by threatening

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[85] Ralphe Earle to Naval Inspector of Ordnance, Sperry Gyroscope Company, SGC. February 16, 1918. Earle's letter summarizes a letter to him by the Commanding Officer of the Texas, January 22, 1918. RG 74 E-30 Box 585 Subject 29758 Folder 301-350. Naval Inspector of Ordnance, Sperry Gyroscope Company to Sperry Gyroscope Company, February 25, 1918, emphasis original. RG 74 E-30 Box 585 Subject 29758 Folder 301-350. These records contain numerous complaints about ruggedness and synchronization in the Sperry systems, see EAS to F.C. Martin, June 13, 1917 (Folder 151-200), BuOrd to Naval Inspector of Ordnance, Sperry Gyroscope Company, Sperry Gyroscope Company September 10, 1918 (Folder 151-200), Captain, Arkansas to BuOrd, March 3, 1918 (Folder 251-300), "Report on Installation Tests of Sperry Target Turret System USS Michigan, April 25, 1918 (Folder 400-450), Electrical Officer to Commanding Officer, USS Utah May 21, 1918 (Folder 400-450), Earl to Naval Inspector of Ordnance, Sperry Gyroscope Company, September 13, 1918 (Folder 450-500), Earle to EAS, July 21, 1919 (EAS Papers, Box 32), Jorel to EAS, September 29, 1924 (EAS Papers, Box 32, Capt. William McEntree, Naval Constructor Folder). For the review of the system by the National Bureau of Standards, see Earle to Sperry Gyroscope Company, May 1, 1918, RG 74 E-30 Box 585 Subject 29758 Folder 351-400 and technical note to Ralph Earle, handwritten, March 12, 1918, Folder 301-350.

[86] W.R. Van Auken, "Report of Visit to works of Sperry Gyroscope Co. and Ford Instrument Company, March 12, 1918."

[87] EAS to Van Auken, December 18, 1917. SGC Box 32 Folder Capt. William McEntree, Naval Constructor. For Sperry's 1918 reorganization, also see Hughes, Elmer Sperry 210-11. Sperry Gyroscope's new organization chart, as published in Sperry's public newsletter, does not list the secret fire control division.

to "spread scandal in the newspapers." Gillmor sought to head off Allen's accusations before they reached the navy by discrediting him, "this man has made it appear highly probable that he is mentally unbalanced." In an attempt to win back the department's favor, perhaps with a tinge of envy for Ford's new position, Sperry Gyroscope offered "to consider itself an auxiliary of the Government service."[88]

Although Gillmor improved the situation at Sperry Gyroscope, the company's relationship with BuOrd had suffered permanent damage, especially as the bureau had other reasons to be uncomfortable with the contractor. Fire control made up a small part of Sperry's business. With its gyrocompass and other control devices, the company had a dynamic and growing commercial operation. BuOrd did not object to commercial sales *per se*, but the corporate culture required to support commercial sales, especially internationally, was incompatible with the bureau's preoccupation with secrecy. Sales organizations, marketing literature, and the general level of publicity to promote industrial products fostered the very exchange of information that BuOrd sought to restrict. BuOrd did approve the sale of Sperry Gyroscope's Target Bearing Instruments and Turret Control Systems to some foreign navies, but it was less comfortable with other more threatening rivals. Elmer Sperry was beginning a personal fascination with the Japanese that included sharing American technology and supplying equipment to the Imperial Navy.[89] Japanese naval officers toured the Sperry plant and inspected the Battle Tracer system. The bureau had not purchased the rights to the Battle Tracer, and indeed had lost interest in the device, but it still worried that such a demonstration would reveal to the Japanese other parts of the fire control system which operated in the American fleet. The Russian navy also adopted the Sperry Gyrocompass; the Russians deemed the device so successful that the Sperry representative in Moscow was decorated by the Czar.[90]

BuOrd saw the company as an untrustworthy source of fire control technology. Ford Instrument, in contrast, had no salesmen, no foreign connections, no offshore plants; in fact Ford Instrument had no public image at all. Ford Instrument by nature was highly responsive to the demands of its sole customer and its sole source of income. Sperry's technical problems, its

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[88] Gillmor to Assistant Secretary of the Navy, June 13, 1918. RG 74 E-30 Box 584 Subject 29758 Folder 450-500.
[89] Hughes, Elmer Sperry, 395-303.

troubled relationship with BuOrd, and its internal difficulties finally caught up with the company. In 1920, Sperry Gyroscope lost a key contract for fire control systems for the new Battleships Colorado and Maryland. The company dropped out of the fire control business. It continued to supply the navy with gyrocompasses and gyropilots for submarines and surface ships, as well as naval searchlights and some smaller naval instruments.

Fire Control in the Twenties

Sperry's departure from fire control was part of a broad demobilization. With the end of World War I, military work of all types was scarce; The 1922 Washington Naval Treaty (and its 1930 London successor) set strict limits on numbers and sizes of naval warships; new building during the following decades was sporadic at best. Only three new battleships (Maryland, Colorado, and West Virginia) were laid down between the end of World War I and 1937. Still, within that limited sphere, fire control played an important role and did require new development. Treaty limitations measured tonnage, but improved fire control could increase the striking power of a given vessel with negligible added weight. The technology then, with its relatively low cost, mass, and volume (compared to ships), had high leverage for sea power. And the navy, with growing understanding of how to control development within its captive contractors, made the most of its limited resources to promote exclusive American leadership. As one path toward that leadership, the navy brought in the premier American technology company.

Though BuOrd had helped Ford Instrument get started in order to foster competition, disenchantment with Sperry Gyroscope left the bureau again with a single contractor. In 1918, then, the bureau sought new industrial talent to help solve the synchronization problem and create a new generation of control systems. The new contractor had vast technical resources and an established reputation within the navy as a supplier of electrical equipment: General Electric. G.E. was widely recognized as the leader in industrial research and certainly on the cutting edge of electrical technology. Nonetheless, the bureau's choice represented the established route, displaying the conservatism for which the bureau system was renowned. Rather than depend solely on small companies, with their attendant friction, instability, and unreliability, the navy sought familiar expertise. Beginning in 1920, then, General Electric not only brought a full

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[90] EAS to J. Strauss, July 17, 1916. BuOrd to SGC, August 14, 1916. RG 74 E-30 Box 586 Subject 29758 Folder 101-150. NIO, SGC to BuOrd, March 21, 1916 and March 23, 1916. J. Strauss to SGC, March 23, 1916. D.M.

research organization to the problem, but solid electrical skills as well — elements lacking at both Sperry Gyroscope and Ford Instrument. As a consequence, naval control system technology became both more stable and more electrical.

The choice of General Electric also reflected the background of the new post-war head of the Fire Control Section, William R. Furlong. A 1905 graduate of the Naval Academy, Furlong had earned a Master's degree in electrical and radio engineering at Columbia University in 1914 (he would go on to head BuOrd from 1937-41). During the World War I, he brought an electrical perspective and a keen inventive eye to his work in the fleet as a gunnery officer. In early 1918, Furlong sailed with the British Grand Fleet to evaluate fire control. He then returned to the United States to head the fire control section of BuOrd. Full of new ideas, Furlong immediately began looking to staff his department with electrical engineers.[91]

General Electric's Synchronous System

General Electric had no prior experience in fire control, but it had built a follow-the-pointer system for commanding the motion of the doors of the locks in the Panama Canal. The control system maintained a miniature simulation of the canal's locks, doors, and water levels in a central control room, much as a fire control system represented the field of battle in the plotting room. After learning of this system, in mid-1918, the bureau procured a motor from G.E. for testing, along with samples of a position indicator the company had designed. The devices, which were fully synchronous, seemed suitable for fire control, but General Electric had never worked in the area and had no engineers with experience in the technology — they would need navy direction. BuOrd chief Van Auken recalled, "no citizen or private manufacturer at that time had sufficient knowledge of director firing, or the complete needs of fire control, as to initiate a system." Still, G.E. had credibility and reputation within the navy, supplying a host of components to naval shipyards including the numerous electric motors required to run modern naval vessels. The New Mexico, launched in 1915, had been hailed as the "all electric ship," with everything

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Mahood to BuOrd, March 29, 1916. RG 74 E-30 Box 587 Subject 29758 Folder 51-100.

[91] W.E. Furlong Papers, Library of Congress, Box 4. As early as 1905 Furlong thought about improvements to fire control. His diary as an ensign aboard the Franklin that year criticizes the communications systems based on voice tubes, "as long as the sense of hearing is depended on there are going to be mistakes...instead of rigging up these temporary range transmission gadgets we should be trying to develop something that is practical under battle conditions." Furlong Papers, Box 5, General Correspondence, Military File, Franklin Diary, 1905. For Furlong's hiring of electrical engineers, see Box 1, General Correspondence Folder.

from steering gear to ammunition hoists to kitchen appliances run by G.E. electric motors. The period 1916-1920 also saw the brief heyday of G.E.'s advanced but short-lived turboelectric power drives for propulsion.[92]

William Furlong had also seen the sample G.E. synchronous device. When he returned from Europe he immediately began development of a synchronous fire control system for new battleships West Virginia, Colorado, and Maryland. For three years, Furlong literally tutored G.E. engineer Edward Hewlett and his assistant in fire control. Hewlett, the company's premier switch and switchboard designer from Schenectady, had designed the motors and switchboards for the Panama Canal. The men met several times a month; Furlong sketched ideas and Hewlett designed and implemented them in a system. Technical exchange between the navy and its contractors reduced to the interaction of these two men. Between 1918 and 1920 Hewlett developed a new fire control system built around the self-synchronous motor, or "selsyn." Hewlett and his assistants built models, laid out systems for range and deflection transmitters, bearing indicators, compass relays, directors, control towers and a broad variety of devices and systems.[93] Together, Furlong and Hewlett matched military experience to technical expertise.

The G.E. self-synchronous system, or "selsyn," as it became known, used A.C. rather than D.C. electrical signals. It kept two dials, connected by three wires, exactly in line with each other. [*Figure 2-11: AC Selsyn Principle] Thus if one rotor is connected to a "master" dial, a rangefinder for example, and the other one connected at some distance to an indicator dial, the dial will read exactly the setting on the rangefinder. G.E. called this technology "Selsyn" but BuOrd copyrighted "synchro" to mean the same thing regardless of manufacturer.[94] The basic idea was not unique to G.E.; other companies suggested similar systems, including the Pioneer Instrument company and Ford Instrument. During World War II, several companies (including

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[92] Wilbur R. Van Auken, "Adoption of General Electric Fire Control System," July 23, 1929. RG 74 Box 1740, Folder General Electric Fire Control — General. Also see John Winthrop Hammond, Men and Volts: The Story of General Electric (Philadelphia: J.B. Lippincott Co., 1941) 356-8, 370-1. For the "all electric ship," see William R. McBride, "Strategic Determinism in Technology Selection: The Electric Battleship and U.S. Naval-Industrial Relations," Technology and Culture 33 (no. 2, April, 1992) 248-277.

[93] W.R. Furlong to Admiral Larimer, January 8, 1932. Furlong Papers, General Correspondence, Military File, Box 4. This letter supports a commendation by the Navy of Hewlett for his work on the selsyn fire control system and narrates the development process. Also see General Electric Company, "Report of Expenditures up to October, 1920 for Development of Fire Control Apparatus as Shown by the Following Special Manufacturing Orders Since February 14, 1919," RG 74 Box 3251 Subject 39117 Folder 1-50.

[94] "Elements of the Synchro System," in U.S. Naval Academy, Postgraduate School, Fire Control Installations PGS No. 105, 1939.

Ford Instrument, Arma, and G.E) manufactured these devices in large numbers for a wide variety of control applications (rotating radar sets, for example, were all built around this technology).

BuOrd had not sought to exclude Sperry Gyroscope. Van Auken mentioned the selsyn to Gillmor and hoped the company would incorporate it into a data transmission system. Gillmor declined. Furlong also recalled he tried to interest the company, but "I could not get Sperry or others to tackle the job." Only when Sperry was losing contracts to G.E. did Elmer Sperry develop a fully synchronous system. They came too late. Sperry submitted its earliest patent application for a synchronous system in December 1919, by which time General Electric was well on its way.[95] Clearly angry about having been squeezed out, Elmer Sperry accused G.E. of violating his patents and criticized the company as unsuitable for fire control work because, "like all large commercial companies, [they] are built up and maintained to manufacture commercial products in quantity for long periods of time without change in design....they are too unwieldy to incorporate valuable ideas and suggestions coming from naval officers and others without going to tremendous expense."[96] Ironically, the shortcomings Elmer Sperry pointed out in G.E. reflected at least some of the failings of his own company in the navy's eyes (G.E. and Sperry eventually settled, and the navy admitted it violated the Sperry patents, claiming it had to in order to move the technology forward).[97]

In 1920 the bureau contracted with General Electric instead of Sperry for fire control for battleships Colorado and Maryland. By the end of that year General Electric had no less than six types of gun directors in design and a number of other fire control projects underway, all using synchronous transmission (although otherwise similar to earlier systems). Ford Instrument provided Range Keepers and a number of other instruments as a subcontractor. During the 1920s G.E. consolidated earlier fire control designs and converted them to fully synchronous operation.

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[95] W.R. Van Auken, "Report of Visit to works of Sperry Gyroscope Co. and Ford Instrument Company, March 12, 1918." Furlong to Larimer, January 8, 1932. EAS to William S. Furlong, September 18, 1919, EAS Papers. R.C. Hyatt, USN Bureau of Ordnance "Discussion of Modern Fire Control System," NWC, 1.

[96] Elmer A. Sperry to Comdr. C.S. McDowell, September 3, 1924.

[97] For BuOrd patent policy, see O.G. Murfin, Memorandum for File, "Patent Rights for Fire Control Material," August 12, 1933. R.K. Davis, Office of the Judge Advocate General, to Admiral Furlong, "Bureau of Ordnance, New Patent Clause," April 6, 1938, RG 74 Box 1740 Ford Instrument Company Folder. For Sperry's position regarding G.E., see Sperry Gyroscope Company, "An Analysis of the Fire Control Patent Situation in the U.S. Navy," n.d., ca. 1920, and associated correspondence in RG 74 E-30 Box 2924, 37186. This folder contains a handwritten note, 1922, that "G.E. reports that it has arranged with Sperry, Hammond, Ford to use apparatus. This is covered by clause in contract allowing not over $9000 for patent rights."

G.E. built directors not only for the big main battery guns, but also for secondary "broadside" batteries on battleships West Virginia, Colorado, and Maryland.[98]

G.E. brought two key innovations from electric power into fire control: power drives and switching. Power drives coupled a producer of information, either an observer or a rangekeeper, directly to the motion of the guns, and switching allowed the entire system to be "programmed." In 1922, Ernst F. Alexanderson, the G.E. engineer who had done pioneering work in high-frequency alternators and other fields of power electronics, became involved in Hewlett's fire control work. Alexanderson studied vacuum tube drives for selsyn motors which allowed synchronous instruments to drive high power equipment. This technology eliminated follow-the-pointer indicators, allowing instruments, even delicate calculators, to move the turrets directly, without human intervention. Moving large machinery with small signals easily leads to oscillation and instability. Much of G.E.'s research in the mid twenties consequently related to "antihunt" devices for stabilizing these servo loops. In 1930 Alexanderson and G.E introduced "thyratron" control for high power motors. The thyratron was an electrical tube, but unlike a traditional "vacuum tube," it contained specific amounts of inert gasses. While not able to amplify small analog signals like a standard tube, the thyratron could switch high currents on and off with small electrical inputs. Later in the thirties Alexanderson and his group introduced the "amplidyne," a power amplifier based on a dynamo also capable of amplifying small signals into immense amounts of electrical power. The amplidyne was incorporated as an electric drive on navy turrets, and thyratrons drove motors in smaller applications.[99] G.E.'s work in power drives automated the

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[98] In these "master gun" setups, a gunnery officer aimed at the target with a single, instrumented gun mount (equipped with a Mark II "Baby Ford" rangekeeper), and gunners on the other mounts followed-the-pointer to track the master's movements. For a good summary of fire control in the early twenties, see Comdr. R.C. Hyatt, USN, "Discussion of 'Modern Fire Control System,'" Lecture to Naval War College, February 2, 1925 NWC XOGF-44. For a list of G.E.'s fire control work in 1920, see NIO, General Electric, to BuOrd, "Maryland and Colorado Contract #2992, Fire Control, General Information of Progress of Work," November 15, 1920. RG 74 E-30 Box 3251 Subject 39117 Folder 1-50. Friedman, US Naval Weapons, 35 appraises fire control in the twenties. For a technical description of one of these systems, see "Main Battery Fire Control System, U.S.S. West Virginia," Chapter IV, in U.S. Naval Academy, Postgraduate School, Fire Control Installations PGS No. 105, 1939. Also United States Naval Academy, Notes on Fire Control 1940 250-70.

[99] James E. Brittain, Alexanderson: Pioneer in American Electrical Engineering (Baltimore: Johns Hopkins University Press, 1992), 204, 219-222, 237-242. See also United States Naval Academy, Dept. of Ordnance and Gunnery Naval Ordnance and Gunnery Volume 1: Naval Ordnance (Washintgon: Bureau of Naval Personnel, USGPO, 1955), Chapter 10 section d., "Amplidyne Follow Up System," 221-6. Also Stuart Bennett, A History of Control Engineering, 1930-1960 (London: The Institution of Electrical Engineers, 1993), 10-12.

articulation of the control system, converting low-power information generated by rangekeepers into high-power signals required to move guns.

Where power drives coupled information to articulation, switchboards routed information between sensors and processors. Fire control became generalized: each sensor produced a signal, calculators translated signals, and output actuators turned signals into motion. For redundancy, warships had two conning towers with two each of gun directors, spotting telescopes, and rangefinders. Turrets also contained their own rangefinders and instruments. The plotting room usually had multiple rangekeepers, and eventually an entire second plotting room was added with wiring physically and electrically separate from the main control room. With the switchboard, the ship's crew could easily switch signals between all these components, and hence optimize the system for different applications. For example, in *divided fire* the main battery could split between fore and aft turrets and engage two targets simultaneously, using two directors. In the 1920s, battleships began carrying one or two seaplanes for spotting, allowing *indirect fire* when the ship itself could not see the target or the salvo splashes. Provision had to me made then, for entering spotting corrections after receiving them by radio from a spotting plane rather than from the spotter in the conning tower, as well as for switching quickly to standard optical sighting if the target came into view — all by means of the switchboard. [*Figure 2-12: System configuration and switching] With the switchboard, the control system took on new structures depending on these different operating modes. *Primary control*, *secondary control*, *auxiliary control*, *local control* all referred to different combinations of directors, rangefinders, and plots determined by the switchboard which made them "interchangeable."[100]

G.E.'s switchboards made the fire control system programmable; they could reconfigure it for a broad variety of contingencies. Covering the walls of the plotting room, switchboards connected individual elements to connect to common "busses;" in a director or a turret, different signals could be employed by connecting equipment to different busses. By changing the switch settings, the system could be given a new configuration. These diverse arrangements, of course, played an important role in battle, since the "system" needed to be robust to the loss of any of its

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[100] Comdr. G.L. Schulyer, "The Present Status of Ordnance Developments in the US Navy," Lecture to Naval War College, Newport, Rhode Island, March 9, 1928. RG 15, Naval War College, Newport, Rhode Island. Annual lectures on ordnance by BuOrd personnel to to the War College trace the development of fire control during the

individual elements. Redundancy, then, was built not only into the components themselves, but into the very system structure. Flexibility meant reliability. Such systems required G.E.'s synchronous data transmission: the Sperry step-by-step type would need to resynchronize every time a switch was thrown, but the selsyns came into line automatically.

In modern terminology, switchboards made naval fire control a hybrid analog and digital systems. The rangekeepers, as well as the various telescopes and rangefinders, produced analog signals: continuous, smooth data. The switchboard, however, formed a digital matrix; by routing those analog signals to different places, it programmed the system for different data flows. Fire control manuals portrayed fire control in block diagrams representing data flow, both within the computers themselves and aboard the entire ship. These manuals contained pre-determined tables of switchboard setups, which operators dialed in for a particular application.[101] As we shall see, this hybrid system paralleled other systems of the same period: the telephone network, which routed analog phone calls through a matrix of switching relays, and Vannevar Bush's Rockefeller Differential Analyzer, which combined analog computing elements through a set of telephone relays. The plotting room became a "control center," where information was processed, switched, and routed to its destination. "A machine is a little system..." wrote Adam Smith, and "a system is an imaginary machine." [*Figure 2-13: Analog/Digital system]

The Arma Engineering Company

As Ford Instrument became dedicated to fire control, the navy still had no second supplier of gyrocompasses, and hence still dependent on Sperry Gyroscope's monopoly. They then turned to another young company formed by an ex-Sperry employee, the Arma Engineering Company, a partnership of its two founders David H. Mahood and Arthur P. Davis (taking the first two letters of Arthur and the first two of Mahood formed "Arma"). Mahood worked in the fire control division of the Sperry Company during the war, and then as the chief civilian in the Ship's Electrical Apparatus Testing Laboratory of the Brooklyn Navy Yard. This facility handled virtually all the new equipment emerging from both Sperry and Ford for installation on navy vessels, so Mahood's position brought him into intimate contact with control technologies of the

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twenties. For a clear, diagrammatic example of the switchboard system, U.S. Navy Bureau of Ordnance, "Main Battery Fire Control System," pamphlet OP 1387, June 14, 1948, 5.

[101] U.S. Navy Bureau of Ordnance, "Surface Fire Control," OP 1701 n.d., probably mid 1940s. U.S. Navy Bureau of Ordnance, "Main Battery Fire Control System." OP 1387

time (as well as their problems). Davis, a young, self-educated engineer, had worked on switchboards at G.E. in Schenectady. Davis and Mahood formed the Arma company on January 30, 1918 with a capital of about $1500. Like Ford, Arma was originally located in Manhattan but moved later to larger quarters in Brooklyn and eventually to Long Island. In 1919, Arma obtained a navy contract for searchlights, in direct competition with Sperry Gyroscope.[102]

At the end of the first world war, the navy captured the design for the German Anschütz gyroscopic compass which inspired Elmer Sperry's original design. The Navy gave the Anschütz to Arma which built a business supplying gyroscopic control and stabilization systems for several navy bureaus. The company soon applied its energies to fire control. In 1924, Arma competed with G.E. for gun directors for the new aircraft carriers Lexington and Saratoga. It won the contract by underbidding G.E. by a factor of two, thus beginning a relationship with the Bureau of Ordnance.[103] Arma, like Ford Instrument, became a captive supplier, fully responsive to the needs of BuOrd. Arma seemed like a small version of Sperry Gyroscope, but optimized for BuOrd's secret work. Founder D.H. Mahood contrasted Arma to Sperry Gyroscope: having responded to a request by the navy "to enter fields of work which were then in the hands of a monopoly [Mahood wrote]...we have had no other customer but the U.S. Navy Department, have never sought any foreign or commercial contracts and have maintained the fullest secrecy which would have been impossible otherwise." Mahood wrote, "We have considered ourselves part of the Navy Department."[104]

Arma's became specialized in applying gyroscopes to fire control. In 1929, Arma introduced the "Stable Element" which employed a gyroscope to maintain an absolute horizontal and vertical reference for a fire control system. It recreated the horizon mechanically in the bowels of the plotting room, analogous to the "artificial horizon" instrument in an airplane (introduced by Sperry Gyroscope at about the same time). Arma did not invent the idea. The British Navy, Sperry Gyroscope, even the National Bureau of Standards had all experimented with or designed

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[102] S.J. Davy, "A Case Study: The American Bosch Arma Corporation," January 21, 1958, Term Paper, Sloan School of Management, MIT. Davy, a fire control engineer at Arma, compiled this history from annual reports and interviews with Arma employees.

[103] Davy, "Case Study." Hyatt, "Discussion of Modern Fire Control System," 8.

[104] D.M. Mahood to Chief of BuOrd, December 22, 1931. RG 74 E-25 Box 1740 Arma Folder.

similar devices before 1920.[105] Like Ford Instrument with its rangekeeper, Arma created a workable device that filled a niche for the bureau, and Arma was willing become a captive contractor.

The Arma machine, located in the plotting room next to the rangekeeper, now fired the guns with a trigger.[106] It could command continuous aim by sending orders to the guns to track the target. In rough seas, the guns could not move rapidly enough to maintain continuous aim, so *intermittent aim* kept gun train and elevation fixed in relation to the deck, moving with the ship's pitch and roll. When the gunnery officer depressed the firing key, the stable element waited until the flat point of the roll and then automatically sent the firing signal. The stable element anticipated the proper angle by a certain finite period of time so the ship would be at the exactly correct angle, not when the gun fired but a few milliseconds later, when the shell left the barrel.[107] With the stable element, the bulk of the control system had now moved to the plotting room. The gun director in the conning tower merely tracked the target in range and bearing.

Fire Control in the 1930s

Surface fire control achieved a certain technical maturity around 1930. The essential configuration did not change much until well into World War II, except for myriad incremental improvements. Despite the depression and treaty limitations, new ships continued to be built, some with funds from Roosevelt's National Industrial Recovery Act. Battleship construction began again in 1937. While the closed and secret world could allow rapid innovation, it also separated the community from advances in other fields. No evidence indicates fire control engineers were aware of parallel work in feedback amplifiers or servomechanisms until the late 1930s. Even the newer systems in use as of Pearl Harbor were basically designed around 1930;

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[105] SGC to BuOrd, August 19, 1915. RG 74 E-30 Box 587 Subject 29758 Folder 51-100. Gillmor to EAS, January 17, 1917, SGC Box 32 Folder Comdr. F.C. Martin. Cresap to EAS, February 11, 1918, SGC Box 32 Folder Lt. Logan Cresap Fire Control Correspondence. Earle to NIO, SGC, March 17, 1918. RG 74 E-30 Box 585 Subject 29758 Folder 301-350. EAS to Earle, April 15, 1918 and Earle to EAS, April 27, 1918, RG 74 E-30 Box 585 Subject 29758 Folder 400-450. B.A. Wittkuhns to BuOrd, July 2, 1931. RG 74 E-25 Box 1741 (S-71) Sperry Gyroscope Company Folder.

[106] It measured the pitch and roll and resolved them into *level* and *cross level*, the components of the motion the line of fire. The elevation of the gun then could correct for level, and cross level contributed to a quantity called *trunnion tilt*, which affected both the guns' elevation and angle of fire.

[107] Bureau of Naval Personnel, Naval Ordnance and Gunnery Volume 2: Fire Control (Washington: 1955) 120-8, 140-8.

they remained operational through the war. On December 7, 1941 all American battleships except five still used the Ford Rangekeeper Mark I, originally designed in 1915.[108]

The technical maturity paralleled an organizational and technical conservatism. Hannibal Ford became president of Ford Instrument in 1930, removing him from daily operation of the company (he would retire in 1943). Given Ford's influence over his company's technology, the change surely had practical effects, but it was symbolic as well, signaling the company's stability as an engineering firm and government contractor. The company did, however, begin to hire established technologists. Two important engineers joined the firm: Edward Poitras and James Tear. Poitras had been a student of Harold Hazen and Vannevar Bush at MIT, before going to G.E.'s Schenectady works and Tear's laboratory.

A new holding company, the Sperry Corporation, acquired Sperry Gyroscope in 1930 and then the Ford Instrument Company in 1933. This acquisition, however, put the bureau in a quandary, as their animosity toward Sperry had not yet cooled. Because "the Sperry Company has proven unmindful of protecting American interests of secrecy in the past and there is no assurance that they will become less careless in this respect" BuOrd threatened to restrict Ford Instrument's access to Naval technology. Probably because of these threats, even under the same umbrella Sperry Gyroscope and Ford Instrument had very little contact with on another, and they retained their separate corporate cultures. Ford engineer William Newell, who joined the company in 1926, recalled having "practically no contact" with Sperry Gyroscope.[109]

By the 1930s fire control matured as an integrated system composed of General Electric, Ford Instrument, and Arma equipment. It culminated the bureau's second, interwar phase of control systems engineering. General Electric's synchronous data transmission system brought data to and from sensing instruments and its switchboard routed the signals to different places. The company's power controls tied these signals to the movement of the guns, and hence to the gunnery officer in the plotting room. Ford Instrument's rangekeepers collected data from the system, bringing the target's motion into the machine where it could be tracked, predicted, and sighted. Arma's stable element stabilized this pitching, rolling, heaving apparatus, not by keeping

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[108] United States Navy, Administrative History of the U.S. Navy in World War II Volume 79, Fire Control (Except Radar) (Washington: 194XX), 17.
[109] C.C. Badger, "Memorandum for Chief of Bureau," RG 74 E-25 Box 1740, Fire Control-General Folder. William Newell interview May 12, 1995.

it physically still but by providing reference signals, minute corrections which could be subtracted and factored out of the calculations. Still, despite its sophistication (and because of it), the control system began to push the limits of its dynamics, "Having grown up like Topsy, it varied to a certain extent from ship to ship, depending on the date of installation and the progress of modernization, but the equipments shared several things in common: they were able to provide adequate fire control for main battery guns, and they were rough in operation, unpopular with crews, and far from the ultimate in fire control equipment."[110] [*Figure 2-14: Main Battery Fire Control System, 1940]

Emergence of the Antiaircraft Problem

By the 1930s, control technology had essentially caught up to the capability of the big guns, which stayed comparatively constant since before World War I. A new problem emerged, however, for which engineers had few adequate solutions: antiaircraft fire control. Antiaircraft inherited the difficulty of the surface fire control problem, but with added complexity: aerial targets were smaller and moved faster and in three dimensions, and shells had to be fuzed not to explode not on impact but after a finite time period, another variable in the system. Antiaircraft guns were smaller and more numerous than surface batteries, typically five or six inches instead of fourteen or sixteen, so gun directors had to be smaller and faster. The introduction of the 5-inch 38-caliber (5"/38) "dual purpose," gun, which could fire at airborne or surface targets, introduced a standard, high-quality secondary battery gun which could benefit from precise director fire.[111]

Ford Instrument built the first naval antiaircraft director in 1926; it became operational as the Mark 19 the following year. This device had 55,000 moving parts and integrated an entire control system into a single unit, including a rangekeeper, a stable element, and level and tracking telescopes. It used the same calculations as for surface fire control but handled a target at a different altitude from the firing ship.[112] While an impressive solution to a difficult problem, the Mark 19 took only a first step. In the words of one history, "its continued use represented principally a monument to the difficulty of obtaining sufficient peacetime appropriations for naval development." On December 7, 1941, forty two of these devices were installed in the fleet.

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[110] Buford Rowland and Wlliam B. Boyd, <u>US Navy Bureau of Ordnance in World War II</u> (Washington: Bureau of Ordnance, Department of the Navy, 1953), 373.

[111] Jurens, "The Evolution of Battleship Gunnery," 257-8.

[112] William Newell interview May 12, 1995.

Several successors also entered the service, including the Mark 28, built by G.E. with a Ford Rangekeeper and an Arma stable element. Then in 1934 came the Mark 33, built by the Naval Gun Factory in Washington, DC, which included the same rangekeeper and stable element, but added power drives to move the ever heavier machinery. The Mark 33 mounted on a pedestal atop the ship, earned the appelation "apple on a stick" because of its top-heavy appearance. And top heavy it was, the whole unit weighted nearly 20,000 pounds and wobbled considerably in all but the lightest seas.[113]

The Mark 37, introduced in 1939, was state of the art in 1940. It was not self-contained but rather divided its functions between the director and a room below deck. Hence it was no longer called a director but rather a *gun fire control system*. The Mark 37 employed a Ford rangekeeper, but its designation was changed to Mark I *computer* in recognition of the increasing ability of the machines to track more information than range. It also incorporated an Arma Stable Element, essentially the same as the devices controlling surface fire. The Mark 37 was hailed as the first gun director specifically designed to anticipate the inclusion of radar, but this distinction seems to derive from little more than the unit's flat top for mounting an antenna.[114]

Most important, the Mark I computer incorporated "fully automatic rate control," which automated the feedback loop for course correction. The operators, rather than adjusting the course and speed of the target to match the observed data, merely tracked the target, and the Mark I computer converged on the solution by itself. This closed-loop feature saved the operators some effort, but it brought an essential difficulty which pressed the limit of technical knowledge in BuOrd and its contractors. The tracking and convergence feature of the Mark 37 had a stability problem in its servo loop and would oscillate when perturbed by disturbances. When radar was added in 1941, the problem became still worse and led to a complete breakdown. How each of these loops interacted and fed back on each other was poorly understood, and "when the first complete director-to-gun system was tested, operation was entirely unsatisfactory." The equipment was already in production, and war was rapidly approaching. Still, the Mark 37 became the most prominent antiaircraft director of the World War II era, and its presence is a

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[113] For an excellent summary of the state of naval fire control in 1940 by a participant, see United States Navy, Administrative History of the U.S. Navy in World War II, 137-145 and Jurens, "The Evolution of Battleship Gunnery," 259-60.

[114] United States Navy, Administrative History of the U.S. Navy in World War II, 146-7.

visual landmark and nearly all U.S. warships of the time. More than eight hundred of these units were eventually produced, in 92 separate modifications.[115] [*Figure 2-15: Mark 37]

Naval Fire Control in 1940

By 1940 the action of naval warfare, what John Keegan has called "the face of battle," had shifted. Buried deep in the armor-protected hull, officers who operated machines and supervised data flow became those who actually fought the enemy. Naval gunnery took place, in the words of the foresighted Fire Control Board of 1905, through a "system of information." Fire control joined the airplane and the machine gun in displacing people from the immediacy of combat, creating technologically-mediated war. By the 1930s no realm of warfare had become as mechanized, precise, and remote as naval gunnery.

This displacement could not proceed in isolation; it necessarily accompanied parallel shits in infrastructure. As technology changed, the critical industries shifted from gun manufacturers to instrument manufacturers to electrical and electronics companies. The Bureau of Ordnance, with its fire control section, supervised and directed these shifts. An esoteric technology like fire control had no commercial applications, so the navy had to educate and train each new company it brought into its secret fold. Officers had to ensure the contractors and their technology continued to suit the navy's needs, to keep their secrets, and to deliver their equipment. Fire control technology had to be controllable.

Aircraft were making the battleship itself obsolete, and increasingly the ship's resources went toward defending itself. Centralized fire control, in fact, had largely reached its limits by 1940 The antiaircraft directors of the 1930s, reversing the earlier trend, began to distribute fire control around the ship. These devices, however, which merely adapted surface fire control to aerial targets, could not counter the threat. Interwar fire control remained both cumbersome and open loop: human operators still closed the primary feedback loop, observing targets, plotting shell splashes, and making corrections. To successfully fight aircraft, fire control needed to be quicker and more autonomous, if less precise. Toward this end, the navy followed two strategies. First, return the guns to the gunners; allow them to again move the guns with their bodies, and use technology to enhance their perception. And second, close the control loop in the machine; automate perception and tracking. These solutions required cheaper, mass-produced gun

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[115] Rowland and Boyd, US Navy Bureau of Ordnance, 377-8.

directors, radar for tracking, servomechanisms to move the guns, and new techniques of control and systems to maintain stability. None of these technologies, however, was under the control of the Bureau of Ordnance as war approached.

APPENDIX 2-A: ALGORITHM OF THE FORD RANGEKEEPER MARK I

The own ship has a course and speed (Co and So), as does the target (Ct and St) (see Figure 2-6). The essential problem is to determine the course and speed of the target ship. Consider an imaginary line, the *line of fire* which connects the two ships, which could also be called *line of sight*. The length of this line, of course, is the *target range*. The course of the target with respect to the line of fire is known as the *target angle*, Bt. Consider then that each ship has two velocity components relative to the line of fire, Xo and Yo for the own ship, and Xt and Yt for the target ship. Y, for both ships, indicates the component of the ships motion along the line of fire (in the direction of range). The sum of Yt and Yo is identical to how quickly the two ships are closing or opening in range, and hence is the *rate of change of range*. Xo and Xt are the components of the ships motion normal to (at right angles to) the line of fire, and their sum is known as *deflection*. To hit at a moving target, one would not aim the gun directly along the line of sight but at some angle ahead of it, and that is *deflection*.

Figure 2-16 shows the basic layout of the Ford Rangekeeper's data flow and algorithm. [*Figure 2-16, FRK Mk I Algorithm].[116] Starting at the top, the course and speed of the own ship come into the machine from a gyrocompass repeater, and a revolution counter, respectively. Then initial guesses for the target's course and speed are cranked in by hand, usually taken from a Battle Tracer or hand plot (1) (the own ship's course is subtracted from the target bearing to give target course (2)). The two *component solvers*, resolve these data into their components relative to the line of fire, i.e. Xt,Yt, and Xo, Yo. Differential gears subtract these components from each other, to give dR, the change in range, and RdB, or change in deflection.(3) dR then goes into an integrator which produces R, or range, which varies linearly at a rate determined by dR. This R is only an incremental range, though, so it needs to be added to an initial observed range, Ro to produce an accurate "present range," which is read off a numerical dial (4). To calculate advance range, the range of the target at some time in the future, the range rate dR needs to be multiplied by that time interval. The time interval, Tp (time of prediction), consists of two factors. First, is

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[116] This description of the Ford Rangekeeper is compiled from data in Ford's original proposal for the device, Hannibal Ford, "Ford Range and Deflection Predcitor," May 15, 1915, BO E-30 Box 696 Subject 30199 as well as Terril, "Notes on the Theory of the Ford Range Keeper," "Rangekeeper, Mark I, Mod. 3" in U.S. Naval Academy, Postgraduate School, Fire Control Installations PGS No. 105, 1939.

the time of flight of the shell — the amount of time it takes the shell to reach the target after firing. The Ford Rangekeeper treats the time of flight as linearly proportional to range, which is only an approximation. The second component of Tp is the amount of time between when the "advance range" is read off the dial and when the gun is actually fired, or *transmission interval*. This number includes delays in data transmission to the turret, loading the shell, elevating the gun, etc. The transmission interval, To, is cranked in by hand. The sum of the time of flight and To are then multiplied by the range rate, which is then added to the previously-computed present range, to derive advance range.(5) Advance range can be adjusted up or down manually, depending on spotting corrections. Thus if the spotter sees that a shot falls 100 yards short, Jr is adjusted to subtract from the calculated advance range. RdB, or the rate of change of deflection, is then divided by R, the calculated range, to derive the change of bearing.(6) This quantity, dB, is then integrated to produce bearing, which, when added to initial bearing, calculates a *generated true bearing*.

A final mechanism derives the deflection. Deflection itself is expressed in knots, as the speed at which the target ship is sailing perpendicular to the line of fire. For a given range, this can be converted to an angle, the amount off the line of sight the gun needs to aim, and depends on four factors: (1) the target's change of bearing during the time of flight, (2) *drift*, the tendency of the trajectory to curve to the right due to the fact that the projectile is spinning, (3) wind, and (4) spotting and ballistics. The mechanism, a set of cams and multipliers, takes as input the present range (from which it calculates a time of flight), and the deflection rate or RdB. Jd, the spotting correction (in the form of "left" or "right" a certain number of yards), is entered in by hand. The system then calculates drift via another cam, and outputs on a dial the deflection setting for the guns.

Although elegantly executed, with the exception of the improved integrators these calculations are not qualitatively different from those in the British fire control machines. A key innovation of the Ford Rangekeeper, however, comes in the final stage of output. [*Figure 2-7, FRK Speed Error] This setup allows direct comparison of the "guessed," quantities entered in the beginning of the calculation and the calculated quantities produced by the machine. A cartoon of the target ship appears on a rotating dial. Its angle indicates the target ship's bearing, and a small "button" within the cartoon itself indicates the speed of the ship, as entered in the initial estimation. Ford's innovation is to derive "range rate" back out of the calculated "advance range" as it changes. This he accomplishes with an ingenious and subtle use of feedback, connecting the input and the output of an integrator together through a differential. If the speed of the shafts coming into the differential are the same, the output shaft doesn't turn at all, and the mechanism is in a kind of equilibrium. Since one of these shafts represents advance range, and the other the output of the integrator, they will only turn at the same rate when the integrator is adjusted so that the its output speed exactly matches the rate at which the advance range is changing. Otherwise, the output of the differential changes the position of the balls on the integrator, moving them toward the equilibrium position. At the equilibrium point, the position of the balls is proportional to the rate of change of the advance range. Thus, by taking a shaft rotation as an input and producing an output proportional to its speed, this arrangement acts as a differentiator (like a tachometer). Through a feedback loop, Ford inverted the function of the integrator — an accomplishment not repeated in other fields until a decade later.

The ship's own speed along the line of fire, Yo, is then subtracted from the output of the mechanism, producing the target's speed. This *calculated target component along the line of fire*, or the rate at which the range to the target is changing, then drives a needle, called the *horizontal wire*, that overlays the indicator which reads the observed speed button. Thus another feedback loop is set up, this one involving the human operator. He looks at the needle and the button, which implicitly compares the estimated values of target speed and course, with the calculated quantities based on other observations and integration. His job then, is to reduce the "error" indicated by the distance between the button and the needle, which he does by adjusting the estimates of target speed and course accordingly. This cycle of correction continues until the dials and needles match up. At that point the rangekeeper has converged on a solution for the target's course and speed which matches both the estimates and observations, and the predicted advance range will be accurate for setting the guns.

A similar cycle works for bearing. An integrator converts the rate of change of bearing into an incremental bearing, which, when added to the initial observed bearing produces a generated true bearing [*Figure 2-7, White and Gold Pointer]. This reading drives a needle called the *gold pointer* on a bearing scale. On the same scale, a *white pointer* reflects the observed present bearing, as indicated by the target bearing instrument. The operator then observes the difference between these two. Because these bearings may differ only slightly, a *vertical wire* exaggerates the difference to make it easier for the operator to read. Where the horizontal wire indicates errors in Yt, the white pointer and vertical wire indicate a need to correct Xt. It is worth noting, however, that the range comparison is based on advance range, the result of a prediction into the future, whereas the bearing comparison works off present bearing. The first model of the Ford Rangekeeper made no bearing predictions.

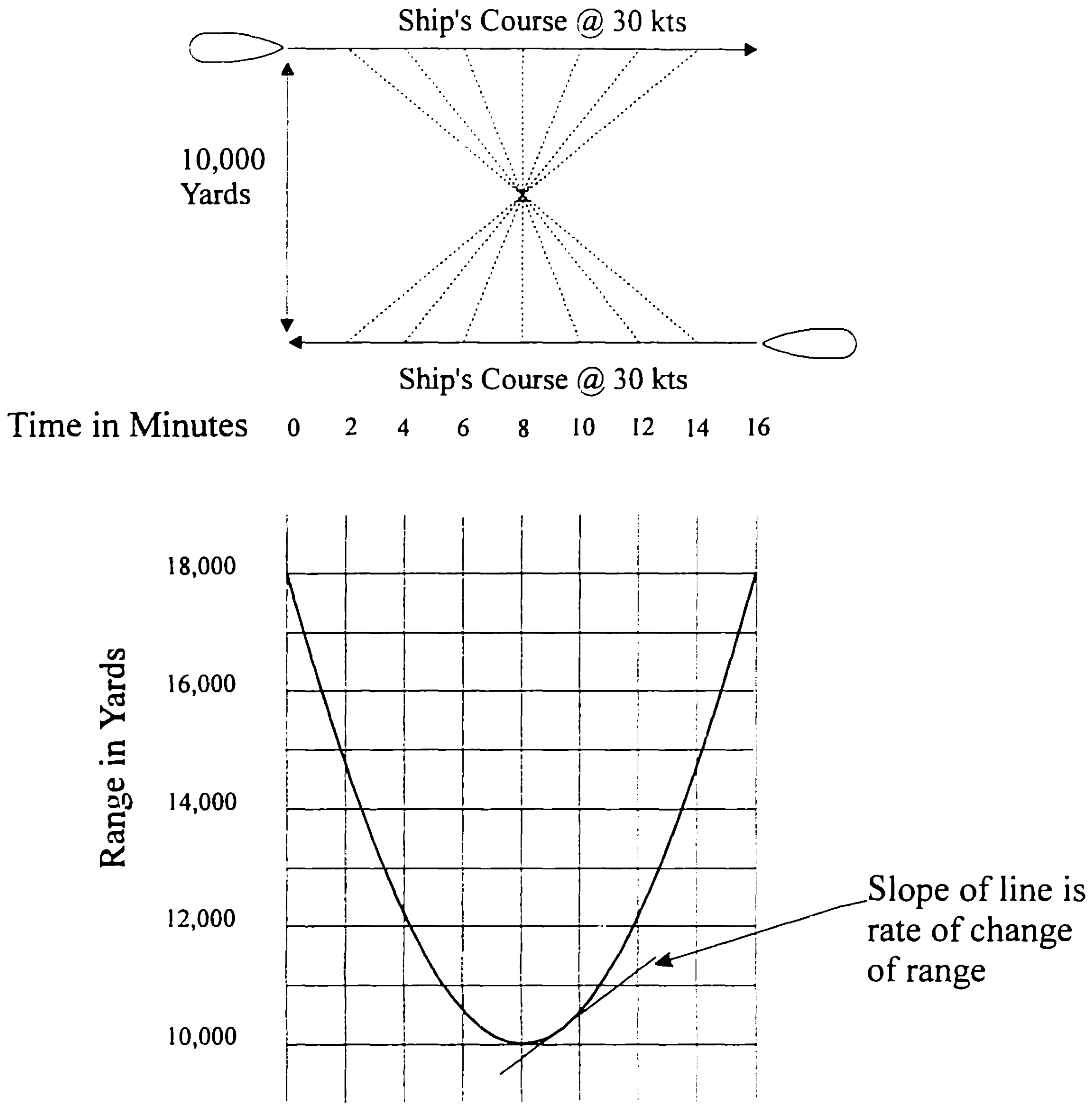


Figure 2-1: Illustration of change in the rate of change of range for two ships approaching each other on parallel courses 10,000 yards away @ 30 kts. (from Hannibal Ford's original proposal for a rangekeeper, May 15, 1915).

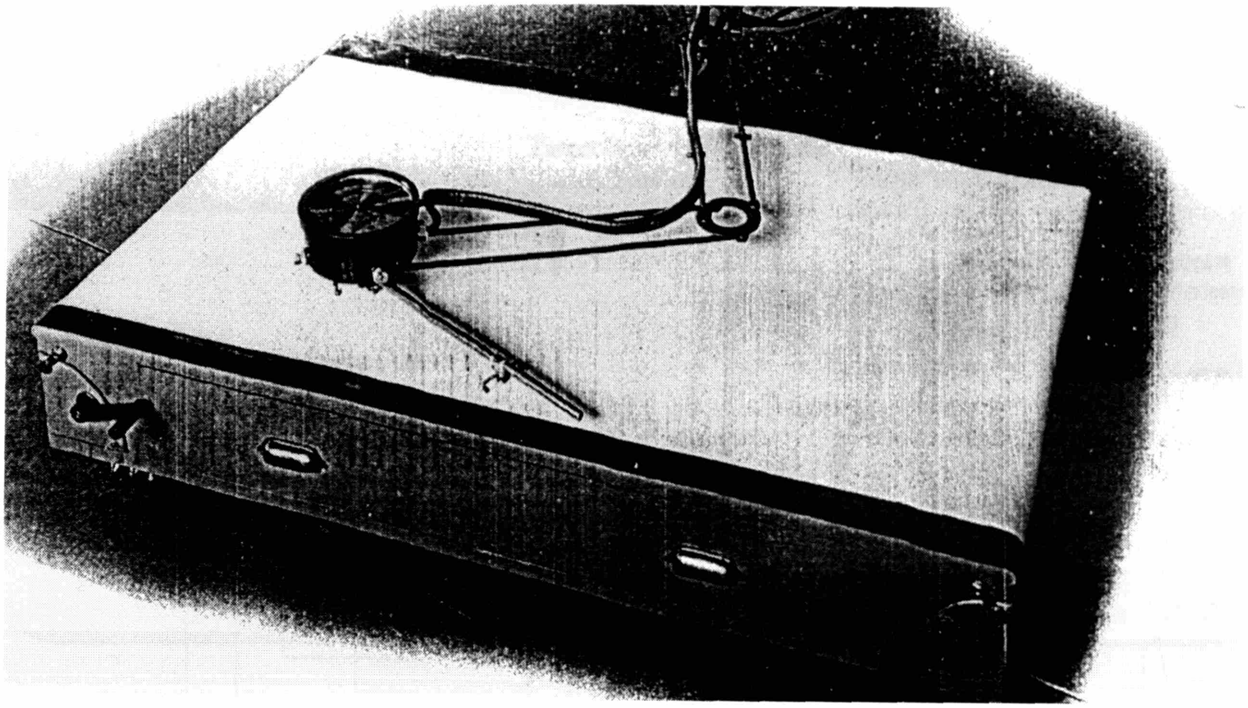


Figure 2-2: Sperry Battle Tracer (Hagley Museum & Library)

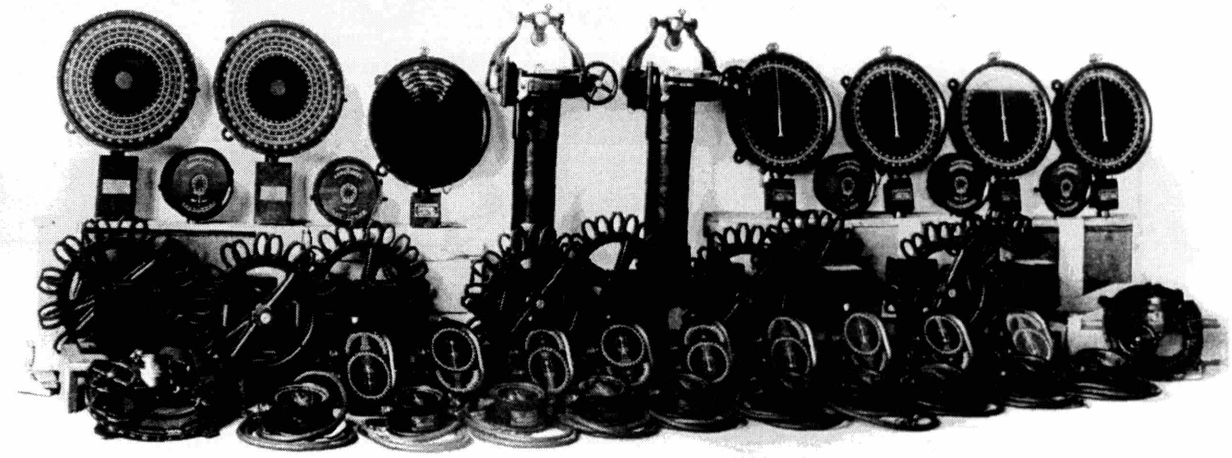


Figure 2-2a: Collected equipment of Sperry Naval Fire Control System
(Hagley Museum & Library)

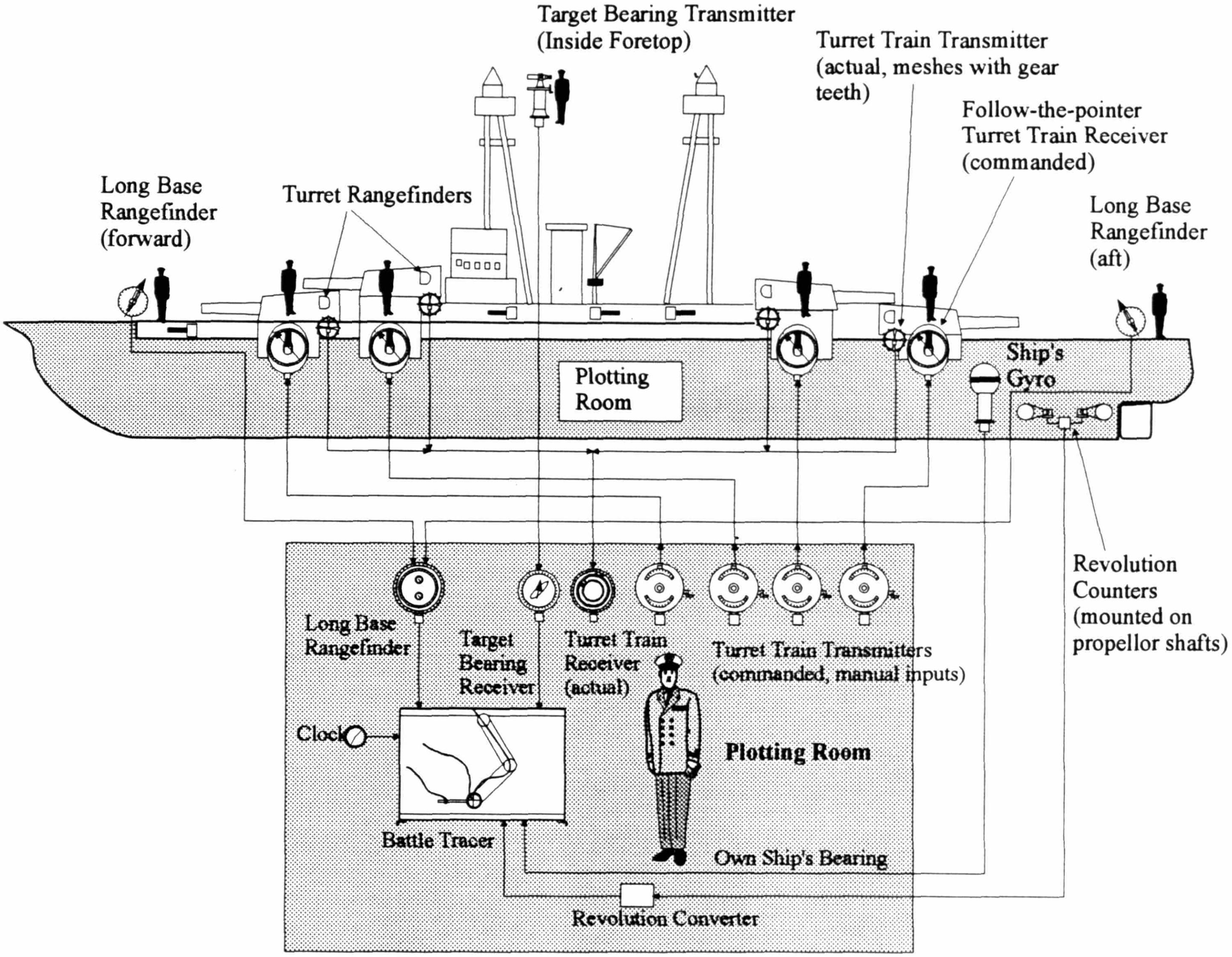


Figure 2-3: Sperry Battle Tracer and Fire Control System Layout (ca. 1916)

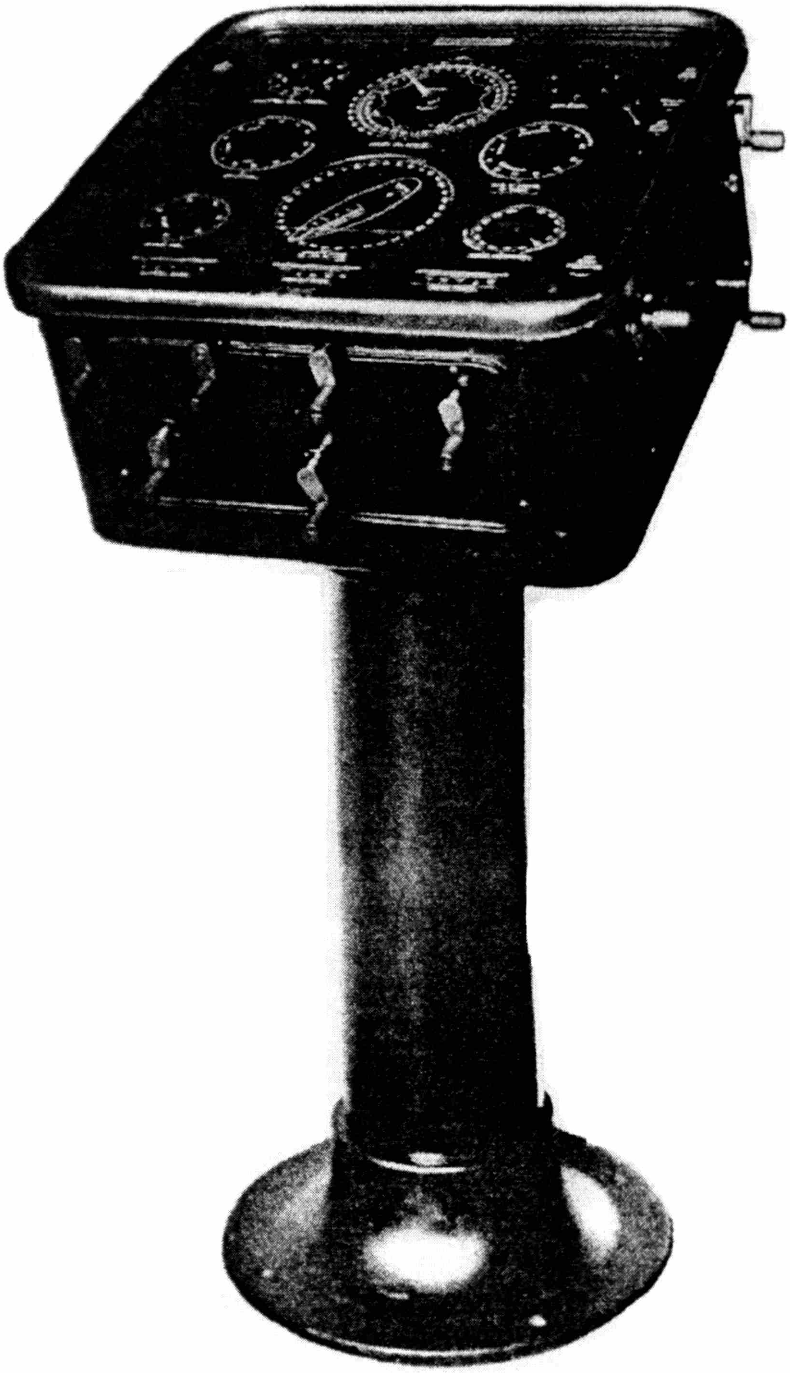


Figure 2-4: Ford Rangekeeper Mark I (1916)

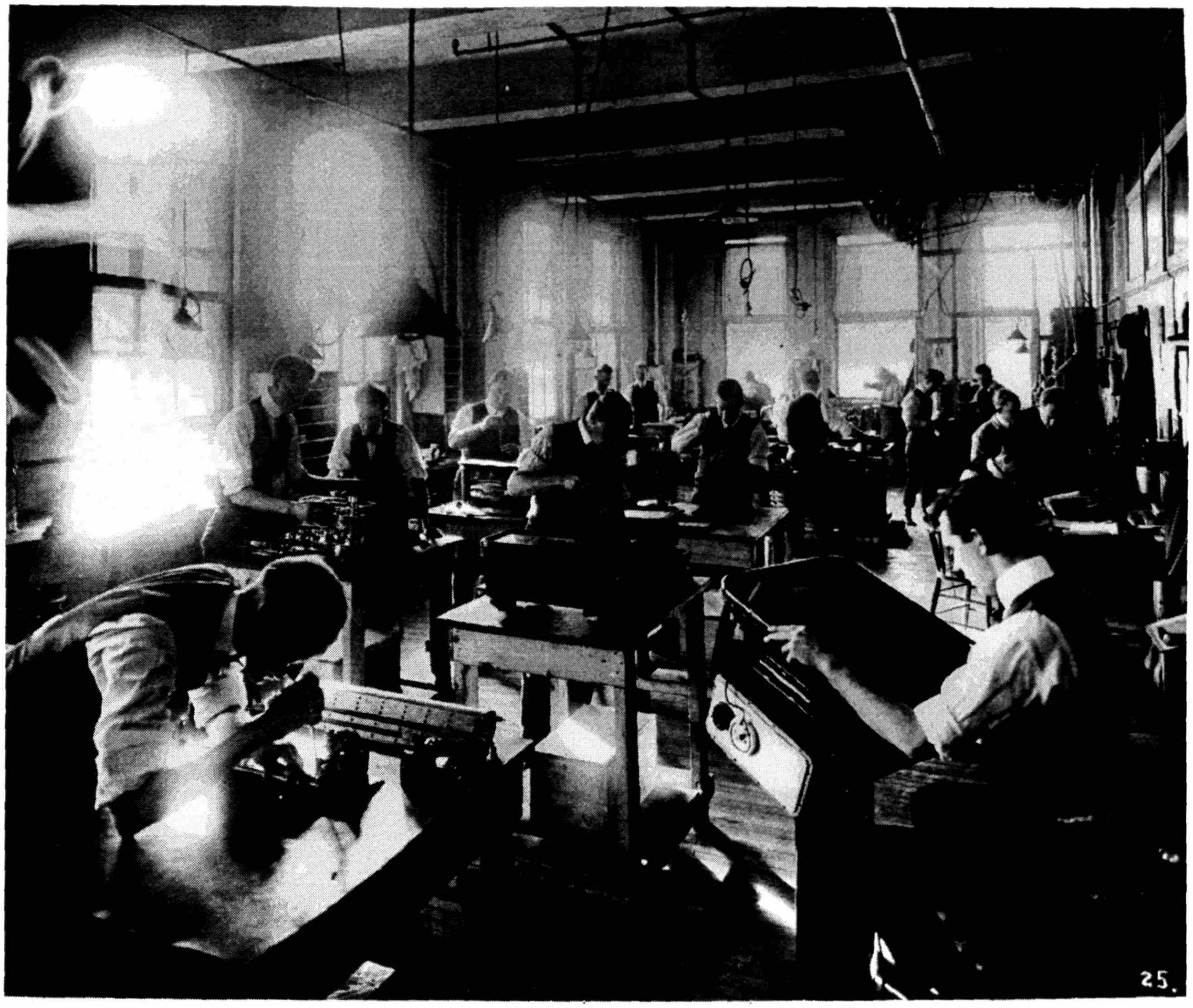
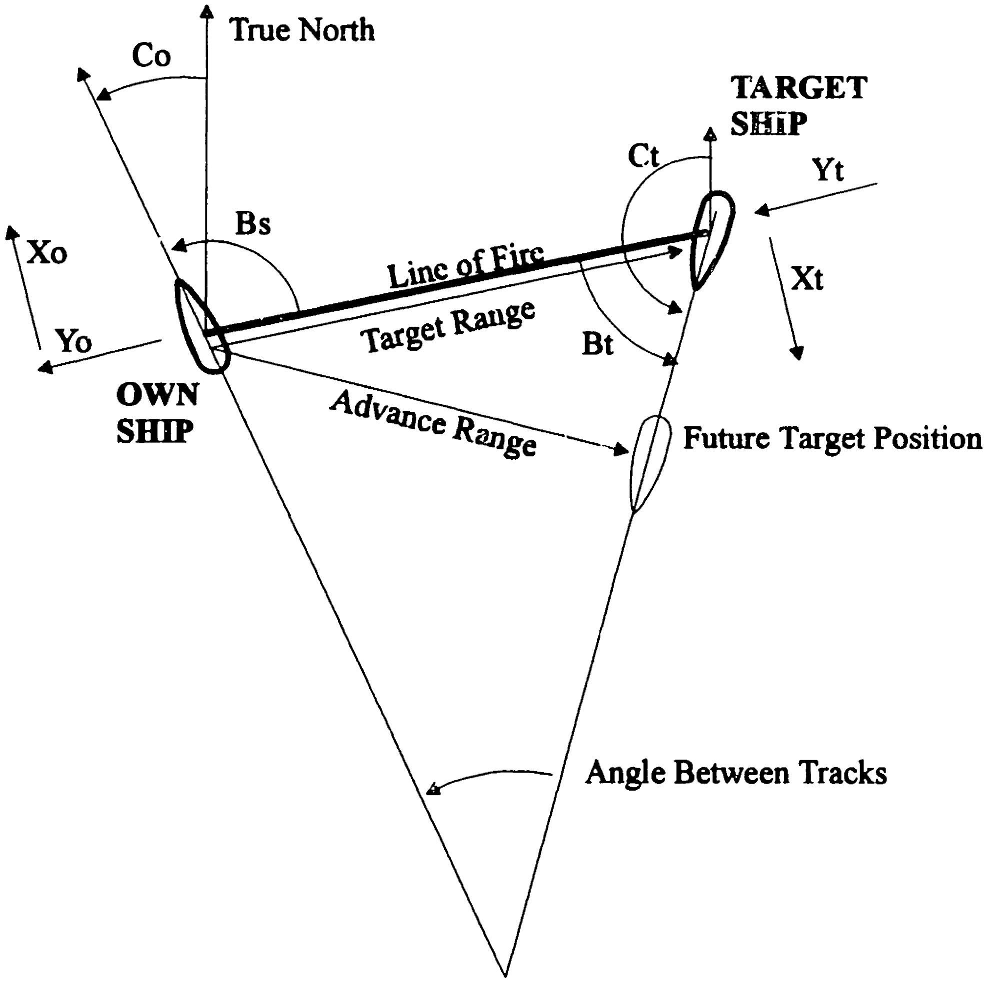


Figure 2-5: Rangekeeper production at the Ford Instrument Company, ca. 1916 (Hagley Museum & Library).



Co - Own Ship's Course
So - Own Ship's Speed

Ct - Target's Course
St - Target's Speed

Bt - Target Angle
(Target's course w/
respect to line of fire)

Xo - Component of Own Ship's Speed Normal to Line of Fire
Yo - Component of Own Ship's Speed Along Line of Fire

Xt - Component of Target Ship's Speed Normal to Line of Fire
Yt - Component of Target Ship's Speed Along Line of Fire

Bs - Relative Target Bearing

Figure 2-6: Angle measurements in Ford Rangekeeper

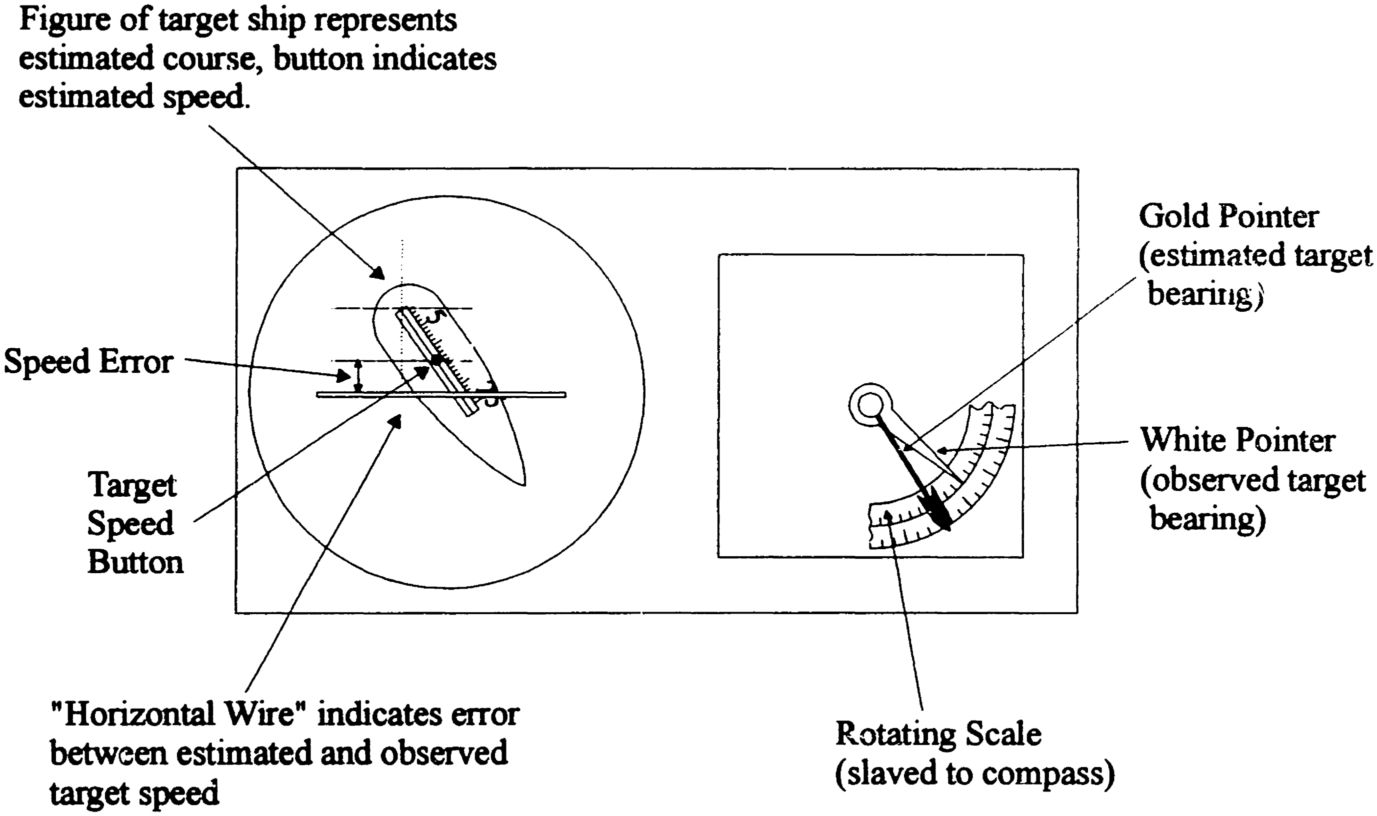
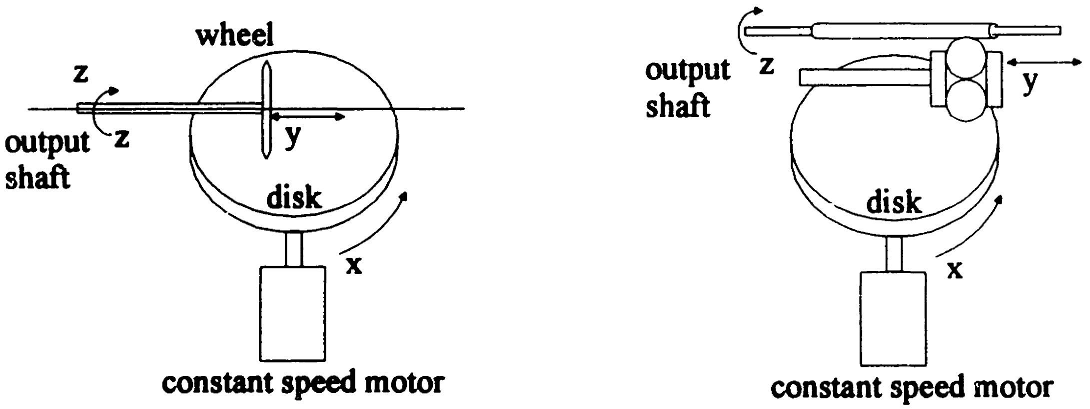


Figure 2-7: Subset of dials on Ford Rangekeeper. "Horizontal Wire" indicates error between observed and estimated target speed. "Gold Pointer" and "White Pointer" indicate difference between observed and estimated target bearing, against a rotating scale which indicates compass bearing.

Figure 2-8: Mechanical Integrators



Wheel and Disk Integrator
$z = \int y dx$

Ford Integrator
$z = \int y dx$

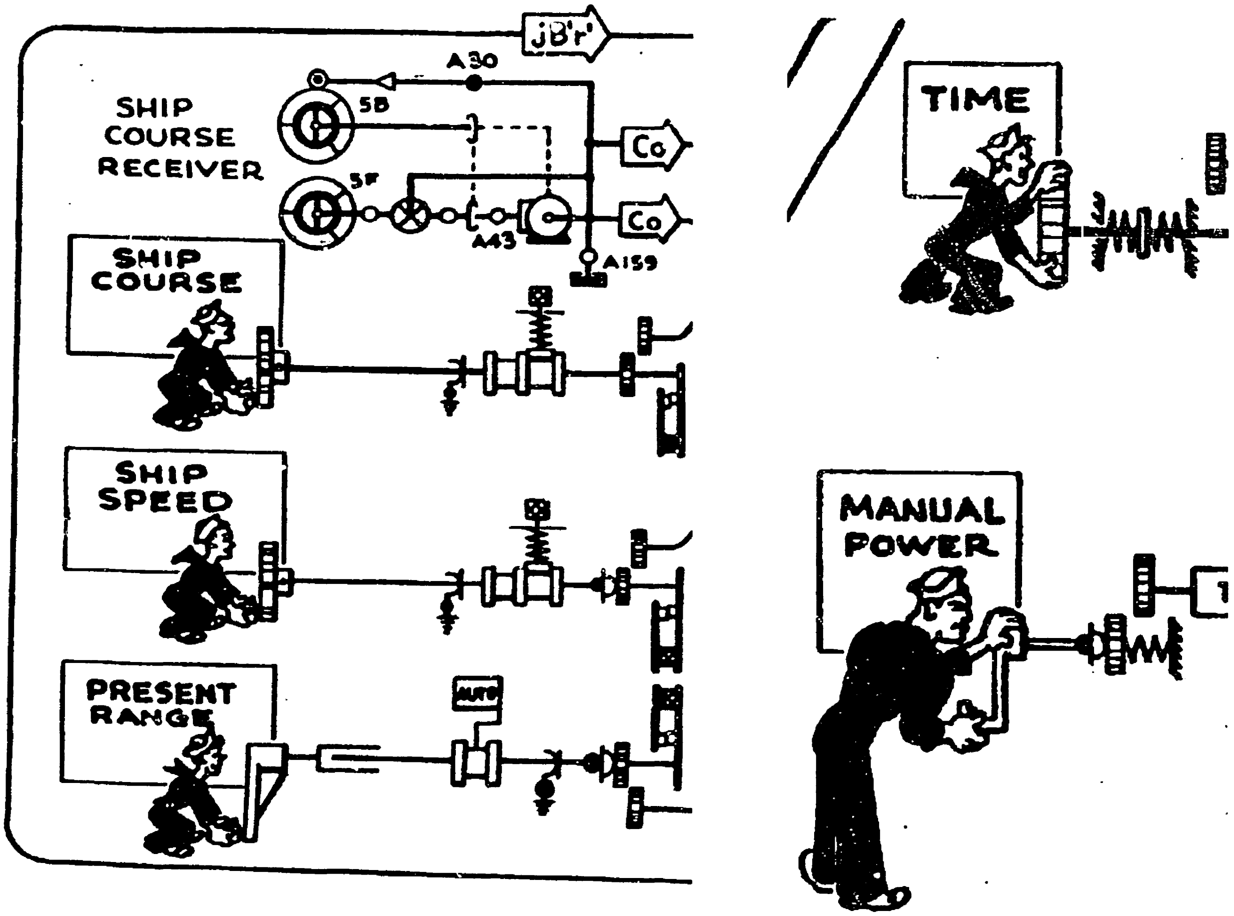
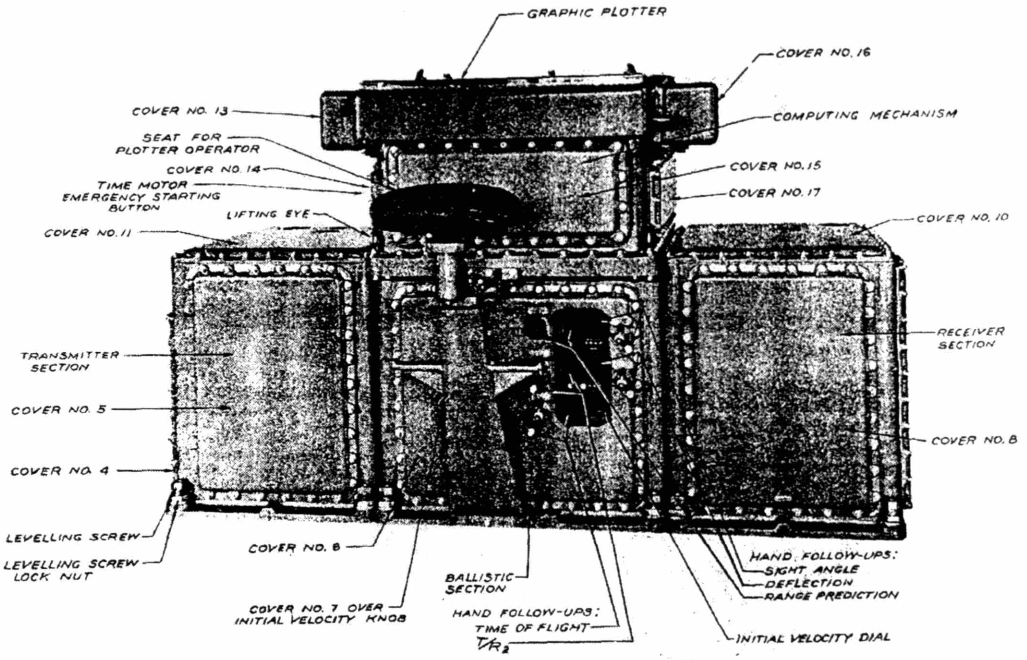
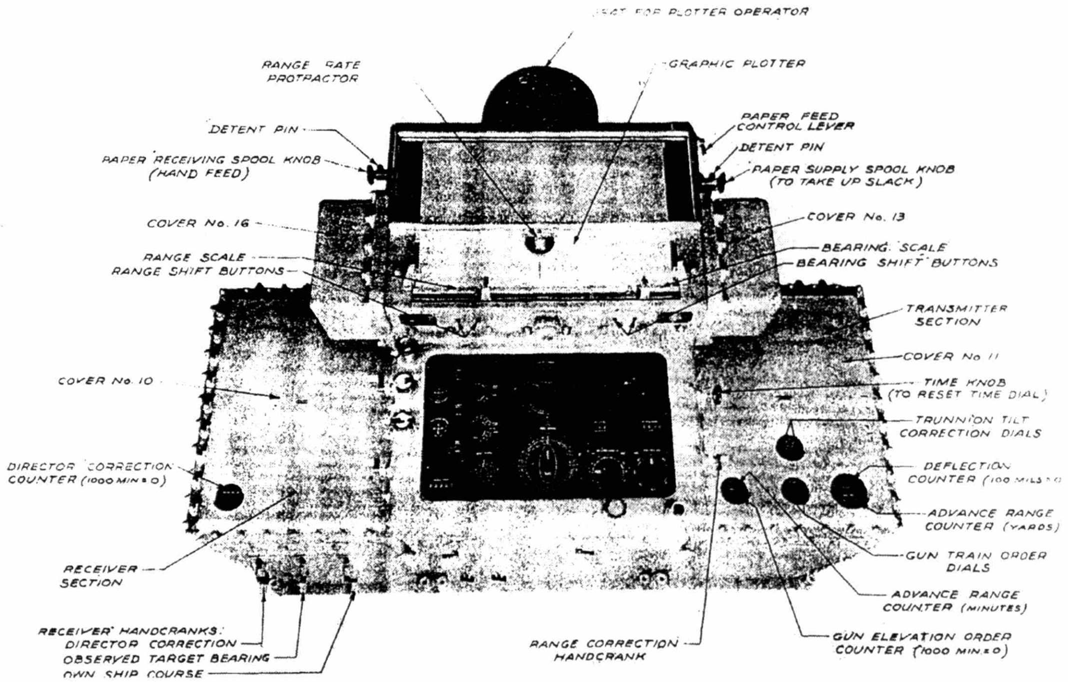


Figure 2-9: Mark 8 Rangekeeper, selections from system diagram showing user inputs. This clip represents about 5% of the total rangekeeper system diagram (courtesy John Testuro Sumida).

Figure 2-10: Two views of the Mark 8 Ford Rangekeeper (Mod 2), *circa* 1930. Note operator's seat and the five separate sections, receiver, calculator, ballistics, transmitter, and plotter. This machine was standard on battleships and cruisers from the 1930s on, and remained in service on battleships retired in the mid 1990s ("Fire Control Installations," Postgraduate School, U.S. Naval Academy PGS 6 no. 105, 1939, Chapter 9, courtesy John Testuro Sumida).



RANGE KEEPER MK. VIII, MOD. 2
REAR VIEW



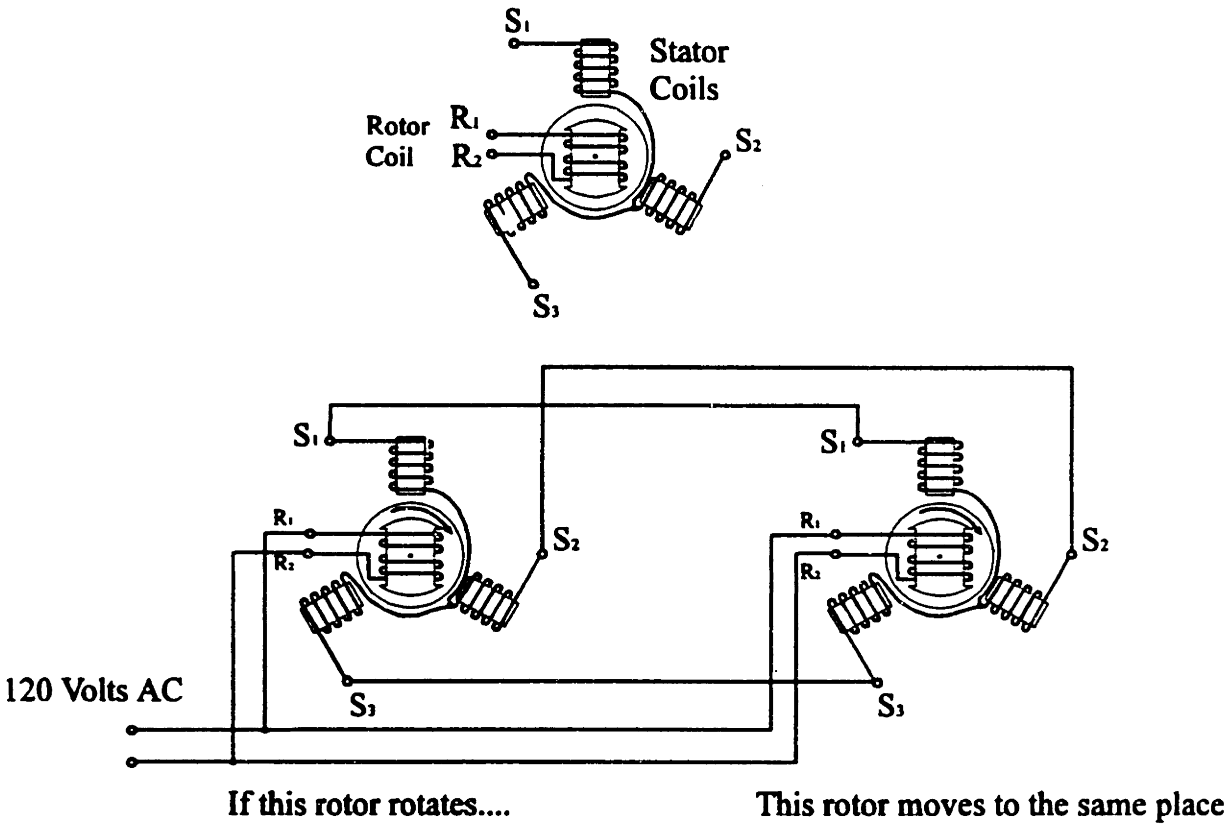


Figure 2-11: AC Selsyn Principle

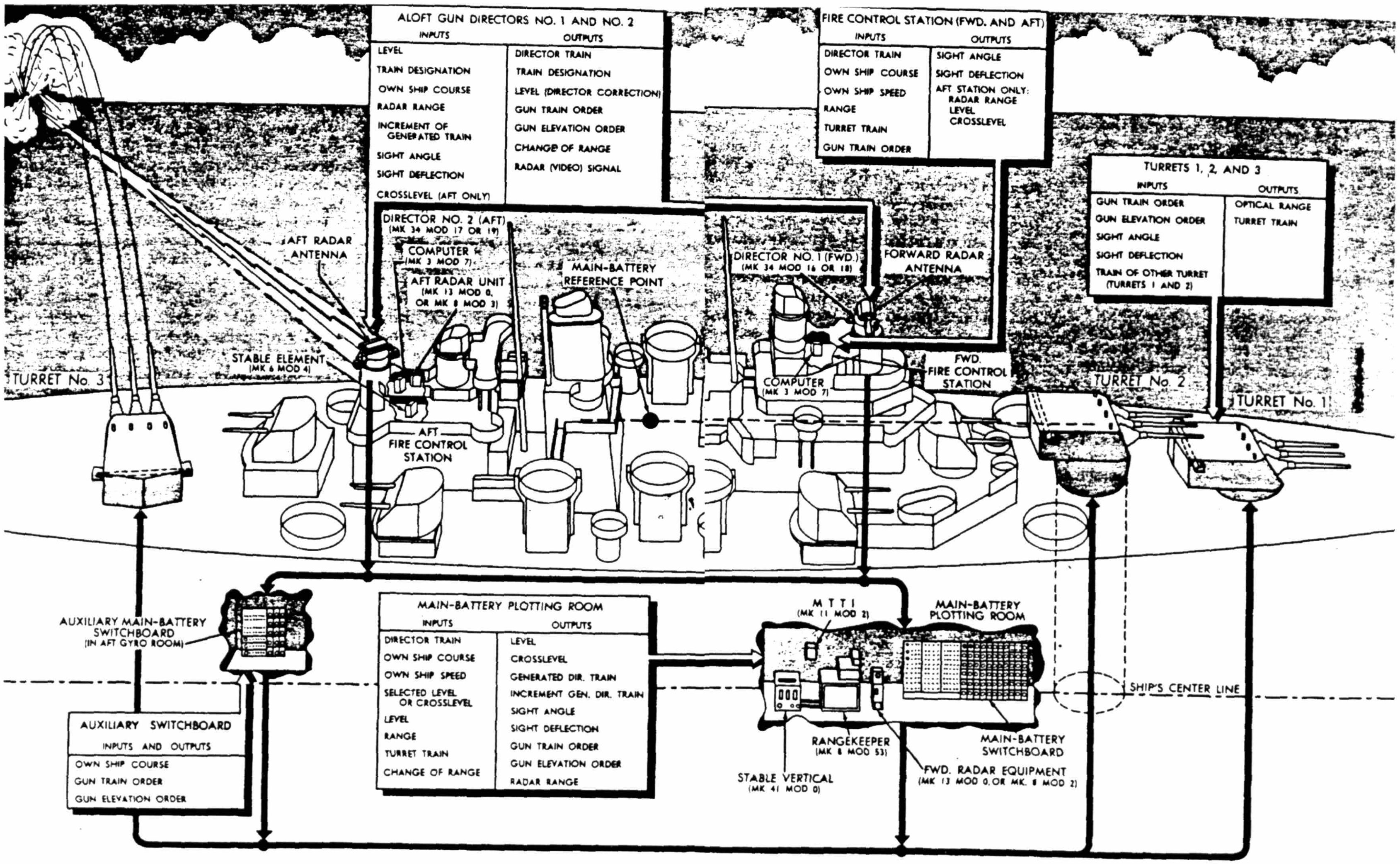


Figure 2-12: System configuration and switching aboard a cruiser, *circa* 1940 technology. Note the main and auxiliary switchboards and the multiple directors which can control the fore and aft batteries separately. Each turret has its own optical rangefinder, which it can use in "local control," if the system is damaged (from "Main Battery Fire Control System CA 68, 72, and 122 Class Ships," OP 1387 U.S. Navy Bureau of Ordnance, 1948, 82).

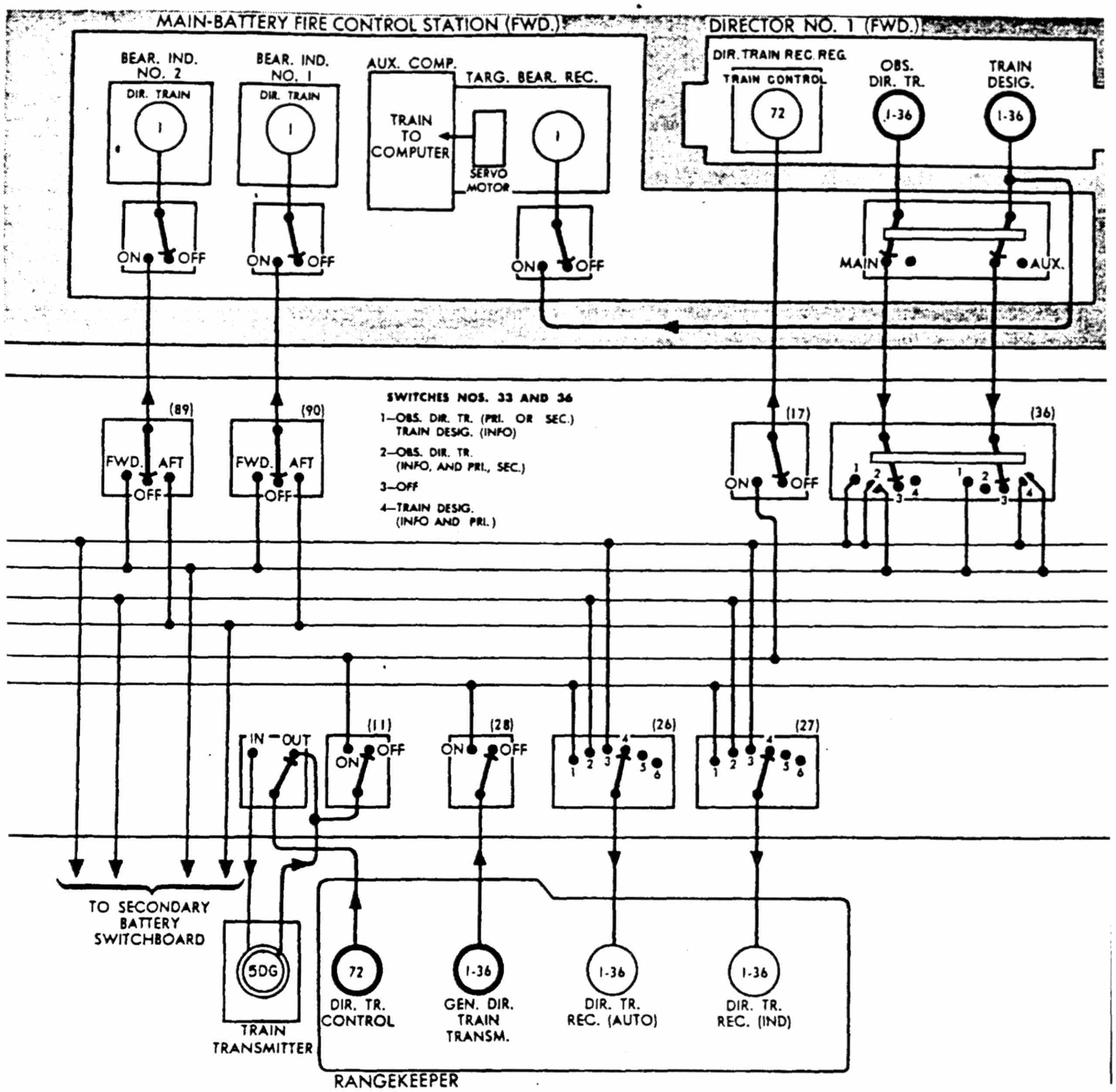


Figure 2-13: Subset of switchboard signal routing in a fire control system. The fire control station and the director have local switchboards and connect to common wiring busses that travel throughout the ship. The main switchboard in the plotting room routes signals from the busses into and out of the rangekeeper. This shows a small fragment of the total system aboard a cruiser (from "Main Battery Fire Control System CA 68, 72, and 122 Class Ships," OP 1387 U.S. Navy Bureau of Ordnance, 1948, 47). Note the similarity of this notation to the Differential Analyzer program in Figure 5-9, page 299.

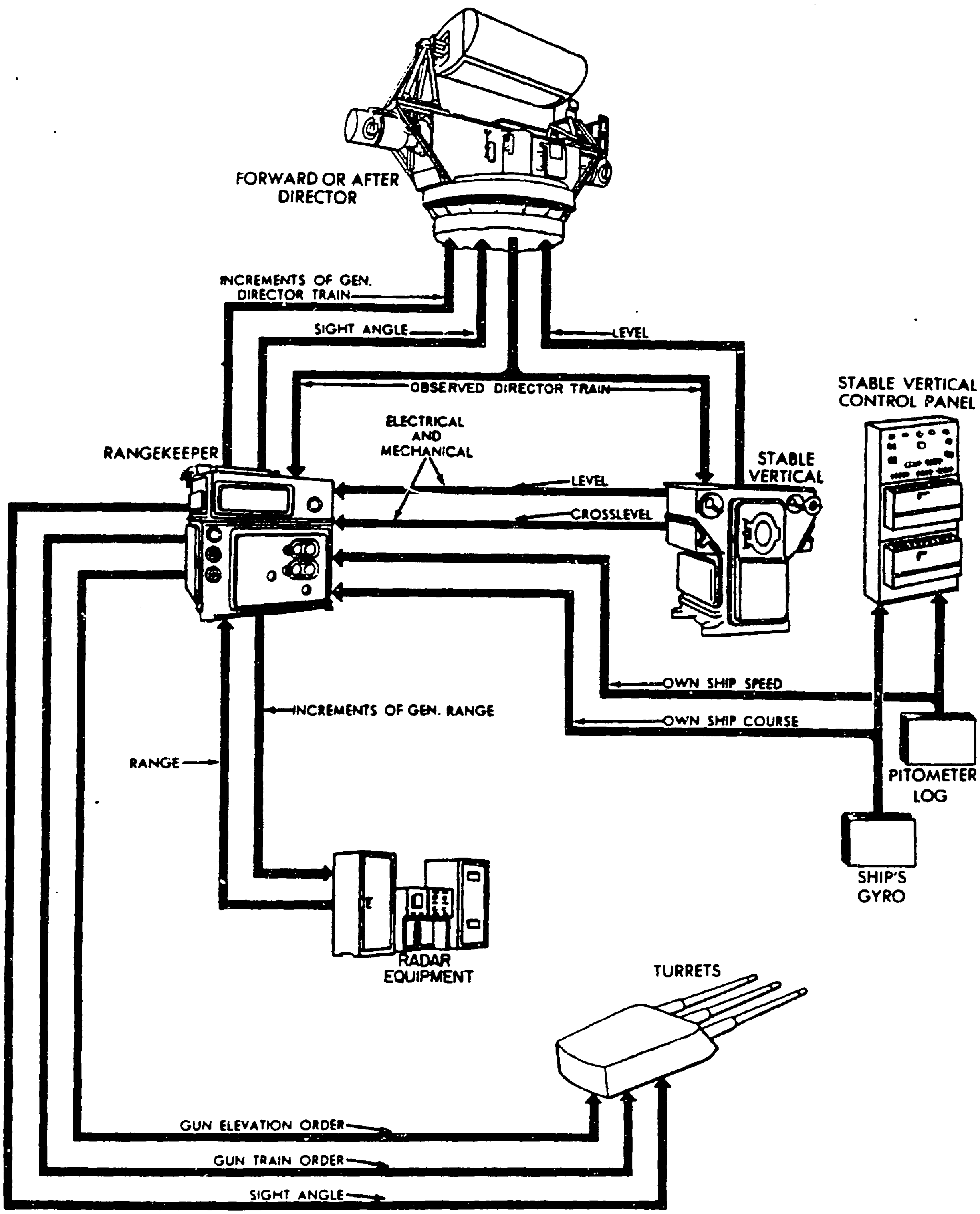


Figure 2-14: Main battery fire control system from a cruiser including Ford Rangekeeper Mark 8, Arma Stable Vertical Mark 6, and General Electric Mark 34 Director, circa 1940. Note optical rangefinder integrated into director (from "Main Battery Fire Control System CA 68, 72, and 122 Class Ships," OP 1387 U.S. Navy Bureau of Ordnance, 1948, 82).

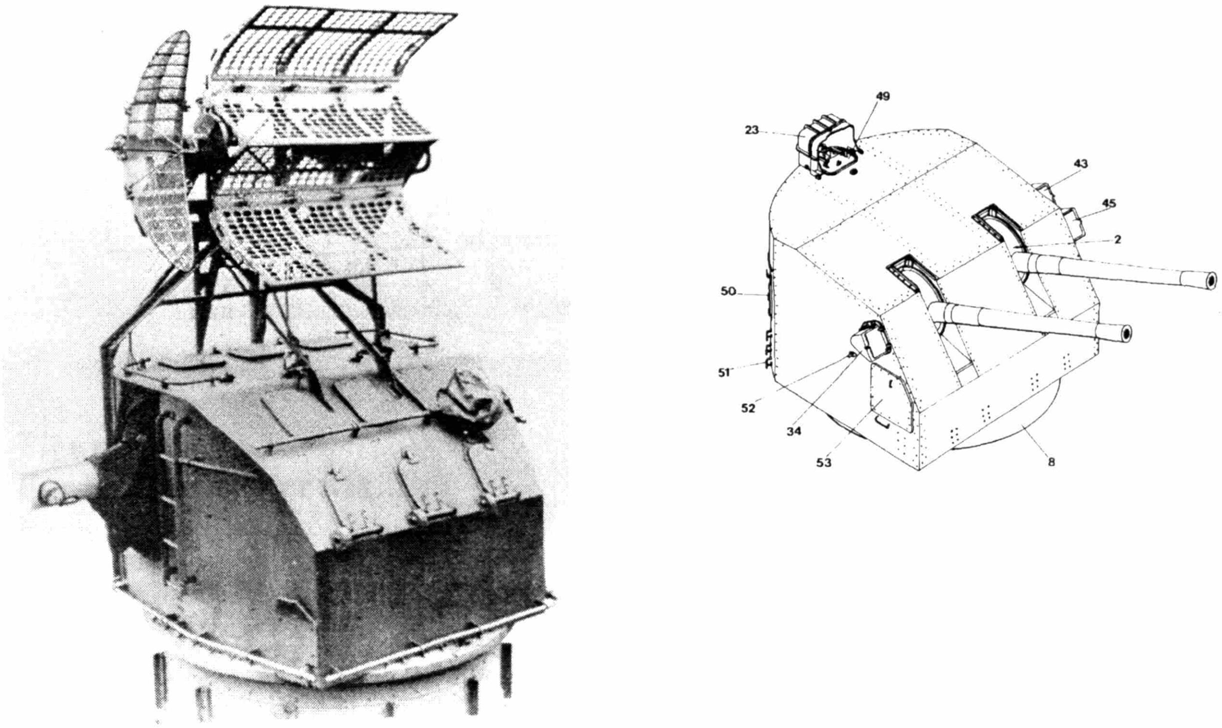
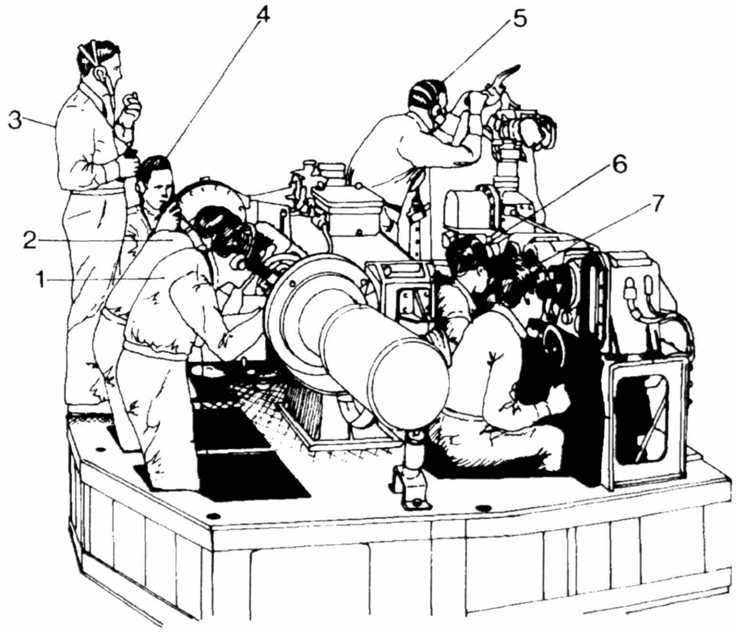


Figure 2-15: Mark 37 Director (left) with Bell Labs' fire control radar (ca. 1940). Note optical rangefinder protruding from side of director. This director primarily drove five-inch 38 caliber "dual purpose" (antiaircraft and surface fire) guns (right). It connected to a Mark I "Computer," (a late-model Ford Rangekeeper) below deck.

Translating perception to articulation: Mark 37 internal crew positions (Friedman, <u>US Naval Weapons</u>, 83).



1. Range Talker
2. Rangefinder Operator
3. Illumination Control Officer
4. Talker
5. Control Officer
6. Pointer
7. Trainer

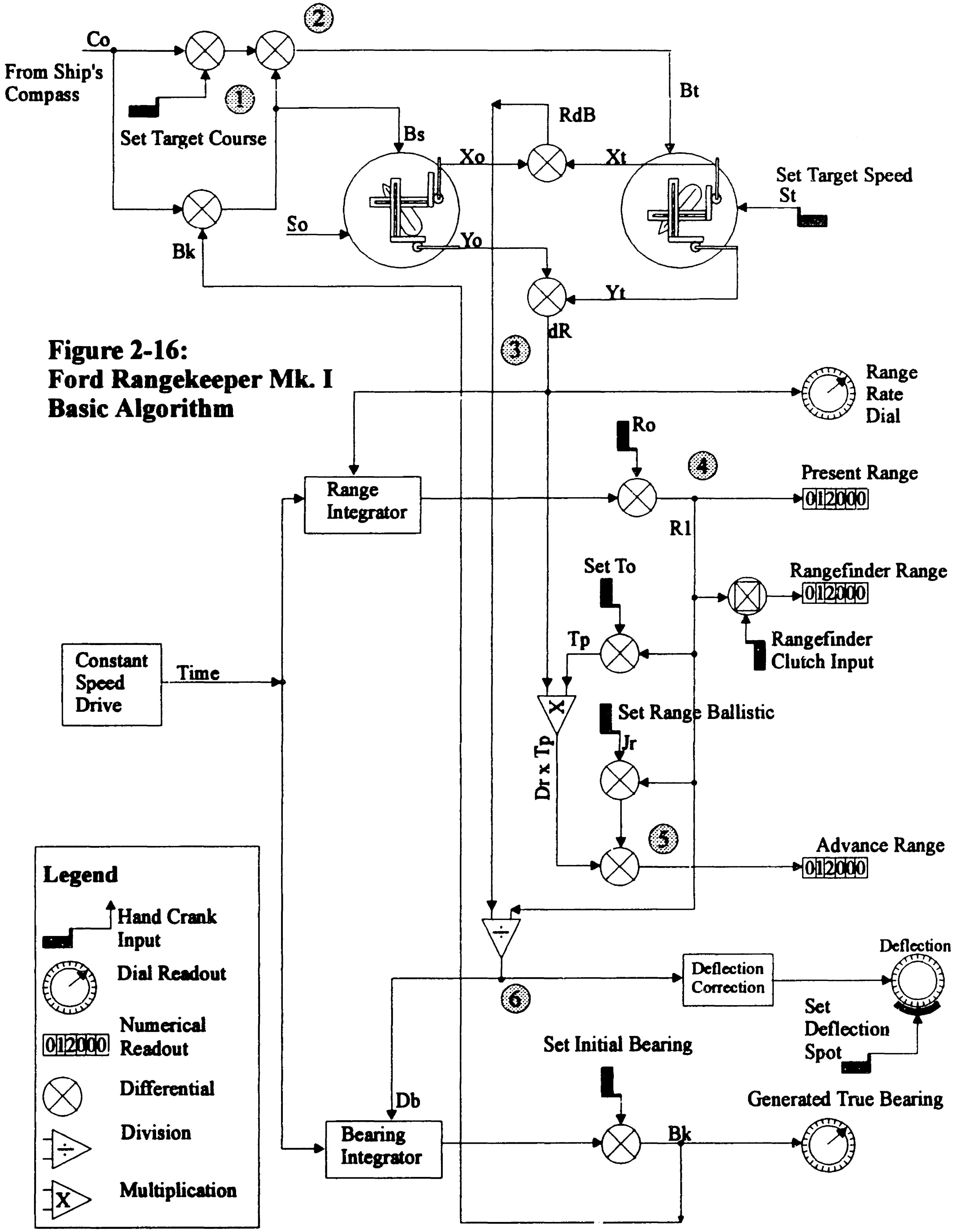


Figure 2-16: Ford Rangekeeper Mk. I Basic Algorithm

Chapter 3

Taming the Beasts of the Machine Age:

The Sperry company

Sperry Gyroscope's falling out with the Bureau of Ordnance did not eliminate the company from the field. In fire control, it merely switched services: the army too needed to counter the increasing threat from aircraft. Sperry Gyroscope brought its naval experience to the army's version of the problem. Where naval fire control integrated naval weapons within a naval warship, army fire control worked quasi-independently in the field. The latter befit Sperry's talents, as the company envisioned a relationship between human and machine more suited to these smaller, dynamic machines than to the ponderous, intricate naval systems. Sperry's products retained the basic functions of the coxswain or governor: perception, integration, and articulation. Where naval fire control responded solely to the needs of the Bureau of Ordnance, Sperry responded to the marketplace and its demands for technology — taming powerful and potentially wild machines. Corporate culture, economics, and differing technical context gave rise to variations on the basic governor structure.

Sperry's post-World War I fire control work was part of a broad business stabilizing ships, airplanes, and guns, all of which were developing higher performance than human operators could control. Increasing size, performance, and complexity pressed new machines to the edge of stability, Sperry Gyroscope's control systems reined them in, adding precision to the power. The company rarely designed the machines themselves, but rather added to developments in other areas. Sperry's regulators brought human operators into new combinations with new machinery, brought technological power into the range of human reaction, endurance, and precision. Sperry Gyroscope did not replace the coxswain with a governor; it amplified the coxswain's power to govern the machine.

The company inherited its practical style of control from its founder. Until his death in 1930, Elmer Ambrose Sperry, led Sperry Gyroscope in applying regulators and governors in diverse fields. At first glance, his career appears fragmented: Sperry moved from arc lighting, to power generation, to marine machinery, to aircraft stabilization. On closer analysis, as his biographer Thomas P. Hughes has shown, we see that Elmer Sperry's work consistently improved existing machinery through the application f feedback mechanisms — making smoother arcs, steadier power, straighter courses, and more stable flying.[1] Elmer Sperry founded several companies, but Sperry Gyroscope, founded in 1910, was the last and the largest.* After Sperry's death the Sperry Corporation emerged, a holding company for Sperry Gyroscope, Ford Instrument, and a number of other manufacturers, reflecting the maturity of its organization and its technology but retaining the founder's vision. Elmer Sperry's greatest contribution may have been the very notion of a company that specialized in control systems as a discrete technology.

Hughes has noted Elmer Sperry's conception of the airplane as "a beast of burden obsessed with motion," and this suggestive metaphor captures the Sperry Gyroscope vision.[2] It projects onto machinery animism and autonomy — not an animism of intelligence like "thinking machines," but an animism of the body, more akin to horses than automata. Seeing machines as beasts equates the human relationship to machinery with our relationship to animals. As with the stirrups on a horse, control systems do not create autonomy in the machine but rather remove it, bringing its independence under the will and intention of the human rider. This human/beast metaphor of technology parallels the division between information and power. To tie it to Chandler: horse is to rider as railroad is to telegraph. The notion of "harnessing" technology follows from this idea of machines as beasts of burden. Sperry's control systems, rather than seeking "autonomous" machinery, sought to bring machines into close connection, physical and intentional, with the human operator.

Gradually, however, the idea of control systems as taming beasts evolved. The distinction between people and beasts became more complex and less clear; human and machine performed more of each other's functions. Control moved beyond taming to become a system, integrating information, interconnecting people, and replicating social and political relationships — as in naval fire control. This evolution, from beasts to systems, was neither linear, causal, nor complete. As a commercial enterprise Sperry responded to its customers, who often did not require the most

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[1] Thomas P. Hughes, Elmer Sperry: Inventor and Engineer (Baltimore: The Johns Hopkins University Press 1971), 284-85.

* To avoid confusion, I will use the terms "Sperry," or "Sperry Gyroscope" to refer to the company, and "Elmer Sperry" to refer to the man, except where syntax makes "Sperry" clearly the man.

advanced, systematized automation. Sperry's main attempt at selling large scale control systems, its naval Fire Control System, failed, as eventually did its antiaircraft fire control and other systems projects. Instead, the beast vision, far from being displaced by such information systems, had surprising staying power; it produced lightweight controllers easy to manufacture and use — critical for the country in World War II. Sperry engineers in the twenties and thirties worked out a set of compromises on complexity, cost, and automation. The beast vision of control and the system vision coevolved, and their dialectic tension drove the technology. In this chapter I examine Sperry's antiaircraft devices as the nexus of this coevolution; they struck a curious balance between beast control and system integration, incorporating "human servomechanisms" to integrate separate components into a single system.

In time, Sperry came to see the control system as making the machine the extension of a person, literally grafted onto the senses, the body, or the mind, what Hughes has called "technological appendages."[3] By 1940, the company could write coherently about "the inability of the unaided man to operate his weapons:"

> His airplanes have become so big and fly so far that he must have automatic pilots instead of flying by hand. The machine gun turrets must be moved by hydraulic controls. The targets of his antiaircraft guns now move so fast in three dimensions that he can no longer calculate his problems and aim his gun. It must all be done automatically else he would never make a hit...There has come into being a whole new field of scientific accessories to extend the functions and the skill of the operator far beyond his own strength, endurance, and abilities.[4]

This vision culminated three decades of control engineering at Sperry. During that time, however, the idea of control as an extension of the human was still evolving and company could not articulate its goals so clearly. Rather, Sperry engineers worked out their ideas and expressed themselves by building machinery with different degrees of automation, different roles for the human operator, and different types of systems. They developed skills not only in negative

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[2] Hughes, Elmer Sperry, 173. Sperry's words have a biblical ring: "Of all vehicles on earth, under the earth and above the earth, the airplane is that particular beast of burden which is obsessed with motions, side pressure, skidding, acceleration pressures, and strong centrifugal moments...all in endless variety and endless combination."

[3] Hughes, Elmer Sperry, 173.

[4] "Introduction," to Sperry Company History, n.d., probably 1942, Sperry Gyroscope Papers, Hagley Museum and Library, Wilmington Delaware (hereafter referred to as SGC) Box 40. Similarly, Sperry Corporation President Thomas Morgan wrote in the 1943 Annual Report, "The primary value of Sperry's military products is that they extend the physical and mental powers of the men in the Armed Forces enabling them to hit the enemy before and more often than the enemy can hit them." This annual report refers not only to Sperry Gyroscope but to the other companies, including Ford Instrument, under the Sperry Corporation.

feedback mechanisms, but also in electrical instrumentation, data transmission, analog computing, and power drives. What eventually emerged as Sperry's "philosophy" of automation during the second world war was the result of engineering research, commercial imperatives, production compromises, and military demands.

Military Demands and Automation

Sperry Gyroscope's innovations in military control systems depended on its unique institutional culture. Heavily emphasizing engineering, the company relied on technology for competitive advantage, often leaving a field when it generated competition (and, as with naval fire control, sometimes being forced out). They built long-term relationships with officers and military organizations, frequently hiring military personnel and lending people to the services for special projects (Sperry reportedly recruited graduates of the Naval Academy who had failed the eye exam). Before the war, military customers paid Sperry Gyroscope to make complex mechanical devices with very high precision in small numbers, at relatively high costs. *Fortune* magazine reported Sperry had the largest collection of high-precision machine tools under one roof in the country, and characterized the company's production by this fact: the Sperry Direction Gyro sold for $350, and its raw material cost was thirty-nine cents.[5] These strengths were Sperry's limitation as well, as the company sometimes relied on its skills in precision mechanical design, machining, and manufacturing to the exclusion of newer electrical and electronic techniques (although it get an early start, with brothers Russel and Sigurd Varian, in klystron radar tubes).[6] Still, during the thirties, when military funding declined and government arsenals could not keep pace with new technology, Sperry sustained and developed control systems which otherwise would have stagnated. The Sperry Corporation research and development budget rose steadily, although maintaining an average 2.5% of sales from 1933-1940.[7] When the time came to ramp up production for war, the company was ready.

The military was no steady customer, especially between the wars, so Sperry adapted to the changing economics of defense. The company's rhetoric maintained that cycles of armament

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[5] "Sperry: The Corporation" Fortune, May, 1940. This article provides a detailed description of the state of the company in 1940.

[6] Dorothy Varian, The Inventor and the Pilot: Russel and Sigurd Varian (Palo Alto: Pacific Books, 1983).

[7] Data taken from Sperry Corporation Annual Reports, 1933-1940. R&D spending at the company actually went down as a percentage of sales during the war due to the radical increase in sales.

and disarmament called for automation at every point. Rapid mobilization required machinery that a man could use with minimal training. Wartime required weapons he could use under the stresses of combat. Post-war drawdown required technology that extended his powers to compensate for limited resources. Each needed Sperry's automatic machinery.

To accommodate these fluctuations, Sperry retained a paradoxical view of human operators as either the "weak link" or the "strong link" in a system, depending on the situation. As the weak link, humans were unreliable, obstreperous, and failure-prone. As the "strong link," people could smooth noisy data, evaluate patterns, use judgment, and make intelligent decisions. Which view one adopted depended on whose role was being automated (e.g. officers or enlisted men), the current availability of personnel, training capacity, and the limitations of achievable mechanisms. Often "weak link" and "strong link" language shaped the perception of a machine to present its strengths and limitations in the best possible light. An automated gunsight could extend the powers of an experienced gunner, or it could make a novice gunner shoot like an experienced one with minimal training.[8] Publicity and sales notwithstanding, good engineering dictated using the strengths and weaknesses of operator and machine to complement one another.

Sperry's military work felt these issues more keenly than its other projects, for military automation has a different set of imperatives from industrial automation. Automation on the battlefield or at sea carries social, psychological, and technical baggage that overlaps only partially with automation in the factory. At the start of a war, for example, when the nation mobilizes, the military has too much labor — the problem becomes how to utilize it effectively, not how to reduce it. For an armed service, labor is cheap and plentiful, and the rigid hierarchy can overlook user problems and complaints and order compliance with new techniques. Automation in the military concerns issues of performance, precision, combat stress, and, above all, the technological extension of a warrior's capabilities (although production is still a major issue in wartime). In contrast, industrial automation tends to address cost, efficiency of production, and worker autonomy. While other historical studies have analyzed automation in the military, they have

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[8] See, for example, "Bomber Defense from a Little Black Box," Sperryscope 9 (no. 12, July, 1943).

looked primarily at production, where they can directly compare military and industrial contexts.[9] Automatic machinery in combat has no such clear civilian counterpart.

Elmer Sperry And The Gyrocompass

The Ship Stabilizer

Elmer Sperry founded the Sperry Gyroscope Company in 1910 spurred by the "stability problem" of ocean vessels. Traditional sailing ships had enjoyed a natural stability due to do the balance of wind and water forces, but steam ships tended to roll severely, making life aboard rather unpleasant for passengers. Sperry designed a large spinning gyroscope to be installed in a ship's hold which could tilt fore and aft on its mount and apply the force of its precession to stabilize roll. While others had already built similar devices, Sperry added an innovation to make it more effective. The earlier gyrostabilizers were passive, i.e. they relied only on the precessional force of the gyroscope to counteract the rolling of the ship. These devices were only minimally useful because the action of the gyroscope would lag the roll of the ship and would not exert much force until the vessel was already rolling with considerable momentum. Sperry made his system "active:" he added a pendulum which sensed the slightest roll (eventually a small gyroscope replaced the pendulum as the sensing element). This sensor caused a motor to tilt the gyroscope, thus countering the motion while the ship was still nearly vertical and moving slowly with little momentum. Thus the gyro seemed to "anticipate" or "lead" the roll, keeping it to relatively small excursions from the vertical (later a similar lad would be used to predict the future positions of aircraft). Active stabilization provided markedly better performance than earlier passive gyrostabilizers. Elmer Sperry used the gyroscope as a reference for stability, echoing the long tradition of regulators and governors which imposed regularity on mechanical motions. His early goals were to provide regular and even motion more than dynamic control.

Between 1910 and 1915, Elmer Sperry developed the gyrostabilizer further, in close cooperation with Captain (later Admiral) David W. Taylor, delivering a unit to the US Navy for testing aboard the USS Worden in 1912. The ship stabilizer displayed the key characteristics of a control system: control of a high power device (the large gyro) by a low power device (the

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[9] Hugh Aitken, Taylorism at the Watertown Arsenal (Cambridge: Harvard University Press, 1960). David Noble Forces of Production (New York: Alfred A. Knopf, 1984). Harry Braverman, Labor and Monopoly Capital: The Degradation of Work in the Twentieth Century (New York: Monthly Review Press, 1974).

sensor). "Sperry did not use the terminology of automation, but he incorporated in his stabilization system a sensor, feedback, a programmed controller, and a servomotor."[10] Sperry Gyroscope built and marketed a number of different versions of the gyrostabilizer, including one for luxury yachts. The company eventually installed the devices on more than forty ships between 1915 and 1935. Gyrostabilizers never really succeeded in the market, however, and were eventually superseded by active external fins. They were not to be Elmer Sperry's most important invention.[11]

The Gyrocompass

By the turn of the century, the increasing power of steamships, combined with their expanding complements of electrical machinery, exposed the limitations of magnetic compasses. They wobbled, reacted to iron hulls, and pointed only to magnetic and not true north. Corrections could partially compensate for these errors, but new equipment and the need for greater accuracy made the magnetic compass an increasingly unacceptable source of heading data. In military applications the problem was worse. On a warship, rotating the heavy steel guns would change the ship's magnetic signature, and the compass could be thrown off by 180 degrees when the guns fired. For submarines, running underwater depended on large electric motors, whose magnetic fields would disturb the compass even more. This limitation proved especially critical since dead reckoning, the only way to navigate below the surface, depended on an accurate heading reference. In a bid to sail to the North Pole by submarine, German inventor Hermann Anschütz-Kaempfe had invented a device that employed a gyroscope to provide for undersea navigation in 1905.[12] In 1909, while in Germany promoting the gyrostabilizer, Elmer Sperry had seen the Anschütz gyrocompass, which eliminated problems of the magnetic compass by pointing to "true north." The spinning gyro literally sensed the rotation of the earth and aligned itself to the earth's rotational axis without regard to magnetic fields.

Elmer Sperry introduced the gyrocompass in 1911, and its success, resting in part on naval applications, won Elmer Sperry acclaim as a great American inventor, and access to the upper echelon of American technology (including the Naval Consulting Board, charged with bringing

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[10] Hughes, Elmer Sperry, 114.

[11] "The Products of The Sperry Corporation," n.d., probably 1935, SGC, Box 12.

[12] Donald MacKenzie, Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance (Cambridge: MIT Press, 1990), 34-35.

new inventions to the war effort). By the end of World War I, the gyrocompass had become an accepted and reliable technology, and the company profitably busied itself outfitting the world's fleets, both civilian and military. Yet even with its internal feedbacks, the gyrocompass remained essentially a sensing device a stable reference, and an "open loop" device. It was an ideal candidate to be incorporated into a larger closed-loop control system. Such a system made sense as a marketing strategy as well, for customers who already owned the sensing device could be sold additional equipment to complete the control loop. [13]

Resuming a project it had started but shelved before the war, in 1922 Sperry Gyroscope introduced the "Gyro-pilot." [*Figure 3-1, Sperry Gyropilot] This device connected the sensing element of the gyrocompass back through a ship's wheel in a negative feedback loop that would keep the ship automatically on course. It truly tamed the beast. In promoting the device, Sperry emphasized its recording capability and registered a weak-link view of humans in automation:

> Our studies include many hundreds of automatically made records showing the movements of the ships head, together with the movements of the rudder. These records clearly show that even the best of men are not constitutionally adapted to perform this purely mechanical task [steering]. The man's powers of attention quickly become fatigued, he fails to detect small deviations from the course, these small deviations quickly become large deviations, too much or too little rudder is applied and the ship performs a sinuous course. The result is waste of power, both in the steering engine and the main engines. [14]

Here, the rhetoric of automation reflects larger cultural currents: the concern with "waste of power" echoes the efficiency craze of the 1920s.

Although operated by a simple feedback loop, the Sperry Gyropilot nevertheless had the uncanny sense of automatic machinery. It truly seemed to be alive. Sperry sales literature played on the novelty and mystery of the device. One promotional pamphlet, "A True Story of the Devil" tells a racist tale of a ship captain sailing in the Mediterranean. He invites an Arab captain, an experienced and able seaman, aboard the vessel to see it operate under automatic steering by the Sperry Gyropilot. The Arab sees the wheel operating "by itself" and stands in awe. After much searching for hidden ropes or some other source of the trick, he remains incredulous, convinced the ship must be possessed by the devil. The American captain explains to him the ship is being driven by an angel which only Christian believers can see. If he were to convert from his "godless"

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[13] Hughes, Elmer Sperry, 275-279.

[14] Sperry Gyroscope Company, "The Gyro-Compass and Gyro-Pilot: Their Operating Principles, Construction, and uses," SGC.

ways, he too would see how the ship drives itself.[15] The ship might still have been a beast, but now an intelligent, domesticated one.

Other advertising played on shipboard labor politics, always a present issue with automatic devices. A common ad portrayed the Gyropilot as a man looking for work, in a tone that cannot have been comforting to working sailors:

> Wanted - a permanent position on board ship as a wheelsman. Have had experience in steering every type of merchant ship, can steer courses more accurately than others and use less rudder. Am sober, intelligent, strictly attentive to business, never ask for time off, do not talk back, am not affected by bill of fare or poor cooking, in fact do not eat at all. Wages wanted, only 54 cents per day for 24 hours service.
>
> [Signed]
>
> Sperry Gyro Pilot.[16]

These familiar claims often surround the introduction of automated devices: improved accuracy, greater obedience, and more reliability than human operators. Furthermore, Sperry sold a "course recorder" which could accompany the gyropilot and record on paper the precise course of the vessel during an entire trip, thus demonstrating the superiority of the gyropilot for accurate course keeping. The course recorder also exposed differences between individual helmsman, as well as any variations in the quality of steering during a particular watch. *Fortune* magazine reported in 1940 that "helmsman regarded the course recorder as a kind of mechanical company spy, and the marine gyropilot as a wicked device meant to send them into technological unemployment."[17]

Nevertheless, the gyropilot was a hit with industry and the public, and quickly acquired the nickname "Metal Mike." Still, despite Sperry Gyroscope's promotion of the "weak link" view of the human steersman, customers, especially in the traditional maritime trades, hesitated to relinquish such an important function as steering to a machine. The Company therefore hastened to assure prospective users that after ceding control, they could grab it back anytime. The Gyropilot included a feature (in the form of a large lever) to physically disengage the unit, returning it to manual operation, assuring that "the regular ships *control is instantly available for emergency.*"[18]

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[15] Sales Pamphlet, n.d., "A True Story of the Devil," SGC, Box 1.

[16] Advertisement, n.d., SGC, Box 3.

[17] "Sperry: The Corporation" Fortune, May, 1940.

[18] Automatic Steering for Naval Vessels. Publication no. 19-3, 1932. SGC Box 2. Emphasis original.

The Gyropilot then, while it usurped control of the steering function, gave the human another task: control of the gyropilot. Originally the pilot had only one way to control the system, turning the wheel; the gyropilot increased the number of inputs to seven. The original wheel still worked as before, when the autopilot was disengaged. "Metal Mike" itself had a smaller wheel, used for setting the heading to hold (in another mode, this wheel could also operate the rudder directly). As mentioned above, a clutch could physically remove the gyropilot from the system. The "weather adjustment" allowed the ship to yaw a certain amount without initiating a correction (known as a "deadband" in today's parlance). "Initial rudder adjustment" provided a means for "meeting" the help as it returned to course, easing off on the rudder as it approached the proper course (analogous to "derivative gain" in today's terms). "Rudder ratio adjustment" determined the amount of rudder required to bring a particular ship back on course ("gain" in today's terms). While the last tended to be a permanent setting, varying only from ship to ship, others needed to be tweaked more frequently. In fact, the control system required proper "tuning" (as do all closed-loop systems) to perform most efficiently, and sometimes even to operate at all.

This is not to imply that "Metal Mike" did not save labor: it genuinely kept the ship on course and relieved the operator of significant workload. Rather than eliminating the steersman, the gyropilot altered the character of his job. The pilot set the desired course, and changed it in accordance with navigation. He also adjusted the instrument for different weather conditions and different speeds. And, most important, he controlled when the automation was in effect. Entering a harbor or avoiding an obstacle, for example, would not call for automatic control. The helmsman thus engaged and disengaged the autopilot according to the circumstances, literally trading or exchanging control between human and machine. Man and beast worked together, each covering the other's limitations.

As with the earlier gyrocompass, the navy's interest in the gyropilot extended beyond that of commercial users. It cared less about labor problems and barely at all about labor costs, emphasizing instead accuracy, precision, and performance. The Gyropilot allowed better course tracking for maneuvers, and more accurate courses meant more constant speeds and better firing solutions. In its constant search for accuracy, naval gunnery sought to measure its world, to control its environment by bringing more and more variables into the machine. Closed loop control of ship's heading stabilized a major component in the complex equation of warfare at sea.

Sperry catered to the navy's special interests. In other applications the company touted the ease and uniformity of operating automatic machinery, where the operator only performed "mechanical" functions. In the navy, however, this vision of a robot-like operator broke down under conditions of extreme stress.

> It has been assumed that many of the helmsman's reactions under the stress of battle conditions will be mere automatic reflexes, inculcated by previous training until the familiar tasks may be performed without conscious thought. While it is a fact that predictions of human reactions must be based on averages, the man at the wheel is unfortunately not an average but an individual.

Here Sperry display's a certain sophistication concerning the role of the human operator. It presupposes machinery designed to match average human operators trained to perform reflexively. The unique individual, with all his judgment, skill — and possibly panic, uncertainty and mistakes — reemerges from this average in battle, invalidating the automation. The solution to this quandary was not less automation but more, to shift the operator's burdens to less stressful time periods. Once setup and adjusted the gyropilot would literally sail through battle:

> The consistent, machine-like precision of the Gyro-Pilot cannot fail to enhance the qualities which are all-essential in battle. True, man can never be displaced by the machine, but his function may well become that of the stand-by observer, rather than prime-mover in the action where perfection in every detail must ever remain the objective.[19]

Combat, that most chaotic and unpredictable human situation, calls for the highest precision and certainty technology can provide. The steersman need only stand by and watch. More than merely taming the beast of the machine, however, the control system plays a dual role, taming and standardizing the human operator as well.

Wiley Post and the Sperry Automatic Pilot: Putting on the Machine

The sea was not the only area in which Sperry sought to stabilize motion. Before World War I Elmer and Lawrence Sperry had experimented with gyroscopic stabilization of aircraft. During the war they built, under government contracts, an unsuccessful autonomous flying bomb or "aerial torpedo." This device suffered from the "afterthought" nature of control engineering; even Sperry did not have a broad enough view to design the airplane mechanism with automatic controls in mind.[20]

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[19] Ibid.

[20] Hughes, Elmer Sperry, 269.

Sperry first achieved commercial success in aviation not with robots but with instruments, giving pilots feedback on the state of the airplane which they could not get from other sources. These devices helped expand the operating envelope of aviation into adverse weather conditions. Aviation needed reliability to make the transition from a technical curiosity to a mainstream business. Where the military needed to bring the chaotic field of battle into its quantified purview, commerce needed to bring the wild airplane into the world of acceptable risk and reliable scheduling.

During the first world war, Sperry conducted a difficult development program in gyroscopic aircraft instruments for the navy (Elmer Sperry's navy liaison for this program, Luis de Florez, an MIT graduate, would later be the primary sponsor for the Whirlwind digital computer). Sperry introduced a gyroscopic turn indicator in 1918, then a "directional gyro" and a gyroscopic "artificial horizon" in 1930. These devices became part of the standard suite of airplane instruments and remain so today. Sperry's first truly high-volume products, the directional gyro and the artificial horizon were installed on nearly all airplanes produced in this country through World War II.

Just as it had introduced the "Metal Mike" gyropilot to close the loop around marine gyrocompasses, Sperry closed the loop around its aviation instruments with an autopilot. The company built on its flying torpedo technology with Sperry Automatic Pilot for aircraft, a pneumatic-hydraulic device. As early as 1914 Lawrence Sperry had demonstrated gyroscopially stabilized flight, but war interrupted development. After Lawrence's death in 1924, it remained for Elmer Sperry Jr. to develop a fully automatic autopilot, in cooperation with the army from 1925-29. Similar to the navy's interest in the gyrocompass, the army wanted the autopilot to keep an airplane stable during a bombing run. The first Sperry device, designated A-1 had reliability and maintenance problems which were corrected in the A-2 model.[21] Sperry's A-2 autopilot, the first such device to be practicable, became enormously popular in the thirties. TWA equipped its entire fleet with the device in 1933; it earned the appelation "Elmer" after its designer. The autopilot's rise to popularity began in the hands of a man who single-handedly brought together pilot and airplane, tightening the man/beast relationship into a much more intimate affair.

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[21] Stephen L. McFarland, America's Pursuit of Precision Bombing 1910-1945 (Washington: Smithsonian Instiution Press, 1995), 36-9.

Wiley Post, an Oklahoma farm-boy with training in mechanics and electronics, left a job in the oilfields for a career in the small and marginal profession of commercial pilot. From the beginning, Post made his mark as an operator particularly close to his machine. An early colleague recalled "he didn't just fly an airplane, he put it on." Another remembered Post as, "as near to being a mechanical flying machine as any human who held a stick." [22] In Post's words, when flying

> I tried my best to keep my mind a total blank...I do not mean that I paid no attention to the business of handling the ship. I mean that I did it automatically, without mental effort, letting my actions be wholly controlled by my subconscious mind. [23]

In 1926 an oilfield accident cost Post his left eye, and he literally replaced it with a machine, using workmen's compensation to buy his first airplane. He found a job ferrying Lockheed's Vega airplanes from the factory to its customers. Here Post gained experience with the problems of "blind flying" (what today we would call instrument flying) through clouds and bad weather, relying on Sperry gyroscopic instruments to keep his bearings. In 1931, Post and partner Harold Gatty (who had trained Lindbergh in navigation) made headlines by piloting a Vega, named "Winnie Mae," around the world in record time.[24] For this flight Post grouped his instruments right in front of his eyes and modified his cockpit to fly with one foot on the rudder pedals and one hand on the wheel. As Sperry President Preston Bassett later described Post's piloting, "As many will recall, Wiley had only one good eye. So, all combined, the setup was that of a man flying around the world with one eye, one arm, one leg, and two instruments. You will see that we are building toward a very good servomechanism."[25]

In 1933 Post replaced Gatty with an early automatic pilot donated by Sperry Gyroscope. He repeated the trip around the world solo, relying heavily on the Sperry device. The machine freed Post for other functions like navigation, but primarily it allowed him to nap, considerably reducing his fatigue.[26] Post took over full manual control when the autopilot experienced

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[22] J.H. Conger, quoted in Stanley R. Mohler and Bobby H. Johnson, Smithsonian Annals of Flight Number Eight: Wiley Post, His *Winnie Mae*, and the World's First Pressure Suit (Washington, Smithsonian Institution Press, 1978), 5. Will Harris in Oklahoma City Times, August 16, 1935, 1, quoted in Mohler and Johnson, 116.

[23] Wiley Post and Harold Gatty, Around the World in Eight Days: The Flight of the Winnie Mae, (London: John Hamilton, Ltd., 1932), 26.

[24] Ibid.

[25] Preston R. Bassett, "Servomechanisms: Controlling Vehicles in the Air," From an address given before the New York Section of the American Rocket Society, Sperryscope 13 (no. 1, second quarter, 1953), 22.

[26] *New York Times*, July 23, 1933, 1. Wiley Post, "Destination — New York," Sperryscope 7 (no. 2, October, 1933). Post's Sperry Autopilot remains on display at the Smithsonian Air and Space Museum. Events dramatically demonstrated the importance of the Sperry Autopilot's reducing Post's fatigue. The day after Post landed, two

mechanical failure. Once repaired, the autopilot took control when Post needed sleep. The two worked together, trading control depending on the circumstances, playing on each other's strengths and alleviating each other's weaknesses. In Wiley Post's imaginative hands, the Sperry autopilot brought the beast closer to the pilot. No longer was the machine the body/power and the man the mind/intention. Rather, the machine become part of the man's body. Technology began to fuse governor and governed.

Public exposure of Post's seven-day flight not only promoted the Sperry Automatic Pilot but brought automatic control into public consciousness. The New York Times declared "the days when human skill and an almost bird-like sense of direction enabled a flier to hold his course for long hours through a starless night or over a fog are over. Commercial flying in the future will be automatic."[27] [*Figure 3-2, Wiley Post and Winnie Mae] In his later career, Post further mechanized his body, developing the first pressure suit to allow him to fly into the stratosphere (this may have been what his friend meant when he said Post "put on" the airplane). [*Figure 3-3, Pressure Suit] In 1935 he and Will Rogers died in a plane crash in Alaska.

The Sperry Corporation in the 1930s

By 1929, the year before his death, Elmer Sperry sold his stake in the company. It was a heady, chaotic time for the airplane industry, and Sperry Gyroscope became part of North American Aviation which included several airlines and aircraft manufactures. Tom Morgan became President of Sperry Gyroscope, and soon after President of North American. General Motors acquired North American in 1933, and the fallout from that transaction created the Sperry Corporation, with Tom Morgan as President, a holding company for smaller firms brought in through acquisitions (including Sperry Gyroscope Ltd., the English subsidiary).[28]

Reginald Gillmor, who had overseen installation of the earliest gyrocompass aboard the Delaware, and who had transferred British fire control technology to the United States, now headed Sperry Gyroscope. The company's business divided into 30% naval, 30% military, 30% marine, and 10% aeronautical markets. The last sector grew most quickly. Morgan committed the

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Italian aviators were critically injured landing in New York after a transatlantic flight. The cause of the crash: pilot fatigue. *New York Times*, July 24, 1933.

[27] *New York Times*, July 24, 1933, 2.

[28] "Sperry: The Corporation," *Fortune*, May, 1940.

company to the field with a substantial fraction of the research and development budget going to aircraft. By 1937 aviation accounted for one-half of Sperry Gyroscope's business.[29]

For the Sperry Corporation overall, control encompassed more than aircraft; during the early thirties it took shape as an integrated control system company. It purchased Ford Instrument in 1933, which remained under the leadership of Hannibal Ford, who found himself once again a Sperry Employee. Ford Instrument still exclusively made naval fire control computers for the Bureau of Ordnance, operating out of three separate plants in Long Island City. Though owned by the same holding company, Sperry Gyroscope and Ford Instrument shared remarkably little technology, well into the 1940s.

Where Sperry Gyroscope and Ford Instrument specialized in the precision components required for perception and integration, the feedback part of the control process, several acquisitions during this period brought the company expertise and products for articulation, the output of control. In 1935 Sperry acquired the Waterbury Tool Company of Waterbury Connecticut, maker of large, variable-speed hydraulic transmissions for the navy. Waturbury's hydraulic gear moved turrets for large naval guns and shell hoists, cranes, and numerous other shipboard machines. In 1937 Detroit-based Vickers Inc., the country's largest maker of oil hydraulic machinery (not related to the British arms firm, Vickers Ltd.), was brought into the fold. Vickers specialized in small, high-powered, reversible power control for industrial applications, including paper making and cable manufacture. Harry Vickers founded the company in 1920, with financial backing from Fred Fisher of the Fisher Brothers. Fisher now became the single largest stockholder in the Sperry Corporation, with about two and a half percent of its shares (he remained on its board until his death in death in 1941, when he was replaced by his brother Charles). In 1940 Waterbury was incorporated into Vickers to create a product line that covered the full range of hydraulic power devices. Morgan chose not to integrate these companies as divisions under a single corporation, but rather to keep them subsidiaries to retain their separate cultures (Waterbury eventually became a division of Vickers). Sperry corporate structure mapped the functions of the governor; they frequently sold systems comprised of components from multiple subsidiaries.

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[29] Sperry Corporation, Annual Report, 1937.

By 1930, Sperry Gyroscope itself had grown to about 1,000 employees with annual sales of over $5 million — it typically accounted for half of the Sperry Corporation's sales (although this percentage declined as the hydraulics businesses expanded). Its product line consisted of a number of control systems and components, but also reflected the company's history. The flagship products remained marine gyrocompasses and gyropilots, both commercial and military. Although excluded from naval fire control, Sperry Gyroscope's naval roots survived in searchlights, ship stabilizers, and a number of marine instruments, including a salinity recorder for boiler water. Within the Sperry Corporation, fire control for the navy was now the exclusive domain of Ford Instrument. In the twenties, naval fire control was itself starting from scratch on the new problem of antiaircraft defense, and Sperry Gyroscope teamed up with the army to attack this difficult challenge of control.[30]

Antiaircraft Artillery Fire Control

As Sperry Gyroscope improved airplanes, it also built a business destroying them. Control technology, as it had with steam-engines, railroads, and naval gunnery, responded to progress in other types of machinery. The aerial combat of World War I, combined with postwar fears of aerial bombing, created a military demand for sophisticated fire control for antiaircraft artillery. Shooting an airplane out of the sky is essentially a problem of "leading" the target. As aircraft developed rapidly in the twenties, their increased speed and altitude rapidly pushed the task of computing this lead out of the range of human reaction and calculation. Fire control equipment for antiaircraft guns was a means of technologically aiding human gunners to accomplish a task beyond their natural capabilities.

Sperry's work in antiaircraft control culminated its interwar work, synthesizing the beast and system visions of control. Human operators worked in close concert with automatic machinery to the point where the "system" represented people connecting different machines, or machines connecting disparate people, depending on one's perspective. As automation progressed, human operators were gradually reduced, but in a piecemeal, almost hesitating way — the distinction between mechanical and human function blurred to the point where substituting one for the other required subtle judgments.

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[30] "The Products of The Sperry Corporation," n.d., early 1930s. SGC.

During the first world war, antiaircraft fire control had undergone some preliminary development. Artillery officers used slide rules to calculate lead angles based on optical sighting of targets. These slide rules were incorporated into boxes; an operator would dial in data with knobs, read out an answer on a dial, and telephone azimuth and elevation to those operating the guns ("Azimuth" is the term used by the army for the gun's rotation, where the navy uses "train" for the equivalent parameter. Both services use "elevation.") Elmer Sperry, as chairman of the Aviation Committee of the Naval Consulting Board, was familiar with this technology and the Army Ordnance Department invited him to submit a proposal for an antiaircraft instrument based on his work in naval fire control.[31] Sperry came up with two instruments: a goniometer or rangefinder, and a pretelemeter, or calculator, both of which included rudimentary data transmission systems. Sperry Gyroscope was unable to produce these devices in quantity during the war, however, because of other wartime obligations; several French-manufactured equivalents were used, but only in small numbers.[32]

When the war ended in 1918, the army undertook virtually no new work in antiaircraft fire control for several years. In the mid-1920s, however, they began to develop components for antiaircraft equipment including stereoscopic height-finders, searchlights, and sound location equipment, the latter two of which involved Sperry Gyroscope. Sperry had made its first searchlights in 1916 and sent them to war in 1917.[33] After the war, searchlights grew to a significant fraction of Sperry sales, for both military (navy and army) and commercial applications, and would continue so well into World War II. Sound location equipment did not enjoy the same longevity. It worked by exaggerating the distance between an operator's ears, which allowed him to locate incoming airplane sounds, within three or four degrees. The basic physics of the method had problems, however, for sound traveling in air is highly sensitive to turbulence, wind, and temperature variations, all of which reduce accuracy. Furthermore, improved propellers reduced engine noise and higher aircraft altitudes made airplanes harder to ear on the ground, rendering sound locators virtually useless by the late thirties. Radar, which began as radio direction finding around 1930, eventually superseded this method. Nonetheless, sound locators illustrate Sperry's

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[31] Rose to Sperry, August 11, 1917. Elmer A. Sperry Papers, Hagley Museum and Library, Wilmington Delaware (hereafter referred to as EAS) Box 32.

[32] Elmer Sperry to T. Wilson, Frankford Arsenal, July 10, 1925. EAS, Box 33.

[33] "Ordnance History of the Sperry Gyroscope Company, Inc.," SGC Box 33, Fire Control Folder.

evolving philosophy of augmenting human capabilities, literally grafting machinery onto the operator's senses. [*Figure 3-4, sound ranging equip.]

The army had responsibility for coastal defenses; large coastal guns were similar to naval guns and faced similar problems of coordination when shooting at distant marine targets. After World War I, the army studied the navy's director firing and eventually incorporated the technique, including computers, into its coastal artillery.[34] In 1925, Major Thomas Wilson, who had participated in that work and was now at the Frankford Arsenal in Philadelphia, began developing a central computer for antiaircraft fire control, also based on the system of director firing in naval gunnery. Wilson's device resembled earlier fire control calculators, accepting data as input from perception components, performing calculations to predict the future location of the target, and articulating direction information to the guns.

Integration and Data Transmission

Even with Wilson's director, the components of an antiaircraft battery remained independent, tied together only by voice telephone. Sperry's Chief Engineer, later President, Preston R. Bassett directed the company's searchlight and sound locator development. He later recalled, "no sooner, however, did the [antiaircraft] components get to the point of functioning satisfactorily within themselves, than the problem of properly transmitting the information from one to the other came to be of prime importance."[35] Tactics and terrain considerations often required different fire control elements be separated by up to several hundred feet. Observers telephoned their data to an officer, who manually entered it into the central computer, read off the results, and telephoned them to the gun installations. This communication system introduced both a time delay and the opportunity for error. The components needed tighter integration; that meant automatic data communication. With its gyrocompass repeaters, the Sperry Gyroscope Company could provide such a system.

Because of Sperry's experience with fire control in the navy (at least the positive part of it), as well as Elmer Sperry's earlier work with the goniometer and pretelemeter, the army

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[34] Sperry Gyroscope eventually made computers for army seacoast guns. For a history of Sperry's involvement with coast artillery, see Sperry Company Report, "Gun Data Computer, M1, February 7, 1944. SGC, Box 40.
See correspondence in NARA RG-38 Entry 178 Box 3 Folder 59/65. Courtesy John Testuro Sumida.

[35] Sperry Company memorandum, probably by Preston R. Bassett, "Development of Fire Control for Major Caliber Anti-Aircraft Gun Battery," 2. SGC, Box 33.

approached the company for help with data transmission for antiaircraft fire control. To Elmer Sperry, it looked like an easy problem: the calculations resembled those in a naval application, but the physical platform, unlike a ship at sea, anchored to the ground. It also had its own electrical system, and stood physically separate from the guns, which could help avoid the synchronization problems Sperry's system experienced aboard ships. Sperry engineers visited Major Wilson at the Frankford Arsenal in 1925, and Elmer Sperry followed up with a letter expressing his interest in working on the problem. He stressed his company's experience with the navy, as well as its recent developments in bombsights, "work from the other end of the proposition."[36] Bombsights had to incorporate numerous parameters of wind, groundspeed, airspeed, and ballistics, so an antiaircraft director was really a reciprocal bombsight. In fact, part of the reason antiaircraft fire control equipment worked at all was it assumed attacking bombers had to fly straight and level to line up their bombsights. Elmer Sperry's advances to Wilson were warmly received, and in 1925 and 1926 Sperry Gyroscope built two data transmission systems for the army's gun directors.

The original director built at Frankford was designated T-1, or the "Wilson Director." The army had purchased a Vickers director manufactured in England but encouraged Wilson to design one that could be manufactured in this country.[37] Sperry's two data transmission projects added automatic communications between the elements of both the Wilson and the Vickers systems (Vickers would eventually incorporate the Sperry system into its product), one the traditional DC step-by-step type and one the newer synchronous AC system. Major Wilson died in 1927, and Sperry Gyroscope took over the entire director development from the Frankford Arsenal with a contract to build and deliver a director incorporating the best features of both the Wilson and Vickers systems.

With this project Sperry undertook a small but intensive development program in antiaircraft fire control that would last more than fifteen years. The company set up a separate department with its own facilities and personnel, and gradually developed a cadre of experts. Earl W. Chafee headed the effort, an engineer whose strong personality and free managerial hand allowed him to completely dominate Sperry's fire control work well into the 1940s. During the

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[36] Elmer A. Sperry to T. Wilson, Frankford Arsenal, July 10, 1925. EAS, Box 33. For the Sperry Bombsights, see also McFarland, America's Pursuit of Precision Bombing.

[37] United States Army, Ordnance Department, "History of Anti-Aircraft Director Development," n.d., probably prepared in the fall of 1935. SGC, Box 4.

second world war, government officials viewed all Sperry fire control work as carrying Chafee's personal imprint. The company financed its engineering internally, selling directors in small quantities to the army, mostly for evaluation, for only the actual cost of production.[38] Of the nearly ten models Sperry developed before 1935, it never sold more than twelve of any version; the average order was five. Sperry Gyroscope offset some development costs by sales to foreign governments, especially Russia, with the army's approval — exactly the arrangement which had annoyed the navy.[39]

Antiaircraft work was a difficult enough problem by itself, but during these years it faced an additional challenge. Not only did the machine have to track high speed aircraft, but aeronautic technology itself was rapidly changing. In 1925, bombers flew at 100 miles per hour at relatively low altitude. This speed more than tripled in the next ten years, and the bombing altitude rose to well above 15,000 feet. Still, this situation was part of the terrain: in these years control systems by nature were driven by other technologies. In more ways than one, Sperry was shooting at a moving target.

The Antiaircraft Problem

Defense against high altitude bombing drove antiaircraft development between the wars. Only late in the thirties did close in attack and dive bombing emerge as significant problems. A number of different control systems emerged to deal with them, including tracer bullets and "lead computing sights" for smaller caliber machine guns (see Chapters 6 and 7). Sperry's antiaircraft work in the twenties and thirties concentrated on aiming large guns (three to four inches in diameter) firing exploding shells to relatively high altitudes to reach attacking bombers. The shells were not intended to hit the target directly but rather to explode nearby, a predetermined time after firing. Since this scenario represented the most difficult antiaircraft situation at the time, it led fire control technology and its techniques diffused into other applications, including coastal defense and traditional artillery.[40]

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[38] Sperry Gyroscope Company, "Development of Fire Control for Major Caliber Anti-Aircraft Gun Battery."

[39] Ibid. See also Sperry Company Form #1607, "Sperry Universal Director: Information to be Furnished by Customer." SGC, Box 3. This document was intended for foreign governments wishing Sperry to customize directors to different types of guns.

[40] See, for example, G.M. Wells "New Fire Control for Divisional Weapons," Army Ordnance XI (no. 65) March-April 1931 which explicitly suggests extending Sperry's AA fire control system to standard artillery. Sperry also did some work in seacoast artillery fire control in 1929 and 1938, although one report admitted "Our primary

Rising aircraft speeds and altitudes created a number of problems. Once fired, shells travel with ever-decreasing velocity due to gravity and air resistance. Typical guns of the 1930s could easily have a time of flight of fifteen seconds to reach 5,000 feet, and double that to reach 8,000 feet. A plane traveling at 100 miles per hour at 5,000 feet would travel about 750 yards horizontally (toward the target) during this time of flight. Thus the "lead" for the gunner would be 750 yards. A plane traveling at 250 miles per hour at 8,000 feet would travel 3,660 yards during shell flight, a lead nearly five times greater than that for the slower, lower plane. For either case, the shell would need to be fired not at the plane itself, but at the place it was expected to be after the time of flight. The structure of the prediction was the same as in naval gunnery, but the distances and times were different. The longer the time of flight, the more difficult this prediction. Furthermore, the problem had an inherent feedback loop, because prediction could only be accomplished when the time of flight was known, but time of flight depended on the aiming point, itself the output of prediction.

Tactics further complicated prediction. For antiaircraft fire to have real defensive effects it needed to shoot down attacking planes before they released their bombs. This limitation reduced the time available for the director to produce a firing solution: tracking could begin only when the attacking aircraft came into visual instrument range, and the shell must be fired at least one "time of flight" before the bomb release point, which would precede the target by an amount depending on the aircraft's speed and altitude.[*Figure 3-5, Antiaircraft trajectory diagram] Earl Chafee produced the following chart (for a 100 mph bomber, assuming the guns were placed right at the bombing target):

Maximum time within zone of effective fire:	2.5 minutes
Maximum time within range of 60" searchlight:	4 minutes
Maximum time within effective sound locator range:	5 minutes
Maximum time within trajectory of 3" gun:	5.5 minutes
Maximum time within visual observation range under conditions of ideal visibility:	6 minutes
Maximum time within limits of audibility with sound	

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contribution to the seacoast fire control problem has been our sustained interest in a very slow moving field." Roswell Ward, "Gun Data Computer, M1" January 31, 1944. SGC. Also see Constance M. Green, Harry Thompson, and Peter C. Roots, The Ordnance Department: Planning Munitions for War Volume 1 of The US Army in World War II: The Technical Services (Washington, DC: Office of the Chief of Military History, Department of the Army, 1955), 344.

locator: 8 minutes[41]

These values would all diminish by a factor of 2.5 for 250 mph bomber. One way to improve the situation would be to move the antiaircraft director and battery forward of the target, allowing it to engage attacking planes well before their bomb release points. The ability to predict the bomber's position, however, followed from an assumption of straight and level flight. This assumption held during a bombing run, when the plane needed to fly steady to align its own bombsight — for as much as a minute with 1930s equipment. Too far ahead of the target, however, an antiaircraft battery would catch the bomber before its bombing run, when the straight and level assumption was not yet valid.[42] Nevertheless, if the antiaircraft system could completely solve the problem for a given zone, it could force attackers to maneuver or climb to a higher altitude, making their job more difficult and achieving a partial tactical victory.

The T-6 Director

Sperry's new version of Wilson's director was designated T-4 in development. This model incorporated corrections for air density, super-elevation (the need to aim a bit high to compensate for the droop of the trajectory due to gravity), and wind. Assembled and tested at Frankford in the fall of 1928, it had problems with backlash in its gearing and reliability in its predicting mechanisms. Still, the army found the T-4 promising and after testing returned it to Sperry for modification.[43] The company changed the design for simpler manufacture, eliminated two operators, and improved reliability. In 1930 Sperry returned with the T-6, which it tested successfully. In 1931, the army ordered twelve of the units. The army standardized the T-6 (i.e. accepted it as operational) as the M-2 director.[44]

Since the T-6 was the first antiaircraft director to be put into production, as well as the first one the army formally procured, it is instructive to examine its operation in detail. Such an analysis also clarifies the basic features of the antiaircraft problem, which would drive the

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[41] Sperry Gyroscope Company, "Anti-Aircraft Gun Control" Publication No. 20-1640 (Brooklyn, New York: Sperry Gyroscope Company Inc., 1930), 7.

[42] Earl W. Chafee, "Study of the Requirements for a Satisfactory Antiaircraft Fire Control System," Fire Control Design Division, Frankford Arsenal, Philadelphia, Pa. February 15, 1943. This was the final report from the "Chafee Inquiry" which Chafee conducted for the NDRC fire control division to asses the importance of radar in AA fire control systems. It includes, however, a detailed history of Sperry's fire control development written by its primary participant.

[43] "History of Anti-Aircraft Director Development," 12-14.

[44] "History of Anti-Aircraft Director Development," 9-16.

development of control systems through the end of World War II. A technical memorandum dated 1930 explains the theory behind the T-6 calculations and how the system solved equations. Although this publication lists no author, it was most likely written by Chafee, Sperry's director of fire control engineering.[45]

Chafee begins by placing the highly technical tract into strategic context. He addresses "the influence of public opinion" and points to "the fear of air raids which has been built up through the public press," which may cause "a dangerous division of air forces" in wartime as scarce airplanes divert from offensive purposes to defending cities. Automatic antiaircraft devices can perform this defensive function, setting the public's mind at ease and leaving aircraft to fight forward battles. Chafee's description of public fear of air raids foreshadows the effects of civilian bombing of World War II. Similarly, in the Cold War, public fear of air raids would drive the development of computerized air defense control systems such as SAGE, a descendant of these early Sperry systems.

"The heart of the gun control system is the Computer," writes Chafee, using the term "computer" for a calculating machine a decade earlier than recent observers have noted.[46] Chafee describes in detail the workings of a mechanical analog computer that connected up to four three-inch antiaircraft guns and an altitude finder into an integrated system. [*Figure 3-6, Antiaircraft system layout] Just as with Sperry's naval fire control system, the primary means of connection were "data transmitters," similar to those that connected gyrocompasses to repeaters aboard ship. The director takes three primary inputs. First, target altitude comes from a stereoscopic range finder, similar in design and construction to those used in naval fire control. This device has two telescopes separated by a baseline of twelve feet; a single operator adjusts the angle between them to bring the two images into coincidence. Slant range, or the raw target distance, is then corrected

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[45] Sperry Gyroscope Company, "Anti-Aircraft Gun Control" Publication No. 20-1640 (Brooklyn, New York: Sperry Gyroscope Company Inc., 1930), SGC. This document does not list an author, but its language and explanations are quite similar to those in an article published by Chafee, "A Miss is as Good as a Mile," in *Sperryscope*, the official Sperry Company organ, in April, 1932. Also see Earl W. Chafee, Hugh Murtagh, and Shierfeld G. Myers, U.S. Patent no. 2,065,303, "Apparatus for the Control of Gunfire," filed January, 1933, issued December, 1936.

[46] Paul Ceruzzi writes that before the 1940s, the term "computer" meant a person who performed mechanical calculations in "When Computers Were Human," Annals of the History of Computing 13 (no. 3) 1991, 237. Also see Ceruzzi, Reckoners: The Prehistory of the Digital Computer (Westport, Conn.: Greenwood Press, 1983), 5. Paul McConnel notes that "calling their calculating devices 'computers' appears to have been an accepted practice with aviators as early as 1926." "Some Early Computers for Aviators" Annals of the History of Computing 13 (no. 2) 1991, 174. Chafee's use of the term for antiaircraft directors probably derives from its use in aviation.

by elevation angle to derive its altitude component. The second and third variables come from two additional operators, each with a separate telescope, who track the target for azimuth and for elevation (these telescopes physically mount on the director). Each sighting device has a data transmitter that measures angle or range and sends it to the computer. The computer receives these data and incorporates manual adjustments for wind velocity, wind direction, muzzle velocity, air density, and other factors. It calculates three output variables: azimuth, elevation, and a setting for the fuze. The latter, manually set before loading, determines the time after firing at which the shell will explode (corresponding to the time of flight).

The director performs two major calculations. First, *prediction* models the motion of the target and extrapolates its position to some time in the future, based on an assumption of constant course, speed, and altitude. Prediction corresponds to "leading" the target and parallels the function of the Ford Rangekeper as well as the lead of Sperry's ship stabilizer gyros. Second, the *ballistic* calculation figures how to make the shell arrive at the desired point in space at the future time and explode, solving for the azimuth and elevation of the gun and the setting on the fuze. This calculation corresponds to the traditional artilleryman's task of looking up data in a precalculated "firing table" and setting gun parameters accordingly. Ballistic calculation is simpler than prediction, so we will examine it first.

The T-6 director solves the ballistic problem by directly mechanizing the traditional method, employing a "mechanical firing table." Traditional firing tables were printed lists of solutions for given angular heights of the target, horizontal ranges, and a number of other variables. The Wilson Director had these tables graphically printed on a cylinder, on which an operator set a needle to read the data. The T-6 replaces the firing table with a "Sperry ballistic cam." A three-dimensionally machined cone-shaped device, the ballistic cam or "pin follower" solves a pre-determined mathematical function. [*Figure 3-7, Sperry ballistic cam] Two independent variables are input by the angular rotation of the cam and the longitudinal position of a pin which rests on top of the cam. As the pin moves up and down the length of the cam, and as the cam rotates, the height of the pin traces a function of two variables: the solution to the ballistics problem (or part of it). The T-6 director incorporates eight ballistic cams, each solving for a different component of the computation including superelevation, time of flight, wind correction, muzzle velocity, air density correction. Replacing the ballistic cams with a new set machined according to different firing tables could adapt a director to different guns.[47] Foreign governments, for example, could supply Sperry with firing tables and the company could machine custom cams and produce a special director. The ballistic cams comprised a central component of Sperry's mechanical computing technology.

Sperry literature touted the utility of ballistic cams, even that they could be adapted to more general problems, "this method of solving a mechanical problem is so flexible that it can be used for any range problem in which a desired solution can be expressed in terms of two known coordinates."[48] The cams had one major drawback, however, being extremely difficult to manufacture. To make one, a rough cam would be cast, and then a machinist would drill hundreds of small holes, working from a blueprint derived from a firing table usually supplied by the army's Aberdeen Proving Ground. He needed to continuously examine the point of the drill for wear, for each hole needed to be drilled to an accuracy of one ten-thousandth of an inch. He would then ground the cam smooth and polish it. Later, in an attempt to reduce costs and increase throughput, Sperry built a special machine to shape several cams at a time from a master template, similar to the 19th-century Blanchard lathe. Even with this improvement, the difficulty of ballistic cam manufacture proved a major limitation on Sperry's production of directors.

Ballistic cams essentially formed the permanent memory or stored program of the director computer (although far from a "stored program" machine in the way we think of it today). Numerical calculations from the Army Ordnance Department provided the data from which to fabricate the cams, the machinist's blueprint was an intermediate step which could be eliminated. Cam production became increasingly automated, from the ballistics calculations that produced the data up through actual fabrication. This process, with its flow of information from ballistics to machine control, gradually approached "numerically controlled" machining. In chapter 9 we shall see how how this process drove developments in digital computers and early numerically controlled machine tools.

The T-6 director performed its other computational function, prediction, in an innovative way as well. Though the target came into the system in polar coordinates (azimuth, elevation, and range), targets usually flew a constant trajectory (it was assumed) in rectangular coordinates -- i.e.

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[47] Robert Lea, "The Ballistic Cam in Dean Hollister's Lamp," SGC.

[48] "Universal Fire Control Director For Defense Against Attack from Air, Land, or Water," publication no. 14-8053. SGC, Box 3.

straight and level. Thus it was simpler to extrapolate to the future in rectangular coordinates than in the polar system, and also simpler to include wind and parallax corrections. So the Sperry director projected the movement of the target into a horizontal plane, derived the velocity from changes in position, added a fixed time multiplied by the velocity to determine a future position, and then converted the solution back into polar coordinates. This approach loosely parallels the plotting of the Sperry Battle Tracer, which resolved range and bearing onto a Cartesian map. It became known as the "plan prediction method" because of the representation of the data on a flat "plan" as viewed from above; it was commonly used through World War II. The plan prediction method was a pure analog of the world, "the actual movement of the target is mechanically reproduced on a small scale within the Computer and the desired angles or speeds can be measured directly from the movements of these elements."[49] The familiar radar target display, introduced years later, in which a beam rotates sweeps around a round tube to reveal targets, became known as the PPI or "plan position indicator," an appellation inherited from this method of computation.

Together, the ballistic and prediction calculations form a feedback loop. [*Figure 3-8, Detailed T-6 data flow] Operators enter an estimated time of flight for the shell when they first begin tracking. The predictor uses this estimate to perform its initial calculation, which feeds into the ballistic stage. The output of the ballistics calculation then feeds back an updated time of flight estimate, which the predictor uses to refine the initial estimate. Thus "a cumulative cycle of correction and recorrection...brings the predicted future position of the target up to the point indicated by the actual future time of flight."[50]

The T-6 director, a square box about four feet on a side, mounted on a pedestal on which it could rotate [*Figure 3-9, T-6 Director photo]. Three crew members sat on seats and one or two stood on a step mounted to the machine, revolving with the unit as the azimuth tracker followed the target. This arrangement provided comfortable, stable positions for the tracking operators. As the unit and the trackers rotated, the remainder of the crew, who stood on a fixed platform, had to shuffle around with it. While the rotation angles were small for any given

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[49] "Anti-Aircraft Gun Control," 21.
[50] "Anti-Aircraft Gun Control," 32.

engagement, it must have been awkward. The director's pedestal mounted on a trailer, on which data transmission cables and the range finder could be packed for transportation.

The T-6 computer required only three inputs, elevation, azimuth, and altitude (range), and yet it required *nine operators*. These nine did not include the operation of the range finder, which was considered a separate instrument, or the men tending the guns themselves, but only those operating the director itself. What did these nine men do?

Human Servomechanisms

The operators were "manual servo-mechanisms." One specification for the machine required "minimum dependence on 'human element'." Sperry Gyroscope explained, "All operations must be made as mechanical and fool proof as possible; training requirements must visualize the conditions existent under rapid mobilization." The memory of World War I rings in this statement. Even at the height of isolationism, with the country sliding into depression, design engineers considered the difficulty of raising large numbers of trained personnel in a national emergency. Designers also considered the ability of operators to perform their duties under the stress of battle. Thus, nearly all the work for the crew was in the "follow-the-pointer" mode, derived from naval systems: each man concentrated on an instrument with two indicating dials, one the actual and one the desired value for a particular parameter. With a hand crank he adjusted the parameter to match the two dials. The control system domesticated not just the beast but its operator as well.

Still, it seems curious that the T-6 director required so many men to perform this follow-the-pointer input. When the external rangefinder transmitted its data to the computer, it appeared on a dial and an operator had to follow the pointer to actually input the data into the computing mechanism. Similarly, the machine did not explicitly calculate velocities. Rather, two operators (one for X/north-south and one for Y/east-west) adjusted variable-speed drives until their rate dials dial matched that of a constant-speed motor (the adjustment on the drive then equaled velocity). When the prediction computation was completed, an operator had to feed the result into the ballistic calculation mechanism. Finally, when the entire calculation cycle was completed, another operator had to follow the pointer to transmit azimuth to the gun crew, who in turn had to match the train and elevation of the gun to the pointer indications.

Figure 3-10 shows the crew arrayed around the T-6 director, in an arrangement that today seems almost comical [*Figure 3-10, Crew around T-6]. Strange as these operations seem, they reveal Sperry engineers' conception of the human role in the operation of an automated system. The numerous follow-the-pointer operations were clearly preferable to data transmission by telephone; in that sense the system was automated. Operators literally supplied the feedback that made the system work, although Sperry's idea of feedback was rather different than the modern one:

> In many cases where results are obtained by individual elements in the cycle of computation it is necessary to *feed these results back* into the mechanism or to transmit them.

The operators provided this feedback in part to drive the heavy ballistic cams. The Sperry document acknowledges the possibility of automating these operations, but does not find it the preferable option:

> When mechanical methods are employed, it is necessary to use some form of "servo-motor," and electrical servo-motors are used to a limited degree for "feeding back" data into the computer.
>
> It has been found in many cases to be much easier to rely on a group of operators who fulfill no other function than to act as servo-motors....This operation can be mechanically performed by the operator under rigorous active service conditions.[51]

Here Sperry promotes the view of automation that best matches the strengths and weaknesses of its own products. Human operators connected "individual elements" into an integrated system. The men were amplifiers, and hence quite similar to servomechanisms in other mechanical calculators of the time, especially Vannevar Bush's differential analyzer (see Chapter 5).

The term "manual servomechanism" itself is an oxymoron: by the conventional definition, all servomechanisms are automatic. Just using the term acknowledges the existence of an automatic technology which might replace the manual method. With the T-6, this process was already underway, for though it required nine operators, two had already been eliminated from the previous generation T-4. Servos replaced the operators who fed-back superelevation data and transmitted fuze setting. Furthermore, in this early machine one man corresponded to one variable, and the machine's requirement for operators corresponded directly to the data flow of its computation. Thus the crew that operated the T-6 director was an exact reflection of the algorithm inside it.

Why, then, were only two of the variables automated? While the Sperry literature proudly trumpets human follow-the-pointer operations, it barely acknowledges the automatic servos, and even then provides the option of manual follow-ups "if the electrical gear is not used." This partial, almost hesitating automation indicates there was more to the human servo-motors than Sperry wanted to acknowledge. As much as the company touted its weak-link view, "their duties are purely mechanical and little skill or judgment is required on the part of the operators," men were still required to exercise some judgment, even if unconsciously. The data were noisy, and even an unskilled human eye could eliminate complications due to erroneous or corrupted data. Furthermore, noisy data did more than corrupt firing solutions. The mechanisms themselves were rather delicate, and erroneous input data, especially if it indicated conditions that were not physically possible, could lock-up or damage the mechanisms.[52] As in naval fire control, the operators performed as strong-link integrators in both senses of the term: they integrated different elements into a system, and they integrated mathematically, acting as filters to average out noise.

Later Sperry Directors

When Chafee wrote this report in 1930, his engineers were already at work on a newer generation director. The T-8 was called the "universal director" because it could direct fire to both airborne and ground targets. Chafee intended this machine to be lighter and more portable than earlier models, as well as less expensive and "procurable in quantities in case of emergency."[53] The company still emphasized the need for unskilled men to operate the system in wartime, and their role as system integrators. They were "mechanical links in the apparatus, thereby making it possible to avoid mechanical complication which would be involved by the use of electrical or mechanical servo motors." army field experience had shown Sperry directors to be difficult to use, and that operators were not receiving proper training. The T-6 had also demonstrated that servo motors were a viable way to reduce the number of operators (and hence reduce the training problem) and improve reliability, so the requirements for the T-8 specified that wherever possible "electrical follow-up motors shall be used to reduce the number of operators to

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[51] "Anti-Aircraft Gun Control," 24-25.

[52] Anti Aircraft Defense (Harrisburg, Pennsylvania: 1940). This book reprints the manuals for the Sperry M-2 director and discusses the mechanical problems that can be caused in this generation of Sperry directors by contradictory input data.

[53] "Anti-Aircraft Gun Control," 18.

a minimum."[54] Thus the T-8 continued the process of automating fire control, reduced the number of operators to four and aimed to be lighter, more portable, and less expensive than the T-6. Two men followed the target with telescopes, and only two were required for follow-the-pointer functions (for the two rate follow-ups). The other follow-the-pointers had been replaced by follow-up servos fitted with magnetic brakes to eliminate hunting (the inclusion of these brakes suggests that the hesitating use of servos in earlier models was partly due to concerns about their stability). Several experimental versions of the T-8 were built, and it was standardized by the army as the M3 in 1934.

In 1936 Sperry let a contract to Professor Nicholas Minorsky (1885-1970) to study the possibility of replacing the calculation mechanisms of its mechanical directors with electrical components. Minorsky had worked for Steinmetz at General electric and had done pioneering work in the theory of control systems, especially for ship steering, in the 1920s (see Chapter 5). He then moved to the navy's David Taylor Model Basin and to the University of Pennsylvania. Minorsky proposed a design for an electrical director, which a Sperry engineer, Bruno A. Wittkuhns, evaluated. He found it "entirely too complicated and impracticable" but came up with a scheme of his own to convert the M3 director to electrical computation. Where Wittkuhns employed "follow-up" motors to implement sines, cosines, multipliers, and differentiators, his scheme still involved mechanical cams. He noted electrical equipment is "well suited to mass production," and,

> While there seems to be no field now for a director of this type at this time, it is entirely possible that if developed in a sufficiently small size and light weight and comparable accuracy interest in this machine can be found, if not for Army work then possibly for Naval installations where the resistance against electrical devices seems to be less noticeable.[55]

The company had been spending significant amounts of money on director development at this time, and selling relatively few units, so it took no further action on developing an electrical director. In 1940, an engineer at Bell Telephone Laboratories would "invent" exactly this device, based on an electrical replacement for the ballistic cams (see Chapter 8).

Throughout the remainder of the thirties, Sperry and the army fine-tuned the director system as embodied in the M3. Succeeding models automated further, replacing the follow-the-

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[54] "Universal Director and Data Transmission System," Sperry Gyroscope Publication no. 14-8051, August 1, 1932, 6. SGC, Box 2. This document is essentially a specification for the T-8.

pointers for target velocity with a velocity follow-up servo which employed a ball-and-disc integrator, derived from Hannibal Ford's device. The M4 series, standardized in 1939, resembled the M3 but abandoned the constant altitude assumption and added an altitude predictor for gliding targets. About 200 M4s were eventually produced.[56]

The M7, standardized in 1941, was essentially similar to the M4 but added a number of modifications, including the provision for radio range finding and full power control to the guns for automatic pointing in elevation and azimuth. Automating the pointing of the gun was a more difficult problem than data transmission because it involved significant power amplification. Sperry Gyroscope purchased the rights to the Neiman torque amplifier system from Bethlehem Steel in 1926, which could provide precision high-power outputs in response to low power torque inputs (the Neiman torque amplifier would find its way into Vannevar Bush's differential analyzer, see Chapter 5). During the next several years Sperry applied power controls to a number of individual guns. None were incorporated into actual systems, however, due to army concerns about reliability and perhaps also to problems of stability. No further work was done on power controls for guns for almost ten years. Not until 1939 did Sperry begin to develop an electro-hydraulic remote control system for the army's new 90mm antiaircraft gun. Sperry's acquisition of Vickers and Waterbury provided corporate skills with hydraulic drives. Still, the company had to build its power controls around existing guns, and could not suggest changes to the mount to make it more adaptable to the power control. This machinery was undergoing testing when reports from the Battle of Britain indicated that automatic power controls for gun pointing "was not only desirable but was absolutely necessary." The army then contracted Sperry to produce power controls for all its 90mm guns, which it began in 1941. The company delivered more than 3500 of the devices in 1942, and more than 4000 in 1943, although most did not accompany Sperry directors.[57]

The later Sperry directors had eliminated errors to the point where the greatest uncertainty in the system was the varying time it took different crews to manually set the fuze and load the shell into the gun. Automatic setters and loaders could improve the situation, but crews found

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[55] Bruno A. Wittkuhns to E.W. Chafee, P.R. Bassett, and H.H. Thompson, June 15, 1936. SGC, Box 33.
[56] "History of the AA Director Development (Army Ordnance)," Sperry Gyroscope Memo, n.d., probably Fall, 1935. SGC, Box 4. For an explanation of the Sperry velocity servo, see Allan G. Bromley, "Analog Computing Devices" in William Aspray ed. Computing Before Computers (Ames, Iowa: 1990), 190.

them unreliable and dangerous to the integrity of their fingers, and would not use them. When, in 1940, the National Defense Research Committee began studying the antiaircraft problem, one of their first tasks was to conduct efficiency studies of people doing fuze setting. The other weak link was the range finder, because readings from the stereo viewfinder depended greatly on the skills of the human operator, even changing from day to day without the same operator. The M7 model included a provision for entering azimuth observations from radio locator equipment, anticipating the addition of radar for target observations. At the start of World War II, the M7 was the primary anti aircraft director available to the army, although the M4 was considered state-of-the-art as well.

The M7, culminating fifteen years of work at Sperry, was a highly developed and integrated system, optimized for reliability and ease of operation and maintenance. As a mechanical computer, it was an elegant, if intricate, device, weighing 850 pounds and including about 11,000 parts. The design of the M7 capitalized on the strength of Sperry Gyroscope: manufacturing of precision mechanisms (especially ballistic cams), data transmission, and intimate involvement with technical officers in the armed services.

These capabilities, however, became scarce as the United States prepared for war. Sperry reluctantly subcontracted director production to the Ford Motor Company, but it remained a "real choke" and could not keep up with production of the 90mm guns, well into 1942 (Sperry subcontracted many of its products for volume production during the war).[58] The army had also adopted an English machine, known as the "Kerrison Director" or M5 for lighter guns. It was less accurate than the M7 but easier to manufacture. Sperry redesigned the M5 for high-volume production in 1940, but passed on manufacturing responsibility to the Singer Sewing Machine and Delco companies in 1941.[59] When the National Defense Research Committee was formed in 1940, among its first projects were to create standardized setups for testing antiaircraft director performance. Such tests proved the Sperry machines to be seriously flawed in their firing solutions and by 1943 an electronic computing director developed at Bell Labs superseded the M7, which ceased production.

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[57] Sperry Company Report, "Power Controls," February 7, 1944. SGC, Box 40.

[58] Harry C. Thompson and Lida Mayo, The United States Army in World War II: The Ordnance Department Volume 2: Procurement and Supply (Washington, DC: 1960), p. 86.

The Sperry antiaircraft directors of the 1930s were transitional, experimental systems. Exactly for that reason, however, they allow us to peer inside the process of automation, to examine the displacement of human operators by servomechanisms while the process was still underway. Skilled as Sperry Gyroscope was at data transmission, it only gradually became comfortable with the automatic communication of data between subsystems. Sperry could brag (perhaps protesting too much) about the low skill levels required of the operators of the machine, but in 1930 it was unwilling to remove them completely from the process. Men were the glue that held integrated systems together.

As products, Sperry Gyroscope's antiaircraft gun directors were only partially successful. A decade and a half of development produced machines that could not negotiate the fine line between performance and production imposed by national emergency. Still, we should judge a technological development program not only by the machines it produces but also by the knowledge it creates, and by how that knowledge contributes to future work. Sperry's antiaircraft directors of the 1930s were early examples of distributed control systems, technology that would assume critical importance in the following decades with the development of radar and digital computers. When building the electronic and radar controlled antiaircraft directors of World War II, engineers at Bell Labs, MIT, and elsewhere incorporated and built on Sperry Gyroscope's experience, grappling with the engineering difficulties of feedback, control, and the augmentation of human capabilities by technical systems.

Conclusion: Survival of the Beast vision

In 1940. Sperry Gyroscope listed the following as its product line: aircraft gyropilot, automatic (radio) direction finder, directional gyro, gyro horizon, incandescent searchlight, high-intensity searchlight, course recorder, ship gyropilot, rudder indicator, electromechanical steering system, gyro-compass, sound locator, antiaircraft searchlight, universal (antiaircraft) director.[60] Of

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[59] Sperry Company memo on M-5 and M-6 directors. SGC, Box 33. Also see Bromley "Analog Computing Devices," 186-191.

[60] Sperryscope listed the company's products inside the front cover. This list comes from Volume 9 (no. 2, April, 1940). Because of the developmental nature of many of Sperry Gyroscope's products, dates of introduction are open to interpretation. The following list of the dates of introduction of Sperry's 1940 products is compiled from a chart in Sperryscope 7 (no. 7, April, 1935) page 16, and a "family tree" of Sperry products from "The Story of the Sperry Corporation," n.d., probably 1943. SGC, Box 40: aircraft gyropilot (1931), automatic (radio) direction finder, directional gyro (1918), gyro horizon (1930), incandescent searchlight (1924), high-intensity searchlight (1916), course recorder (1918), ship gyropilot (1922), rudder indicator (1920), electromechanical steering system

these, only two were introduced after 1930, the automatic direction finder in 1931 and the universal antiaircraft director, M4, in 1936. Despite Sperry Gyroscope's emphasis on new technology, and despite its consistent engineering efforts, most of the company's catalog in 1940 did not represent important new inventions. The other devices had undoubtedly matured in the previous ten years, but antiaircraft fire control was the company's only significantly new product. This stagnation probably reflects the effects of the depression and the passing of Elmer Sperry as a creative force. Several development programs did not produce lasting products: Sperry fire control lost out to Ford and G.E., Sperry bombsights lost out to Norden, Sperry antiaircraft directors lost out to Bell Labs, and its aerial torpedoes and gyrostabilizers proved impracticable. Sperry tried several failed product lines for every one that stayed in production; they had great difficulty developing complex, high-performance control systems and deploying them in the field. In fact, the company's history with automatic machinery is as remarkable for its difficulties as for its successes. When World War II came, the company's value lay in its production capacity and engineering vision as much as its research department. The major pre-war product line, antiaircraft fire control, was discontinued at the height of the wartime production boom because of manufacturing complexities. Groups with no experience in fire control were able to learn the field quickly and build better systems than Sperry's, although building on Sperry experience.

In the year or two before 1940 Sperry engineers had begun work on new controls which combined perception and articulation and left integration to the human operator. The company had recently supported klystron research by Russel and Sigurd Varian, which had not yet produced commercial products but gave the company an advantage when radar growth exploded during World War II.[61] 1940 saw the introduction of a number of new products which assured the company's success during the war: simple and easily manufactured controls for fire control aboard aircraft. Unlike battleships, most World War II bombers did not use director fire to coordinate their guns. Machine gunners defending flying fortresses from attacking fighters each worked individually, with no coordination or centralized control (except through voice intercom). Beginning in 1940, the Sperry Corporation produced these individual controls, hydraulic turrets

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(1930), gyro-compass (1914), sound locator (1928), antiaircraft searchlight (1923), universal (antiaircraft) director (1936). The "family tree" is somewhat suspect because it does not list the Battle Tracer, or any of Sperry's World War I naval fire control work. It altogether skips over the year 1916, when these devices were introduced.

[61] Varian, The Inventor and the Pilot.

for machine gun defenses of B-24 and B-17 bombers. These devices allowed a gunner to rapidly and smoothly swing around himself and his machine guns to fend off attacking airplanes. Sperry Gyroscope built on its strength in aviation instruments and its corporate tradition, going back to the original gyrocompass, of reference and measuring devices. The company built instruments of perception; gyroscopic aviation instruments coupled to visual indicators called "lead computing sights" imposed scales on the gunners' vision and indicated where to aim. Vickers, in complement, made the system's articulation component: small, electro-hydraulic power controls to move the turret. Subcontractors made the glass and steel structure.[62] Thus Sperry's corporate organization mapped two functions of the governor, perception and articulation, leaving the third, integration, for a strong-link human operator.

These machines, especially the famous "Ball Turret," comprised a popular image of mechanized air combat during World War II, and their production occupied much of Sperry's wartime resources. [*Figure 3-11, Sperry Ball Turret] These simple but effective machines placed heavy emphasis on the human operator, aiding his mind and his body at critical points but leaving command in his hands. At Sperry, at least, the beast vision survived. Only the B-29, operational late in the war, incorporated "central station" control of its air defenses (with deadly effects but few enemies over Japan in 1945). Sperry Gyroscope developed a prototype of this system, but again lost out to a General Electric design (partly because of Sperry's overburdened production lines). During the war Sperry made not the most advanced or intricate products, but rather those that effected simple, tight assemblages of mechanical and human functions and which could be produced in large numbers. Even the Bureau of Ordnance needed these devices. As Chapter 8 will recount, Sperry sponsored a university researcher, Charles Stark Draper, to apply flight instruments to defending ships, and his work brought Sperry back into naval fire control after a twenty year hiatus.

These simple, human-centered controls produced great rewards for the Sperry Company, as it was ideally suited to wartime demands and devoted itself exclusively to war production. Sperry Corporation sales doubled from 1941 to 1942, and quadrupled the following year. 1942 sales peaked at seventeen times 1939 figures and equaled the nine previous years combined.

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[62] "Aircraft Fire Control," Sperry Gyroscope Company report, SGC, Box 22. "Aircraft Computing Sights," Sperry Gyroscope Company, Vinson report edited by Roswell Ward, February 8, 1944. Report, SGC, Box 40. Roswell Ward, "Aircraft Turrets: Description of Product Development and History," February 16, 1944. SGC Box 40.

[*Figure 3-12: Sperry Sales Chart]. The government built the company a $20 million plant at Lake Success on Long Island, and in 1943 Sperry employed 50,000 people, ten times the 1939 number. Profits rose so high that the company voluntarily returned money to the government (several valuable project histories in the Sperry archives were produced for these negotiations).[63]

Sperry trumpeted its vision of automation as the extension, rather than replacement, of the human operator's capabilities, brought forth by its experience between the wars:

> There has come into being a whole new field of scientific accessories to extend the functions and the skill of the operator far beyond his own strength, endurance, and abilities....The importance to the Government of having these organizations [the Sperry companies] carrying on continuous research along these highly technical lines independent of governmental authority or even popular support is borne out by the fact that now the products of this twenty years of Sperry development must be produced in quantities much greater than the companies can handle.

Mass conscription would have little affect without increases in production; Sperry argued its products brought the wartime mobilization of manpower together with the mobilization of industry.

> Over a billion dollars of this material [control systems] must be produced by us within the next two years. But this billion dollars' worth of technical equipment will fill the vital gap between the one hundred billion dollars' worth of weapons and the thousands of men who must operate them. With this equipment, neither men nor weapons would be effective.[64]

Sperry's control systems united the beasts procured by the services with the men who would ride them into battle.

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[63] Data taken from Sperry Annual Reports, 1939-45. Also see the "family tree" in "The Story of the Sperry Corporation," and page 18 of that booklet for photos of these devices. Project histories can be found in SGC, Box 40, folder "Renegotiaion Documents."

[64] "Introduction," to Sperry Company History, n.d., probably 1942. SGC, Box 40. Emphasis added.

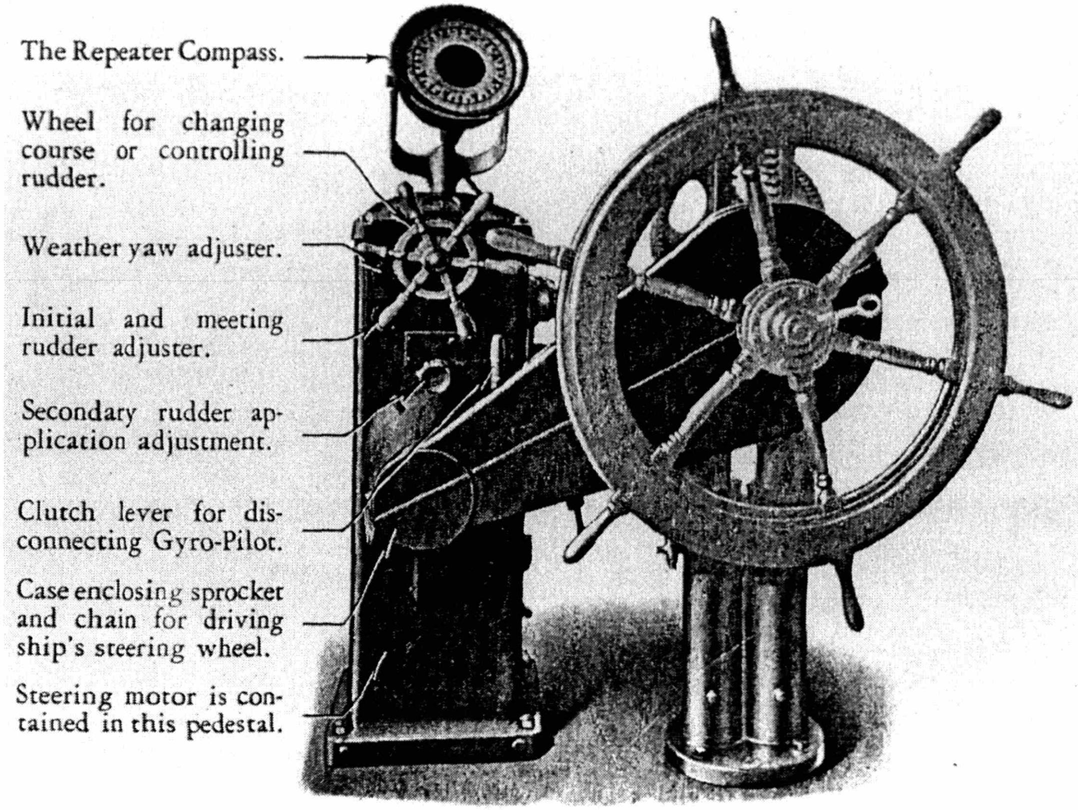


Figure 3-1: Sperry Gyropilot, "Metal Mike."

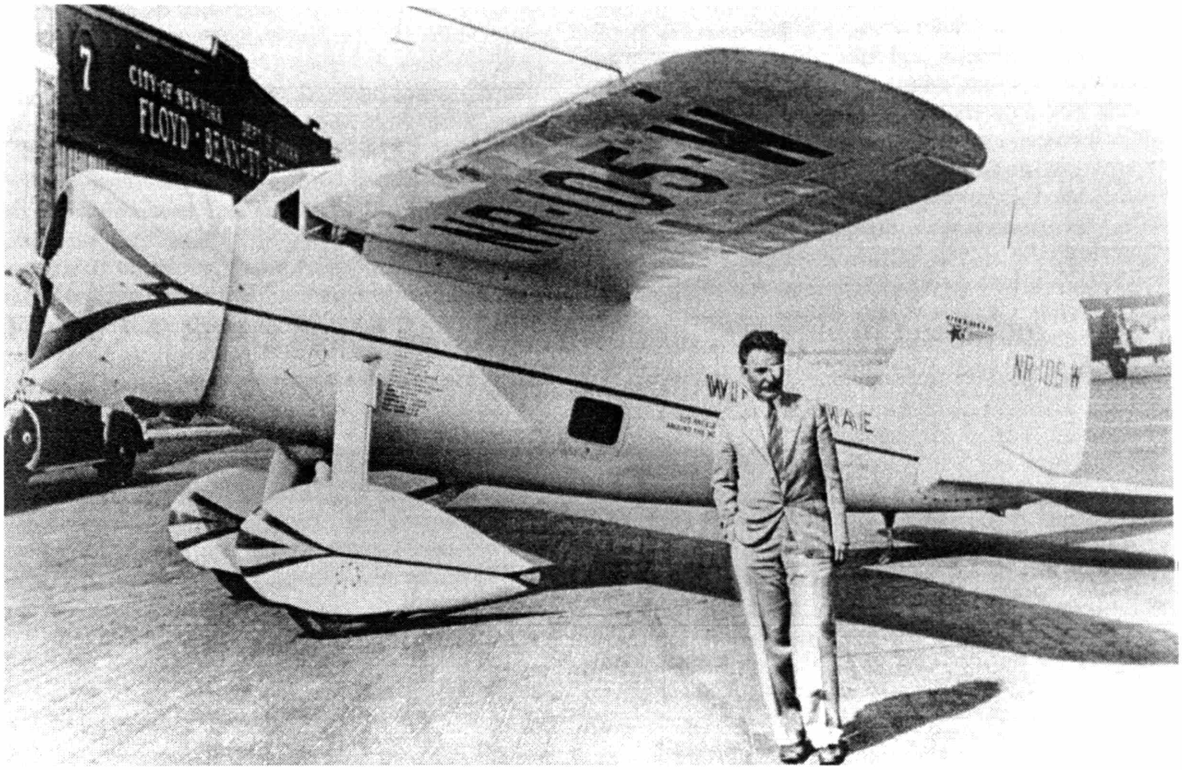


Figure 3-2: Wiley Post and his airplane, Winnie Mae, in which me made his around-the-world solo flight with a Sperry Autopilot (From sales pamphlet, "'Round the World with the Sperry Pilot.," SGC).

Figure 3-3: Wiley Post in his pressure suit. Note the oxygen tank feeding into the helmet next to Post's eye patch.

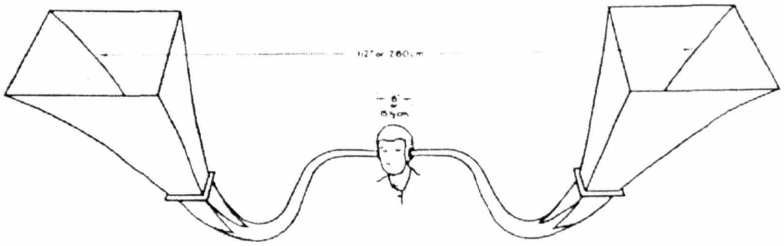


Figure 3-4: Sperry Sound Ranging Equipment, principle of operation.

Figure 3-4a: Sperry Sound Ranging Equipment, human operator.

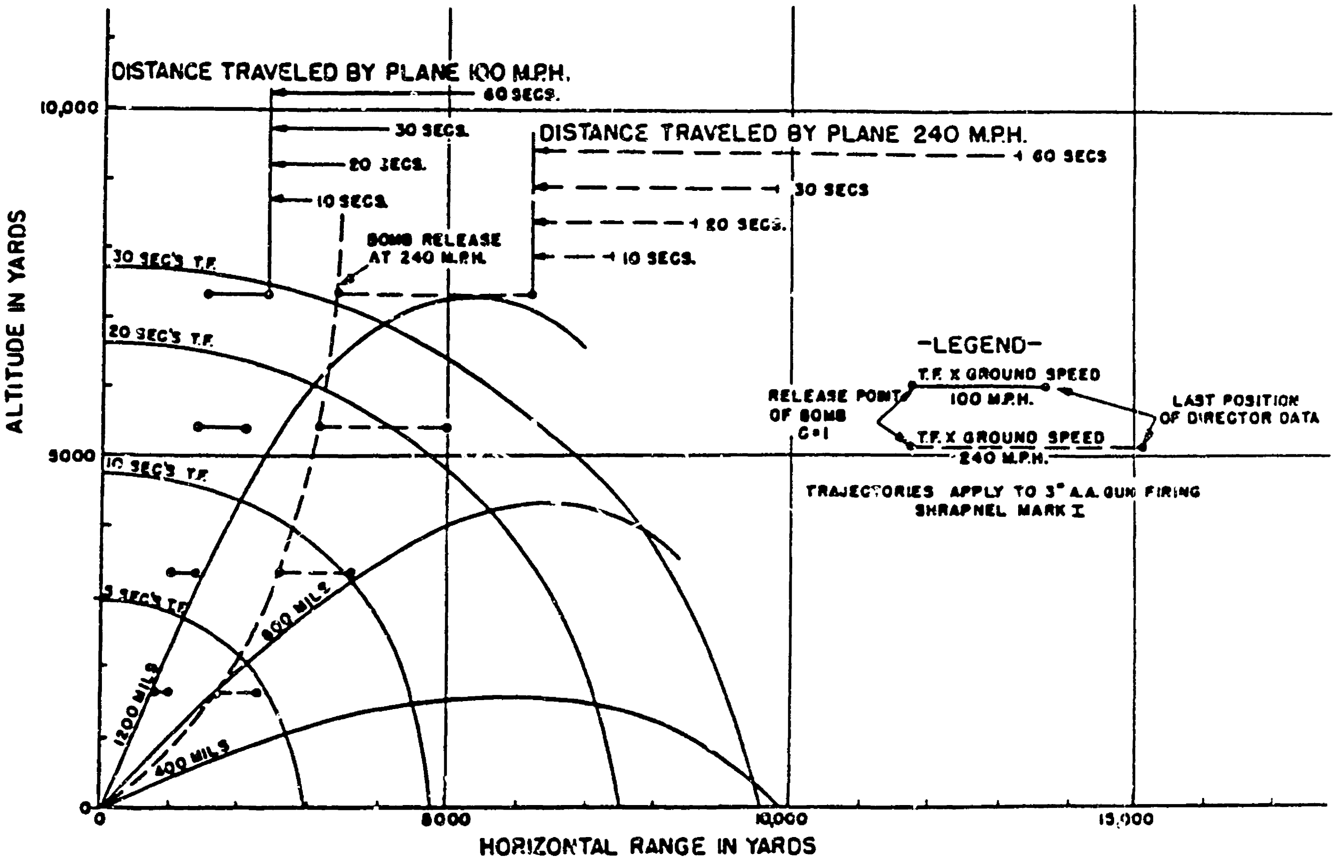


Figure 3-5: Antiaircraft trajectories and leads for different bomber speeds (From Chafee, 1930).

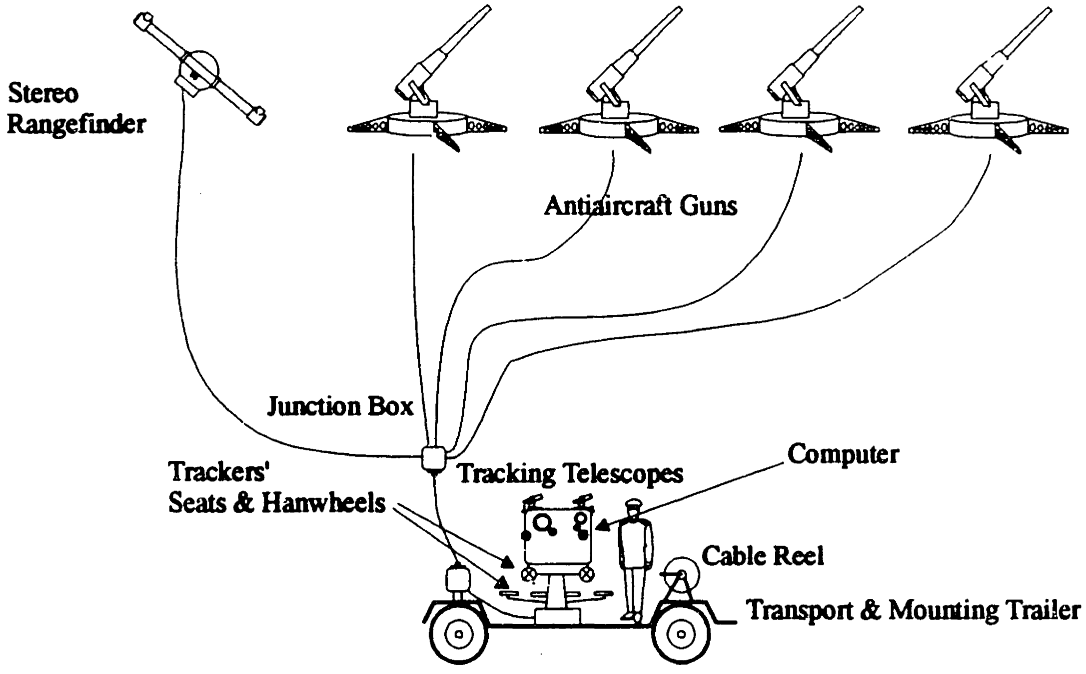


Figure 3-6: Layout of Sperry T-6 Antiaircraft Director (human shown for scale only)

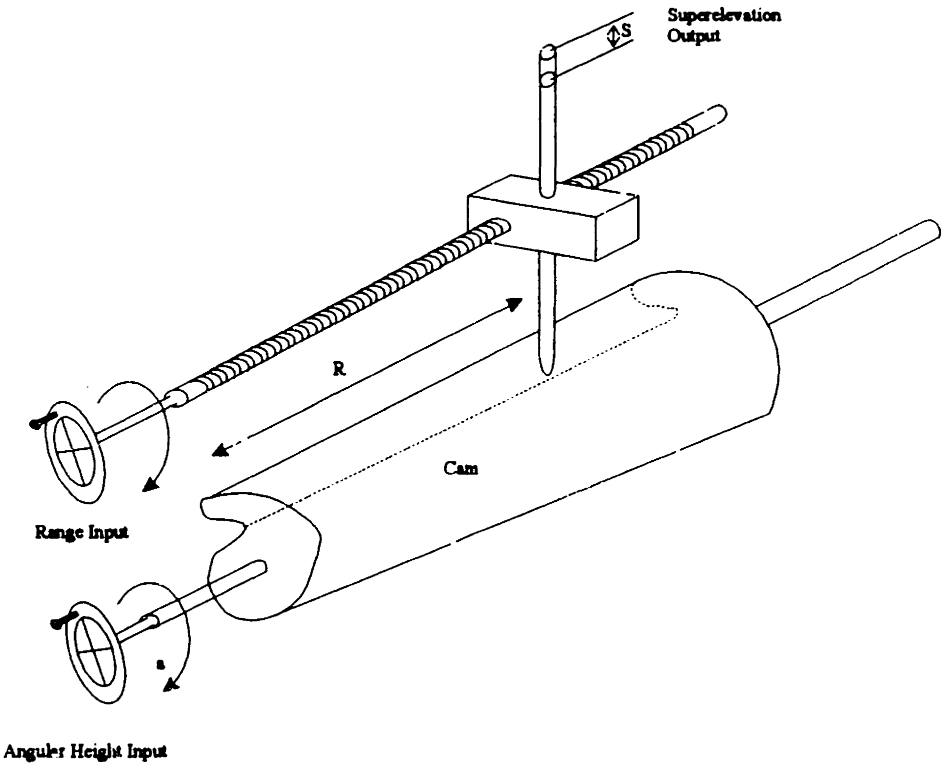


Figure 3-7: Operation of Sperry Ballistic Cam. Two variables are input by rotation of handles at left. Pin rides along cam as it rotates, height of pin S provides output value for feeding into another mechanism. In this example, for a given range and angular height of a target, the ouput S is the superelevation, or the firing angle of the gun. Handles would likely be rpelaced by shaft inputs in real machine.

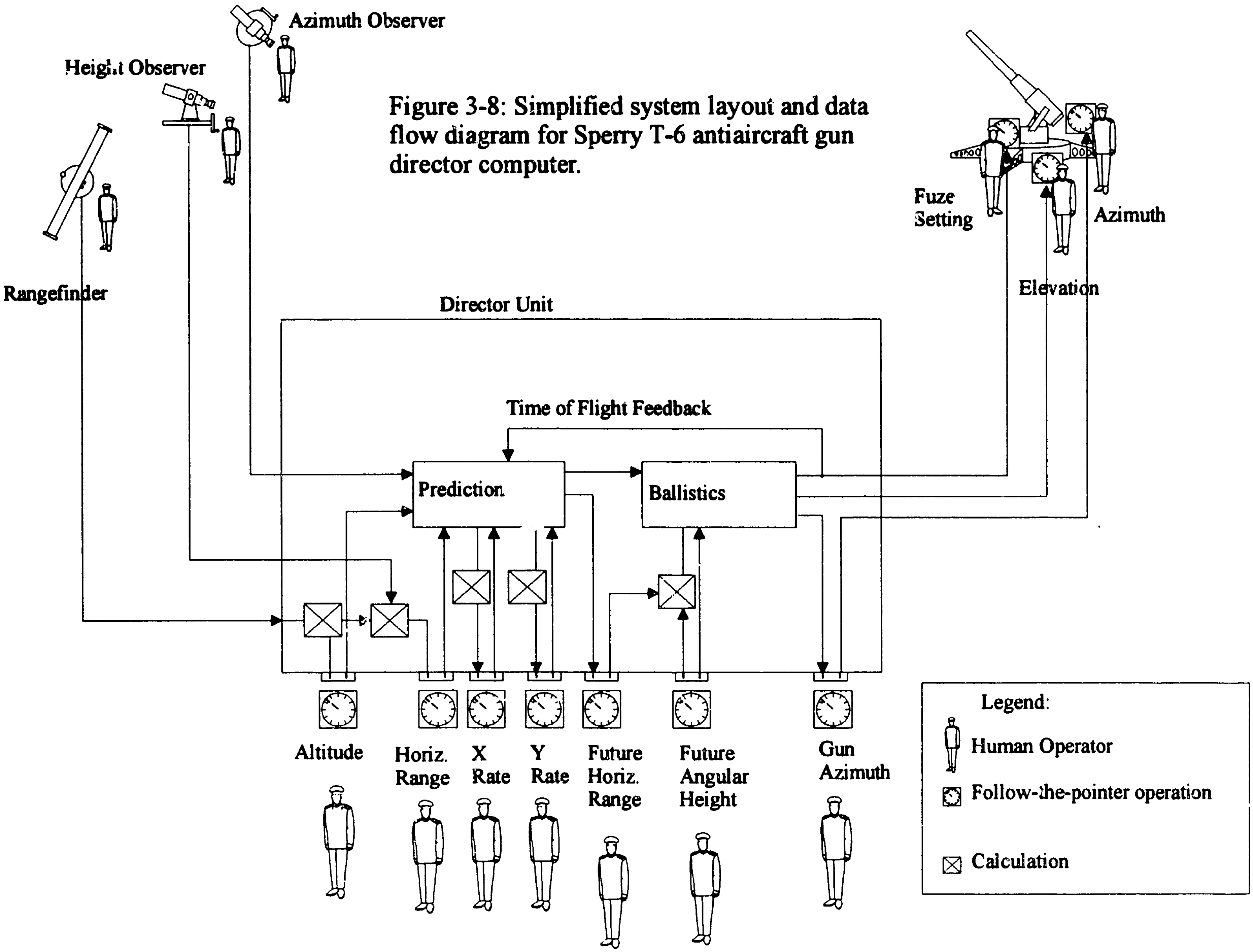
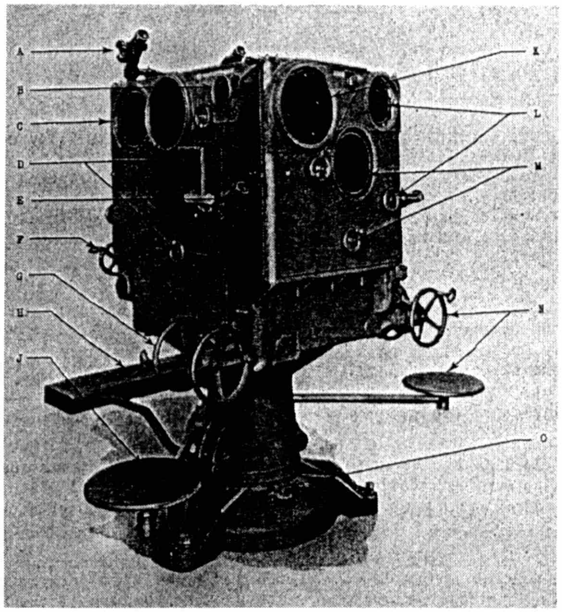


Figure 3-8: Simplified system layout and data flow diagram for Sperry T-6 antiaircraft gun director computer.

Figure 3-9. The Sperry T-6 Director

A. Spotting Scope
B. North-South Rate Dial and Handwheel
C. Future Horizontal Range Dial
D. Super-Elevation Dial and Handwheel
E. Azimuth Tracking Telescope
F. Future Horizontal Range Handwheel
G. Traversing Handwheel (Azimuth Tracking)
H. Fire Control Officer's Platform
J. Azimuth Tracking Operator's Seat
K. Time of Flight Dial and Handwheel
L. Present Altitude Dial and Handwheel
M. Present Horizontal Range Dial and Handwheel
N. Elevation Tracking Handwheel and Operator's Seat
O. Orienting Clamp.



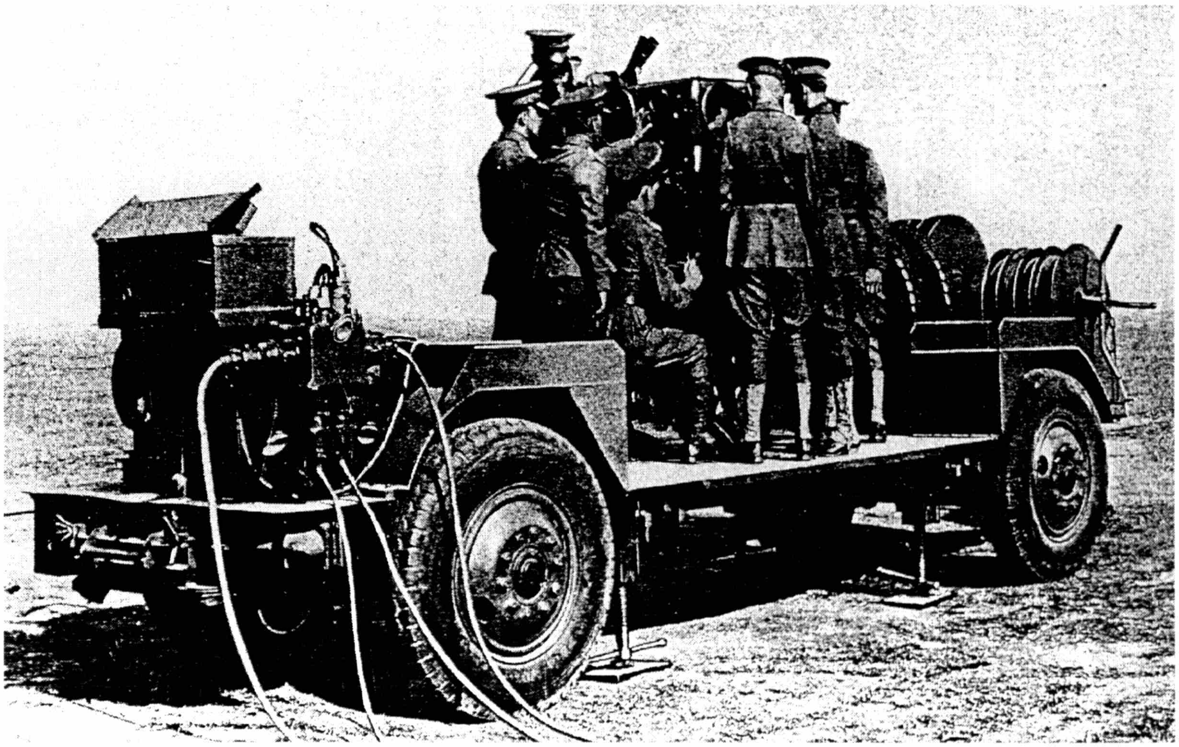


Figure 3-10. The Sperry T-6 Director mounted on a trailer with operators. Note power supply at left and cables to other system elements. (Courtesy Hagley Museum and Library)

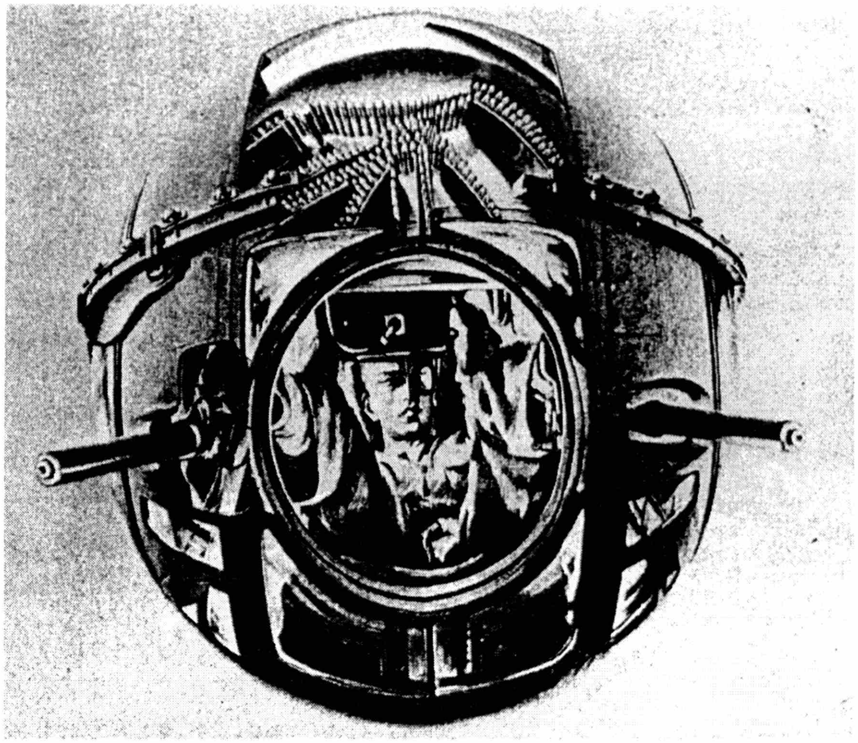
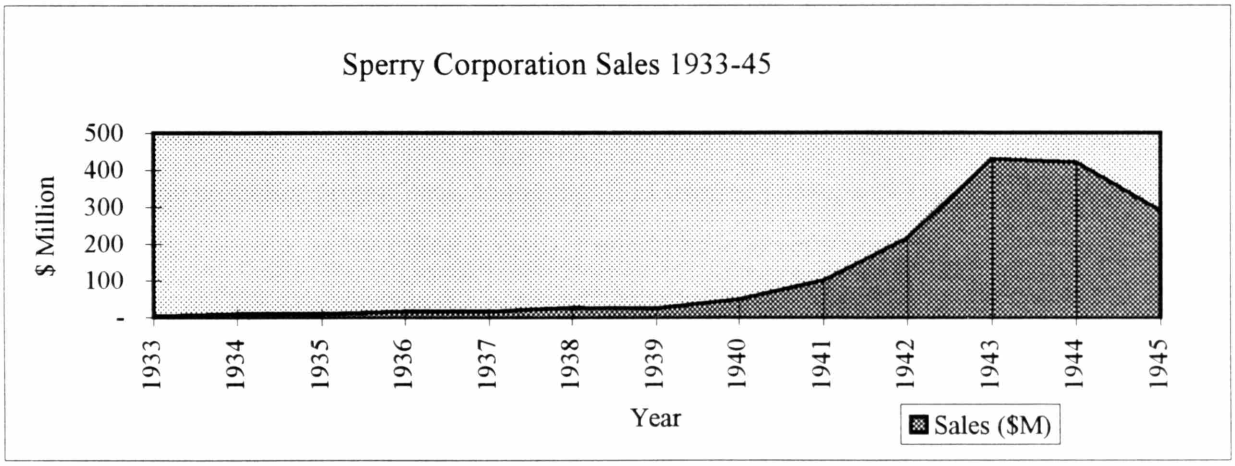


Figure 3-11: Sperry Ball Turret for the B-17 bomber, 1941. Note eyepiece for the lead-computing gunsight.

Figure 3-12: Sperry Corporation Sales and R&D Expenses, 1933-45
(Source: Sperry Corporation Annual Reports)

Year	Sales ($M)	R&D Expenses	R&D as a Percentage of Sales
1933	3.5716	$112,451	3.15%
1934	7.8310	$216,370	2.76%
1935	8.6901	$254,194	2.93%
1936	14.6841	$291,033	1.98%
1937	15.2773	$352,433	2.31%
1938	25.3992	$546,527	2.15%
1939	24.8561	$789,437	3.18%
1940	47.5145	$1,049,046	2.21%
1941	99.8195	$2,211,313	2.22%
1942	216.2819	$3,483,221	1.61%
1943	429.0160	$4,902,265	1.14%
1944	420.1860	$6,783,536	1.61%
1945	288.9337	$6,663,513	2.31%



Chapter 4

Feedback Amplifiers and Mixed Emotions

At Bell Telephone Laboratories

> People assimilated telephony into their minds as if into their bodies — as if it were the result of a new step in human evolution that increased the range of their voices to the limits of the national map.
>
> John Brooks, Telephone: The First One Hundred Years[1]

> The engineer who embarks on the design of a feedback amplifier must be a creature of mixed emotions.
>
> Hendrik Bode, 1940

Opening Black's Box: Rethinking Feedback's Myth of Origin

Like any modern epistemology worth the name, the theory of feedback has a myth of origin. On a sunny August morning in 1927, Harold Black, a 29 year-old systems engineer, rode the Lackawanna ferry to work at the Bell Telephone Laboratories. Many Bell engineers lived in New Jersey; on the early morning ferry rides across the Hudson to the Manhattan laboratories they frequently gathered on the forward deck for informal technical conferences. This morning, Black stood alone, staring at the Statue of Liberty, and had an epiphany: "I suddenly realized that if I fed the amplifier output back to the input, in reverse phase, and kept the device from oscillating (singing, as we called it then), I would have exactly what I wanted: a means of canceling out the distortion in the output."[2] As it happened, the New York Times that day contained a blank page and Black sketched his idea, "a simple canonical diagram of a negative feedback amplifier plus the equations for the amplification with feedback." He rushed into work, asked a technician to wire up a prototype, and gave birth to a foundational circuit of modern electronics. This story has become enshrined as one of the central "flashes of insight" in electrical

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[1] John Brooks, Telephone: The First One Hundred Years (New York: Harper and Roe, 1975), 142.

[2] Harold S. Black, "Inventing the Negative Feedback Amplifier," *IEEE Spectrum* 14 (December, 1977), 54-60. George Stibitz's memoir has memories of the early morning ferry rides, The Zeroth Generation: A scientist's recollections (1937-1955) from the early Binary Relay Digital Computers at Bell Telephone Laboratory and OSRD to a fledgling Minicomputer at the Barber Coleman Company (unpublished MS, 1993), 54. Courtesy Paul Ceruzzi.

engineering in this century, periodically retold as an inspiration for engineers.[3] One current textbook on control engineering prints the story of Black's vision verbatim in the first chapter.[4]

This canonical version of Black's invention follows typical engineering ontology — a cleaner, more intellectual lineage than the military and industrial tales of the previous two chapters. The inventor brings forth a seminal idea with far reaching implications. The engineering community recognizes its value, expands on the idea, and formalizes it into a set of design rules. In the feedback story Harry Nyquist and Hendrik Bode add the formalism, producing a set of stability criteria and design methods. Then practicing engineers make it their own, using the rules in routine design and as building blocks for larger systems. At Bell Laboratories from 1927 to 1940, the legend goes, Black, Nyquist, and Bode laid the foundations of control theory, which engineers then applied to all types of closed-loop systems from servomechanisms to thermostats, fire control systems to automatic computers.[5] More than Sperry or BuOrd's control systems, this story of feedback earned a place in engineering legend and college textbooks. It produced design methods and graphical techniques which carried their author's names (the Bode plot, the Nyquist diagram) and earned telephone engineering a claim to priority in feedback history.

But like the later Cybernetics, feedback's origin myth effaces its sources. It reveals little about the concrete problems these men worked on when they produced their solutions. It skips over the relations between the men, and how their backgrounds and prior experience influenced their work on feedback. The story also removes feedback theory from the landscape of telephone engineering between the world wars. Nor does it account for the relationship of the feedback amplifier to the long tradition of governors and self-regulating machinery. How did feedback

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[3] For other accounts of Black's invention, see Hendrik Bode, "Feedback: The History of an Idea," Proceedings of the Symposium on Active Networks and Feedback Systems, Polytechnic Institute of Brooklyn (Polytechnic Press, 1960) reprinted in Richard Bellman, ed., Selected Papers on Mathematical Trends in Control Theory (New York: Dover, 1964). M.J. Kelley, "Career of the 1957 Lamme Medalist Harold S. Black," *Electrical Engineering 77* (August, 1958), 720-22. Prescott C. Mabon, Mission Communications: The Story of Bell Laboratories (Murray Hill, New Jersey: Bell Telephone Laboratories, 1975), 39-40. Of historian's accounts, most thorough is Stuart Bennett, A History of Control Engineering 1930-1955," (London: Peter Peregrinus, 1993) Chapter 3, "The Electronic Negative Feedback Amplifier." Also see E.F. O'Neill, ed. A History of Science and Engineering in the Bell System: Transmission Technology (1925-1975) (Murray Hill, New Jersey: AT&T Bell Laboratories, 1985) Chapter 4, "Negative Feedback." Ronald Kline, "Harold Black and the Negative-Feedback Amplifier," IEEE Control Systems, August, 1993, 82-85. Also see a short film, "Communications Milestone: Negative Feedback," (Bell Telephone Laboratories, 1977).

[4] Richard C. Dorf, Modern Control Systems (Reading, Mass.: Addison-Wesley), Fifth Edition, 1995.

[5] Hendrik W. Bode, Synergy: Technical Integration and Technological Innovation in the Bell System (Murray Hill, New Jersey: Bell Telephone Laboratories, 1971), 138-40

theory reconfigure the governor and its functions of perception, integration, and articulation? We can begin to answer this question by connecting Black's heroic tale to longer trends in electrical engineering and to the immediate context of the telephone network. Telephone engineers had their own version of the governor. It coupled human beings to the system. It connected different locales with transmission media. It listened and it spoke.

Thus a reexamination of the sources is in order, one which reveals a history at once less simple and more interesting. On the ferry, Black did not understand as much about feedback and stability as he later recalled. To make his idea credible, Black needed Nyquist's solution to the thorny problem of stability. And Bode, bringing the subtle analytics of network theory to bear on the feedback problem, actually sought to limit feedback, outlining the tight constraints which a stable feedback amplifier must meet. Black, Nyquist, and Bode all worked on a network which strove to extend its reach, to expand its capacity, and to translate ever more of the world into transmissible messages. This translation required ever closer coupling of human and mechanical elements through the medium of sound, a coupling which left a discernible mark on feedback theory.

Rethinking the work of Black, Nyquist, and Bode in this way clarifies the history of negative feedback and elicits how researchers at Bell Labs conceptualized systems, stability, and human operators in the years prior to World War II. Toward these goals, this chapter asks the following questions: What was the engineering culture at Bell Labs in the late 1920s? What was the historical and technical background of the organization? Who worked there? What difficult problems did they face? What were their new technologies? Relating feedback control in telephony to fire control in the navy and to Sperry Gyroscope raises further questions: What was the system? What was stability? How did the system connect to its human operators? How did the feedback theory of Black, Bode, and Nyquist map onto the governor's perception, integration, and articulation?

Negative feedback grew in the context of extending the telephone network across the continent, increasing the network's carrying capacity, and making it work predictably in the face of changes in season, weather, and landscape — the context, that is, of building a large technical system and operating it over a diverse and extended geography. Engineers' increasing facility with creating, manipulating and switching signals in that system prompted them to reconceptualize the network. They began to understand electric waves as abstract signals and analyzed those signals as sums of frequencies. Then the Bell System became not merely a conduit for voice conversations but a generalized system, capable of carrying any signal as a new currency: information.

Technical and Historical Background: Network Geography

The Bell System was an engineer's dream: geographically expansive, reaching into all types of difficult terrain and climates, and yet always in control, tied to the central office, never released to survive autonomously in the terrifying world. The phone system reached interregional and national scale in 1910; by comparison the other large system, electric power, dealt with numerous smaller systems and did not become comparably interconnected until the late 1920s. Still, in the first decade of the century American Telegraph and Telephone company did not yet have its familiar hegemony, it controlled only about half the telephones in the country. Long-distance was the key to expanding that share, as competing local operators could not offer the service. The Bell System thus followed its own frontier on a western expansion — often literally along the tracks of the railroads (and, of course, the telegraph). But beyond the Rocky Mountains the problem became extraordinarily difficult. Transmission over such distances posed a critical problem for AT&T engineers; it required adding energy to the network.[6] AT&T's chief engineer, J.J. Carty, translated corporate goals into geographical expansion, and transmission was the key to that translation. From the turn of the century until the 1930s, AT&T expressed its milestones in geographical terms: the New York/Chicago line stood for carrier frequency transmission; the New York/San Francisco transcontinental line stood for vacuum tube repeaters; the Morristown trial simulated the entire country and represented the negative feedback amplifier.

Defining the Signal: Time and Frequency

At the turn of the century the telephone network remained a passive device, as it had been since the time of Alexander Graham Bell. Carbon microphones added energy from a battery to the weak acoustic signal from a speaker's voice, but once the wave entered the line it traveled to the

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[6] For a general history of the Bell System, see Brooks, The First One Hundred Years. Thomas Shaw, "The Conquest of Distance by Wire Telephony," *BSTJ* 23 (no. 4, October, 1944). Leonard Reich, "Industrial Research and the Pursuit of Corporate Security: The Early Years of Bell Labs," *Business History Review* 54 (Winter, 1980), 511. See also Leonard Reich, The Making of American Industrial Research: Science and Business at GE and Bell, 1876-1926 (New York: Cambridge University Press, 1985, Chapters 7-8.

receiver without further gain. In fact, resistance in the wire imposed considerable losses, as did capacitance and inductance, whose loss varied with frequency and hence introduced distortion. Increasing the thickness of the wire could reduce this attenuation, but the additional copper proved heavy and expensive. Because it increased with cross-sectional area, cost went up with the square of distance. Around the turn of the century, then, the telephone network ran up against the limits of transmission, both in extension, where it determined the furthest distance a signal could travel, and in economy, where it determined the cost of more moderate distances.

Weather stressed the problem further. The standard type of transmission, even for long distances, was "open wire," which literally meant each circuit had its own separate wire, separate from the others by a few inches [*Figure 4-1, Open Wire System]. This separation wires minimized "crosstalk," where one conversation leaked to an adjacent wire, and also kept losses to a minimum. Telephone poles with tens of wires characterized this technology, similar in appearance to the telegraph cables that ran along railroad tracks. But these lines were particularly vulnerable to snow and ice storms, in addition to cluttering the urban landscape. Cables, an alternative to open wire, bundled numerous smaller wires together and could be buried, so they were more immune to weather and and cheaper to install. [*Figure 4-2, Cable Route] But because of the small diameter of the wires and their tight packing, cables had higher losses than open wire, easily 20-30 times more signal attenuation, and further pressed the limits of transmission.

Solving the transmission problem required rethinking the telephone signal, transcending a direct-current model based on the telegraph. In the late 19th century, Enlglishman Oliver Heavyside argued for seeing telephone signals not simply in terms of Ohm's Law of voltages and currents, but as electric waves traveling down the line. Heavyside observed that, over certain frequencies and distances, increasing inductance could actually reduce the attenuation of the wire. Thus, adding passive inductors placed at discrete intervals along the wire, "loading coils," decreased attenuation by a factor of three or four, and thus increased transmission distance proportionally. Michael Pupin of Columbia University and George Campbell of Western Electric, working simultaneously, made the loading coil a practicable electrical device.[7] It began

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[7] James E. Brittain, "The Introduction of the Loading Coil: George A. Campbell and Michael I. Pupin," *Technology and Culture* 11 (no. 1, January, 1970), 36-57. See also discussion of Britttain's Paper by Lloyd Espenschied, Joseph Gray Jackson, and John G. Brainerd, *Technology and Culture* 11 (no. 4, October 1970), 596-

commercial installation in 1904, and rapidly proliferated through the network, especially on cabled routes. Spacing of the loading coils illustrates the difference between transmission media: open wire required loading coils every eight miles, whereas cables required them every mile.[8] Still the loading coil remained passive — it facilitated the propagation of the wave down the line but added no additional energy.

Heavyside's contribution, however, went beyond spurring this important invention. His "operational calculus" reduced the solution of complex differential equations to simpler algebraic manipulation. He introduced a "step function" (which still bears his name) to analyze a circuit, network, or system in terms of its response to a sudden shock. This "indicial admittance," — how a system received the shock ("impulse response" in today's terminology), determined the response of a system to any arbitrary input. The technique was analogous to hitting the system with a hammer, and watching vibrations as they died out. John Carson at AT&T showed that the shape, frequency, and decay of the vibrations provided sufficient information to calculate the response of the system to any input. Heavyside's work was formalized, simplified, and applied to practical problems by Carson and Vannevar Bush at MIT, among others.[9]

This "transient response" approach described short, instantaneous events. It found wide application in telephony, since a voice signal, semi-random in character, could be seen as a long succession of these events. In contrast, "steady state" methods described systems in their long-term, stable conditions. Much of Steinmetz's work on power systems, for example, used complex algebra to describe steady-state phenomena of alternating current.[10] Early in this century, however, engineers (including Steinmetz) became increasingly comfortable with describing electricity from both points of view. The translation between transient, time domain and steady-

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603. Neal Wasserman, From Invention to Innovation: Long-Distance Telephone Transmission at the Turn of the Century (Baltimore: The Johns Hopkins University Press, 1985).

[8] Fagan ed., *The Early Years*, 241-252.

[9] Carson gave a series of lectures on Heavyside's operational calculus at the Moore School of Engineering at the University of Pennsylvania in the spring of 1925, which he also published in the *BSTJ* in 1925-26. These were compiled in a book, Electric Circuit Theory and the Operational Calculus (New York: McGraw Hill, 1926). See also Vannevar Bush (with an appendix by Norbert Wiener) Operational Circuit Analysis (New York: John Wiley & Sons, 1929). For background on Heaviside, see Stuart Bennett, A History of Control Engineering 1800-1930 (London: Peter Peregrinus, Ltd., 1979), 195-200. Paul J. Nahin, Oliver Heaviside: Sage in Solitude (New York: IEEE Press, 1988). Ido Yavetz, From Obscurity to Enigma: The Work of Oliver Heaviside, 1872-1889 (Basel: Birkhauser Verlag, 1995).

state, frequency domain representations was greatly aided by Fourier methods (Fourier series, the Fourier integral, and the Fourier transform) which expressed signals as sums of sine waves.

Engineers now saw telephone signals simultaneously as transient and steady-state phenomena and described and manipulated them in both the time and frequency domains. Modulating a signal, for example, shifted it up or down the spectrum; a radio transmitter modulates a signal from voice frequencies up to radio frequencies for transmission and then the receiver modulates it back down to audio. Another frequency manipulation technique, the the electric wave filter (also invented by George Campbell), selects a particular set of frequencies and excludes others. In a trend complimentary to that in communications, power engineers used transient analysis to understand their systems during rapid changes, when steady-state analysis were inadequate (see Chapter 5). Fourier analysis and operational calculus formed the intellectual tools for attacking the transmission problem, and the backdrop against which feedback theory developed at Bell Labs. These techniques allowed engineers to manipulate signals on paper; and embodied in modulators and filters they manipulated electric waves in the circuits themselves.

Telephone Repeaters: Linking Geography, Technology, and Corporate Goals

These analytic trends were supported by organization and policy. John J. Carty, chief engineer of the Bell System in 1907, had a clear vision of the social role of the telephone network, as "society's nervous system." He and his engineers vigorously pursued the goals of AT&T President Theodore Vail's famous motto, "One policy, one system, and universal service." Carty strongly supported science within the company, the modern vision of industrial research. Corporate research translated corporate goals into technical problems to be solved in the laboratory — in AT&T's case as much for protection against competition as for advancement. [11]

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[10] Late in his career, Steinmetz did work on transient phenomena, and made important contributions to the understanding of transients in electric power systems. See Ronald Kline, Steinmetz: Engineer and Socialist (Baltimore: Johns Hopkins University Press, 1992), 138-49.

[11] M.D. Fagan, ed. A History of Engineering and Science in the Bell System: The Early Years (1875-1925) (Murray Hill, N.J.: Bell Telephone Laboratories, 1975), 32-35,44. Ironically, Carty closed Western Electric's Boston engineering department which had been investigating Lee deForest's audion for use as an amplifier. Hugh Aitken argues that Vail's closing of the lab may have cost the company several years toward making a practicable telephone amplifier. A proposed contract with Reginald Fessenden for radio technology also became a casualty of Vail's consolidation. "What slipped through the Telephone Company's fingers, in short, was a unique opportunity to come to grips with electronic technology," Aitken argues, countering other historians (Hoddeson and Reich) who see the move to a single department in New York as progressive toward industrial research. See Hugh Aitken, The Continuous Wave: Technology and American Radio 1900-1932 (Princeton: Princeton University Press, 1985), 75-78. Lillan Hoddeson, "The Emergence of Basic Research in the Bell Telephone System, 1875-1915,"

One of Carty's long-time associates recalled him as a system-builder in the Hughesian sense: "He recognized the interrelationship in the telephone business of operating methods, design of the plant, and the rate structure...He had in mind that all of these factors must be considered in relations to one another."[12] On all of these facets, Carty believed, science could be brought to bear.

And science he needed. By 1911, the state of the transmission art had hit its practical limit: "loaded lines" reached the 2,100 miles between New York and Denver. But the frequency-dependent nature of attenuation so mangled voice transmitted over that distance it was barely understandable. Still, the Bell System sought to further extend its reach, all the way across the United States with a transcontinental line. In 1909 AT&T's technical management initiated a project to extend the Denver line to California.

This geographical problem had a technical core. Bridging the distances required an amplifier or "repeater," an active device which added energy to the signal, as opposed to the passive loading coils, which merely stemmed its decay. Developing a repeater had a strategic dimension as well: the rapid rise of new wireless communications seemed a threat to wired communication, and repeaters would give the company the opportunity to control radio technology, which required similar types of amplifiers. Toward these goals, in 1911Carty organized a special Research Branch of the Western Electric Engineering Department, with E. H. Colpitts as its head.[13] The two men shared the belief that long-distance transmission posed the most challenging, and rewarding technical problem. Thus Colpitts and the Research Branch sought a repeater which would renew the signal periodically along the line, to counter the energy dissipated by the resistance of the wire.

Technology and Culture 22 (1981), 530 notes that the term "fundamental research" began to appear in the Company's rhetoric about 1907. Horace Coon, American Tel and Tel: The Story of a Great Monopoly (New York: Longmans, Green and Co., 1939), 197 also makes the point about fundamental research. See also Reich, "Industrial Research" and The Making of American Industrial Research for the defensive stance of early industrial research.

[12] Bancroft Gherardi, "The Dean of Telephone Engineers," *Bell Laboratories Record (BLR)* 9 (no. 1, September, 1930).

[13] Shaw, "The Conquest of Distance," reprints Carty's original proposal for the transcontinental line. Reich quotes Carty's understanding of the strategic importance of the repeater, "A successful telephone repeater...would not only react most favorably on our service where wires are used, but might put us in a position of control with respect to the art of wireless telephony, should it turn out to be a factor of importance." The organization chart of the AT&T/Western Electric Engineering departments in 1905, 1907, 1909, 1911, 1915, and 1925 is reprinted in Shaw, 400-406 and Fagan ed., The Early Years, 43-55.

Mechanical telephone repeaters, logical extensions of simple and common telegraph repeaters, had existed for some time. These devices coupled acoustic energy from a speaker into a microphone, amplified the signal and retransmitted it. This approach amounted to connecting two telephone circuits end-to-end, and numerous such devices were patented before 1900. More elegant solutions used the same principle but combined the elements into a single unit. Because of inertia, the mechanical coupling lagged the electrical signal and the output was not very linear with input, which meant that mechanical repeaters introduced significant distortion. No more than a few could be connected in series, and the delicate devices proved especially sensitive to temperature variations. The Bell System did employ mechanical amplifiers to a limited extent in the first decade of the century, and they briefly carried the transcontinental line. As a transitional technology, mechanical repeaters clarified the requirements for improved amplifiers. Bell engineers clearly needed a better solution, and they began thinking about new approaches.[14]

Where Colipitts and his lab carried out fundamental research on repeaters, Carty gave responsibility for the overall construction of the transcontinental line to a young physicist, Frank Baldwin Jewett, assistant chief engineer at Western Electric. Jewett came to the company in 1904 from a stint as an instructor in electrical engineering at MIT. He earned his doctorate in physics at the University of Chicago, where he worked under Albert A. Michelson and became friendly with Robert Millikan. In 1910 Jewett, when faced with the problem of making repeaters for the transcontinental line, realized that a solution, "in order to follow all of the minute modulations of the human voice, must be practically inertialess."[15] It might lay in the electron physics he had studied at Chicago, Jewett thought. At Jewett's request, Millikan sent several recent Ph.D.'s to AT&T, and they formed an important axis of the company's research for years to come: Howard D. Arnold, H. J. van der Bijl, H.W. Nichols, John Mills, Karl K. Darrow, Harvey Fletcher, and Merton J. Kelley. In his quest for the repeater, Jewett had enlisted Millikan, the electron, and the very discipline of physics in support of his industrial goals — now it remained for him to solidify

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[14] See Shaw, "The Conquest of Distance" 376-79 for a detailed discussion of mechanical and mercury arc repeaters. Also see Fagan ed., The Early Years, 241-256.

[15] Jewett to Millikan, quoted in Fagan ed., The Early Years, 258. Jewett and Millikan had boarded together at Chicago, in a group which also included Thorsetien Veblen and Harold Ickes. Jewett was the best man at Millikan's wedding. Robert A. Millikan, The Autobiography of Robert A. Millikan (New York: Prentice Hall, 1950), 52-3. Millikan recounts the story of Jewett's approach to him, 116-17. Millikan remained a consultant in long-distance telephony and his testimony helped settle the protracted suit between G.E. and AT&T over the vacuum tube, 120-122.

the alliance as equipment in the network. Frank Jewett's became intimately associated with long distance transmission; when he retired in 194[illegible], BTL published an "implicitly biographical" tribute, not a description of the man's life, but a detailed technical history of the transcontinental line.[16]

Harold D. Arnold came first from Chicago, and he joined Colipitts's new Research Branch. Arnold began to investigate Lee De Forest's audion, which the inventor demonstrated as an amplifier of radio waves. The telephone company purchased de Forest's patent rights. Arnold, with fellow Millikan disciple van der Bijl, analyzed electron behavior within audion tubes, characterized their behavior as circuit elements, and engineered them for mass, interchangeable production. By 1913, Arnold's "high vacuum thermionic tube," later known simply as the vacuum tube, could amplify signals in telephone repeaters.[17] The repeater enabled the transcontinental line, which opened on schedule at the San Francisco exhibition in 1915 with great fanfare. Alexander Graham Bell on the East Coast repeated his famous first conversation with Thomas Watson, now in California. The line consisted of 130,000 poles, more than ninety-nine percent on open wire (the few cables forded streams and rivers). It had loading coils every eight miles, and eight vacuum-tube repeaters amplifying the signal in both directions, working over copper wire .165 inches in diameter. Calling across country was far from routine; a three-minute call cost more than twenty dollars, and included only a third of the bandwidth of standard lines (and hence reduced quality).[18]

Repeater amplifiers on long distance lines, made the telephone network no longer a passive device. Now it actively added energy along the route. The network became a machine. The repeater, amplifying and renewing the signal as it lost strength, effectively decoupled the wave from its physical limits. Electricity now became merely a carrier and not a means of power,

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[16] Shaw, "The Conquest of Distance." Hoddeson, "The Emergence of Basic Research," 533. Van der Bijl, with a doctorate from Leipzig, came to Western Electric in 1913. Nichols, in 1914. Bruno Latour uses Jewett's appropriation of the electron as an example of "machines" as abstract apparatus for tying together interested groups, in Science in Action (Cambridge, Harvard University Press, 1987), 125-6.

[17] Shaw, "The Conquest of Distance," 375, 379-82. Hugh Aitken argues that Arnold simply had a fundamentally different vision of the audion's potential than did de Forest. "Arnold...saw in it...something its inventor did not see: the possibility of making it into a high-vacuum device, operating by pure electron emission," whereas de Forest saw it as a gas-discharge device. Still, in Aitken's view, the distance between telephony and wireless delayed the Bell system's adoption of the audion for a number of years. Hugh Aitken, The Continuous Wave, 244-5, and 546.

[18] E.H. Colpitts, "Dr. H. D. Arnold," *BLR* 6 (no. 6, June, 1928), 411-413. Gradually, more repeaters were added and the number of loading coils reduced; the coils reduced the bandwidth of transmission, and reduced the speed of signal propagation, which led to problems with echoes. Shaw, "The Conquest of Distance," 389-92 provides a

"useful only as a means of transmitting intelligible sounds while it would have no appreciable value purely from the power standpoint."[19] As part of this evolution, the product of the network became standardized. No longer did the system merely deliver conversations, now it delivered signals within a specific frequency range, at a specified volume, and with a specified amount of noise. This transformation required standard measures; the "mile of standard cable," was replaced by the "transmission unit," renamed the "bell," and eventually standardized in the "decibel," smaller by a factor of ten (still the standard measure of attenuation). Noise became a measurable quantity, and the limiting factor in quality.[20] The message was no longer the medium, now it was a *signal* — which could be understood and manipulated on its own terms, divorced from its physical embodiment.

The Establishment of Bell Laboratories

The success of the transcontinental line proved to Carty and AT&T the value of Jewett's alliance of physics, electronics, and telephone engineering. The transcontinental line so solidified the alliance technically that loading coils were gradually removed from the network.[21] But duplicating this success would require an organizational solidity as well. On January 1, 1925, then, the AT&T and Western Electric engineering departments combined to form the Bell Telephone Laboratories Incorporated (BTL). BTL was responsible to AT&T for fundamental research, and to Western Electric for the products of research, and the two companies funded the lab accordingly. The new lab, at 463 West Street in Manhattan, had 3,600 employees, including 2,000 scientists and engineers. Carty (now "General Carty" after his role in the World War) was the chairman of the board, which also included vice presidents of Western Electric and AT&T. Frank Jewett became President, and Harold Arnold Director of Research. While an important milestone for corporate research, it's easy to overestimate the importance of the foundation of the

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detailed technical description of the transcontinental line. The line was not permanent, but was "build up by switches" when needed, as was the New York/Denver line. Fagan ed., The Early Years, 263-4.

[19] H.H. Nance, O.B. Jacobs, "Transmission Features of Transcontinental Telephony," *J. AIEE* 45 (November, 1926), 1062.

[20] W. H. Martin, "Transmitted Frequency Range for Telephone Message Circuits," *BSTJ* 9 (July, 1930) 483-6. W.H. Martin, "The Transmission Unit and Telephone Transmission Reference Systems," *BSTJ* 3 (no.3, July, 1924), 400-408. R.V.L. Hartley, "TU Becomes 'Decibel,'" *BLR* 7 (no.4, December, 1928) 137-9. J.B. Johnson, "Thermal Agitation of Electricity in Conductors," and H. Nyquist, "Thermal Agitation of Electric Charge in Conductors," *Phys. Rev.* 32 (July, 1928), 97-113.

laboratory itself. The new organization mostly resembled the old Western Electric engineering department, with only moderate changes.[22] Research conducted at Western Electric carried on largely unaltered, as did the careers of the engineers. Until 1934, BTL did not include the Development and Research (D&R) department of AT&T, which had 1,100 engineers and scientists.

The Technical Agenda

The completion of the transcontinental line overcame the major distance hurdle in North America, and a major technical goal of the Bell engineers. Now they aimed to bring down the cost of the connections. That meant distributing the capital of the line over several channels, and Bell engineers turned their attention toward improving the capacity of a wire, putting more conversations onto a single line. The most promising method, "carrier multiplex," modulated several voice signals onto higher frequency "carrier" frequencies. If these modulations occur in distinct frequency bands, they may all be sent over the same line, much in the same way separate radio stations occupy the electromagnetic spectrum. At the receiving end, a wave filter, invented by George Campbell, separates out the voice channels. [*Figure 4-3, Carrier modulation] The idea had been around for a long time; and both Elisha Gray and Alexander Graham Bell had investigated carrier techniques in their telephone research.[23] But with the introduction of the vacuum tube, carrier telephony became practicable (it became known as "wired wireless") as did continuous wave wireless transmission.[24] The first commercial carrier system, type "A" was

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[21] The transcontinental line was fully unloaded in 1920, more than tripling the velocity of transmission, which reduced echo effects and improved the "sense of nearness" of the speakers. Shaw, "The Conquest of Distance," 396.

[22] In 1926 BTL opened a laboratory in Whippany New Jersey for radio research. In 1941, it moved most of its operations to a new campus in Murray Hill, New Jersey. Fagan, ed., The Early Years, 54-55 compares BTL with the old AT&T and W.E. engineering organizations. Also see the organization charts in Shaw, "The Conquest of Distance," 406 for its similiarity to the initial BTL organization outlined below.

[23] E.H. Colpitts and O.B. Blackwell, "Carrier Current Telephony and Telegraphy," *J. AIEE 40 (*April, 1921) 301-315 has a detailed history of carrier methods in telephony, as well as an elegant explanation of carrier modulation and transmission.

[24] John Stone Stone, "The Practical Aspects of the Propagation of High Frequency Electric Waves Along Wires," *Jour. Franklin Inst.* 174 (no. 4, October, 1912) described high-frequency multiplex telephony as "identical with that of the new continuous wave train" radio, and included the Alexanderson alternator as an element a telephone design. Also see Lloyd Espenschied, "Application of Radio to Wire Transmission Engineering," *BSTJ* 1 (no.2, October, 1922) 117-141. For "wired wireless," see Fagan ed., The Early Years, 282.

installed in 1918, putting four two-way channels on open-wire pairs.[25] [*Figure 4-4, carrier type A] [*Figure 4-5, Carrier on open wire]

Another means of increasing capacity, cables, carried ten times as many circuits as open wire lines. But cables were still a difficult and demanding technology, installed for interregional distances like Washington to Boston (which opened in 1913), but difficult for continental spans. Cables had such high attenuation that they required repeaters every five miles, whereas open wire lines required them every 250. In October 1925, a cable opened between New York and Chicago, but only with difficult and precise construction pushing the limits of the medium. The success came at a massive cost in equipment and material, requiring an expensive, large diameter cable and extensive loading and repeater equipment.[26] Making long cables practicable and economical required many repeaters along the same route, and massive manpower (distributed along the route) to maintain the delicate devices. But such a serial connection needed amplifiers of an extraordinarily high quality, otherwise distortion would accumulate from repeater to repeater, rendering the voice signal unintelligible after only several stages. These problems, of carrier transmission, cable attenuation, and high-quality amplifiers defined the culture of Bell Labs at its founding.

Bell Labs' Organization and Engineering Culture

Bell Labs' publications describe the technical problems and reveal how they shaped the organizational culture. Beginning in 1922, The *Bell System Technical Journal* (*BSTJ*) published scientific and technical work by researchers in the Bell System, including Bell Labs and Western Electric (its editorial board included Carty, Jewett, and Colipitts). Industrial research derived a certain credibility by publishing results in the manner of a university, and also provided a quasi-academic outlet for the many scientists recruited away from university careers. Unlike most scientific journals, however, *BTSJ* represents a cross section of work at a single institution, because it contains articles almost exclusively by Bell System authors. These articles often appeared in mainstream engineering journals simultaneously with their appearance in *BSTJ*, but compiled in one place they paint a detailed picture of the interdisciplinary engineering surrounding

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[25] In 1924 the "C" system went into service, incorporating lessons from the more experimental A and B systems. C carrier systems were so successful the last one was not removed from service until 1980. O'Neill, ed. Transmission Technology, Chapter 1, "The State of the Technology (1925-1930)," 3-14.

[26] H.P. Charlesworth, "General Engineering Problems of the Bell System," *BSTJ* 4 (October, 1925), 515-41.

the system. A typical issue might include a primer on electron physics, a discussion of circuit theory, an analysis of speech signals, and a statistical study of quality control in manufacturing.[27]

Less technical and more focused on Bell Labs itself, the monthly magazine *Bell Laboratories Record*, a typical "company organ" of the time, began publication in September, 1925, nine months after BTL's founding. The *Record* had news about employee activities, clubs, awards, and retirement, in addition to technical articles aimed at the educated lay reader, describing research at BTL with an emphasis on engineering practice.[28] The first several issues contained articles describing each department in turn, its key executives and researchers, as well as their educational and professional backgrounds. Together, the *Bell System Technical Journal* and the *Bell Laboratories Record* provide a technical, personal, and organizational view of communications research reflective of its comprehensive, scientific approach to the problems of "The System."

Bell Telephone Laboratories divided into six departments: Systems Development, Research, Apparatus, Inspection Engineering, Commercial Development, and Patents. Inspection Engineering set and maintained quality standards, both of equipment itself and of the product, the voice signal. For example, the group developed statistical sampling techniques for measuring mass production lots of equipment with only minimal interference.[29] A young engineer in this group, Donald A. Quarles, would become secretary of the Air Force and Assistant Secretary of Defense during the Cold War.

Only the Research Department performed "basic" industrial research in the classical sense. Headed by Harold Arnold and comprising five hundred people, its mission was "to find and formulate broadly the laws of nature, and to be concerned with apparatus only insofar as it serves to determine these laws or to illustrate their application in the service of the Bell System." Research covered nine main areas: speech, hearing, conversion of energy between acoustic and electric systems (i.e. speakers and microphones), electric transmission of intelligence, magnetism,

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[27] For the *BSTJ*'s mission, see "Foreward," to the first issue, *BSTJ* 1 (no.1, July, 1922). The remainder of the editorial board was, E.B. Craft, H. P. Charlesworth, B. Gherardi, L.F. Morehouse, O.B. Blackwell, and R.W. King (editor).

[28] Company organs of the time typically had several audiences, including employees, prospective customers, and the engineering profession, under the guise of technical journals. See David Nye's discussion of *General Electric Review*, a similar publication during the same period, in Image Worlds: Corporate Identities at General Electric (1890-1930) (Cambridge: MIT Press, 1985), 62-64.

[29] Franis J. Hallenbeck, "The Inspection Engineering Department," *BLR* 2 (no. 6, August 1926), 243-7.

electronic physics, electromagnetic radiation, optics, and chemistry. The Research Department also included a vacuum tube group under Mervin J. Kelley which not only studied tube design and fabrication, but actually manufactured the tubes for the Bell system (in 1925, tube manufacture remained a delicate, manual affair).[30] Still, within the Bell System, the Research Department did not have a monopoly on fundamental exploration, because the Development and Research (D&R) department of AT&T, with a similar charter, remained separate from BTL for the labs' first ten years.

The Apparatus Department served "to bring to commercial completion certain of the studies of the Research Group." Under the direction of John J. Lyng and including seven hundred employees, Apparatus also designed new equipment not covered by the systems group, improved existing equipment and reduced its cost, and compared Bell System equipment with that of other manufactures. It maintained measurement standards for "fundamental electrical quantities, such as inductance, capacitance, and frequency." It also designed radio equipment for telephone transmission over impassable terrain, talking movies, television, train dispatching and power station control systems, and public address systems. The Appratus Department also included a "General Development Laboratory," which provided engineering services to BTL overall.[31]

Where apparatus focused on laboratories and equipment, the Systems Development Department had a broader view of the network. It served as a liaison with the operating companies, determining their needs and translating them into engineering requirements. The eight hundred people of Systems Development also studied the growth of the system as a whole, projected future needs and spawned research or development programs accordingly. They designed the actual telephone circuits for the network, including equipment structures, office layouts, and the electric power systems required to run the equipment.[32] While this group had the widest scope of the engineering departments, its vision of the system remained concrete — the actual groups of wires and switches which made up the network.

One other group served as a consultant to the rest of the laboratory, The Mathematical Research Department, under the direction of Thornton C. Fry. Fry, who specialized in applying

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[30] Paul B. Findley, "The Research Department," *BLR* 2 (no. 4 June, 1926), 164-70. Mervin J. Kelley, "The Manufacture of Vacuum Tubes," *BLR* 2 (no. 4, June 1926), 137-144.

[31] J.J. Lyng, "The Development of Apparatus," Paul B. Findley, "The Apparatus Development Department," *BLR* 2 (no. 3, May 1926), 113-120.

probability to engineering problems, came to the phone company from a teaching post at the University of Wisconsin and a year at MIT. He maintained a staff of mathematicians and “calculators,” women who performed numerical tasks required by the projects. While Fry’s group did not provide “computing” services to the departments, it did maintain a set of mechanical calculating instruments for use in its own work. These included a Millionaire, a mechanical multiplier, a Coradi Integraph, for finding the area under curves, and eventually an “Isograph,” which Fry designed for finding the roots of equations. Fry analyzed, for example, the glass-to-metal seal of the vacuum tube, and developed an equation for describing it which designers then widely employed.[33] George Stibitz, a recent Ph.D. in mathematical physics from Cornell, joined this group in 1930; he built mechanical calculators for the department and would lead BTL’s work in digital computers during World War II.

Over time, the department undertook its own mathematical investigations, but originally Mathematical Research furnished only “expert advice,” to other investigators. Fry strongly believed in the role of mathematicians in industry, not as quasi-engineers but as liaisons between science and industrial research, “to give council and assistance...to translate the abstract language of science into terms more suitable for scientific explanation.” “The mathematician in industry,” Fry wrote in 1941, “is a consultant, not a project man.”[34] As we shall see in later chapters, Fry played a central role in applying Bell Labs experience and knowledge to military problems in World War II. In structure and responsibility, the Applied Mathematics Panel of the NDRC, which Fry joined under Warren Weaver, strongly resembled this group at BTL.

It would be inaccurate to characterize all of BTL’s work as “industrial research.” in that it addressed fundamental scientific problems which might be of use in the phone systems. Most of BTL engaged in the routine, if creative work of designing telephone equipment and making it work. Only one section, the actual Research Department, performed exclusively “fundamental” or “exploratory” investigations. Furthermore, despite the system-oriented organization of BTL, its members did not do “system engineering” in any modern sense. The System Development Department did not formulate the most abstract vision of the system overall, but in fact had the most concrete job: planning wiring, power supply, and equipment layouts. No one at Bell Labs

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[32] Paul B. Findley, “The Systems Development Department,” *BLR* 2 (no. 2, April 1926), 69-73.

[33] “Mathematical Research,” *BLR* 1 (no. 1, September 1925). Riech, *Industrial Research, 213.*

[34] Thornton C. Fry, “Industrial Mathematics,” *BSTJ* 20 (no. 3, July 1941), 258.

specifically addressed the system as an abstract entity — all focused on particular pieces of the overall problem, with no systematic integration of all the activities. The work at BTL overall, however, did represent "systematized research," in Director of Research Arnold's words, as a concerted attack on a related set of problems.[35]

The New Problems Facing BTL

Among the chief problems facing BTL, long distance transmission continued to play a critical role. After the New York to San Francisco line in 1915, there wasn't much further to go with wires (trans-oceanic telephony was a radio problem). But it was one thing to span the continent and quite another to offer high-capacity economical service of that distance. Increasing the capacity over existing long-distance routes thus began to drive technical development. Just meeting demands for growth proved a constant problem, about 800,000 new lines were added in 1925 alone. This task required planning and forecasting future requirements based on the rate of growth, and detailed cost analysis to determine when new cables were required.[36] Engineering studies considered a series of tradeoffs between the diameter of the wire, the number of repeaters, the cost of the terminal equipment, and the number of available channels.

What was the state of transmission technology when Bell Laboratories was established in 1925? The system had matured, but it had yet to employ several of the new technologies. Vacuum tubes had proliferated, with a total of 7,500 tube repeaters in the nearly three-million miles of circuits. Still, sixty percent of the system still operated by voice-frequency on open-wire, and thirty-nine percent by voice frequency on cables. Under two percent utilized the new carrier methods on open wire; virtually none used carrier on cables.[37] Both carrier and cable, and especially the two in combination, still posed difficult challenges.

Carrier transmission, because of its higher frequencies, suffered greater attenuation than voice band signals. Thus carrier on open wire required more repeaters than voice-band signals, more still on cables. The original transcontinental line, a voice-band system, required fewer than ten repeaters across the continent, (which gradually increased to twenty). But to make this transmission economical, a carrier system was required, and forty repeaters. An equivalent

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[35] H. D. Arnold, "Systematized Research," *BLR* 6 (no. 4, June, 1928), 316-17.
[36] Charlesworth, "General Engineering Problems."
[37] O'Neill, ed., Transmission Technology, 11.

transcontinental cable would employ two hundred.[38] Herein lay the problem: both carrier modulation and long strings of repeaters required amplifiers with extremely pure characteristics, otherwise unacceptable distortion would accumulate along the line; amplifiers had to be highly linear.

An ideal amplifier is a pure multiplier, producing as output a simple multiple of its input. This means the amplifier must have a linear relationship between input and output — literally a straight line whose slope is the multiplication ratio or the *gain*. But the output vs. input curve of a vacuum tube tends to be more s-shaped. [*Figure 4-6, Vacuum Tube Non-linearity]. This non-linearity introduced harmonic distortion, which caused two problems. First, with a modulated signal the non-linearity produced extraneous harmonics outside of the desired signal band. This becomes a problem when one starts to add several signals on the same wire, with closely spaced spectra. The harmonics from one signal bleed over into the band of other signals, causing "crosstalk"— where one conversation bleeds through into another. [*Figure 4-7, intermodulation products] Second, distortion gets progressively worse when passed through several non-linear amplifiers, as it did on a long line with several repeaters. The signal itself is cumulatively distorted until the speech is hopelessly garbled after only a few stages. Thus, as the line became longer and longer, and as more and more signals were squeezed onto a single wire, the amplifiers had to become correspondingly higher in quality. This was a system problem *par-excellance*: the behavior of the individual components was determined by the expected performance of the system overall.

The Negative Feedback Amplifier

The Search for the Linear Amplifier

It was to this problem of linear amplifiers which Harold Black turned his energies when joined the Systems Engineering department of Western Electric in 1921. A Massachusetts native, he had graduated that year from Worcester Polytechnic Institute in electrical engineering. At that time, the new type "C" carrier systems, which had not yet entered service, were having problems with distortion and cross talk. The first approach, and the logical starting point, was to make the vacuum tubes themselves more linear —a common line of attack at the time. Toward this goal

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[38] See table in O'Neill, ed., Transmission Technology, 63.

Black worked with Mervin Kelley and the vacuum tube department, but with little success. Despite their utility as circuit elements, vacuum tubes remained complex, unruly — and non-linear — devices. "The problem lay with the unwanted frequencies generated by the vacuum tubes," Black recalled, "particularly the second-order harmonics and other [distortion] products that predominated."[39]

Black realized that the distortion and modulation products "contributed by a string of x amplifiers are virtually x times that contributed by a single amplifier." Thus in a string of 1000 amplifiers, each would have to be 1000 times better than one operating alone. Black plotted a chart of the linearity requirements and distortion effects of a string of amplifiers, where the number in the string varied from one to 3,000. This number was way beyond an expected requirements of the time; Black claimed that recognizing the possibilities of this radical increase in performance led him to consider fundamentally new approaches to the problem.

Black's version of the legend then invokes the great engineer of the time. In 1923 Black attended a lecture by Charles Stienmetz at an AIEE meeting in New York, less than a year before his death. Impressed by the simplicity and clarity of Steinmetz's presentation, Black rethought his own problem. He began to think not of making a highly-linear amplifier, but of removing distortion products from the output. He reconceptualized the output of the amplifier as containing a pure, wanted component, the signal, and an impure, unwanted component, the distortion. The problem, then, was to somehow separate the two, and keep only the pure signal. Black's invocation of Steinmetz may be apocryphal or irrelevant, but it links Black's thought process to the transformation in electrical engineering, both in power and communication, toward thinking about abstract signals, as opposed to concrete electricity (a transformation for which Steinmetz was a key intellect and icon).

Black came up with an arrangement which manipulated his signal by clear, if inelegant, means. [*Figure 4-8, Feedforward amplifier] The output of the amplifier was reduced in amplitude, and the original input signal subtracted from it, which produced in pure form the unwanted part of the output, the distortion. This distortion could then be amplified back up to the

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[39] This account is based on Harold S. Black, "Inventing the Negative Feedback Amplifier," *IEEE Spectrum* (December, 1977) and Harold S. Black to A.C. Dickieson, June 16, 1974, ATT. For a typical effort to design linear vacuum-tube amplifiers, see E.W. Kellogg, "Design of Non-Distorting Power Amplifiers," *Electrical Engineering* 44 (1925), 490.

level of the amplifier output, and subtracted from it, leaving only the pure signal at the output of the amplifier. Black built a laboratory prototype, which achieved the desired result, and applied for a patent in 1925.[40] Though this setup proved that a low-distortion amplifier was possible, it was far from practicable. It required two power amplifiers, for example, an inefficient application of energy. Furthermore, because of the many additions and subtractions, each amplifier and signal element had to be perfectly adjusted and balanced, else it would introduce *more* distortion. Black's new amplifier required hourly adjustment, which engineers could do in a testing lab but not in a complex system deployed in the field.

Black's Feedback Vision

For three years then, Black struggled with simplifying his solution. Finally, in 1927, he had the ephiphany on the ferry: if the gain of the amplifier were reduced by some amount, and that amount fed back into the input, the linearity could be vastly improved. In fact, distortion was reduced (i.e. linearity improved) by the same factor that the gain was reduced. A simple explanation of the idea appeared in a paper of Black's in 1934. [*Figure 4-9, Black's 1934 fb diagram] Black showed the gain of the amplifier depends only on the feedback network, b, and not on the gain mof the amplifier itself. This assumption holds to within 1/ m, so if the amplifier gain is 100, then 1 percent of the gain is determined by the vacuum tube, and 99 percent by the feedback network. Since the latter can contain only passive elements, such as resistors, capacitors, and inductors, it can be much more precise than vacuum tubes, and much more stable with respect to temperature and other changes over time. The higher the gain m, the it contributes to the final result. Even then, gains 10,000 or 100,000 were achievable, and Black's invention *reduces the distortion and non-linearity by that same amount.* Thus a feedback amplifier with a vacuum-tube gain of 100,000 has on the order of .001 percent of the distortion of an open loop amplifier. The price, of course, is to throw that gain away, and settle for an overall amplifier gain that's much lower, say one, two, or ten. On December 29, 1927, Black and BTL engineers succeeded in making a feedback amplifier whose distortion was reduced 100,000 to 1 (and whose gain was reduced accordingly).[41]

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[40] Harold S. Black, Patent no. 1,686,792, "Wave Translation System."

[41] Harold S. Black, "Inventing the Negative Feedback Amplifier."

Stabilizing Black's Box

Still, Black had no easy time convincing others at Bell labs of the utility of his idea. He recalled that Jewett supported him in his research, but that Arnold refused to accept a negative feedback amplifier and directed Black to design conventional amplifiers instead.[42] To the generation of engineers who had struggled to make the vacuum tube amplify at all, throwing away the hard-won gain seemed absurd. Furthermore, no one could understand how an amplifier's output could be fed back to its input without a progressive, divergent series of oscillations. They knew the difficulty of making a high-gain amplifier even without explicit feedback. Subtle, uncontrolled feedback would arise through, for example, stray capacitance between wires, or even between elements within the tube itself, and cause the amplifier to go into "parasitic oscillation" or "singing." Two BTL engineers, H.T. Friis and A.G. Jensen, studied this phenomena of "feed-back or regeneration" occurring through the tube, which "makes the total amplification vary irregularly in a very undesirable manner and also makes the set 'sing' at certain frequencies."[43] They sought to eliminate feedback as a means to good design, not to explicitly incorporate it as Black did; Black's work ran counter to the grain of the regenerative amplifier designers.

Black had similar difficulties with the U.S. Patent Office . His application for a "Wave Translation System," originally filed in 1928, was not granted until 1937. The British Patent Office treated it the same way the would a perpetual motion machine, and would not approve the invention without a functioning model, which Black submitted while the device was in engineering trial. Black insisted "the long delay resulted because of my refusal to the U.S. Patent Office reject a single claim," and that "the patent had to teach a new art: the negative feedback amplification principle."[44]

Black interpreted the resistance to his ideas as evidence of their radical nature. But as an engineer with a bachelor's degree the Systems Department, he did not possess the analytical sophistication, the communications skills, nor the prestige of the top research minds at BTL at the time. His lab assistant during this period, Alton C. Dikieson, recalled Black as in constant conflict with his own management, and with the rest of BTL. Dikieson's recollections of Black's troubles

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[42] Black, "Inventing the Negative Feedback Amplifier," 59-60.

[43] H.T. Friis and A.G. Jensen, "High Frequency Amplifiers," *BSTJ* April, 1924.

[44] Black to Dickieson, June 16, 1974. ATT. Harold S. Black, patent application 298,155, August 8, 1928. "File History of Black Application Serial No. 298, 155," ATT.

parallel the inventor's own accounts, so his memory seems credible.[45] Such conflicts were one thing for a lucid genius, but Black was far from self-explanatory. "A compulsive, non-stop talker...[Black] was inventive and intuitive, but not particularly clear at exposition." His negative feedback scheme was only the last in an series of attempts and ideas over a period of several years, all of which Dickieson wired up and built, but, as he recalled, "none of the schemes we tried showed any real promise." Dickieson also recalled "quite a bit of rivalry" between the circuit designers in the Research Department and Black, from the lower-status Systems Development department. "There seemed to be some feeling that *exploratory* development was the exclusive province of the research people. Mathematicians like Thornton Fry found Black's mathematics beneath contempt."[46] Black — restless, creative, and a bit arrogant — was traversing the established boundaries of the organization.

Credible as Dickieson's recollections seem, no contemporary accounts exist to support or refute them. The documents do allow, however, a thorough analysis of Black's ideas, and show how Black himself had to transform them (or enlist others to transform them) in order to win their acceptance. A key point surrounds his claim that the epiphany on the ferry included a concern for dynamic stability, that if he "kept the device from oscillating (singing, as we called it then)" it would work — he implies he understood "stability" of the amplifier as the central problem. But a look at Black's conception of stability at the time reveals it to be different from this standard meaning of "freedom from oscillation." In fact, Black's conceptions of both negative feedback and stability differed markedly from much of the engineering community at the time, although they would have been common to practicing telephone engineers.

Differing Conceptions of Feedback and Stability

The idea of *feed-back* had become current with the introduction of the "regenerative amplifier," a positive feedback device. Positive feedback, or "regeneration," in radio engineering increased the sensitivity of a receiving tube by sending a wave back through an amplifier many times. Today's common notion of "negative feedback" derives from the element of subtraction — the feedback signal subtracts from the input signal (as opposed to adding it in positive feedback). Put in terms of alternating currents, we say the signal shifts by 180°, to the negative counterpart

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[45] See, for example, "Inventing the Negative Feedback Amplifier," 59-60 for Black's conflict with H.D. Arnold, and intimations of consistent conflict with his superiors.

of the cycle, in the feedback network. Put in terms of the steam engine governor, we say the faster the balls spin around, the more they *retard* the motion of the engine. Black's earlier feedforward amplifier made explicit use of this subtraction, as the circuit amplified the distortion products and subtracted them from the output. In Black's time, however, even this specific-sounding term "negative feedback," had yet to settle on a definition. Black insisted it referred not to the subtraction, but to the fact that gain was reduced by the addition of feedback, as opposed to "positive feedback." In terms of the steam engine governor, this sense of "negative" means the energy required to spin the balls reduces the energy available to the engine — not a significant effect in this case. In their 1924 paper, Friis and Jensen, had made the distinction Black used between "positive feed-back" and "negative feed-back," not by the sign of the feedback itself, but rather by its effect on the amplifier's gain.[47] Serious misunderstanding did not arise over these differing notions of negative feedback, but they do demonstrate the confusion that existed over the most basic matters of definition.

Confusion and misunderstanding, however, did arise over the issue of stability. Dikieson recalled why those concerned with singing in amplifiers did not take Black seriously, "Harold did not even approach the question of stability — he simply assumed that it did not sing...[he] knew about oscillations and that the circuit would sing if the gain and phase around the loop were zero, but he did not have the mathematical tools to analyze the stability problems." Documentary evidence supports Dickieson's memory. Black's first published paper on the topic of feedback appeared in 1934, and its title, "Stabilized Feedback Amplifiers," reflects Black's central concern with stability. Discussing Black's paper in *Electrical Engineering,* BTL engineer Homer Dudley listed freedom from singing as one of the two most important problems of the amplifier. But to Black stability referred to long-term behavior of components in the telephone network, not to freedom from oscillation. Stability meant,

> When many amplifiers are worked in tandem...it becomes difficult to keep the overall circuit efficiency constant, variations in battery potentials and currents, small when considered individually, adding up to produce serious transmission changes in the overall circuit.[48]

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[46] A.C. Dickieson to M.J. Kelley, July 6, 1972. ATT 43 09 03. Emphasis added.

[47] Friis and Jensen, "High Frequency Amplifiers," 204.

[48] Harold S. Black, "Stabilized Feedback Amplifiers," *BSTJ* 13 (1934), a paper presented at the Winter Convention of the A.I.E.E., New York City, January, 1934, and also published in *Electrical Engineering* January, 1934. See

Temperature changes, aging of components, changes in the power supply, and any number of other factors could affect the characteristics of an amplifier. Life in the network exposed a telephone repeater to a harsh world, and Black sought to insulate the signal from the brutal reality. He wanted to use feedback to "stabilize" the characteristics of the amplifier over time. Rain and temperature, by changing the resistance and electrical properties of the wire, caused the attenuation to vary significantly, potentially by a factor of more than a hundred over the course of a single day, and comparably over the change of seasons (an aerial, open-wire cable might undergo half of its annual temperature change in a day).[49] [*Figure 4-10, Temperature variation in line resistance] These alterations could radically alter the physics of transmission, a potentially disastrous effect for a system already operating close to its physical limits. Black was not primarily concerned with the stability against oscillation that others saw as the key problem with a feedback system. *His original patent application, filed in 1928, makes no mention of even the possibility of "singing" or oscillation.*[50] He resubmitted the application in 1932, and added this clarification,

> Another difficulty in amplifier operation is instability, not used here as meaning the singing tendency, but rather signifying constancy of operation as an amplifier with changes in battery voltages, temperature, apparatus changes including changes in tubes, aging, and kindred causes...Applicant has discovered that the stability of operation of an amplifier can be greatly improved by the use of negative feedback.[51]

He acknowledges the other meaning of stability, but assigns it unequivocal second billing:

> Applicant uses negative feedback for a purpose quite different from that of the *prior art* which was to prevent self-oscillation or "singing." To make this clearer, applicant's invention is not concerned, except in a very secondary way...with the singing tendency of a circuit. Its primary response has no relation to the phenomena of self-oscillation.[52]
> [emphasis added]

In his explanation in the patent, Black "simply assumed" that the amplifier did not oscillate — due in part to his concerns with the daily, as opposed to the theoretical, behavior of the system. In fact, Black's conception of stability was in line with that of telephone engineers in the

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the discussions by F.A. Cowan (April, 1934) 590; by G. Ireland and H.W. Dudley (March, 1934) 461-2; and by Harry Nyquist (September, 1934), 1311-12.

[49] H. A. Affel, C. S. Demarest, and C. W. Green, "Carrier Systems on Long Distance Telephone Lines," *BSTJ 7* (July, 1928), 384. The third author of this paper was Harold Black's boss.

[50] Harold S. Black, patent application 298,155 August 8, 1928. "File History of Black Application Serial No. 298, 155" ATT.

[51] Harold S. Black, "Wave Translation System," Patent no. 2,102,671, page 2.

[52] Black, "Wave Translation System," 2.

Systems Development department (as opposed to those in Research Department). To them, "stable" amplifiers retained consistent performance in the face of varying conditions experienced by equipment in the telephone network. Consistency, regularity, and "stability" of the circuit elements themselves were critical to transmission systems operating near their physical limits. Black, then, employed this operational conception of stability in the analysis of his amplifier. He used the term like an engineer from the Systems Development who saw "the System" as a concrete, operational entity.

Despite their emphasis on transmission stability, systems engineers would also have been familiar with dynamic stability through the problem of "singing" — which they may or may not have called stability. The old mechanical repeaters had a natural resonance right in the middle of the voice band which caused them to sing audibly. Similarly, early repeater circuits, whether mechanical or electronic, would sing if the signal from one direction of transmission leaked into the other (a full repeater requires two amplifiers, one for each direction of speech). In response to these problems, telephone engineers filtered out the singing frequencies and imposed limitations on the amount of gain in each repeater. Carrier systems also tended to sing, either locally or through the transmission line. In 1921, for example, Colpitts and Blackwell wrote that singing in a carrier system could arise when the gain was greater than one and when there existed "sufficient unbalance," between the circuits.[53] The introduction of the now-familiar telephone handset in the late twenties depended on understanding and preventing "howling" or singing between the earpiece and mouthpiece. In 1926, Harvey Fletcher analyzed the howling telephone as a dynamic electrical system to understand the relationship between impedance, frequency, and the tendency to break into the oscillation.[54]

Stuart Bennett observes that at least some telephone engineers in the 1920s were aware of earlier work on the stability of motion, although they were unsure how to apply it to vacuum tube circuits.[55] In the late 19th century, E.J. Routh addressed "dynamic stability" which meant the

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[53] Colpitts and Blackwell, "Carrier Current Telephony and Telegraphy," 313.

[54] Harvey Fletcher, "The Theory of the Operation of the Howling Telephone with Experimental Confirmation," *BSTJ* 5 (no. 1, January, 1926), 27-49. Fletcher's paper does not employ the terms "stability" or "feedback," in its analysis, although it does analyze electro-acoustic circuits which greatly resemble canonical feedback systems. Shaw, "The Conquest of Distance," 382-3. For the problems of handset howling, see Fagan ed., The Early Years 146-50 and Gherardi and Jewett, "Telephone Communication of the United States," 9.

[55] Stuart Bennett A History of Control Engineering 1930-1955, 77. See also Ronald M. Foster, "A Reactance Theorem," *BSTJ* 3 (no. 2, April, 1924), 266.

absence of oscillatory behavior, and provided a tool for analysis, "Routh's stability criteria." So "stability" as resistance to oscillation would have been familiar to electrical engineers at Bell Labs, possibly even to Black. Those building on Black's work uncritically used stability to refer to oscillation or singing, and not the stability of transmission.

Multiple, overlapping conceptions of critical ideas, negative feedback, regulation, and stability, surrounded the introduction of Black's amplifier. These differing notions help explain why the Research Department would not have taken Black seriously. Feedback and stability meant different things, depending on whether one saw as a vulnerable system surrounded by a hostile geography (and atmosphere), or whether one studied a free, dynamic entity. Simply put, when Black "invented" the negative feedback amplifier, he invented a different machine from the one it eventually became (and the one he remembered). These clashing visions raise questions about the feedback amplifiers' relationship to earlier feedback mechanisms: Did Black's invention draw on the long tradition of regulators and governors that preceded it? Did Black understand his work in relationship to that tradition?

The Feedback Amplifier and Regulators and Governors

In his later memoirs, Black said he did. His patent, as issued, states negative feedback principle applies to more than electronic amplifiers, "the invention is applicable to any kind of wave transmission such as electrical, mechanical, or acoustical...the terms used have been generic systems." But the patent never specifies what those systems might be, and a steam-engine governor or a voltage regulator fits into the category "wave translation system," only with wide latitude. Black likely had in mind more directly analogous systems, such as the numerous electro-acoustic translations required in telephony. Neither the patent, nor any of Black's early writings, nor the writings of any of the BTL feedback theorists for at least ten years, mention regulators, governors, or any of the myriad devices we now understand as employing negative feedback.

Nonetheless, even transmission stability required self regulating mechanisms. To compensate for changing transmission characteristics of the wire, gains in the repeaters would require adjustment. A "pilot wire," with no telephone signal ran down the transmission line and looped back, carrying a dc-current connected to a meter which monitored line resistance. This value indicated on a dial, which a human operator would read and then adjust the amplifier parameters accordingly. Black's stability of transmission was a kind of automation; it relieved network maintenance personnel of adjusting delicate amplifiers.[56] About the same time, in the late 1920s BTL automated the process in another way as well. An "automatic regulator," located in about every fourth repeater station, adjusted the amplifier gain with a feedback loop driven by the resistance of the pilot wire. The New York/Chicago line in 1929 included six regulating stations among the twenty repeaters.[57] Still, no evidence suggests Systems Engineers understood these self-regulating lines as similar to traditional mechanical regulators.

Was the earlier tradition of regulators and governors even present in BTL's engineering culture? Yes. Research at Bell Labs did employ precision regulation, especially of the speed of electric motors for sound movies. The speed of a silent film could fluctuate fairly widely without being visible to the human eye, but a synchronized sound track required tighter control. Variations in speed introduced changes in pitch of the sound which became noticeable and annoying to the listener with less than a percent of fluctuation. Fluctuations in power supply, film mechanisms, or frequency of the AC power supply all would affect the speed of the motors, and hence of the film and sound. Furthermore, the television systems in development at BTL in the 1920s employed large mechanical disks to scan the picture (instead of the later electron beam). These needed regulation and synchronization, as the disks on the transmitter and receiver sets needed to align exactly.

BTL engineers around the time of Black's invention discussed governors for sound movies and television in the context of the tradition of feedback devices. In a series of papers published between 1927 and 1929, H.M. Stoller of the Apparatus Department analyzed governor design including speed of response, "hunting" of the regulator, and means of synchronizing several cameras to a single sound recorder. "It is a well-known property of all forms of governors that if they are adjusted to too great a sensitivity the speed instead of remaining constant will fluctuate up and down around a mean value," Stoller wrote. He acknowledged "the necessity for avoiding hunting or surging of the speed," and clearly understood his speed control as similar to governors and regulators: "This phenomenon is well known in the mechanical governor art and is described by Trinks in his book *Governors and the Governing of Prime Movers*."[58] The "hunting"

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[56] Ireland made this observation in his discussion of Black's paper in *Electrical Engineering*, March, 1934.

[57] E. D. Johnson, "Transmission Regulating System for Toll Cables," *BLR* 7 (no. 5, January, 1929), 183-87.

[58] Hugh M. Stoller, "Synchronization and Speed Control of Synchronized Sound Pictures," *BSTJ* 8 (January, 1929) 184-195. Also see H. M. Stoller and E. R. Morton, "Synchronization of Television," *BSTJ* 6 (October, 1927), 604-

Stoller mentioned paralleled "singing" in an amplifier. In another paper Stoller explicitly compared his regulator to a flyball governor, and included a drawing of the device in the *Bell Laboratories Record.* He used "stability" in the sense of dynamic stability, noting, "when the sensitivity of the governor is made too great...an unstable condition is brought about." He added damping to avoid this instability, to which he also referred as "oscillations." Stoller even used the term "feed back" for the electrical speed regulation in his own circuits.[59]

The presence of Stoller's work at Bell Labs and in BTL publications permits four conclusions about the engineering environment surrounding Black in the late 1920s. First, had Black looked for it, he would have found analysis and citation of traditional mechanical regulators in his own organization and its publications. Second, Stoller clearly had access to knowledge about these governors, and Black presumably would have had similar access. And third, the analogy between a mechanical regulator and an electronic one would not have been a great leap for Black, as Stoller made the connection clearly but without much fanfare. Finally, then, we may conclude that Black did not see his negative feedback amplifier as part of the tradition of governors, he did not see "stability" in the amplifier in the way people understood "stability" of mechanical motion, and he did not see "hunting" in a regulator as analogous to "singing" in an amplifier.

What was the Amplifier?

This critical look at Black's conception of his amplifier provides some perspective on the origin myth of the amplifier. Black's flash of insight, however much it enlightened him on the structure of negative feedback, did not give him an artifact he could sell. But it would be wrong to suggest that had Black would have found a more receptive audience for his invention had he realized the amplifier's stability was a key problem, that negative feedback worked similarly to regulation, that singing in an amplifier resembled hunting in a regulator. These judgments we can only make with hindsight. The important historical point must be made positively: to Black, what *was* the amplifier? It was a means of throwing away gain to achieve linearity in a vacuum tube. It

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15. and H. M. Stoller "Speed Control for the Sound-Picture System," *BLR* 7 (no. 3, November, 1928), 101-105. W. Trinks, Governors and the Governing of Prime Movers, (New York: Van Nostrand and Co., 1919).

[59] H. M. Stoller "Speed Control for the Sound-Picture System," *BLR* 7 (no. 3, November, 1928), 101-105. Stoller also published on voltage regulators, H.M. Stoller and J.R. power, "A precision Regulator for Alternating Voltage," *Trans. AIEE* 48 (1929), 808-811.

was a way of stabilizing the repeaters in the telephone system where wires were subject to variation and hazard. On these points he was always clear, consistent, and determined.

Black's 1934 paper, "Stabilized Feedback Amplifiers," presented the negative feedback amplifier to the world. Before then, Black did not have the resources, intellectual, organizational, and technical, to sell his amplifier, to make it into a black box, literally "Black's Box " So he needed to enroll allies, others who could help him in his cause. He attributed the delay from his 1927 insight to the 1934 paper to corporate secrecy, but that can account for at most five of the seven years. Black's paper, in fact, was not the first word from the phone company on the negative feedback amplifier; that one, which Black cited and discussed, had appeared two years before. It was the work of an ally, to whom Black had turned for help, but who remade the black box. Harry Nyquist rethought negative feedback by redefining stability.

Nyquist and Bode on Stability

Properly defined, Black's amplifier, with its ultra-low distortion and crosstalk, could find a place in the network. During the 1920s, cables carrying voice-band signals were expanding their role in the system and accounted for most of its increased capacity. The ability to add carrier circuits on cables, however, would multiply the capacity of the existing network, but carriers on cable required linear amplifiers with excellent stability. Harry Nyquist, a Swedish immigrant with a Ph.D. in physics from Yale, brought negative feedback from Black's curiosity into the network. Nyquist belonged not to BTL but to the Development and Research department of AT&T; as an outsider he stabilized Black's box by bringing it into the frequency domain.[60]

The Morristown Trial

In May of 1928 Nyquist asked Black to join in developing a new carrier system and to include the negative feedback amplifier in a trial of new transmission techniques. A major project began in 1929 to test a long distance of cable with repeaters every twenty-five miles. Known as the "Morristown Trial," this program installed twenty-five miles of cable terminating in the repeater station in Morristown, New Jersey. The many pairs in the cable connected back on each other at the ends, a total of thirty-four times, adding up to eight hundred fifty miles of transmission distance. Seventy-eight negative feedback amplifier repeaters, spaced every twenty

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[60] Hendrik W. Bode, "Harry Nyquist," Obituary, IEEE Spectrum, April, 1977.

five miles, were all located in the same laboratory in Morristown. Through this trial, Morristown repeated its role in the network, folding over the signal so one place in New Jersey came to stand for the entire country.

The Morristown cables pushed the limits of transmission stability. Compared to open wire, underground cables experienced smaller variations in temperature (by a factor of three) more slowly (by a factor of several hundred), making their transmission qualities more stable. But greater attenuation in the cables offset this advantage, requiring a correspondingly more precise and stable transmission. Cables in fact, added the further complication that the variation in loss with temperature varied according to frequency, which meant that the signal would become distorted as well as attenuated as temperature changed, a phenomena known as "twist." Thus the transmission regulators had to change not only the numerical gain of the repeater amplifiers, but their frequency response as well, twisting the signal back. In the Morristown Trial an "automatic transmission regulating system" operated a self-balancing amplifier which drove a motor. The motor mechanically adjusted electrical equalizer networks to account for the changing frequency response. This setup also included a "centering cam" to avoid hunting.[61] [*Figure 4-11, photo of Morristown regulator]

Regeneration Theory

Before his work on the Morristown trial, Nyquist worked on the problem of transmission stability and regulation. He patented a non-feedback method of "constant current regulation," for smoothing out fluctuations in power supply voltages, and a means for using pilot wire

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[61] For a detailed account of the Morristown Trial, see A.B. Clark, and B.W. Kenall, "Carrier in Cable," *BSTJ* 12 (July, 1933) 251-62, also O'Neill, ed., Transmission Technology, Chapter 5: "Carrier on Cable." Getting the system to work as planned proved no simple matter, but such was the purpose of an engineering trial. Repeater amplifiers did not pose the only problems: cable design (the number, size, and shielding of each of the many wire pairs) proved especially critical as well. Shielding, grounding, and interference between signals plagued the system. Because of the depression, AT&T changed its emphasis from new systems to improving capacity with the existing plant. BTL engineers had several years to refine the results of Morristown, and to work on ways of compressing more transmission onto existing wires. But the Morristown Trial did form the basis for the "K" type carrier system, introduced in the late 1930s, which carried 12 voice channels on cables at frequencies from 12 to 50 kHz for distances up to 4,000 miles. K carrier furnished 70 percent of the increased capacity in the country, which doubled from 1940 to 1947 and remained in service until at least 1980. K-carrier also included a "pilot wire" transmission regulation scheme, with an automatic self-balancing regulator and a self-synchronizing motor, similar to the G.E. selsyns used in fire control. C. W. Green and E. I. Green, "A Carrier Telephone System for Toll Cables," *BSTJ* 17 (January, 1938).

transmission regulators to compensate for phase shifts.[62] With the Morristown trial, Nyquist brought this experience to negative feedback. His 1932 paper, "Regeneration Theory," provided a rigorous set of measurable conditions which determined an amplifier's stability. Nyquist's earlier published work had analyzed signals in terms of their frequency characteristics, their Fourier components, and now he defined stability in terms of transient disturbances. "For the purpose of studying the singing condition, it is permissible," he wrote, "to regard the feed-back phenomenon as a series of waves."[63] If all disturbances impressed upon a circuit die out after a finite period of time, the circuit is *stable*. If the disturbance goes on indefinitely, the circuit is *unstable*.[64] [*Figure 4-12, Nyquist drawing of amplifier and waves]

It was intuitively clear that two simultaneous conditions would make an amplifier unstable and sing. First, the wave coming around the feedback loop would have to be equal to or greater than the input to the amplifier; its gain would have to be greater than one. And second, the feedback wave would have to be inverted compared to the input wave; its phase shift would have to be 180°. If, for any frequency, these conditions are both met, then the amplifier is unstable and will oscillate. Building on Carson's work with Heavyside's operational calculus and on George Campbell's Fourier analysis, Nyquist turned these conditions into a simple, empirical method for determining stability. First, break the loop so the amplifier will not feedback on itself. Then measure its "open loop characteristics," plotting two easily measured quantities, gain and phase, against each other as they vary with frequency. If the resulting curve encloses the point that represents a unity gain and 180° shift, the system is unstable. If the point lies outside the curve, the system is stable.[65] [*Figure 4-13, Nyquist Diagram]. This plot became known as a "Nyquist Diagram;" and remains the "Nyquist stability criterion," or the "Nyquist criterion." Within electronics Nyquist's result had, in Stuart Bennett's words, "enormous practical significance," for

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[62] H. Nyquist, U.S. Patent no. 1,887,599 "Constant Current Regulation;" 1,683,725, "Phase Regulating System." Applications filed 1928 and 1926, respectively.

[63] H. Nyquist, Discussion of H. S. Black, "Stabilized Feed-Back Amplifiers," *Elec. Eng.* (September, 1934), 1311-12.

[64] Harry Nyquist, "Regeneration Theory," *BSTJ* 11 (1932) 126-47. My discussion of Nyquist's paper is based on Bennett, Stuart Bennett, A History of Control Engineering 1930-1955, 82-84.

[65] Nyquist, "Regeneration Theory," emphasis original. In 1934, BTL engineers compared Nyquist's criterion to Routh's test from his 1877 Adams Prize paper on stability in dynamic mechanical systems. They found the two stability analyses compatible, and thus linked the new feedback theory to the older work on dynamic stability. Despite this link, however, their work makes no mention of applying feedback amplifier theory to other dynamic

it reduced a significant amount of complex calculation to a simple procedure, a literary technology and a tool for engineers to think with. Nyquist patented the method.[66]

Nyquist's criterion provided an elegant means for engineers to determine the stability of the systems they were designing. But it still assumed a relatively ideal amplifier. If one designed close to the border of stability, which would produce the highest performance and the most efficient amplifier, variability in the vacuum tubes or any other parameters could easily push the amplifier over the limit. It remained for one more BTL engineer, Hendrik W. Bode, to complete pre-war phase feedback theory. Bode came to BTL in 1926, fresh from a masters degree at Ohio State, where had also done his bachelors (he received a Ph.D. in physics from Columbia in 1935). Bode's expertise was not in feedback, nor even really in amplifiers or vacuum tubes, but in the useful but esoteric *network theory*.

Feedback as a Network Problem

The theory of electrical networks dealt with collections of resistance, capacitance, and inductance represented as complex impedances. *Network analysis*, describing the behavior of existing networks, derived from George Campbell's early work on wave filters. *Network synthesis*, formulating a network based on proscribed behavior, was developed at BTL in the 1920s by R.M. Foster, O. Zobel, Thornton Fry, and others.[67] Filter specifications included frequency response, selectivity, phase delay, and the "flattness" of the passband. Network design techniques compressed a great deal of algebra into standardized building blocks for making complex filters with a minimum of components and optimized for a variety of parameters.

As the Bell System adopted carrier transmission and began to manipulate signals in the frequency domain, electrical networks became increasingly critical to telephony. Filters networks separated specific frequencies out of the spectrum. Phase shifting networks aided in single-sideband transmission. Equalizer networks compensated for the distortion in a transmission line,

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systems. E. Peterson, J.G. Kreer, and L.A. Ware, "Regeneration Theory and Experiment," *BSTJ* 13 (October, 1934) 680-700.

[66] Bennett, A History of Control Engineering 1930-1955, 83. H. Nyquist, U.S. Patent no. 1,915,440, "Regenerative Amplifier." Application filed 1930.

[67] S. Millman, ed., A History of Engineering Science in the Bell System: Communications Sciences (1925-1980) (Murray Hill, New Jersey: AT&T Bell Laboratories, 1984), 16-17. Also see O'Neill ed., Transmission Technology, 204-208. For a good summary of the work on network theory in the twenties and thirties, see Karl L. Wildes and Nilo A. Lindgren, A Century of Electrical Engineering and Computer Science at MIT 1882-1982 (Cambridge: MIT Press, 1985), Chapter 9, "Network Analysis and Synthesis: Ernst A. Guillemin."

returning the voice signal to its original shape. With these networks, as with repeaters, each element required proportionally more quality as the size of the system increased. In the early thirties, Bode recalled, he "plodded through a long program intended to reformulate certain areas of network theory related to equalizers as a study of the analytic behavior of some particular classes of rational functions in the complex plane."[68] In 1934, he developed and published a general theory which accounted for all types of filters.[69] Bode called this work "a sort of algebra of the transmission characteristics of dissipative networks" which construed as design parameters the poles and zeros of the network's characteristic equation in the complex plane.

In feedback amplifiers, Bode realized, networks became critical because they shifted the burden of performance from the active vacuum-tube element to the passive feedback path, itself an electrical network. His network work merged with feedback amplifiers in the context of yet another new transmission medium, coaxial cable. These cables, which had only one conductor surrounded by a conductive shield, could carry much higher frequencies on a single wire than the older cables which had bundles of twisted pairs. The millions of cycles per second (MHz) range allowed several hundred conversations to be multiplexed together and could also carry the new broadband television signals. In addition, coaxial cables had much better "stability" of transmission, that is their losses varied with temperature simply and uniformly. Still, as with the jump from open wire to cable, the jump to coaxial cables placed heavier demands on repeaters, equalizers, and system performance overall.[70] In 1934, Bode the network expert was brought in to design an equalizing network for the feedback path of an amplifier for a coax repeater which required 1Mhz bandwidth. The overall amplifier behaves like the *reciprocal* of its feedback elements — when the feedback path divides, for example, the amplifier multiplies. So for an equalizer, which mimics the inverse of the transmission line to cancel out its effects, the feedback path had to simulate the transmission line exactly, and to follow the line's complex fluctuations

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[68] H. W. Bode, "Feedback, the History of an Idea."

[69] H. W. Bode, "General Theory of Electric Wave Filters," *J. Math. & Physics* 13 (November, 1934), 275-362.

[70] L. Espenschied and M.E. Strieby, "Systems for Wide-Band Transmission over Coaxial Lines," *BSTJ* 13 (October, 1934), 654-79. M.E. Strieby, "A Million-Cycle Telephone System," *BSTJ* 16 (no.1, January, 1937), 1-9. See also O'Neill, ed., Transmission Technology Chapter 6, "Coaxial Cable," especially 131-139. The system Bode worked on became known as the L1; it was tested on a line from New York to Philadelphia in 1936-38 and put into service just before the war.

with a single adjustment.[71] The trouble was, Bode had to design the equalizer network after the amplifier had already been designed, and such post-hoc modification made the amplifier unstable. Bode recalled "I sweated over this problem for a long time without success," finally, "in desperation," redesigning the entire amplifier, applying the procedure for designing an equalizer to an entire closed-loop system. Where Nyquist had redefined the stability of the amplifier and provided a simple way to determine it, Bode began to address the formulation of the amplifier itself, and its associated feedback network; a formulation not only of a stable amplifier, but one which met desired parameters for performance.

Feedback Amplifiers and Mixed Emotions

Bode's 1940 paper, "Relations Between Attenuation and Phase in Feedback Amplifier Design," remains his best-known and most succinct contribution to feedback theory. The opening pages have a decidedly pessimistic tone, as Bode comments that the stability of a feedback amplifier "is always just around the corner." He begins,

> The engineer who embarks upon the design of a feedback amplifier must be a creature of mixed emotions. One the one hand, he can rejoice in the improvements in the characteristics of the structure which feedback promises to secure him. On the other hand, he knows that unless he can finally adjust the phase and attenuation characteristics around the feedback loop so the amplifier will not spontaneously burst into uncontrollable singing, none of these advantages can be actually realized.

He likens a feedback amplifier to a perpetual motion machine, which always works, "except for one little factor," a little factor which never quite goes away, despite all the tweaking. He sets out to elucidate the relations between gain and phase change around the loop "which impose limits to what can and cannot be done in a feedback design. The relations are mathematical laws, which in their sphere have the same inviolable character as the physical law which forbids the building of a perpetual motion machine." The conditions for stability, he continues, the price of using feedback, "turns out to be surprisingly high." It "places a burden on the designer," and without new tools "he is helpless:" "Unfortunately, the situation appears to be an inevitable one. The mathematical

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[71] H. W. Bode, "Variable Equalizers," *BSTJ* 17 (April, 1938), 229-244. Black wrote in 1934, "For many types of frequency characteristics it is difficult, and for some impossible, to construct a passive network having the exact inverse characteristic [as the transmission line]. With this type of [feedback] amplifier, however, it is only necessary to place in the feedback circuit apparatus possessing the same characteristic as that to be corrected." "Feedback Amplifiers," *BLR* 12 (no. 10, June, 1934), 294.

laws are inexorable." Bode seems to be addressing Black himself, and those who shared his uncritical exuberance for the benefits of feedback, regardless of stability problems.[72]

Bode shows instead that feedback is in fact a rather complicated affair. Nyquist had recognized broadly that stability was a function of attenuation and phase, but Bode defined a specific integral which related phase shift and attenuation, "broadly, that the phase shift at any frequency is proportional to the derivative of the attenuation on a logarithmic frequency scale." He built on this gain/phase relationship and the limitations it defined "to establish a definite method of design:" again, a set of simple, graphical techniques for plotting gain vs. phase based on observed and analytic quantities, which can be used to determine stability. These graphs, which approximate exponential response curves with easily-drawn straight lines, survive to this day as "Bode Plots." [*Figure 4-15, Bode Plot]

Bode also refined Nyquist's graphic, rotating it around 180° to erase the effect of a vacuum-tube amplifier (which is inherently inverting and thus adds 180° phase shift). Nyquist's stability conditions did not account for variations in vacuum tube performance due to temperature or manufacturing differences. Bode introduced the concepts of "phase margin," and "gain margin," which in effect, answer the questions: When the gain reaches 1 how much phase is left before 180° (and instability)? When the phase reaches 180°, how much gain is left before 1 (and instability?) These measures "bridge the gap between a purely mathematical formula...and a physical amplifier, whose ultimate loop characteristics vary in some uncontrollable way." Nyquist's criterion implied it would be beneficial for the gain to cut-off as quickly as possible outside the useful band of frequencies. Bode showed that if the gain cutoff was too rapid, it could alter the phase in such a way as to induce instability. Thus "the amplifier should cut off, on the

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[72] H.W. Bode, "Relations Between Attenuation and Phase in Feedback Amplifier Design," *BSTJ* 19 (July, 1940), 421-454. For other discussions of this paper, see Bennett, A History of Control Engineering, 1930-1955, 84-86. Millman, ed., Communications Sciences, 29-30. O'Neill, Transmission Technology, 68-70. In later years, Bode displayed some aversion to Black's version of events. He wrote to A.C. Dickieson in 1974, after reviewing Black's account for the History of Engineering and Science in the Bell System, "this is not exactly how one ordinarily writes formal technical history [interestingly, Bode had some notion of "formal technical history"]...Have you thought of a less personalized treatment in which pieces of Black's account are woven in with expository text of your own?...It might be possible to eliminate, for example, the references to Steinmetz and Hartley, which seem to me to be irrelevancies. In a less personalized account, it might be possible to present basic technological issues in a more satisfactory way. For example, as the paper now stands it seems to imply that Black deserves credit for the pioneer investigation of nonlinear effects in long systems. I doubt whether this is really accurate....I was also a little disturbed by Harold's claim that he outfaced the U.S. Patent office on every one of 126 claims. I didn't know that

whole, at a well defined rate which is not too fast." With a determined cutoff rate, then, the amplifier actually needed to work in a range higher than the useful frequency band in order to be stable, and the more feedback, the higher that band was extended. Practically, this meant "we cannot obtain unconditionally stable amplifiers with as much feedback as we please" because at some point these out-of-band frequencies would become impractical.[73] The amplifier which Bode had originally been asked to examine, for example, would need to work up to 30MHz just to stay stable, even though it only needed to amplify signals up to 1Mhz. Bode imposed limits on the possible performance of the feedback amplifier. But he brought the sophistication of the network designer to the problem and brought the negative feedback amplifier fully into the frequency domain.

Bode's name is permanently associated with feedback, but he always linked it to its network roots, "it is still the technique of an equalizer designer...I can imagine that the situation may well seem baffling to someone without such a background."[74] He spent the years between 1934 and World War II refining his work. He taught an "out of hour course," at BTL in the winters of 1938-39 and '40-41, the notes for which were published internally, and which became a book in 1945 (Harold Black taught a similar course on feedback amplifiers in 1935-36). Bode's title, Network Theory and Feedback Amplifier Design reflects his primary experience in networks, with secondary application to amplifiers. Before publication, Bode and BTL widely distributed this manuscript during the war to other laboratories working on control systems.[75] Bode acknowledged a certain amount of "unnecessary refinement," of the design methods in the book, but explains they were required for telephone repeater amplifiers, with their unusually high standards for performance.[76]

Speaking Machines and the Transmission of Information

Bode and Nyquist brought negative feedback and the vacuum tube within the realm of signals, networks, and frequencies — within the emerging communication engineering. A high-quality, linear repeater effectively separated the message inherent in the telephone signal from then

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the Patent Office gave ground that easily. In any case, credit should probably go to the long-suffering patent attorney who wrote all those letters." Bode to Dickieson, September 17, 1974, ATT.

[73] Bode, "Relations Between Attenuation and Phase," 426-35.

[74] Bode, "The History of an Idea," 117.

[75] H.W. Bode, Network Analysis and Feedback Amplifier Design (New York: Van Nostrand Co., 1945), iii.

[76] Bode, *Network Analysis*, iv.

energy required to transmit it down the line. But the repeater alone did not transform the telephone signal, it coevolved with a conception of the network as a social device, and of machines as active speech producers. Technically, this vision incorporated both telegraphy and telephony, text and speech, into a theory of processing signals, manipulating them in the frequency domain, and defining the bandwidth of transmission channels. Even before Black's invention, Nyquist and Ralph Hartley were already addressing the problem of telegraph channel capacity, and at work on theories of messaging.

The Network Machine

Where repeater amplifiers made the transmission network an active device, validating J.J. Carty idea of the telephone network as "society's nervous system," others at BTL thought of the network in human terms as well. In 1925, for example, a BTL employee with an interest in amateur microscopy published a pair of photographs in the *Bell Laboratories Record*, displaying a cross section of a modern telephone cable side-by-side with a human spinal cord. The similarity, proclaimed the *Record*, "may be carried further than mere physical resemblance," when one considers the function of the nervous and telephone systems, "The spinal cord of an individual is the conduit of the main nerves which go out from the brain and over which intelligence may be flashed to any part of the body...In similar manner the long-distance cables of the modern telephone plant connect physically the widespread members and communities of the social and economic structure of the nation."[77] The attribution of "singing" and "howling" to the repeater further illustrates this anthropormphic (or lupomorphic) vision of the system. When the network became active it quickly acquired human (or wolf-like) qualities.

While repeater amplifiers redefined transmission as similar to human nervous activity, during the 1920s automatic switching redefined routing, transforming the telephone network into an active information system. AT&T was relatively late to introducce automatic switching, but between 1923 and 1924 the number of automatic switching stations nearly doubled, to just under one million. By 1927 there were nearly two and a half million. In 1935, thirty five percent of all phones had dials.[78] For local calls, users now dialed a telephone number directly, without the help

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[77] "The Spinal Cord of a Nation," *BLR* 1 (no. 2, October, 1925).

[78] AIEE Committee on Communication, Annual Report, "Recent Advances in the Communication Art," *Trans AIEE* 44 (June, 1925) and Annual Report, "Electrical Communication," *Trans. AIEE* 46 (June, 1927). The annual reports of all AIEE Committees track a wide range of electrical technologies during this period. For the social

of the operator (who was still needed for long-distance connections). This meant, of course, the user had to know the number of the party he or she was dialing, or call a central operator to get the number. Dialing "information" connected the user to a "centralized information bureau," where a number of operators had telephone books. Automatic dialing replaced the switching function of the human operators, but it still needed the information center as a central storehouse for the telephone numbers. Bell Engineers paid close attention to both the signal routing and the ergonomics of these facilities, designing them so the operators looked up the numbers as rapidly as possible, using specially designed "information desks."[79] Added to the nervous system, automatic switching seemed to make the network autonomous, even intelligent. In 1926 the *Record* called the new automatic network "A Mechanical Brain." The switching network automatically selected among thousands of possibilities to make the right connection, an activity, the *Record* asserted, which clearly indicated intelligence.[80]

Repeater amplifiers and automatic switching exemplified the Bell System's constant redefinition of the human role in network operations (as both "users" and "operators,"), as machines and people talked to each other in novel ways. Even the problem which drove feedback regulator design at BTL, talking movies, represented a kind of automation, combining human and machine capabilities and replacing a textual representation (subtitles) with an anthropomorphic one. Machine-mediated actors' voices also appropriated the sounds of the local orchestra. Mechanical reproduction of the moving image had its own silent mystery, but adding the voice truly made the machine come alive — as any number of popular reactions to the new technology argued.[81] BTL engineer Hugh Stoller even suggested the governor, while its main goal was proper pacing of the sound track, might also regulate a silent film, allowing the orchestra conductor to stay in tight synchrony with the image — rationalizing the orchestra as well as the machine.

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history of automatic switching, see Kenneth Lipartito, "When Women Were Switches: Technology, Work, and Gender in the Telephone Industry, 1890-1920," *American Historical Review* (October, 1994). A.E. Joel, Jr. Ed., A History of Engineering and Science in the Bell System: Switching Technology (1925-1975) (Whippany, New Jersey: Bell Telephone Laboratories, 1982). Robert J. Chapuis, 100 Years of Telephone Switching (1878-1978) (Amsterdam: North Holland Publishing Co., 1982). Also see Brooks, Telephone, 193.

[79] J. F. Dahl, "Improved Equipment for Information Service," *BLR* 8 (no. 7, March, 1930), 328-332.

[80] "A Mechanical Brain," *BLR* 3 (no. 3, November, 1926), 78-81.

[81] Sheldon Hochheiser, "What Makes the Picture Talk: AT&T and the Development of Sound Motion Picture Technology," *IEEE Trans. Education* 35 (no. 4, November, 1992), 278-85.

"The Invisible Orchestra:" Coupling Users to the Network

In the years before World War II, engineering research at BTL studied the borderline between human and machine, where the network connected to its users — the translation between electrical and acoustic energy. Much of this work had gotten under way during World War I, when Harvey Fletcher, Ralph Hartley, Thornton Fry and a number of other BTL engineers worked on detecting attacking airplanes with binaural sound.[82] Engineers at Western Electric developed microphone detectors and binaural (what today we would call stereo) direction finders for antiaircraft systems (as in Sperry's sound locator devices), loudspeaking intercoms for battleships, and telephone sets for fire control applications for both the Navy and the Army.[83] Afterward they continued this work, studying listening and the nature of speech. Fletcher studied noise, intelligibility, the structure of the human ear, and created hearing aids and an artificial larynx — all applying the "matched-impedance" techniques of electrical transmission theory to electro-acoustic systems. Others analyzed articulation, acoustics in auditoriums, and pitch sensitivity of the ear.[84]

From this and related work in electronics emerged the new field of high fidelity audio. The Director of the Philadelphia Orchestra, Leopold Stokowski, saw his musical creation as requiring technological as well as orchestral elements and collaborated with Fletcher and BTL on numerous high-fidelity projects. On April 27, 1933, the Philadelphia Orchestra played remotely to a capacity crowd in Washington. The music was transmitted via Black's new repeater amplifiers, taken straight from the Morristown trial.[85] Stokowski did not conduct the performance in Philadelphia, but rather operated electronic volume and tone controls in Washington. "Seated at his controls, Dr. Stokowski superimposed his interpretation on that of the invisible orchestra" the New York

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[82] See Findley, "The Research Department," and R.V. L. Hartley and Thornton Fry, "The Binaural Location of Complex Sounds," *BSTJ* 1 (no.2, November, 1922), 33-42.

[83] "Western Electric Wartime Developments, 1917-1918," ATT 177 06 03 01.

[84] See Harvey Fletcher, "The Nature of Speech and its Interpretation," *BSTJ* 1 (July, 1922), 129, "Physical Measurements of Audition and their Bearing on the Theory of Hearing," *BSTJ* 2 (October, 1923), 145, "Useful Numerical Constants of Speech and Hearing," *BSTJ* 4 (July, 1925), 375-386. Robert E. McGinn, "Stokowski and the Bell Telephone Laboratories: Collaboration in the Development of High-Fidelity Sound Reproduction," *Technology and Culture* 24 (no. 1, January, 1983), 43. *BSTJ INDEX* Volumes 1-10 (1932) articles under the heading of Speech, Acoustics, Audition, Sounds of Speech, Sounds and Words. See also Millman, ed., Communications Sciences, 93-102. The tone and loudness controls on modern audio equipment emerged from this research.

[85] Black to Dickieson, August 14, 1974. Stokowski, Bode, and Fletcher maintained a long collaboration. Stokowski to Bode, March 8, 1940 and March 30, 1940. Bode Papers, Harvard University. Box 1, Folder 1.

Times reported.[86] Stereophonic hearing, artificial organs, "invisible orchestras," — each stressed, in their own way, the further integration and extension of human activity by the telephone's spreading network.

BTL researcher Homer Dudley also dramatically blurred the human/machine boundary. In "The Carrier Nature of Speech," Dudley likened human speech to "a radio wave in that information is transmitted over a suitable chosen carrier." Dudley built two speech synthesizers, the "vocoder" and the "voder." The first device analyzed spoken language in terms of frequency components, transmitted each component separately, and then recombined them at the receiver. The "voder," relied instead on a skilled operator to produce the speech components by tapping at a typewriter-like device with "spectrum keys." Dudley modeled the human as an integral part of the system of voice transmission, and the transmission media (wire or radio) as replicas of the human vocal tract. He effectively extended transmission theory into the human brain: "Communication by speech consists in sending by one mind and the receiving by another of a succession of phonetic symbols with some emotional content added."[87] AT&T displayed the Voder with much fanfare at the 1939 World's fair. Sound recording, synchronized movies, high-fidelity transmission — all indicated an increasing facility for generating, recording, and reproducing sound, now abstracted as audio signals. [*Figure 4-16, Voder block diagram and photo]

Measuring Text and Speech

> While all forms of electrical communication are merely variant adaptations of common physical phenomena, they can nevertheless be divided for convenience into two groups. The basis for this division is not, however, one of the methods of transmission or even of the kind of service given. Rather it is a differentiation based on our physical senses of sight and hearing.
>
> If at the receiving station the interpretation of the message is conveyed to the brain through the ear in the normal function of hearing, it is telephony; if through the eye, it is telegraphy.
>
> Frank Jewett, to the National Academy of Sciences, 1935

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[86] *New York Times* April 13, 1933. Quoted in McGinn, "Stokowski," 59. According to McGinn, Stokowski manipulated the controls with such enthusiasm that he sometimes irritated those who preferred the orchestra's own volume variations.

[87] Homer Dudley, "The Carrier Nature of Speech," *BSTJ* 19 (no. 4, October, 1940) 495-515.

Parallel to the shifts in human/machine boundaries, the line between text and speech began to blur as well. Theoretical work at BTL extended the facilities of translation achieved by the vacuum tube to all messages, beginning with the oldest form of electrical communication, telegraphy. Telegraphy had not disappeared with the rise of voice communications. In fact the "Telegraph" in the title AT&T remained far more than vestigial — it represented a significant source of revenue. Teletypewriter service emerged in the early thirties as a new business for AT&T, which proudly promoted it, along with a multiple channel carrier telegraph system, at the 1933 Century of Progress Exposition in Chicago.[88] Bell engineers pressed to increase the capacity of telegraph lines just as they did voice lines — and they faced the same limits of attenuation in the new cables. Still, multiplexing merely made the capacity problem into a speed problem. The faster you switched between parallel signals, the more lines you could impose on a single wire, but how fast could you switch? What were the limits of the transmission medium which determined the highest speed of telegraphy? Carrier multiplexing, which modulates telegraph signals in the frequency domain, translates the question into one of bandwidth. How much space does a telegraph signal require on the spectrum?

From James Clerk Maxwell to the 1920s, telephone engineers attacked this problem empirically: they looked at the shape of the telegraph pulses after they traveled down the line, and adjusted transmission speed so the pulses didn't overlap.[89] Few analytical tools existed for relating transmission to bandwidth — given a certain kind of line, how fast a signal could you send down it? Harry Nyquist analyzed this problem in 1924, in "Certain Factors Affecting Telegraph Speed." He divided time into "short intervals of approximately equal duration," and then divided the message up the same way, into "signal elements" (similar to what today we would call bits), thus standardizing and rationalizing the signal. Using these basic units, Nyquist examined the ideal shape for the telegraph pulse, and the ideal code to provide the fastest communication of intelligence for a given line speed. Morse code, for example, did not effect the most efficient transmission, because it was optimized for the human ear to discriminate symbols. Other codes specifically designed for machine transmission did better. Nyquist also defined a "rate of sending

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[88] Coon, American Tel and Tel, 203. "The Bell System Exhibit at the Century of Progress Exposition" *BLR* 11 (no. 10, July, 1933). Multiplexing of telegraph signals had gone on for many decades; Bell was working on a telegraph-multiplexer when he invented the telephone.

of a signal...the speed of transmission of intelligence." In 1928, further elaborating the modules "signal elements" and "time units," he related this "speed of signaling" to bandwidth, which allowed modulating several telegraph signals onto carriers. The bandwidth required, Nyquist showed, equaled half of the pulse rate.[90] In his analysis, Nyquist freely alternated between the time the frequency domains, and his results derived from his analysis of the telegraph signal as a steady state and not a transient wave — just as they would four years later with his work on negative feedback amplifiers. He also acknowledged that noise or "interference," would slow down the effective transmission rate, although without quantifying the effect.[91]

Theory of Information

In 1928, Ralph V. L. Hartley, a Rhodes Scholar who joined Western Electric straight out of Oxford in 1913, added his own formulation of transmission capacity. In "Transmission of Information" Hartley sought "a quantitative measure whereby the capacities of various systems to transmit information may be compared." Acknowledging that "as commonly used, information is a very elastic term," he eliminated the "psychological factors" of semantics and meaning by measuring information in a purely physical sense. He declared "The capacity of a system to transmit a particular sequence of symbols depends on the possibility of distinguishing at the receiving end between the results of the various selections made at the sending end." This definition implies that capacity increases exponentially with the addition of more possible

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[89] H. Nyquist, R.B. Shanck, and S.I. Cory, "Measurement of Telegraph Transmission," *AIEE Trans.* 46 (February, 1927), 367-376.

[90] H. Nyquist, "Certain Factors Affecting Telegraph Speed," *BSTJ* 3 (no. 3April, 1924), 324-46. B. P. Hamilton, H. Nyquist, M.B. Long, W.A. Phelps, "Voice-Frequency Carrier Telegraph System for Cables," *Trans. AIEE* 44 (February, 1925), 327-39. This paper (which erroneously lists Nyquist's first initial as N.) also includes a discussion of the precision governor required for a generating carrier frequencies for this telegraph system, suggesting Nyquist had exposure to regulation before his 1932 paper on feedback, "Regeneration Theory" (he had patented line regulating devices). For telegraph sampling, the main paper was H. Nyquist "Certain Topics in Telegraph Transmission Theory," *AIEE Trans.*, 47 (February, 1928), 617-644. See also the discussion of this paper by Nyquist's son-in-law, John C. Lozier, "The Oldenberger Award Response: An Appreciation of Harry Nyquist," *Journal of Dynamic Systems, Measurement and Control* (June, 1976), 127-8. Nyquist's measure, that a wave must be sampled at twice its bandwidth to be transmitted without distortion is still used today, and the sampling rate is frequently referred to as "the Nyquist rate." A modern CD player, for example, samples the music at 44khz, in order to reproduce music in the audible band of about 20khz.

[91] H. Nyquist, "Thermal Agitation of Electric Charge in Conductors," *Phys. Rev.* 32 (July, 1928), 110-113.

selections, so information, for Hartley, became "the logarithm of the number of possible symbol sequences," a definition Nyquist hinted at as well.[92]

Employing Carson's work with Heavyside's step function, Hartley derived the information capacity of a channel from its response to a single imposed impulse (including a lucid verbal description of the relationship between steady state and transient analysis). He then integrated the transient response over time, making a picture of the steady-state system. From this he concluded, similar to Nyquist: "the maximum rate at which information may be transmitted over a system whose transmission is limited to frequencies lying in a restricted range is proportional to the extent of this frequency range." That is to say, information capacity is proportional to bandwidth, and also to the product of bandwidth times time. A narrowband channel transmitting for a long time has the same capacity as a wideband channel transmitting for a short time (a fact familiar to anyone today used to downloading data through a modem).

Going beyond Nyquist, Hartley generalized his analysis. He grouped all transmission media (wire, radio transmission, or even direct speech) under the general term "line," and characterized each by their bandwidth, or "line-frequency-range." He similarly defined as "messages," the "symbol sequences" sent over each medium, with a corresponding "message-frequency-range," which may or may not equal the line-frequency-range. The problem of communications, then, becomes matching the two ranges, or squeezing the message-frequency-range into the available line-frequency-range. For example, if the two have equal bandwidths at different points on the spectrum, modulation can shift the message up to the band of the line. More complicated, however, is when the available transmission channel (line-frequency-range) is narrower than the message-frequency-range. A long time can compensate for a narrow bandwidth, so the message may be recorded on a tape and then played back more slowly through the line. Then "the frequency range of the message as reproduced from the tape may be made to fit whatever line-frequency-range is available" by playing it back at a different speed. This recording effectively maps the temporal function of the signal onto a spatial function over the

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[92] R.V.L. Hartley, "Transmission of Information," *BSTJ* 7 (July, 1928), 535-63. See brief discussions of Nyquist and Hartley by E. Colin Cherry, "A History of the Theory of Information," *Proc. IEE* 98 part 3 (no. 55, September, 1951) 386, and by J. R. Pierce, "The Early Days of Information Theory," *IEEE Trans. on Information Theory* IT-19 (no.1, January, 1973), 3. Comparing Nyquist and Hartley's work on transmission, Pierce writes, without elaboration, that "It is [Claude] Shannon's feeling, and mine, that Nyquist's work was more fruitful." Cherry finds

length of the tape. Hartley took this idea further with the emerging technology of television. He divided the picture into a series of strips, which he then treated like other signals. The bandwidth required for transmitting visual images over telephone lines, he concluded, depends on visual resolution and picture repetition rate.

Hartley and Nyquist's studies of information began to outline "digital processing," but conceptualized through transmission media instead of through mathematical calculation. Hartley's notion of recording data equates to "memory" in the way computers use it today, and his imaginary signal tapes resonate with those of Alan Turing's vision of the general computer a decade later. Moreover, both Nyquist and Hartley considered the number of possible levels of an analog signal, the "number of current values employed," as equivalent to the number of symbols transmitted down the line. Hartley, for example, acknowledged that being able to distinguish an infinite number of signal levels would imply infinite information capacity, an impossible case. He thus assumed some discrete and finite set of distinguishable levels, a figure also proportional to information content (for a picture, this quantity might refers to the number of "gray scales" distinguishable in the image.) Nyquist similarly showed his own results to be "substantially independent of the number of current values employed." Because of his background and interest in telegraphy, he generally employed only *two* levels, corresponding to the key pressed or not pressed. This awareness, on the part of both men, that continuous waves were an idealization limited by the available bandwidth, points to the value of digital, or binary transmission. Neither termed it as such, but Nyquist did use the numbers 1 and 0 for the magnitude of his signal elements.

Dividing up time into discrete elements, dividing up current into discrete levels, dividing pictures into strips and dots: these are the foundations of digital data transmission and processing. Through the Heavyside step function, through the Fourier transform, and through transmission theory, Nyquist and Hartley related the textual messages of the telegraph to the sonorous messages of the telephone. By building on the relationship of time to frequency, of transient to steady-state Nyquist and Hartley related pulses to waves, and words to speech — what today we would call it the relationship of digital to analog.

Hartley's work more general than Nyquist's, and notes "Hartley's has a very modern ring about it...[it] may be regarded as the genesis of the modern theory of the communication of information."

Despite the apparent significance of Nyquist and Hartley's work, however, their results had limited impact. In contrast, Black's innovation, though it required significant theoretical and practical application, did achieve a certain continuous presence within BTL and the system. The chain from Black to Bode and Nyquist is more or less unbroken from 1927 until 1940, mediated by engineering trials, publications, out-of-hour courses, and network installations. But the early theory of information spawned no such lineage before World War II. No further papers mention the topic, Nyquist went on to his feedback work, and Hartley to further work in communications. Not until Claude Shannon's 1948 paper, "Mathematical Theory of Communication," did the theory of information, based on transmission capacity, again emerg. In fact, Shannon Cited Nyquist's two papers on telegraph transmission (1924 and '28) and Hartley's 1928 paper in the first paragraph of his work as "a basis for such a theory [a general theory of communication]." Shannon cited no other papers on information or transmission in the remainder of his paper.

Conclusion

Harold Black's vision of feedback on the Lackawana Ferry in 1927 did not take place in isolation. It connected at every point to the problems of the telephone network, including long distance transmission, carrier modulation, and the relationship of research to the broader system. As a governor, Black's feedback amplifier aimed to regulate transmission, to insulate the performance of the technical network from its physical (meteorological) context. But Nyquist and Bode realized that this long-term stability was straightforward compared to the immediate problems of dynamic stability. Self-regulation could rapidly turn to oscillation, and avoiding instability hence became the driving force of feedback amplifier design. This analytic work, and the graphical techniques it spawned, had not parallel in the world of naval fire control or Sperry's manufactured controls. Neither had a network, physical, social, or financial, extensive enough to support the type of "pure" research carried out in Bell Labs. As we have seen, the role of theory in the telephone network derived not only from the difficulties of technology, but also from its geographical extent. Nyquist and Hartley initiated a still broader, if still tentative, with their attempts to quantify information. By defining information capacity as bandwidth, and equating transmission lines with that bandwidth, Nyquist and Hartley demonstrated the equivalence of diverse types of signals: telegraph messages, voice signals, and television images. This realization had important implications, both technical and political. Frank Jewett echoed the equivalence in a speech to the National Academy of Sciences in 1935. He rejected a notion of communications based on technical artifacts, "We are prone to think and, what is worse, to act in terms of telegraphy, telephony, radio broadcasting, telephotography, or television, as though they were things apart." Instead, Jewett argued, "they are merely variant parts of a common applied science. One and all, they depend for the functioning and utility on the transmission to a distance of some form of electrical energy whose proper manipulation makes possible substantially instantaneous transfer of intelligence."[93] A unified theory of communication reflected corporate goals: regulation persisted in making distinctions between media (radio, telephony, etc.), each controlled by "vested interests." When policy followed science in treating communications as equivalent, Jewett hoped, AT&T, with its "natural monopoly," would emerge as the unified communications company. Almost without realizing it, AT&T, which from the first had sponsored industrial research to exclude competition, fostered technologies of control while in the pursuit of the control of technology.

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[93] Frank B. Jewett, "Electrical Communication, Past, Present, and Future," Speech to the National Academy of Sciences April, 1935, reprinted in Bell Telephone Quarterly 14 (July, 1935), 167-99.

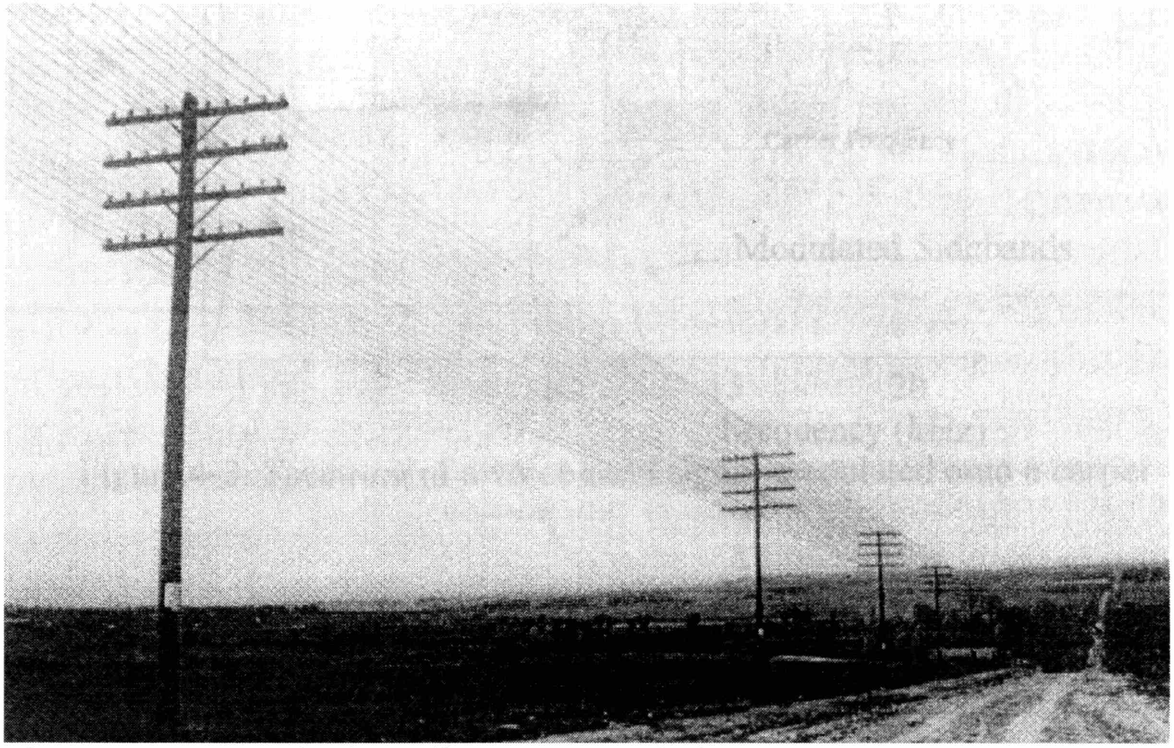
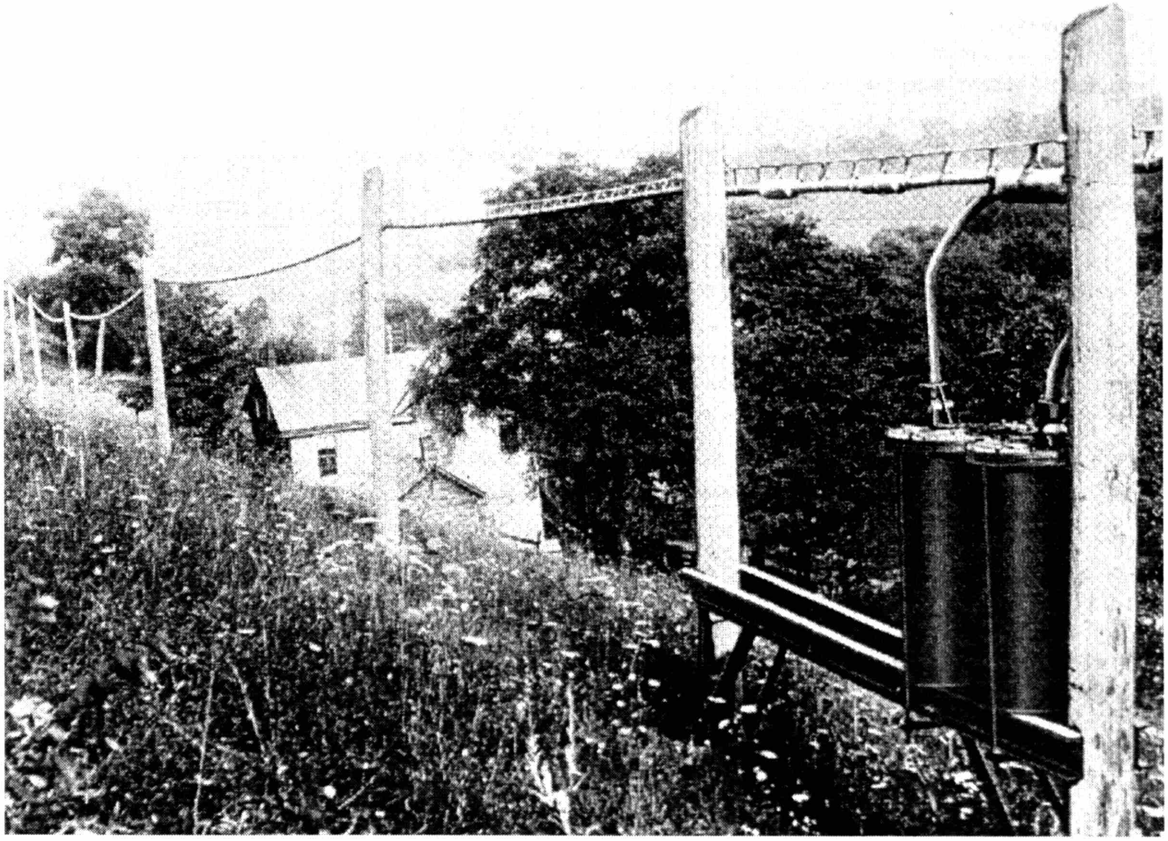


Figure 4-1: Open wire long distance transmission.

Figure 4-2: Cable transmission and loading coils (at right).



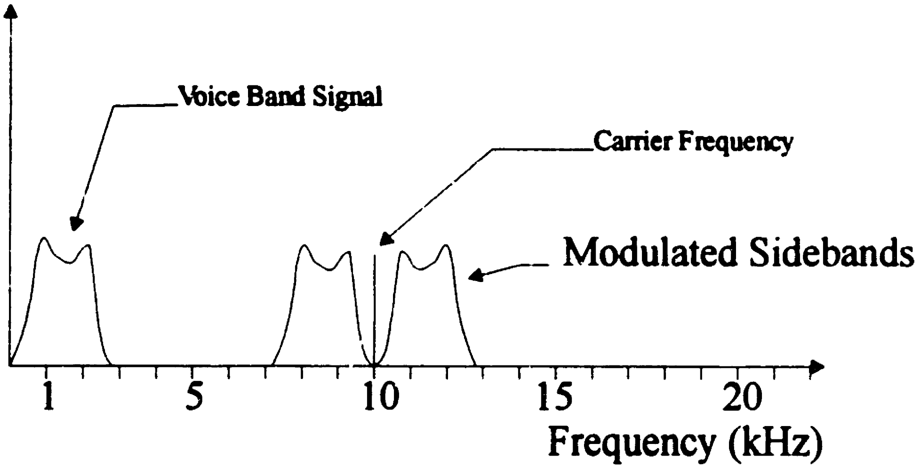
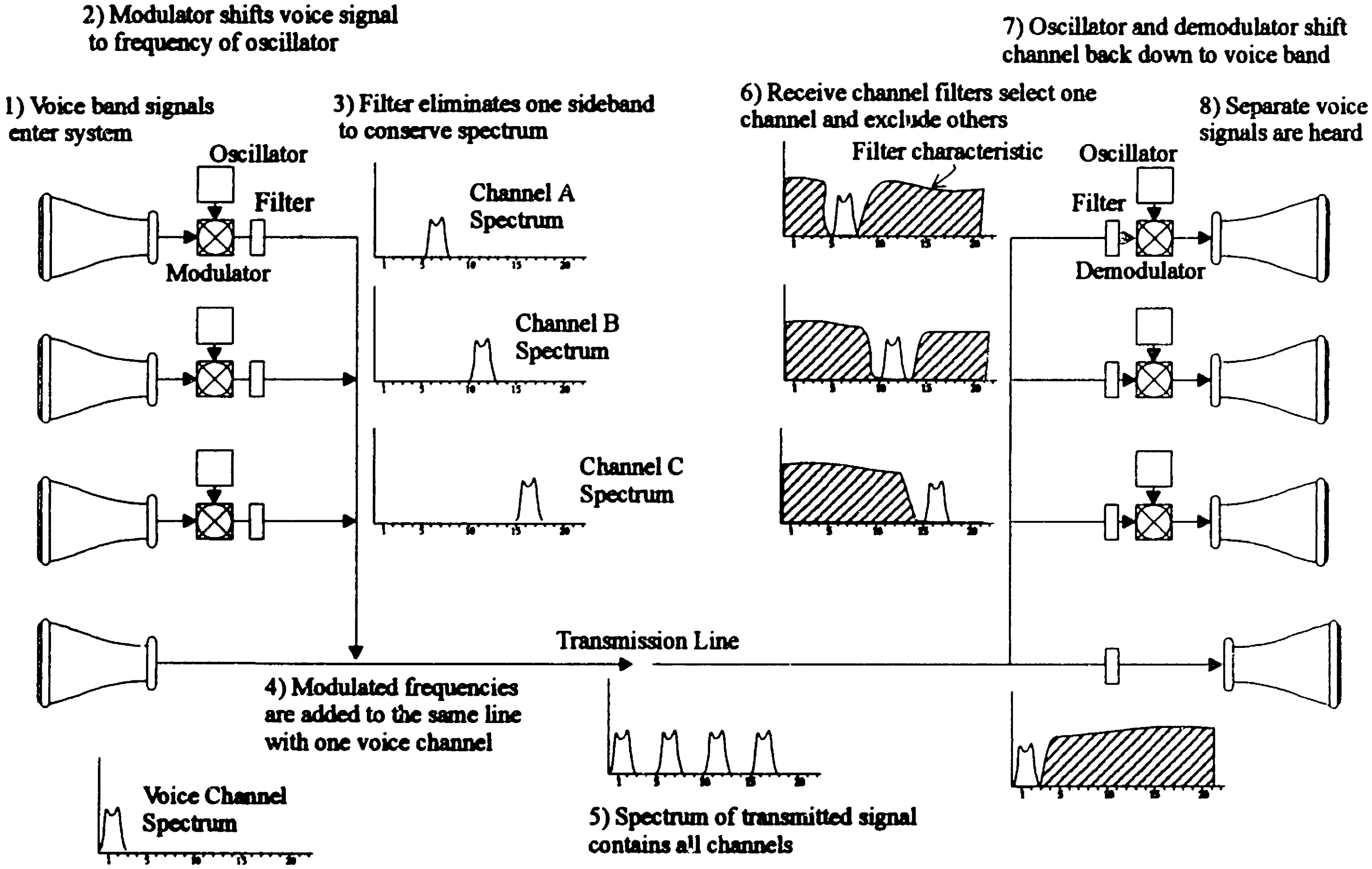
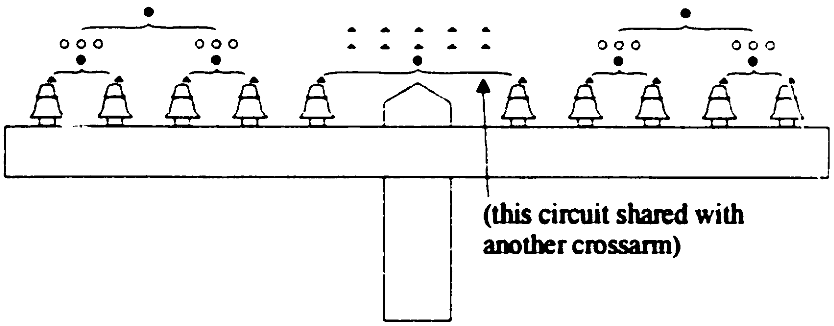


Figure 4-3: Spectrum of a voice band signal modulated onto a carrier

Figure 4-4: Carrier Modulation on Cable

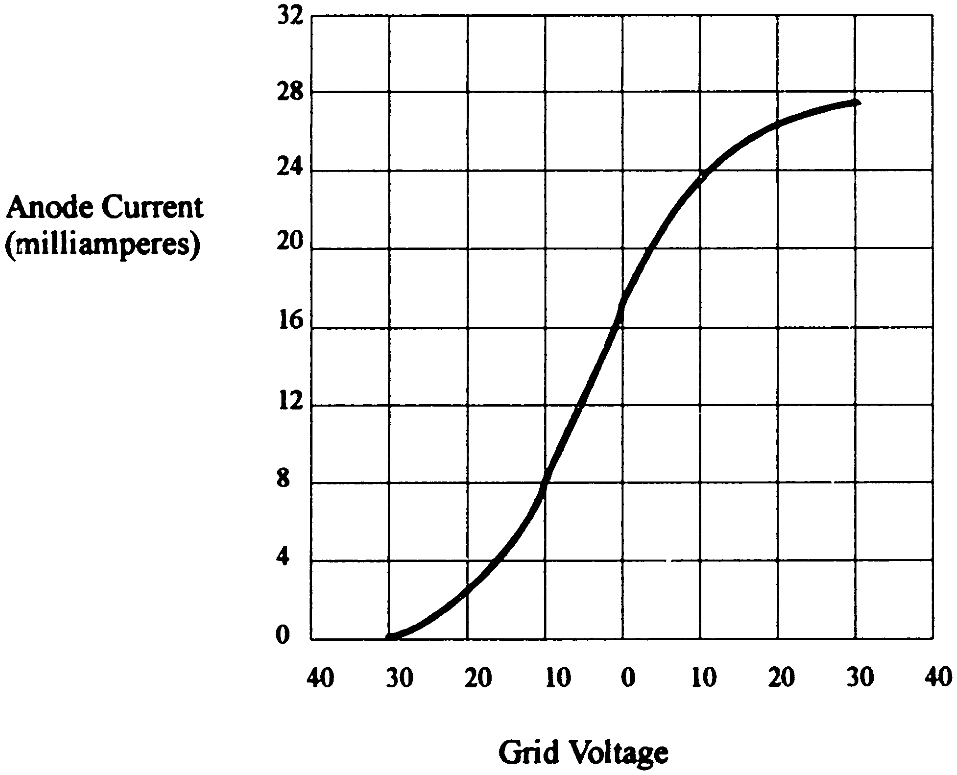




	Telephone	Telegraph
● Voice frequency telephone circuit	7.5	
○ Carrier telephone circuit	12	
▲ Grounded DC telegraph circuit		10
△ Carrier telegraph circuit		10
Total Circuits	19.5	20

Figure 4-5: Arrangement of voice freqeuncy and carrier circuits on a single open-wire crossarm. Carrier multiplexing and "phantom" circuits allow nearly twenty phone conversations and twenty telegraph lines transmitted on ten wires. (Affel, Demarest, and Green, "Carrier Systems on Long Telephone Lines," BSTJ 7, July, 1928, p. 592.)

Figure 4-6: Typical vacuum tube nonlinearity. The output anode current is not a linear function of the input grid voltage.



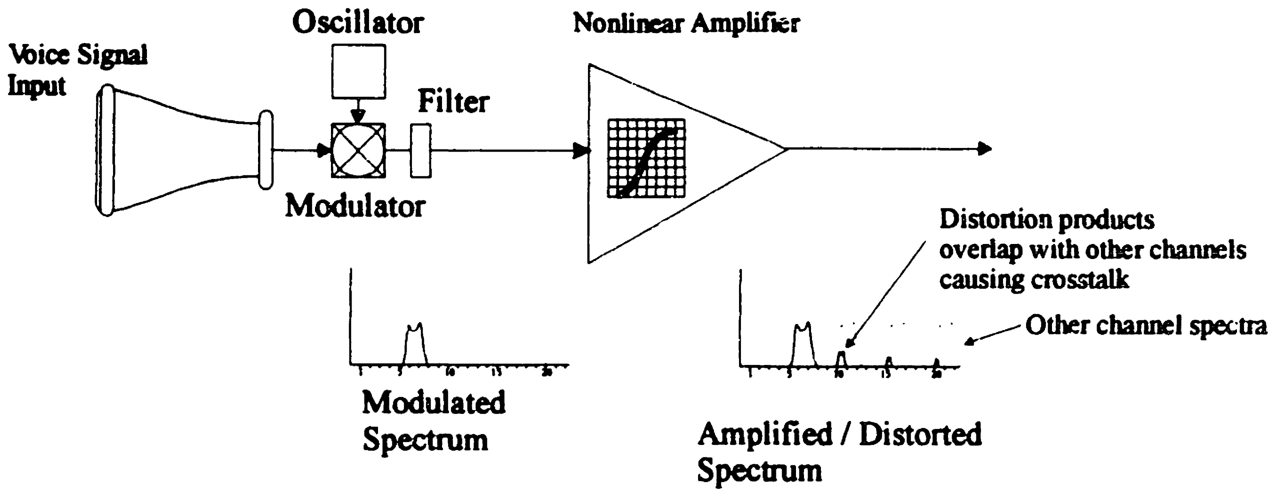
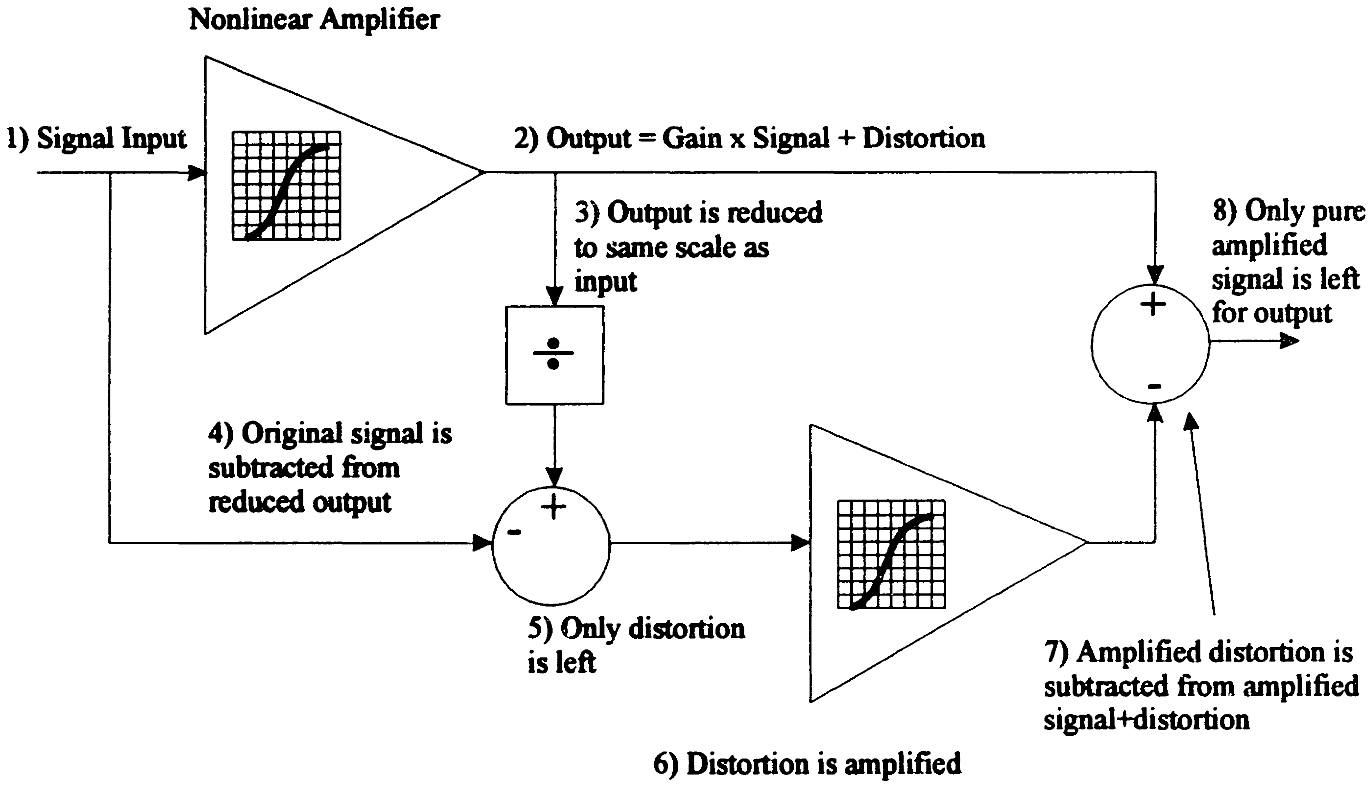
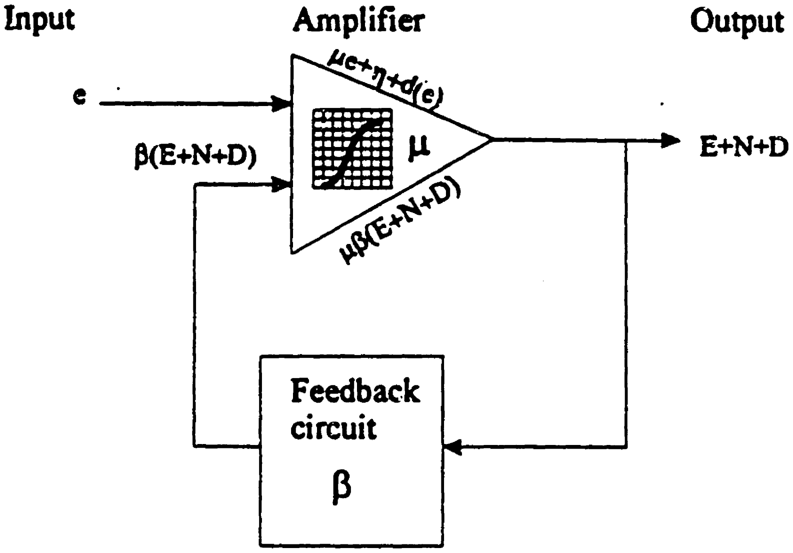


Figure 4-7: Nonlinear amplifier causing distortion and crosstalk in a carrier system.

Figure 4-8: Black's original feedforward amplifier (Harold S. Black, U.S. Patent no. 1,686,792, Figure 2).





Amplifier System with Feedback

e Signal input voltage
μ Propagation of amplifier circuit [gain]
μe Signal output voltage without feedback
η Noise output voltage without feedback
d(E) Distortion output voltage without feedback
b Propagation of feedback circuit
E Signal output voltage with feedback
N Noise output voltage with feedback
D Distorition output voltage with feedback

The output voltage with feedback is E+N+D and is the sum of $\mu e+\eta+d(E)$, the value without feedback plus $\mu\beta(E+N+D)$ due to feedback.

$$E+N+D=\mu e+\eta+d(E)+\mu\beta(E+N+D)$$

$$(E+N+D)(1-\mu\beta)=\mu e+\eta+d(E)$$

$$E+N+D=\frac{\mu e}{1-\mu\beta}+\frac{\eta}{1-\mu\beta}+\frac{d(E)}{1-\mu\beta}$$

If $|\mu\beta| >> 1$, $E = -e/\beta$

Under this condition the amplification is independent of μ but does depend on β. Consequently the over-all characteristic will be controlled by the feedback circuit which may include equalizers or other corrective networks.

Figure 4-9: Harold Black's negative feedback amplifier
(Harold S. Black, "Stabilized Feedback Amplifiers," BSTJ, January, 1934, p. 3.).

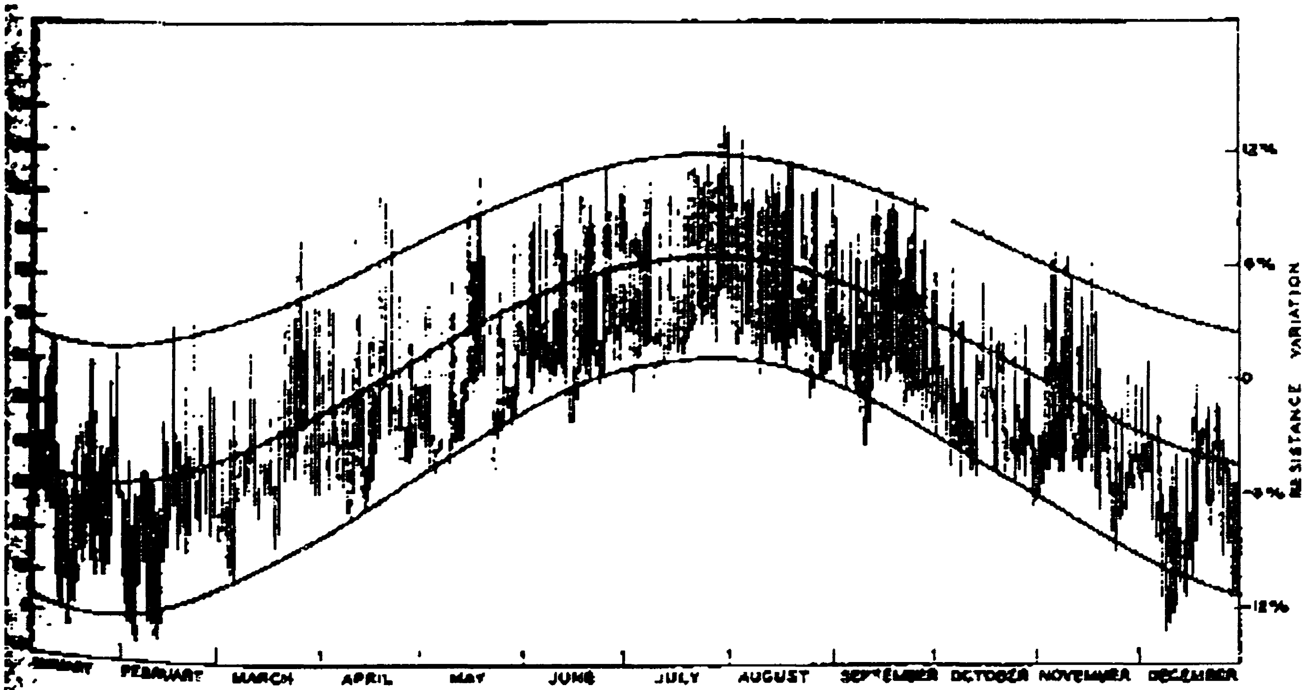


Fig. 1—Why automatic regulation is necessary—daily variations of temperature along the New York-Chicago Cable

Figure 4-10: Annual variation in line resistance along a cable (E.D. Johnson, "Transmission Regulating System for Toll Cables," *BLR* 7 no. 5, January, 1929).

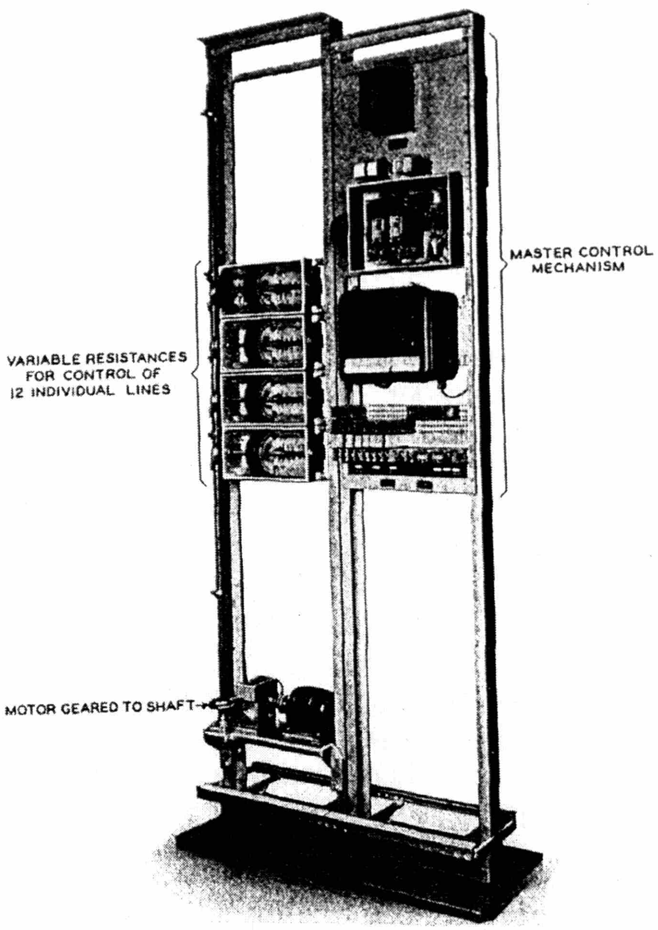
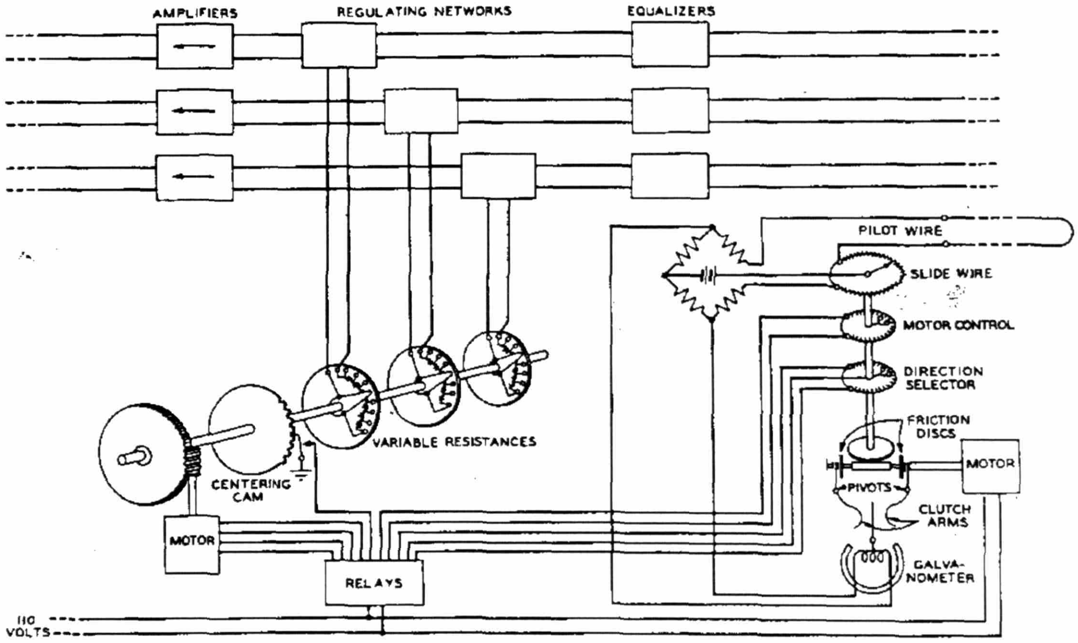
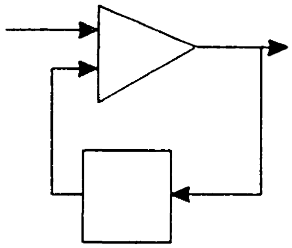


Figure 4-11: Automatic transmission regulator. Motor adjusts variable resistances according to temperature changes to keep transmission constant (A.B. Clark, and B.W. Kenall, "Carrier in Cable," *BSTJ* 12 (July, 1933)).



Feedback amplifier



Progress of successive waves around loop

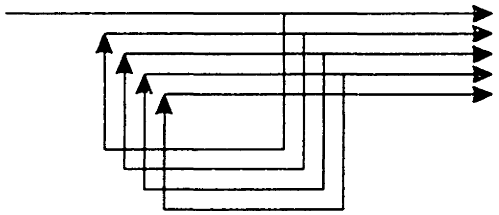
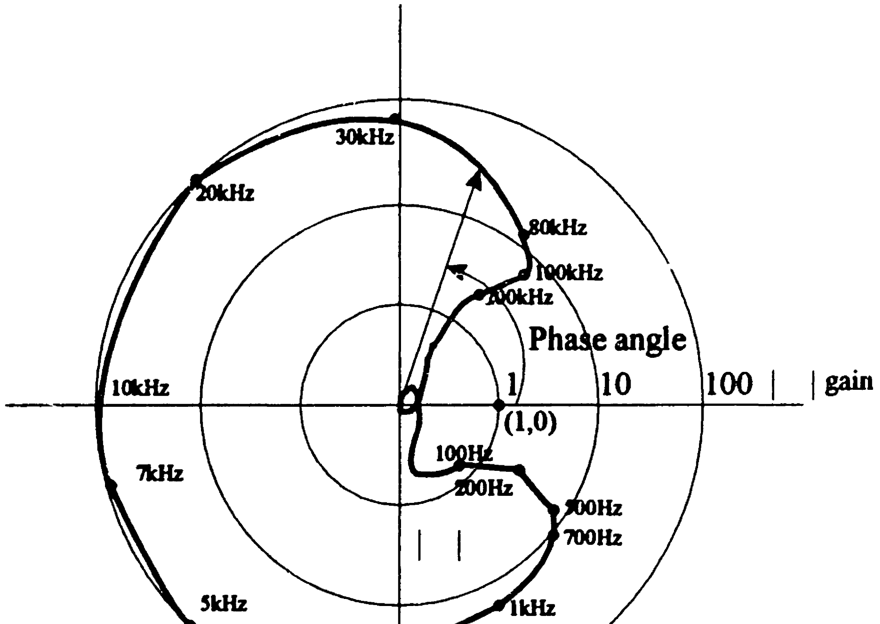
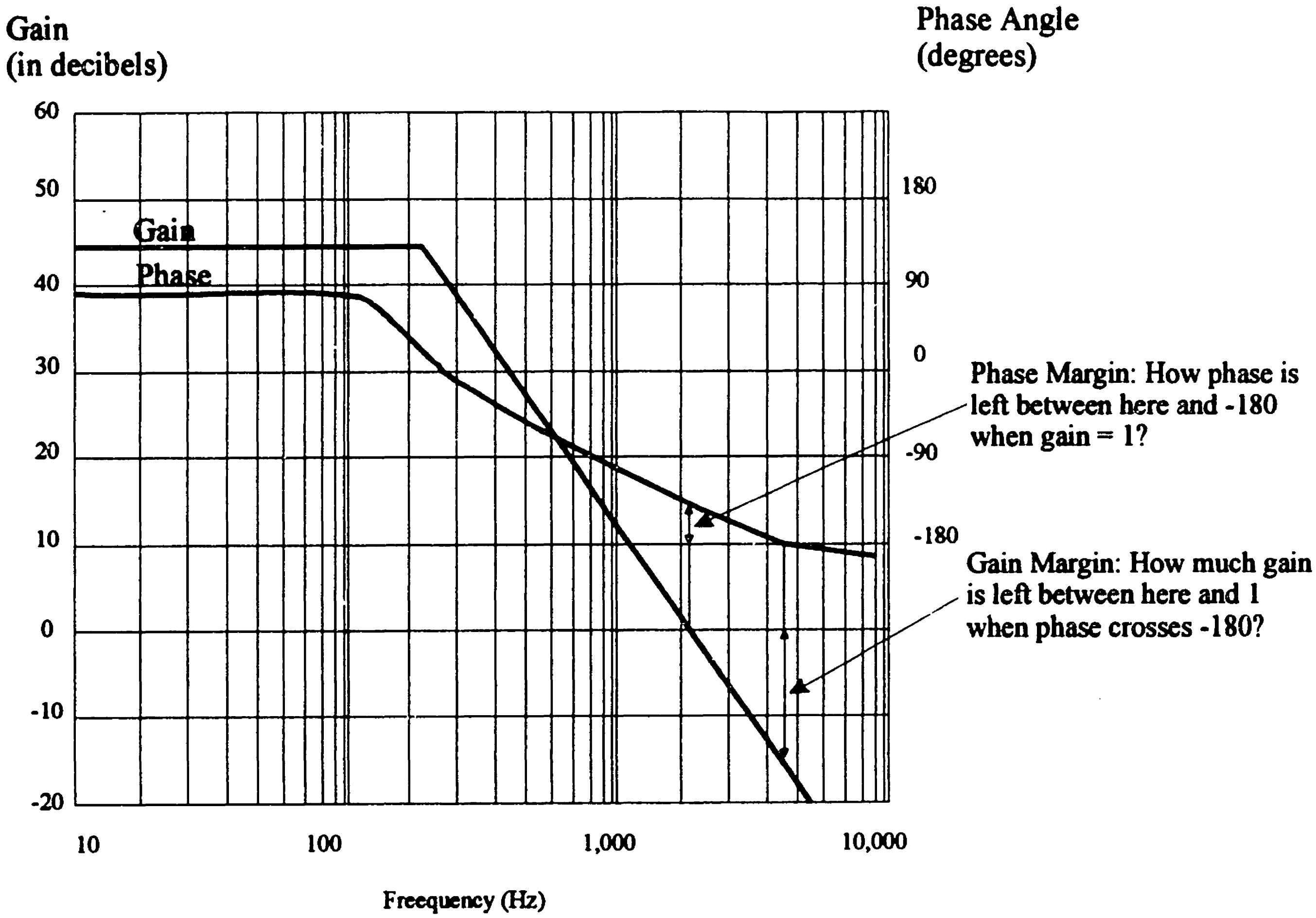


Figure 4-12: Harry Nyquist: "For the purpose of studying the singing condition, it is permissible to study the feedback condition as a series of waves..." (H. Nyquist, discussion of a paper by H.S. Black, "Stabilized Feedback Amplifiers," Electrical Engineering 53, September, 1934, 1311)

Figure 4-13: Original style Nyquist diagram, showing gain vs. phase angle plotted on a polar plot. Since the curve does not enclose the point (1,0), the system is stable. If curve did enclose that point, the system would be unstable (H. Bode, "Feedback: The History of an Idea," 114).

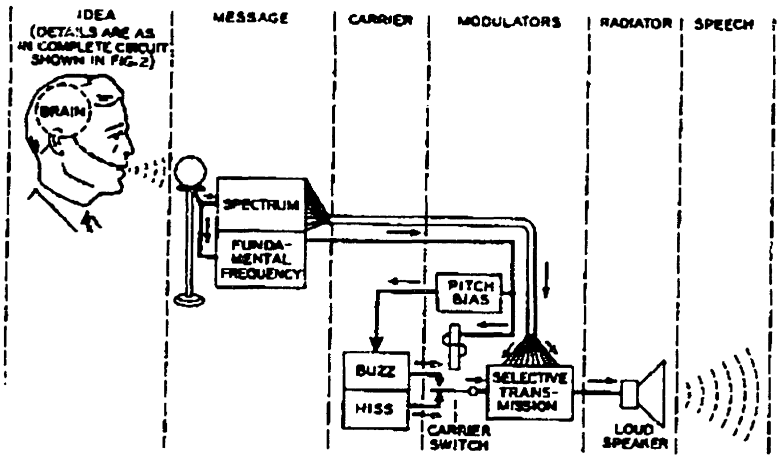




Phase Margin: How phase is left between here and -180 when gain = 1?

Gain Margin: How much gain is left between here and 1 when phase crosses -180?

Figure 4-15: Bode Plot



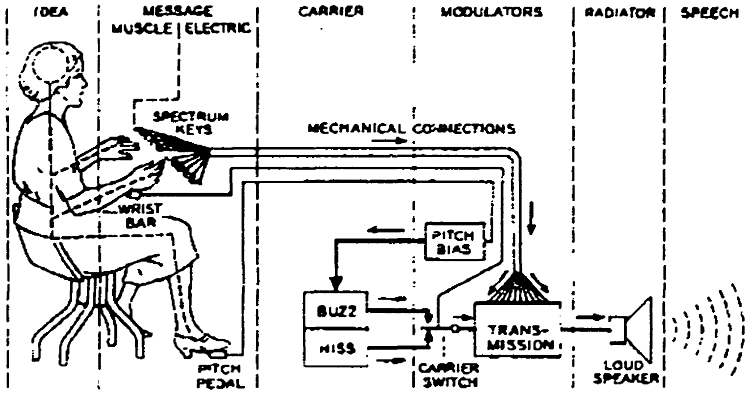


Figure 4-16: Voder and Vocoder block diagrams showing physiological analogies with telephone messaging and information flow across human/machine boundaries (from Homer Dudley, "The Carrier Nature of Speech," BSTJ 19 (no. 4, October, 1940)).

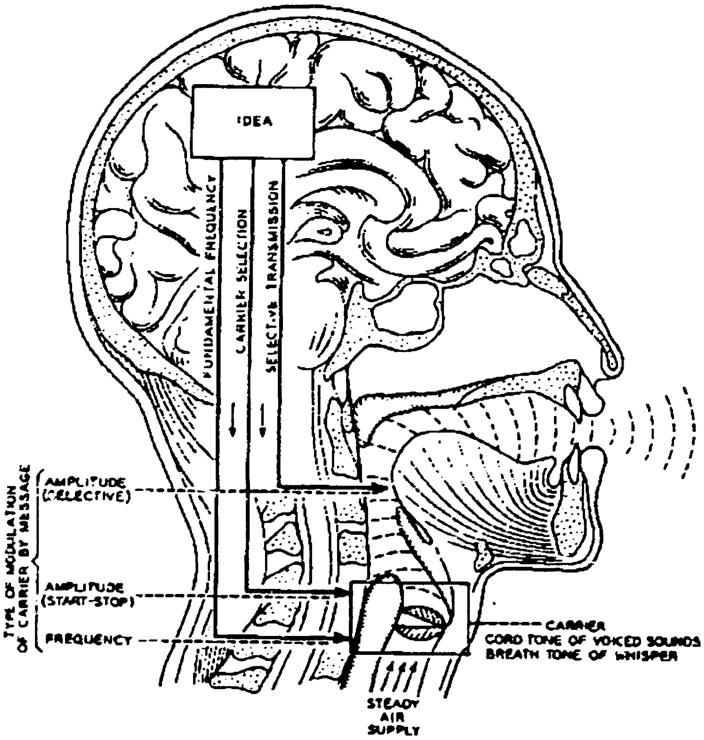


Fig. 1—The vocal system as a carrier circuit.

Chapter 5

"Artificial Representation of Power Systems:" Control at MIT in the 1930s

BuOrd and its contractors developed weighty and intricate fire control systems. Engineers at Sperry Gyroscope tamed powerful machinery. Bell Labs engineers created a feedback theory for the telephone network. To these three, dissimilar threads, we add one more group of engineers who created a theory for servomechanisms. Researchers at MIT shifted the focus of automatic control from static to dynamic behavior, a conceptual shift from "regulation" to "control." They worked neither with communications nor military technology, but with the other large system of the time, electric power. Servomechanism theory emerged in the struggle to stabilize not turrets or airplanes, but powerful, distributed, and synchronous electric systems. This chapter narrates this process and traces its intellectual background in a conception of machinery as a simulation and a representation of the physical world. Through servos, MIT researchers translated problems of electric power systems into problems of computing and data manipulation.

At MIT, Vannevar Bush led and inspired this translation. With his electrical engineering students, Bush built machines to simulate electrical power networks. Bush's student and colleague Harold Hazen combined emerging engineering science with the existing culture of feedback engineering to produce a series of machines, the most famous of which is the Differential Analyzer. Much has been made of these devices as early "computers," but they also contributed to the history of control. Every stage of their development concerned feedback, stability, servomechanisms, man/machine interfaces and the control of machinery. Their most lasting effects were not as hardware but as a spawning ground: the MIT machines influenced automatic control primarily through the people involved. Their experience building differential analyzers contributed to the development of personal technological styles and shaped later work. It also forged an important connection: in the late thirties the Bureau of Ordnance became interested in servomechanism theory applied to fire control and sent junior gunnery officers to study at MIT. When war broke out, these men became the vanguard of a massive effort to bring engineering science to the control of gunfire.

Feedback Culture

Engineers at BuOrd, Ford Instrument, or Sperry Gyroscope belonged more to a common "feedback culture" than to a discrete practice of "control engineering." The latter presupposes a recognition of "control" as a distinct intellectual, theoretical, and professional activity — a recognition that only began to emerge in the 1930s. Rather, "feedback culture" refers to a set of techniques, tools, knowledge, and, above all, a group of people who were skilled in applying feedback mechanisms.[1] Although successful in applying negative feedback mechanisms to a broad array of machinery, feedback culture had two defining characteristics. First, it primarily concerned regulation and stability, the behavior of machines in a *steady state*. One would set the speed of an engine, or the course of a ship, and the regulator or servo would "hunt" back and forth while settling on that speed or course. The feedback mechanism maintained *consistency* in the face of external "disturbances," such as changes in the load on an engine or changes in wind pushing on a ship. One cared less about the machine's behavior while the setpoint was changing than its ultimate stability. Hunting was acceptable as long as settled out in a reasonable amount of time. Sperry's gyrostabilizer and gyrocompass, for example, sought to stabilize a ship along its roll axis and about a particular course. Similarly, automatic pilots kept airplanes straight and level, but they did not provide high maneuverability. Feedback culture emphasized the steady state, and not the transient or dynamic behavior of machines, systems, and processes. Regulation and stability were the order of the day.

The second characteristic of feedback culture was its reliance on a set of practices, techniques, and mechanisms, without a unified theoretical framework. Published work in the field nearly always discussed specific systems not understood to obey the same laws: steam governors, voltage regulators, automatic steering devices, or autopilots. Few discussed general theory, most addressed particular applications, "The Sperry Automatic pilot," "Automatic steering," "Controlling load, maintaining frequency."[2] Some, mostly mathematicians and physicists, had made theoretical analyses of feedback devices, but nearly all examined governors and regulators, not other types of feedback mechanisms. In 1867, James Clerk Maxwell attempted a taxonomy of existing regulators and governors. Maxwell's paper, "On Governors," Otto Mayr points out, was

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[1] The concept of "feedback culture" expands on Donald MacKenzie's idea of "gyro culture" in Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance (Cambridge: MIT Press, 1990), 31.

[2] These are paper titles referenced in Hazen's 1934 papers. See note below.

incoherent in terminology and definition and lacked the idea of a closed feedback loop so fundamental to later conceptions of control. Maxwell had other goals besides understanding and improving feedback mechanisms; he sought to use mechanical gearing as an analogy to electrical circuits. Mayr wrote "in the world of engineering, the paper was ignored," and he found no references to it between 1867 and World War I.[3] Another commentator noted the paper's "terse and inconsecutive style," and suggested "On Governors" was "probably written in a forced attempt to free [Maxwell's] mind from control problems" in order to do physics.[4]

Others also presented theories of the stability of motion, relating it to the problem of stability in mathematical equations. In his paper, Maxwell had called for a straightforward way to determine system stability, and British physicist E.J. Routh responded with such a method. German Adolph Hurwitz independently formulated the same approach. Any system can be described by "characteristic equations," taking into account the physics of the system itself (e.g. laws of motion) and the forces to which it is subjected. Routh and Hurwitz developed a "stability criterion" which uses the coefficients of those equations to determine stability without actually having to solve the equations completely. The Routh-Hurwitz "stability criteria" are in use today by control engineers to determine the stability of a given system (with a yes or no answer), but not to provide quantitative measure of that stability nor to specify system behavior while the setpoint changes or during disturbances.[5] They are analytical tools and not design tools. Furthermore, a successful theory needs a receptive audience, and much of this work remained too theoretical and mathematical to be of use to practicing engineers, who were not widely scientifically trained until the early twentieth-century and hence could not apply theoretical tools with sophistication.

A 1919 textbook provides a representative window into the state of engineering knowledge in regulation theory. Governors and the Governing of Prime Movers by Professor W. Trinks of the Carnegie Institute of Technology, aimed to bring a certain unity to the study of regulating mechanisms, noting that in engineering schools, "instruction in governors is given in a scattered fashion." One learns about steam-engine governors in a course on steam engineering, Trinks pointed out, pressure regulators in a hydraulics course; rarely is the subject treated as a

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[3] Otto Mayr, "Maxwell and the Origins of Cybernetics" in Philosophers and Machines (New York: Science History Publications, 1976), 168-88.
[4] A.T. Fuller, "The Early Development of Control Theory," *Trans. ASME, Jour. of Dynamic Systems. Measurement and Control* (September, 1976), 224-235.

whole. He identified the two common functions of the governor, measuring some quantity and then varying an energy supply to keep that quantity constant: perception and articulation. "A governor, then, is both a measuring device and a motor." Trinks, extensively citing European and American publications, analyzed stability, the "promptness" of the governor's return to equilibrium, the natural period of vibration, and a host of other behaviors for a variety of governors. He raised modern issues in control, always regarding particular mechanisms, never general systems, and made no mention of other feedback systems such as autopilots or ship stabilizers.

An early insight into the theoretical side of control systems, as opposed to regulators and governors, came not from Elmer Sperry or his engineers but from Nicolas Minorsky. Minorsky, a Russian Immigrant engineer, had served in the Russian Navy during the first world war. After emigrating to the United States in 1918, he worked for four years as an assistant to Steinmetz at General Electric in Schenectady, New York. Minorsky himself shared Elmer Sperry's interest in human interactions with machines. At G.E., as well as in Russia, he studied the human eye's ability to perceive a ship's angular motion in deviating off course. Based in part on those observations, Minorsky and G.E. installed a prototype automatic steering gear for testing on the battleship New Mexico (like G.E.'s fire control system, its automatic pilot posed a commercial threat to Sperry Gyroscope). Minorsky's 1922 "Directional Stability of Automatically Steered Bodies," addressed automatic ship steering as a feedback problem.[6] In this paper Minorsky considered not only the desirability of replacing the human operator, but also the naval advantages of better gunfire, speed, and cruising radius — his analysis seems to have in mind the Sperry Gyropilot, also installed on the New Mexico. Minorsky debunked Sperry's claim that its device, or even a human pilot for that matter, "anticipates" a turn in any meaningful way:

> It has often been stated that the human intuition of the helmsman cannot be replaced by any mechanical contrivance whatever its nature may be. Such a standpoint seems to be erroneous, as far as the problem of automatic steering is concerned, since there is not so much question of intuition as of suitable timing based on actual observation. Once the

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[5] Bennett, A History of Control Engineering, 1800-1930, (London: Peter Peregrinus, 1979), 148.

[6] Nicolas Minorsky, "Directional Stability of Automatically Steered Bodies," *J. Amer. Soc. of Naval Engineers* 34 (1922), 280-309. See also Stuart Bennett, "Nicholas Minorsky and the Automatic Steering of Ships," *IEEE Control Systems* (November, 1984), 10-15.

> element of observation is removed from the helmsman, there can be no accurate steering whatever his intuition may be.[7]

Minorsky understood the human's task as a feedback loop: the quality of his steering depends on "suitable timing based on actual observation" and not some magical factor named "intuition." Minorsky went on to discuss a number of different methods of feedback control and introduced what became famous as the "three term controller." This technique, which remains standard practice, feeds back not only the error signal itself, but a weighted sum of the error signal, its time-derivative (i.e. rate of change), and its integral (i.e. its accumulation over time); it thus acquired the name PID for "proportional plus integral plus derivative" control. Still, the work was not widely recognized at the time. Later, in 1937, Minorsky published a complete analysis of the Sperry Gyropilot showing it to be of the "proportional plus acceleration" type.[8]

By the 1930s feedback devices found new uses in a widening array of machines. But these controls, like the Sperry Autopilot, were still built and adjusted by intuition, not based on a rigorous understanding of control or stability. Engineers designed their controllers to mimic the behavior of the human operator. Minorsky's rejection of the idea of "anticipation," represents a first step away from anthropomorphism and toward mathematics. Now one could understand how the behavior and stability of an individual servomechanism related the system overall. What behavior, for example, did the Sperry's "phantom" in the gyrocompass, share with the servo that controlled the rudder, or the larger ship-rudder-gyropilot system? Could one predict the closed-loop behavior of a system in a quantitative way? In addition, engineers, increasing mathematically literate and scientifically trained, were receptive to new formulations.

Theory of Servomechanisms

In 1934, MIT Professor of Electrical Engineering Harold Hazen published two papers in the *Journal of the Franklin Institute* which began to transform the empirical feedback culture into modern control engineering. The two papers, entitled "Theory of Servo-Mechanisms" and "Design and test of a High Performance Servo-Mechanism" overcame the two limitations of the

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[7] Minorsky, "Automatically Steered Bodies," 282-3. Also see Bennett, A History of Control Engineering, 1800-1930, 142-47.

[8] Nicolas Minorsky, "The Principles and Practice of Automatic Control," *The Engineer* 1937. See also Stuart Bennett, "Development of the PID Controller," *IEEE Control Systems* (December, 1993),58-65.

feedback culture at the time.[9] First, Hazen's papers shifted the emphasis from steady-state, regulation mechanisms to dynamic systems relying on continuously varying inputs or setpoints (hence the "high performance" of the second title). And second, the papers proposed a theory of feedback loops of all types, from local servomechanisms to large-scale feedback systems. The second paper built on that theory by describing the design of the highest performance servo yet built. Earlier papers on feedback control appear strange to modern engineers. Hazen's papers so changed the language of the topic, however, that their methods and terminology are easily understood by one familiar with present day control engineering.[10]

The first paper, "Theory of Servo-Mechanisms" provides not only analytical theory, but definitions and taxonomy of feedback mechanisms. Hazen begins by describing the rise of automation and how it parallels the rise of mechanization. Where powerful machinery replaced human muscle power, he writes, automatic machinery will replace human operators. His language resembles that of Sperry Gyroscope at the time, even though he primarily discusses servos that replaced no human function. He distinguishes between "open cycle" and "closed cycle" control (what today would be called open and closed loop control). In open cycle, "control is actuated by some quantity such as time which is independent of the result of the control operation."[11] Such control would not fall under the definition of "feedback control" but is better termed "remote control."

The second type, "closed-cycle," is Hazen's primary concern. Sensors for providing the feedback in such a system, the perception function of the governor, are generally low-energy devices, whereas the machine to be controlled tends to require higher power for articulation. Hence the servomechanism, integrating perception and articulation, is fundamentally an amplifier, amplifying the relatively weak signal from a sensor to drive a more powerful device. Hazen defines a servomechanism as,

> a power-amplifying device in which the amplifier element driving the output is actuated by the difference between the input to the servo and its output.

He gives the example of an automatic ship steering device:

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[9] Harold Hazen, "Theory of Servo-Mechanisms," *JFI* 218 (September 1934), 279-331. Harold Hazen, "Design and Test of a high-performance Servo-Mechanism," *JFI* 218 (November 1934), 543-580.

[10] Stuart Bennett makes this observation in his thorough analysis of Hazen's contribution, A History of Control Engineering, 1930-1955 (London: Peter Peregrinus, 1993), 110.

[11] Hazen, "Theory of Servo-Mechanisms," 279.

> Considering the ship-steering example, this disparity [of power level] exists between the energy magnitude associated with the measuring instrument, a compass, and that associated with driving the rudder. In small craft with a direct wheel-to-rudder drive, the helmsman serves as a human servo-mechanism.[12]

Hazen not only employs Sperry's automatic device as an example but also echoes Sperry's inherently backward-looking term, "human servomechanism." As with Minorsky's work, in these early formulations the notion of the servomechanism was never far from that of the human operator. But Hazen's theory extended Minorsky's move, bringing the servo into a more purely technical world (Hazen cited both the Sperry and Minorsky papers).

Hazen distinguishes as well between "servomechanism" and "control system." In 1873, Frenchman Jean Farcot introduced a device "to put any motor or engine under the absolute control of an operator by the movement of his hand." This was the *servo-mechanism* (meaning slave-mechanism).[13] The servomechanism controls a particular device, which might be part of a larger control system. The term servomechanism usually refers to a device that controls position, as opposed to regulating speed. As in the ship steering example, the servo itself moves the rudder and maintains its position according to a setpoint. In Elmer Sperry's time, such devices (often steam-driven) commonly aided the helmsman in moving the rudder, but they could not perform automatic steering. Another loop attached the rudder servo to a compass or gyroscope in order to keep the ship on course. Sperry closed this loop by including his gyrocompass, the rudder servomechanism, and the ship in his Gyropilot control system. This subtle distinction between servomechanism and control system eventually breaks down; later in the paper, Hazen notes that the two are analytically identical.

This "systematic" classification provided the consistent hierarchy of servomechanisms which Maxwell had attempted without success. "Theory of Servo-Mechanisms" defines three types of servos: relay, pulsed, and continuous. In a relay or on-off servo, "widely used because of its simplicity" the actuating force is constant in magnitude when present. A common thermostat works this way because it only controls the binary state of the furnace, turning it on when the actual temperature is too low (i.e., lower than the desired temperature) and off when it is too high (i.e., higher than the desired temperature). The second type, the pulsed servo, operates during

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[12] *Ibid.*, 281-283.

[13] Joseph Farcot, Le servo-Moteur ou Moteur Asservi (Baudry, Paris, 1873),quoted in Bennett, A History of Control Engineering 1800-1930, 100-101.

regular, fixed intervals, affecting a sort of periodic correction. Although not Hazen's primary interest, it represents common type of "discrete time" servo used by digital control systems today. Hazen was among the first to recognize the difference between these "sampling systems" and on-off relay systems.[14] His primary interest, however, was the third type, continuous control, "in which the restoring force, acting continuously on the output element, is approximately proportional to the deviation of the output," a technique still known as "proportional control."

"Theory of Servomechanisms" analyzes the three types in turn, evaluating each for oscillatory response to an input and for time lag in following an input. He emphasizes time lag because of his interest in dynamic performance; he cares how the servo performs while the input is changing. He concludes that the continuous type is best "where high-speed response and smoothness of control are required," and "has the advantage of being susceptible to rather easy and complete analysis." As part of the analysis, Hazen establishes a unitless "figure of merit" for servos, which could be used both to evaluate performance and as a quantitative basis for design (what today would be called "damping ratio"). His "easy and complete analysis" of the continuous type servos also shifted emphasis away from the simpler relay-type systems and toward the higher performance continuous controllers. He studies the two primary behaviors of these feedback devices, "oscillation and lag." Because of physical effects, the output of the servo does not exactly follow the input, instead it "lags," and if it is too far off, i.e. input and output are "out of phase," the device can oscillate. This situation parallels the phase shift in the feedback network in an amplifier, which will cause oscillation if it reaches 180°.

Hazen argues his theory of servomechanisms can be applied to the speed control of steam turbines and water wheels, the stabilization of ships by gyroscopes, the operation of gyrocompass repeaters, the automatic stabilization and guiding of aircraft, and "in fact the automatic recording or control of almost any measurable or measurable and controllable physical quantity." The mention of this broad field of applications shows that he intended his work to be the beginning of a unified theory, and he adds, "to the writer's knowledge no systematic quantitative treatment of even the simple common types has previously been given." Hazen 's numerous references nearly

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[14] Trinks described a "relay" governor as one which used an additional source of power (such as a hydraulic valve) besides the rotating balls to regulate an engine; a definition different from Hazen's. For sampled data systems, see Eliahu I. Jury, "On the History and Progress of Sampled-Data Systems," *IEEE Control Systems Magazine* (February, 1987), 17.

all mention control mechanisms for specific machinery or classes of machinery; none mention generality or theory.

Hazen not only proposes a general theory of servomechanisms, but suggests it can be expanded to a general theory of systems. He adds "entire closed-cycle control systems are dynamically similar to servo-mechanisms and their operation is investigated by the same methods," thus breaking down the distinction between servos and larger control systems. Numerous diverse kinds of machinery could be considered analytically identical systems.

Still, Hazen's unifying vision did not extend to electronics. While he defines servos as power amplifiers, Hazen distinguishes them from simple electronic vacuum-tube amplifiers (or mechanical amplifiers). "The servo-mechanism," he writes, "differs from the simple amplifier in that the responsibility for the functional relation is not placed directly on the amplifying element of the servo." This statement implies the actual amplifying element in the servomechanism need not be a precision device (it has no "responsibility for the functional relation"), as long as it provides adequate power to bring the feedback signal into correspondence with the input, "such an amplifier element can be a relatively crude affair." Curiously, however, when distinguishing his mechanical servo from an electronic amplifier, Hazen compares his closed-loop example not with a feedback circuit but with an open-loop amplifier — he did not recognize the servo's parallels with closed-loop electronic amplifiers. His statement that "responsibility for the functional relation" need not lie with the amplifying element mirrors Harold Black's discovery that a negative feedback amplifier did not require a high-quality linear vacuum tube but merely a high quality feedback network.[15] Hazen recalled later he was unaware of Black, Nyquist, and Bode's work on feedback theory,

> At this time I was not aware of Routh's and Hurwitz's work. I knew vaguely of Nyquist and Bode's frequency domain work at B.T.L. which I mentally associated only with communications network theory. I did not recognize at the time the intimate and fundamental interconnection between this and the transient analysis approach.[16]

Hazen's association makes sense, for Bode and Nyquist were known in 1934 for network theory more than feedback analysis. Bode in particular was working closely with MIT electrical engineer

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[15] For another discussion of the contributions of Hazen's work, see Stuart Bennett, "Harold Hazen and the Theory and Design of Servomechanisms," *Int. J. Control* 42 (no. 5 1985), 989-1012. Bennett's chapter, "Theory and Design of Servomechanisms" in A History of Control Engineering, 1930-1955, presents a similar discussion.

[16] Harold Hazen to Stuart Bennett, October 22, 1975.

Ernest Guillemin (another Bush student) but only on network synthesis, not on feedback.[17] Even though he sought to address all feedback systems as similar, Hazen's failure to see the connection between servos and feedback amplifiers traces the frontier of his vision in 1934.

But within its sphere, Hazen's "Theory of Servomechanisms" overcame the two limitations of servo theory at the time. Where previous work had considered primarily "stability" it considered "high-speed response" as a desirable characteristic. And where previous work did not address unified theory, this paper initiated the development of general analytical tools for control systems. Generally considered fundamental to the field of control theory, Hazen's work was the subject of intense study for a generation of control engineers. The Franklin Institute awarded these papers its Levy Medal in 1935. In his memoirs, Harold Hazen, soft-spoken to the point of self-effacement, said only "this theory paper has in fact been a standard bibliography item in every subsequent paper or book in the field, and is still widely regarded as a classic."[18] A survey of work in the following ten years confirms this view; nearly all books and papers reference Hazen's paper, many as their first citation.[19]

How did Hazen unite this diverse field? What background enabled him to see that different types of machinery could be controlled according to a single theory of systems? What problems were he and his colleagues working on that led him to this formulation? Oddly, it was not immersion in the feedback culture. Hazen had little experience with the feedback engineering of the time, and certainly none comparable to the work at a leader like Sperry. He had some industrial experience in electrical power at General Electric, but no evidence suggests he had worked with governors while there. His doctoral thesis, written only three years before his servo

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[17] After corresponding on network theory for Guillemin's new book, Bode visited MIT in August, 1933. He visited again the following spring and toured the laboratories of the E.E. department, probably including Bush's laboratory. During this latter trip he participated in a colloquium on electrical filter theory; the program for the meeting lists no reference to feedback amplifiers. Another colloquium on network theory in 1936 included Guillemin, Bode, George Campbell and Thronton Fry as speakers, and Bush, Hazen, and many others from the MIT faculty as attendees, but makes no mention of feedback in the program. Hendrik W. Bode Papers, Harvard University Archives, Box 1 Folder 2. Also for a discussion of Guillemin's work see Karl L. Wildes and Nilo A. Lindgren, A Century of Electrical Engineering and Computer Science at MIT 1882-1982 (Cambridge: MIT Press, 1985), Chapter 9, a for discussion of Guillemin's work.

[18] Harold Hazen, Memoirs: An Informal Story of My Life and Work (Unpublished manuscript, MIT Archives, 1976), 3-9.

[19] See, for example, Hubert James, Nathaniel Nichols, and Ralph Phillips, Theory of Servomechanisms (New York: McGraw Hill, 1947), 16. This book, part of the famous Radiation Laboratory series, was perhaps the most influential control theory text to come out of the war.

papers, lists not a single reference to control or regulation.[20] No, Hazen's insight was not the result of long years in the feedback culture, tinkering and adjusting governors and stabilizers. Rather he came from, and had helped form, a laboratory culture intent on simulation and calculation. Hazen's experience designing and building machines that operated as representations of the physical and mathematical world enabled him, indeed directed him, to understand control as a general principle. To trace the background with which Hazen approached the servo problem, we must first examine the context of electrical engineering at MIT in the decade before his famous papers, and the problems that defined the intellectual climate at that time.

The Stability Problem

By the 1920s, regional electric power systems had proliferated throughout the country. Increasingly, they connected into interregional and national grids, the proposed "superpower" systems.[21] These networks had a number of generators (at hydroelectric, steam, and coal stations), each with feedback devices regulating voltage and frequency. Generators drove transmission lines connected to a series of loads, including factories, streetcar systems, and residential areas. The characteristics of these complex networks, however, were poorly understood. In the first decades of the century, Charles Steinmetz at General Electric laid out analytical techniques for alternating current machinery in its steady-state, and wrote on a similar theory for transient phenomena in 1920. By the early twenties electrical engineers recognized the "stability" of electric networks posed a problem but lacked consensus on how to approach it.[22] What was the stability problem?

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[20] Harold Hazen, "The Extension of Electrical Engineering Analysis Through Reduction of Computational Limits by Mechanical Means" (Sc.D. diss., MIT 1931).

[21] For the history of the interconnection of electrical power networks, see Thomas P. Hughes, Networks of Power: Electrification in Western Society, 1880-1930 (Baltimore: Johns Hopkins University Press, 1983). For a highly technical history of the control of electric power systems, from the point of view of the Leeds and Northrup Company, see Nathan Cohen, "Recollections of the Evolution of Realtime Control Applications to Electric Power Systems," *Automatica* 20 (2, 1984), 145-62.

[22] See Committee on Power Transmission and Distribution, "Annual Report," *Trans. A.I.E.E.* 46 (June, 1927). This committee included Ralph Booth and Vannevar Bush of MIT. For a general review of the subject, see C.L. Fortescue, "Transmission Stability: Analytical Discussion of Some Factors Entering into the Problem," *Trans. A.I.E.E.* 26 (February, 1927),984-994 and discussion 994-1003. For Steinmetz's work, see Ronald R. Kline, Steinmetz: Engineer and Socialist (Baltimore: Johns Hopkins University Press, 1992),148-49.

AC machinery would synchronize or destabilize in response to changes in operating conditions.[23] Steady state analyses, based on traditional transmission line techniques, were reasonably adequate for understanding power distribution during normal operation. But these methods would not suffice to study the reaction of the power network to external disturbances, short-lived events or "transients," such as lightning strikes, sudden applications of load, and short circuits. When a factory started up, for example, or a section of the grid tripped off, a transient in the form of a traveling wave moved through the network. How would this transient propagate? Would it exceed the power limits at certain points? How would the system react to the transient? Ideally, the network would damp the transient and it would die away after a short time. If the transient initiated secondary effects which caused it to grow, however, it could increase indefinitely, or until the network was damaged or shut itself down. Edison himself had experienced similar problems with governors on his DC equipment at the Pearl Street station.[24]

To understand the stability problem, first consider a system with two generators feeding a transmission line with a load at the end. [*Figure 5-1: Example network] In an AC system (which these regional systems were), the generators on the line must be synchronized, as they supply power in the form of sinusoidal waves which must oscillate together. When a new load is applied to the line, its incremental power will have to be supplied by each of the generators. They will slow down and "fail back" in phase, meaning that their rotors are not fully synchronized to the alternating current in the line. After some period of time, the governors (themselves feedback devices) on the engines driving the generators will notice the drop in speed and automatically increase the energy and hence the torque applied, so slowly they will begin to come back "into step." If the generators on the line are of different sizes, and thus have different reaction times, they will come into step differently. Then, as one generator comes into step, it can be "pushed" to far by another generator coming from behind, and it can overshoot the point of synchronism. These interactions and feedbacks can continually occur among different machines. With many different sizes, each with its own characteristics and its own speed governors, the machinery on the system oscillates or hunts about some power point. If these oscillations are small and decay

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[23] Stuart Bennett has noted this connection, but concluded "The problems of power-system stability although recognized early did not lead to any theoretical developments in control systems" in A History of Control Engineering, 1800-1930, 171.

[24] Cohen, "Recollections of the Evolution of Realtime Control."

with each successive cycle, they will die out harmlessly. If however, they grow with each cycle, the system becomes unstable: progressive oscillations will cause it to shake itself apart or to fail by exceeding its power limits.

Beginning in 1923, Vannevar Bush, a young electrical engineering professor at MIT, began to investigate transient phenomena. Bush had come to the institute in 1915 as a graduate student and earned its fifth Ph.D. in engineering the following year. During the first world war, he worked on sonar submarine detection and even published on gyroscopic stabilization of ships.[25] As a graduate student and then as a professor and consultant to Dugald Jackson's consulting firm Jackson and Moreland, Bush applied Heavyside's operational calculus to power networks. In Bush's hands, Heaviside's step-function revealed how a "step" input (analogous to a transient) would affect a system. Bush's first book, *Operational Circuit Analysis*, demonstrated how operational techniques could solve any number of engineering problems, especially in electrical circuits, but in other fields as well. The book included an index by Norbert Wiener on Fourier analysis and frequency domain techniques. Wiener, on the mathematics faculty at MIT, served as a mathematical mentor to Bush, and the two continued a close collaboration for many years.[26] Together, Bush and Wiener applied Heavyside's work to "transient analysis" to investigate the network stability problem. Their work mirrored similar developments at Bell Laboratories in manipulating electricity as signals, and in transforming between the time and frequency domains.

Year	Total E.E. degrees (S.B., S.M., & Sc.D.)	E.E. Degrees related to transients & stability	Percentage of E.E. degrees related to transients & stability
1921	61	0	0
1922	116	6	5%
1923	125	9	7%
1924	112	10	9%
1925	118	10	8%
1926	127	10	8%
1927	143	14	10%
1928	146	17	12%
1929	149	22	15%
1930	106	18	17%

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[25] Vannevar Bush, "Gimbal Stabilization," *J.F.I.* (August, 1919), 199-215. Wildes and Lindgren, A Century of Electrical Engineering, Chapter 4.

[26] Vannevar Bush, Operational Circuit Analysis (New York: John Wiley and Sons, Inc., 1929).

Network stability became a major topic at MIT during the twenties. The table above charts one measure of its importance, the number of theses submitted that addressed stability or transient phenomena. Much of the work at MIT during the period 1924-1931, especially that under Bush's tutelage, concerned these issues. A number of men who would later become leaders in electrical engineering were students during this time and studied power system stability.

One student of Jackson and Bush, Frederick Terman, would build Stanford's electrical engineering program and become "the father of Silicon Valley."[27] His 1924 doctoral thesis, "The Characteristics and Stability of Transmission Systems" addressed the proposed "superpower" systems which would span vast areas of geographic space and "must operate under conditions and near limits not approached by any of the lines now in existence."[28] He argued the problem of stability in electrical power networks intimately related to the behavior of governors, regulators, and feedback mechanisms. Not only did individual devices affect transient behavior, Terman wrote, but the characteristics of the network itself had much in common with feedback controls.

Terman's study attacked both the relationship between steady state and transient characteristics of a network and the relationship between electrical and mechanical transients. He sought to understand the effects of regulators and governors on the stability of the system, and, in turn, how the behavior of those governors were influenced by the characteristics of the network. In doing so, Terman drew explicitly on the feedback culture:

> The general problem of electro-mechanical transients in synchronous machinery is of long standing. It first came up in connection with parallel operation where reciprocating steam engines were used as prime movers and in connection with hunting...The entire reasoning can be applied to our problem with very little modification, giving a very good insight into the synchronous condenser transient.[29]

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[27] Terman's Radio Engineer's Handbook (New York: McGraw Hill, 1943), became a standard text for electronics. For Terman's later work at Stanford, see Stuart W. Leslie, The Cold War and American Science (Baltimore: Johns Hopkins, 1993), Stuart W. Leslie and Bruce Hevly, "Steeple Building at Stanford: Physics, Electrical Engineering, and Microwave Research," *IEEE Proceedings* 73 (July 1989), 1169-80.

[28] Frederick Terman, "The Characteristics and Stability of Transmission Systems" (Sc.D. diss., MIT, 1924), 1.

[29] Ibid., 168.

While Terman concluded that the instability of the individual voltage regulators was insignificant compared to electromechanical effects in the generators themselves, he found that the instability of the system overall shared much with that of an individual governor:

> An electro-mechanical transient develops very similar to the general phenomenon of hunting in which the over-swing makes the momentary power that is drawn from the system exceed that of the load alone at times during the transient.

Furthermore, the electrical transient could develop much more quickly than the generators could respond, and more quickly still than the regulators on the prime movers would kick in:

> The ability of the system to hold together under these conditions is determined by the instantaneous limits, since the action of the transient is so fast that the greatest danger comes before the air gap flux [the magnetic field in the generator] can change appreciably either from armature reaction or regulator pickup and long before steam and hydraulic governors become active.[30]

Terman's thesis clearly stated the stability problem as dependent on the complex system of generators, transmission lines, regulators and governors, each with its own characteristics and response times. The best one could do at this point was to identify the dominant features, stabilize their behavior, and hope that the slower transients would not be a problem.

In 1925, Bush and colleague R.D. Booth published a paper in the transactions of the AIEE entitled "Power System Transients" which put Terman's work into a larger engineering context.[31] Although published later than Terman's thesis, it was probably written about the same time and represents a similar state of thinking. The authors (who list themselves as Jackson and Moreland employees) begin by noting the difficulty of the problem, and three ways to attack it: mathematical analysis, test of laboratory models, and experience. These power systems were too new to have any experience built up for some time, so analysis and test were the favored tools, being "complementary. The final check of theory is by test, and the final attack on the actual problems of system design must be by analysis." As their central problem, Bush and Booth ask the following question:

> What is the degree of stability of such a network [a system of power stations connected by transmission lines and operating close to its power limits] when subjected to disturbances of the types likely to be encountered in practice?[32]

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[30] Ibid., 274.
[31] Vannevar Bush, "Power System Transients," *AIEE Trans.* 44 (1925), 229-30.
[32] Ibid., 229.

To address this problem, Bush and Booth propose a "point by point" method of calculation, whereby one starts with the steady-state of the system and then calculates how it changes for incremental parameters of time during a transient. One can then piece together how a system behaves for a certain time interval. This method requires tedious repetition, unless one has an easy method for figuring the individual points, so Bush and Booth propose a "superposition" method, whereby graphs of the machinery's characteristic curves drawn on paper can be physically overlaid to determine their operating points (i.e. solve their characteristic equations). In the context of this laborious calculation Bush first conceived his famous research program in calculating machines.

Knowledge of feedback control formed a further gap in Bush and Booth's understanding of the system's behavior. During transient disturbances "the behavior of exciters, governors, and regulators comes into play, and the mechanical constants of machines as well as their electrical behavior must be considered. "A problem arises, however, because "unfortunately complete information in regard to the behavior of all types of governors is not yet available in the form necessary." [33] The paper's discussants agreed, "A complete paper could be written on steam and hydraulic governors and such a paper would I regret to say deal chiefly with their shortcomings. I feel that there is room for a great deal of improvement and such improvement will come by studying their characteristics in connection with the problem of stability."[34] More study, of individual machines, their regulation characteristics, and their behavior when connected into systems, was definitely in order.

In addition to Terman, other Bush students examined the stability problem, through a number of approaches. Harold Edgerton came to MIT in 1926 from a period "on test" in the cooperative program at G.E. His 1927 master's thesis, "Abrupt Change in Load on A Synchronous Machine," employed the newly developed "Product Integraph" calculating machine to calculate system stability by the point-by-point method, showing ways to avoid instability by applying loads gradually. For his doctoral work, Edgerton used a stroboscope synchronized to the AC power line to "freeze" the poles of a generator. He attached white cardboard signs marked

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[33] Ibid., 232.

[34] C. L. Fortescue, discussion of Bush and Booth, "Power System Transients," *Trans. AIEE* 44 (February, 1925), 97-103. This discussion, from six commentators, provides a good overview of the state of the stability problem in 1925.

"N" or "S" for the north and south poles. Though the rotor was in motion, the letters would appear to stand still under the flashing light. When a sudden load appeared, the letters seemed to rotate backward, then slowly catch up, overshoot and oscillate about a certain position as the governor acted. [*Figure 5-2: Edgerton & generator] Edgerton's 1931 dissertation, "Benefits of Angular-Controlled Field Switching on the Pulling-into-Step Ability of Salient-Pole Synchronous Motors," presented similar studies but made with a new mercury-arc stroboscope. This device led Edgerton to his famous work in high-speed flash photography.[35]

Kenneth Germeshausen, later founder, with Edgerton and Herbert Grier, of EG&G, wrote a 1931 bachelor's thesis "The Effect of Controlled Field Switching on the Pulling-into-step of a Synchronous Induction Motor." Cecil Green, philanthropist and founder of Texas Instruments, submitted "A Static Study of the No Load Flux Distribution in a Salient Pole Alternator," for his B.S. and master's thesis in 1924. Other theses titles give the flavor of the work during this period: "The Parallel Operation of Alternators Through Long Transmission Lines," "Method of Determining the Steady State Stability of Systems Containing Tie Lines," "A Study of Synchronous Machines Not Running at Synchronous Speed," "An Investigation of the Steady State Stability of a Two Generator System," and "A Study of Induction Motors Electrically Constrained to Run at the Same Speed."[36] For electrical engineers at MIT in the 1920s, the stability problem, and the behavior of electric machinery under transient conditions, shaped the atmosphere within which students like Harold Hazen matured.

Simulation: The Network Analyzer

Two innovative strategies were adopted at MIT to deal with the difficulty of the stability problem, simulation and calculation, and Harold Hazen worked with both. The first concerned steady state solutions. After his work on transients with Booth, Bush began thinking of a way to

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[35] Harold Edgerton, "Abrupt Change in Load on a Synchronous Machine" (S.M. Thesis, MIT 1927), and "Benefits of Angular-Controlled Field Switching on the Pulling-into-Step Ability of Salient-Pole Synchronous Motors" (Sc.D. thesis, MIT, 1931). Wildes and Lindgren, <u>A Century of Electrical Engineering</u>, 145-7.

[36] Gordon Brown and Kenneth Germeshausen "The Effect of Controlled Field Switching on the Pulling-into-step of a Synchronous Induction Motor" (S.B. thesis, MIT, 1931), Cecil Green "A Static Study of the No Load Flux Distribution in a Salient Pole Alternator" (S.B./S.M. thesis, MIT, 1924), Constantine Barry, "The Parallel Operation of Alternators Through Long Transmission Lines" (S.B. thesis, MIT, 1927), Robert Caruthers and O.P. McArthy, "Method of Determining the Steady State Stability of Systems Containing Tie Lines" (S.M. thesis, 1928), Sherman Wang, "A Study of Synchronous Machines Not Running at Synchronous Speed"(S.B. thesis, MIT, 1929), George Ackock and Thatcher H. Mawson, "An Investigation of the Steady State Stability of a Two

model complex power systems in the laboratory. In 1924, he suggested to Harold Hazen, then an undergraduate, that he write a bachelor's thesis on a small-scale circuit that simulated an electrical power network. Hazen had come to MIT in the fall of 1920, having grown up in Three Rivers, Michigan; he would remain at the institute for nearly sixty years. As a youth, his Sunday-school teacher had introduced him to machine shop practice, and he built electromechanical inventions in his father's basement shop.

Power network simulations were not new; at least one was built at Edison's Menlo Park laboratory around 1880.[37] Between 1919 and 1923 O.R. Schurig of General Electric had developed a "D.C. Calculating Table" for analysis of short-circuit conditions in networks. When Schurig built a more generally applicable AC model, however, the machine itself had a stability problem, "hunting itself out of synchronism" and "shaking apart" when more than a few elements (e.g. miniature motors and generators) connected together.[38] While electrical parameters (transmission lines) could be replicated easily in miniature, mechanical components (motors and generators) did not scale well, hence the instability. Small rotating machinery just didn't have enough inertia to represent larger machines. In technical terms a small motor has it has little energy storage (inertia) compared to energy dissipation (friction), whereas a large machine is the other way around. Thus the characteristics of the simulated systems would not adequately model the bigger ones, and would be even more susceptible to instability.

Hazen solved this problem, at Bush's suggestion, by building a miniature power network that substituted "phase shifting transformers" for motors and generators. These devices (built with parts loaned from G.E.) had the same external characteristics as generating stations, but did not actually rotate. An operator could adjust them by hand, however, to vary phase shifts, which corresponded to varying loads and torques. Although the model could simulate only steady-state problems, if one used the "point-by-point" method described earlier, the machine could solve each "point" in steady state. By readjusting the machine (i.e. the phase shifts of each of the

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Generator System" (S.B./S.M. thesis, 1928), Jen Kurkuian, "A Study of Induction Motors Electrically Constrained to Run at the Same Speed" (S.B. thesis, 1929).

[37] Hughes, Networks of Power, 23. Edwin Harder would employ power system simulations and analog computers to study servo and regulator problems for many years after World War II. See William Aspray, "Edwin L. Harder and the Anacom: Analog Computing at Westinghouse," *IEEE Annals of the History of Computing* 15 (no. 2, 1993), 35-52.

transformers) for each successive point, and assembling the points into a curve, one could solve for a transient.

With the phase shifting transformers, Hazen took a step forward in his representation of machinery. His thinking evolved from "models," like architect's models as small physical representations of the world, to "analogs," representations with different physical forms but with similar analytical properties. Hazen's network model became an analog, and with it came the idea that the system's essence inhered in its abstract behavior, not its physical presence. Hazen, with fellow student Hugh Spencer, presented this work at the AIEE convention in New York in 1925. They described the construction of their miniature power system, "a simple, compact, accurate, easily manipulated laboratory scale means of solving networks." The very title of their paper "Artificial Representation of Power Systems," suggests their approach moved beyond "models," "miniatures," and even, perhaps, beyond "analogs" to a newer vision, a vision of *simulation.*[39]

How small could one make the simulation? That depended on the accuracy one needed and the quality of the measuring devices used to observe the model. Attaching a voltmeter to a full-size power network had almost no effect, as the meter imposes a negligibly small load compared to the amount of power in the system. In a miniature, however, the loading introduced by the meters of the time could seriously affect the phenomena under study. In other words, one needed to draw power from the network to drive the needle in the meter. As Hazen put it, "When you put a voltmeter on [a miniature network], it's like whacking a factory load onto the actual power system, and that doesn't do...any parasitic power requirements take a major toll on accuracy"[40] (he might have added that the inductive load of the meter could also affect stability). Thus, when building miniature networks, one started with the loading imposed by the meter, and then scaled the size of the simulation accordingly. Hazen notes, for example, that "with standard portable instruments, the current [in the miniature] must be 5 or 10 amperes with 200 or 100 volts

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[38] Harold Hazen and Hugh Spencer, "Artificial Representation of Power Systems," *J. AIEE* (January 1925), 25. Also see Wildes and Lindgren, A Century of Electrical Engineering, 99 and Harold Hazen Interview, March 2, 1977. Oral History Collection, Computers at MIT, MIT Archives, 9.

[39] Hazen and Spencer, "Artificial Representation of Power Systems," 24-31. For a discussion of models and simulations in engineering, see Per A. Holst, "George A. Philbrick and Polyphemus: The First Electronic Training Simulator," *Annals of the History of Computing* 4 (no.2, April 1982), 144. Eugene S. Ferguson, Engineering and the Mind's Eye (Cambridge: MIT Press, 1992).

[40] Harold Hazen Interview, 12.

respectively to keep errors safely below 10 per cent."[41] To redress the loading problem of the meter, the paper presents in an appendix a proposed design for a "currentless" meter based on a vacuum tube (later known as a VTVM or "vacuum tube volt meter"). This problem, the loss of energy (and hence accuracy) in a simulated system, would persist throughout Hazen and Bush's explorations of simulation and calculation. Countering the effects of loading significantly shaped Hazen's conception of the servomechanism.

After Hazen's graduation, Jackson and Bush encouraged him to stay on and pursue a graduate degree. At that time, however, the value of advanced degrees was not well recognized by the engineering community, where the notion prevailed that one could learn everything important in industry. Hazen recalled that Jackson and Bush had observed "that once a man got out from the educational institution, he never came back. He just stayed with industry."[42] Nevertheless, Hazen went to General Electric "on test" as part of the G.E./MIT co-op program, which allowed him to further pursue the simulation problem (Bush himself had spent a year there). This program, a pet project of Dugald Jackson, reflected his commitment to merge industrial experience with engineering education.[43] At G.E., Hazen (and Hugh Spencer as well) worked in the office of Robert E. Doherty, G.E.'s chief consulting engineer and a close friend of Bush. The environment was, as Hazen recalled, "shall we say, *the* high-brow engineering office of G.E."[44]

The main problem concerning G.E. during Hazen's tenure there remained, of course, power system stability. They company was then studying a 500-mile transmission line to bring Canadian hydroelectric power into New England and New York. This project posed a difficult problem which brought the field together in focused effort: Bush, Jackson and Moreland, and Westinghouse all contributed in addition to G.E. In Hazen's words,

> A five hundred mile line, it was soon found out by those who looked at it, provided a very soft, mushy electrical and energy connection between the generating in far off Quebec and the load center. And what will happen if you just suddenly throw on a little more load? Well, it will oscillate, and you can throw it out of step. The system will break down.[45]

Elsewhere Hazen described the problem as,

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[41] Hazen and Spencer, "Artificial Representation of Power Systems," 25.

[42] Harold Hazen Interview, 7.

[43] W. Bernard Carlson, "Academic Entrepreneurship and Engineering Education: Dugald C. Jackson and the MIT-GE Cooperative Engineering Course, 1907-1932," *Technology and Culture* 29 (no.3, July 1988), 536-567.

[44] Harold Hazen Interview, 20, emphasis original.

[45] Ibid.

> resembling one automobile towing another with a long elastic cable stretched almost to the breaking point. Under these circumstances any short circuit or sudden addition of load would usually snap the cable.[46]

After spending several months on this problem in Schenectady, in the fall of 1925 Hazen returned to MIT to continue his investigations as a research assistant, bringing with him equipment borrowed from G.E. He eventually entered the graduate program, although he became so occupied with building research machines that he took four years to get his Master's. Hazen's 1929 thesis, like his undergraduate work, approached the network problem through simulation. He built on the experience gained with his earlier machine to produce, in partnership with G.E., the Network Analyzer, completed in the same year.

Hazen, Schurig of G.E., and Professor Murray Gardner (who taught MIT's course in transient analysis) presented a paper on the Network Analyzer to the AIEE in 1930. The machine comprised a set of transmission lines and transformers that replicated the steady state behavior of a complex network in miniature in a laboratory setting. Like the Morristown Trial at Bell Labs, the Network Analyzer brought a geographically dispersed technical system into a single place, where it could be studied under controlled conditions. But unlike the Morristown Trial, which system the machine represented was not fixed: it was, in the author's words, "a network computing device...sufficiently extensive and flexible to represent numerous actual systems." Where Hazen's earlier simulator represented a particular network, this machine could adapt to new problems. Given a basic set of components, the user could use a plugboard similar to a telephone exchange to configure and connect the elements to represent any particular system, up to "eight generating stations, 60 lines and cables or other connecting elements, 40 loads, four ratio-changing transformers for closing loops, and any desirable number of synchronous condensers."[47] Just like the fire control systems G.E. built with their reconfigureable switchboards, the Network Analyzer was *programmable*. Hazen referred to it as a "network computer," in terms that would reappear many years later at MIT with the advent of digital computers.

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[46] Harold Hazen, *Memoirs*, paraphrased in Gordon S. Brown, "Eloge: Harold Locke Hazen, 1901-1980," *Annals of the History of Computing* 3 (January 1981), 5.

[47] Harold Hazen, O.R. Schurig, and M.F. Gardner, "The M.I.T. Network Analyzer, Design and Application to Power System Problems," *AIEE Trans.* 49 (July 1930), 872-875. See also Wildes and Lindgren, A Century of Electrical Engineering, 96-105.

The department intended this programmable network as a public simulation facility. It was not only "for purposes of teaching and research" by MIT students and staff, but "for commercial engineering service in the solution of network problems for engineers in operating and designing work." The Network Analyzer served industrial clients and power companies, including the American Gas and Electric Service Corporation, General Electric, Jackson and Moreland, Illinois Power and Light, Union Gas and Electric, and the Tennessee Valley Authority.[48] It remained operational in this capacity at MIT until the early 1950s (when dean Gordon Brown closed it down).

Hazen's work on power system stability brought him into contact with the cutting-edge engineering of the time, as it applied increasingly "scientific" methods and academic research styles to practical problems. The Network Analyzer contained in early form several features which would characterize computers built at MIT and elsewhere. A large, room-sized device, it required several operators (usually photographed in lab coats). [*Figure 5-3: Network Analyzer photo] Because of its generic structure, users could program the Network Analyzer for any given application. The simulation matched not the physical form of the object but rather its analytical characteristics. Finally, as a centralized facility available to different users, both academic and industrial, the Network Analyzer (and, as we shall see, the Differential Analyzer) initiated the institutional form of centralized computing facilities which would become common in the 1950s. Through the Network Analyzer Hazen, Bush, and MIT researchers made analytical machines acceptable and productive parts of an electrical engineering department's research program.

Calculation: The Product Integraph

The second approach to the complex power network problem was calculation, and Hazen participated in these efforts simultaneous with his work in simulation. Spurred by the high demand for calculation imposed by the "point-by-point" method of evaluating transients, Bush began building mechanical calculating machines to evaluate the high-order differential equations which described the networks, particularly the integral of the product of two functions. John Carson, who brought Heavyside's calculus to telephone engineering, had defined this integral as central to the mathematics of transient phenomena.[49]

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[48] Wildes and Lindgren, A Century of Electrical Engineering, 103.

[49] John R. Carson, "Theory of Transient Oscillations in Electric Networks," *AIEE Trans.* 38 (1919).

In 1924, Bush, with associates Herbert Stewart, a graduate student, and Gage, a research assistant produced the "Product Integraph," a mechanical analog computer for evaluating these integrals. This machine's components reflected the influence of both electric power and feedback culture: the Product Integraph used a standard "watt-hour meter" to integrate electricity over time, not unlike the devices still in use today for measuring household power usage. Bush, Gage, and Stewart give examples in their paper calculating the load on a cantilevered beam and, of course, "the problem of transients in circuits due to an applied alternating voltage." The authors note this latter problem "is obtained only by much computation unless mechanical integrations may be readily performed in a continuous manner."[50]

Integrating machines had been built before. "Planimeters," which integrated a curve or an area on a piece of paper had been in use for navigation and surveying for many years. Thornton Fry had several such machines in his mathematics department at Bell Labs. Bush and his associates needed a machine that could integrate not only the area under a curve, but a function whose upper limit had not yet been defined. The new machine became an "integraph," since it recorded the result of an integration in the form of a plot or graph.[51] Bush's point-by-point method for transient problems overlaid graphical curves to solve for the operating points of the machines, and the integraph drew those curves automatically.

To those studying power systems, even its transients, the world was smooth and continuous; the challenge was to build a machine that worked graphically as well, without the messy translations of numerical data. In today's digital world, analog computing techniques are seen as unquestionably inferior to digital methods. But Bush and his colleagues were well aware of digital machinery and digital methods (although they used the term "numerical" instead of "digital"). In fact, for them the analog, continuous nature of the machine was a decided innovation over the "numerical" calculating machines employed in offices:

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[50] Ibid., 81.

[51] Vannevar Bush, H.R. Stewart, and F.D. Gage. "A Continuous Integraph," *JFI* 211 (January 1927), 63-84. For this paper and his other 1927 with Harold Hazen (see below), the Franklin Institute awarded Bush its Levy Medal in 1928. The term "integraph," according to Clymer, refers to "a machine for plotting the solution of a given first order differential equation." "Mechanical Integrators," 57. For a history of mechanical integrating machines, see Allan G. Bromley, "Analog Computing Devices," in William Aspray ed. Computing Before Computers (Ames, Iowa: Iowa State University Press, 1990). Also see A. Ben Clymer, "The Mechanical Analog Computers of Hannibal Ford and William Newell," *IEEE Annals of the History of Computing* 15 (no. 2, 1993), 19-22 and an extended treatment, Clymer, "Mechanical Integrators," (Master's Thesis, Ohio State University, 1946).

> Business office practice has been revolutionized by the advent of computing machines. These deal almost entirely in terms of numbers, as indeed does the business man...Applied physics, and in fact many other branches of science, frequently deal, however, with functions as a whole, and usually resort to figures only as a rather laborious means of dealing with functions or the curves which represent them.[52]

Numbers, the "resort to figures," were seen as unnecessary intermediate representations between the physics of the problem and its solution in the machine. Rather, new machines were needed "which will deal directly with the functions themselves." Those steeped in the culture of power system engineering saw analog computing as an improvement over numerical computing, not a precursor to it.

For complex problems involving experimental data (which rarely comes in the form of neat equations), "the only alternative is the use of *the curve itself* as representing the function." The "functions themselves" took the form of graphs. [*Figure 5-4: First integraph diagram] To use the Product Integraph, two functions were first plotted on paper. They were then fastened to a table or "platen" which moves in one direction, say right to left, at a constant speed. Above the table, fixed sliders or rods each had a "pointer" which could move vertically across the paper as the graph moved laterally. Two human operators would use the pointers to follow the curves as they moved from left to right. These pointers were connected to potentiometers which varied in current according to the pointer position and provided the input for the integrator. "This voltage drop is therefore proportional to the ordinate of the plotted curve when the slider pointer rests on the curve." The watt-hour-meter then integrated the product of the two functions, one in its armature and the other in its field coil (a power company charges for watt-hours, the product of voltage and current accumulated over time. Hence the watt-hour meter measures the integral of a product of two functions). [*Figure 5-5: Photo of Bush & product integraph]

The output of the watt-hour meter, however, came from a delicate spinning disk. The device that measured its output, the next stage in computation (or plotting) needed to avoid loading it and losing accuracy. This situation mirrored the sensing element of Sperry's gyrocompass, which needed a "phantom" to follow the output. Similarly, a servo-motor followed the output of the watt-hour meter "in such a manner that the motor follows exactly the rotation of the watt-hour-meter."[53] This servo motor then drove the vertical position (or ordinate) of a pen

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[52] Ibid., 63.

[53] Ibid., 65.

on a third graph connected to the same table, and plotted the integral (output) curve as the table moved by.

Despite these potential loading errors, human operators contributed the most significant source of error for the Product Integraph. The input to the device, where humans followed curves with pointers, introduced errors in tracking of as much as two or three percent. A number of other mechanical and human errors all stayed within one percent. Thus the "following" required for input was the greatest source of inaccuracy of the device. Since these errors were the input to an integrator, however, they tended to integrate or average out, provided they were as often below the proper mark as above it. Where the human operators of Sperry's antiaircraft computer integrated out noise in the data, here the mechanical integrator averaged out human errors. In both cases, as with continuous aim firing, "tracking" became a difficult problem for control.

Building on the experience gained with the first machine, Hazen and King Gould built a second Product Integraph and completed it in 1927. Key to this second and, eventually, higher order machines, was the addition of a feedback mechanism. A method of "back coupling," tied the output of the integrator to its input, thus enabling the machine to solve differential equations rather than just evaluate integrals. Back coupling made the calculator into a computer. Charles Babbage had recognized the value of taking the output of one calculation and using it as the input for the next one (the key difference between his Difference Engine and his Analytical Engine). With a feedback loop, a machine could operate on its own results — making a mere "calculator" into a "computer."[54] Similarly, Lord Kelvin realized he could connect chains of integrators to solve differential equations by adding the feedback loop "Compelling agreement between the function fed into the...machine and that given out by it."[55] As Bush wrote of a later generation machine, "It is the feedback connection which 'mechanizes' the equal sign in the equation, because it applies the constraint which forces the machine to operate so as to equalize the two

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[54] Allen G. Bromley notes that "The use of feedback here [in Babbage's mechanism] is very similar to that employed in differential analyzers and analog computers." This feedback, in fact, was the primary difference between Babbage's "Difference Engine" and his "Analytical Engine" never completed. Aspray ed. *Computing Before Computers.* Babbage may have come up with the idea of using feedback in his computer when observing rotating-ball steam-engine governors while on a tour of factories for his 1832 "On the Economy of Machinery and Manufactures."

[55] W. Thompson, "Mechanical Integration of the Linear Differential Equations of the Second Order with Variable Coefficients," *Proc. Roy. Soc. London* 24 (1876), 269-271 quoted in Paytner, "The Differential Analyzer."

sides of the equation."[56] This back-coupling also paralleled the continuous cycle of correction Ford embodied in his 1915 rangekeeper, although with the human operator in the loop; the output of the calculation was fed back to compare with observed data, causing the system to converge on a solution.

Working with the back-coupled first machine, Hazen realized it could solve more complex, second-order, problems by connecting its output, the rotation of the watt-hour meter shaft, to the input of another integrator. The second machine, essentially a revision of the first, added a mechanical wheel-and-disk integrator of Hazen's design after the watt-hour-meter. [*Figure 5-7: Hazen/Bush Second Product Integraph] Where all three plotting tables in the first machine moved together, here they moved according to independent variables, which could be selected according to the particular problem. Another input platen was added as well, so the machine could evaluate a more complex integral than the product of two functions, "The net result...is that any platen may me made to move so that its displacement is equal to any one of the three variables x, y, or x."

Curiously, in the second Product Integraph, Hazen did not use Hannibal Ford's integrator. His fire control work remained secret when Hazen built the integraph in the late twenties. He recalled Bush "was privy to what Ford was doing when we were working on the differential analyzer but couldn't, because of security, tell us what he knew about what was Ford was doing."[57] Hazen still could have learned about the integrator: Ford had patented the two-ball integrator as "mechanical movement" in 1919; it was public information.[58] Ford's integrator, more accurate and capable of driving a heavier load than Hazen's wheel and disc design, would have helped solve the problem of loading, which Hazen took quite seriously. Cascading two integrators raised the problem, similar to that of metering in the network analyzer and to the "pick-off" of the watt-hour meter in the earlier machine. The second integrator needed to avoid loading the first stage and hence losing accuracy, and the second integrator also needed to drive the load of the back-coupling and the next stage.

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[56] Vannevar Bush, "A New Type of Differential Analyzer," *JFI* 240 (no. 4, October, 1945), 255-326.

[57] Harold Hazen Interview, 17, 54.

[58] Hannibal C. Ford, U S. Patent no. 1,317,915, "Mechanical Movement." Also see Clymer, "Mechanical Integrators." Curiously, Wildes and Lindgren, <u>A Century of Electrical Engineering</u>, 89, show a picture of the Ford integrator, even though it was not used in any of the MIT machines.

In 1927, the Network Analyzer and Product Integraph were proceeding in parallel, and Hazen must have had both in his head. He and Bush stated the problem and described their solution as follows,

> It is essential that these integrator shafts — in the first stage the watt-hour meter rotor; in the second, the wheel shaft [of the wheel and disk integrator] — be free from all friction and load torque, and hence they cannot directly furnish energy to drive the recording shafts. A servo-motor follower mechanism is therefore used to drive each recording shaft, and this not only reduces the necessary energy output of the integrator shafts to a negligible value, but, as mentioned above, practically eliminates bearing friction on these shafts at the same time. This mechanism is really the key to the success of the machine from the practical point of view.[59]

Mechanical positions in the Product Integraph represented numerical quantities, and the servo transmitted that information form from one stage to the next without distortion. For Hazen, problems feedback and servomechanisms first arose in the context of calculation.

In light of Hazen's use of servos as power amplifiers in the Product Integraph, a theory of servomechanisms was not as great a leap for him as it would have been for an engineer immersed in the feedback culture whose primary goal was the stability of a specific piece of machinery. The operation of the servo in the calculating machine was close to its abstract function as presented in the 1934 paper: it was a power amplifying device, and a means of making a set of elements into a system by eliminating unwanted coupling effects between them. The servo made the successive stages of the integraph into truly modular system blocks — just as human servomechanisms in the Sperry antiaircraft director renewed the information at each successive stage, and just as repeater amplifiers in the telephone network boosted the signal as it flowed through the network. The servomechanism, as a coupling between stages, began the abstraction from machinery to information: no longer were the numbers tied to the shaft positions, rather they could be renewed, *i.e.* amplified, with each successive stage. This development separated signals, which could be manipulated on their own, from their representation in machinery, which was tied to mechanical limits. The Product Integraph was an "active mathematical instrument."[60] Renewing information at each successive stage with minimal loss of accuracy meant the size of the machine was no longer limited by energy or friction. Antiaircraft computers could perform calculations of arbitrary

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[59] Vannevar Bush and Harold Hazen, "Integraph Solution of Differential Equations," *JFI* 211 (December, 1927), 586-88.

[60] Henry M. Paynter, "The Differential Analyzer as an Active Mathematical Instrument," Keynote speech to the 1989 American Control Conference, *IEEE Control Systems Magazine* (December 1989), 3-7.

complexity, telephone signals could travel arbitrary distances, and Bush and Hazen could now build much larger computers and systems.

Calculation: The Differential Analyzer

Soon, the two stages of integration in the Product Integraph extended to six. Bush's mechanical calculators entered their third generation with the construction of the "Differential Analyzer" from 1928-1931. It could perform six levels of integration to one tenth of one percent accuracy. This machine succeeded as a practical calculating device, and was applied to problems in a broad range of disciplines. Like the Network Analyzer (and at about the same time), it became a computing facility at MIT, where scientists from other departments or institutions came to run calculations.

Several institutions around the world built versions of the Bush machine. The numerous visitors to the facility sometimes left with blueprints. One staff member remembered being instructed by Bush not to explain the details of the machine to visitors from Japan.[61] Differential analyzers were reproduced at the Ballistics Research Laboratory of the Army Ordnance Department in Aberdeen, Maryland, and the Moore School of Electrical Engineering at the University of Pennsylvania, and at General Electric in Schenectady. These machines continued to influence the direction of control technology through World War II (See chapter 7). Douglas Hartree of the University of Manchester, England built a version out of an erector set. Others were later built in Ireland, Norway, Sweden, and Russia.[62]

For the Differential Analyzer, Hazen solved the stage-to-stage coupling problem with the application of a Nieman Torque Amplifier, instead of a follow-up servo in his previous type.[63] The Nieman device employed varying friction belts on rotating drums (something like a rope around a capstan on a ship) to provide very high amplification of the torque at the output of the integrator, thus taking the load off the mechanism. Nieman Torque Amplifiers were not servos like the components they replaced because they did not use feedback to hold a particular position. Nevertheless, they had one key characteristic in common with servomechanisms: amplification (Sperry Gyroscope licensed the technology to move guns in 1926). In fact, the torque amplifiers

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[61] Gordon Brown, interview.

[62] Wildes and Lindgren, A Century of Electrical Engineering, 92, See, for example, the G.E. analyzer, H.P. Kuehni and H.A. Peterson, "A New Differential Analyzer," *Trans. IRE* 63 (May, 1944), 221-28 and discussion, 429-31.

[63] Vannevar Bush, "A New Machine for Solving Differential Equations," *JFI* 212 (no. 4 1931), 447-488.

had such high gain (i.e. multiplying factor of input to output), that they could become unstable if the slightest amount of feedback, such as mechanical vibration, were to couple the output to the input. Torque amplifiers could become feedback mechanisms by accident. Bush made an explicit analogy between their behavior and that of electronic circuits:

> Now such a torque amplifier is quite analogous to a two-stage thermionic-tube amplifier, and it has many of the properties of the latter, including the possibility of self-oscillation. It was soon found, in fact, that when the amplification of such a low-input unit was raised to around 10,000 it was very prone to go into a condition of violent oscillation usually ending in disaster. This was presumably caused by a small part of the output being fed back in one way or another into the input. This problem caused quite a struggle.[64]

One of Hazen's key insights into servos, that they served fundamentally as amplifiers, thus related to the mechanisms of the differential analyzer, with its amplifying servos. Still, although they recognized the similarity of mechanical systems to electrical amplifiers in 1931, neither Bush, Hazen, nor anyone at MIT pursued the connections to the stability issues Bell Labs researchers explored at the same time.

A machine that could couple stages together without losing energy, corrupting data, or compromising accuracy could make a truly general system, infinitely extensible. The earlier integraphs had embodied a more or less fixed set of equations. But the Differential Analyzer sought "extreme flexibility," its very structure could change. It presented the user with a set of mechanical elements which corresponded to a mathematical functions and could be arranged in a different way for each problem. [*Figure 5-8: Differential Analyzer language elements] Thus, like the Network Analyzer, this was a general machine, and a programmable one. Bush compared the method of programming the Network Analyzer with programming the Differential Analyzer:

> The scheme of connecting the machine for a specific problem which has been illustrated is quite general; more so in fact than it might at first appear. It has certain features in common with the "plugging" of a desired circuit on a switchboard, and the resulting diagrams have something of an electrical atmosphere about them.[65]

Bush saw the configuration of the machine as more than a mere mechanical activity, but as an intellectual one with some degree of generality. He described the mental exercise thus,

> This [the layout of the machine] is more than a diagram — it is a process of reasoning, and as such it is recommended to those who seek to import to youth the meaning, as contrasted with the formalism, of the differential equation.[66]

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[64] Ibid., 465.

[65] Ibid., 459.

[66] Ibid., 477.

[*Figure 5-9: Differential Analyzer connected for a particular problem] By this time the work had transcended the limited goals of power system analysis, and research focused on calculating machines in their own right and what they could add to engineering and education. Still, continuing efforts in computing led Hazen to further work with servomechanisms.

The Culture of Calculation and Simulation

Hazen's 1931 dissertation, "The Extension of Engineering Analysis by Mechanical Methods," examines contemporary problems in electrical engineering and how they might be adapted to mechanical solutions. He articulates the philosophy of simulation and calculation that had emerged under Bush's leadership in the previous years,

> It is well to state the sense in which mechanical referring to computations is used in this thesis. It represents the idea of expressing [an] abstract quantity as a physical quantity, such, for example, as length, electric current, light flux, or angular displacement; of applying by physical means the mathematical concepts enumerated to this physical representation of quantity; and of obtaining as a result a physical quantity which can be returned to the abstract form.[67]

This eloquent statement of the analog art reveals Hazen's deep dedication to that way of thinking. For Hazen, numerical computation was not only "costly to apply in involved problems" but also inelegant, needlessly complex, and divorced from the physical intuition which made simulation so valuable. Numerical methods, he wrote, "have an artificiality irksome to the physically minded." Hazen thus displays his antipathy toward what would later be called digital computation, a profound, almost temperamental predisposition.

In 1931 Hazen received his Ph.D. and was appointed Assistant Professor. Although he focused more on teaching and less on the Differential Analyzer as time went on, he continued to incorporate feedback into his work. The Differential Analyzer became popular as a general computing facility for research at the institute, and pressure mounted to increase its throughput. Operators input data by curve tracing proved the primary bottleneck. To remedy this situation, Hazen, with his student Gordon Brown, designed an "automatic curve follower" which employed photocells to automatically track the curve and automate the entry of data. This device was

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[67] Harold Hazen, "The Extension of Engineering Analysis," 4.

exhibited at the Chicago Worlds Fair in 1932-33, attracting large crowds, although it was probably never used for calculation.[68]

Gordon Brown had come to MIT from Australia as an undergraduate, and through the thirties (indeed through much of his career) he followed one step behind Hazen. Like Hazen, he would remain at MIT until retirement. Like Hazen, he cut his teeth on the stability problem, collaborating with Kenneth Germeshausen on his 1931 bachelor's thesis "The Effect of Controlled Field Switching on the Pulling-into-step of a Synchronous Induction Motor." In addition to his work on the curve follower, Brown built a special meter for taking power measurements from the Network Analyzer. This device employed a negative feedback amplifier of the type that Black was developing at Bell Labs. Still, although Brown cited Black's work in a paper on the device, nothing suggests Brown perceived an analogy (much less an identity) between servomechanisms and electronic amplifiers with feedback.[69] Brown would follow Hazen as MIT's control systems expert and found MIT's servomechanisms laboratory. Brown then succeeded Hazen in 1952 as department head of Electrical Engineering.

Brown's 1934 M.S. thesis and his 1938 dissertation both dealt with the "Cinema Integraph," a further line of research into methods of integration.[70] Norbert Wiener, who advised Bush's laboratory on calculating machines, suggested a way to further "lighten" the load on mathematical mechanisms. Plot images of functions on film, shine light through the film, and integrate the light passing through with photocell. King Gould built an infrared version of this device in the late twenties, and Truman Gray built a visible light machine, the "Photoelectric Integraph" in 1930. Gordon Brown's device used movie film for images of functions. Although the device anticipated the need for faster electronic integration, it proved a dead-end intellectually, and never became the general-purpose computing facility the Differential Analyzer did. The

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[68] Harold Hazen, Jacob J. Jaeger, and Gordon S. Brown, "An Automatic Curve Follower" *Rev. Scientific Instruments* 7 (September 1936), 354-357. Gordon Brown, interview with author, August 26, 1994. In later years, other groups built automatic curve followers which proved more practicable.

[69] Gordon S. Brown, "An Amplifier Wattmeter Combination for the Accurate Measurement of Watts and Vars," Gordon Brown papers, MIT Archives. This paper cites Black's "Stabilized Feedback Amplifiers," *Electrical Engineering* 53 (January, 1934), 114-120 and B.D.H. Tellegen, "Inverse Feedback," *Phillips Technical Review* 2 (October, 1937), 289-94. See also G.S. Brown, "Field Testing of a New Cosmic Ray Meter in Colorado," *MIT VI-A News* 12 (August, 1934), 1-4.

[70] Gordon S. Brown, "A Photocell Receiver and a Direct Current Vacuum-tube Amplifier for the Cinema Integraph," (S.M. thesis, MIT, 1934), "The Cinema Integraph: A Machine for Evaluating a Parametric Product Integral," (Sc. D. thesis, MIT, 1938).

Cinema Integraph did function, in Brown's words, as "a machine for producing dissertations."[71] It also produced Hazen's servomechanism theory.

The mechanically complex Cinema Integraph used a servo to position the film. Since the machine sought to improve the speed of the earlier integrators, Brown described it with the language of automation, "The adjustments to the position of the balance shutter, and hence the operation of the recording mechanism, do not require the attention of the person operating the machine. They are performed automatically by a small, high-speed servo motor."[72] The Cinema Integraph also included a servomechanism to operate a light shutter to accurately measure light flux through the film by a null-balancing technique. For these problems Harold Hazen designed the "high performance servomechanism" he described in his in his 1934 Franklin Institute paper. At Bush's urging, Hazen generalized the results in the "Theory of Servomechanisms" paper.

Nonetheless, Hazen himself did not see the Differential Analyzer an influence on his formulation of servomechanism theory. The emphasis on dynamic behavior, he explained, responded to the need for speed in the Cinema Integraph,

> The high performance servomechanism...was developed in order to permit the above integral to be evaluated as accurately and rapidly as possible as the parameter y was changing continuously. Thus you will see my interest in servomechanisms was not associated with the Differential Analyzer work, in which I was also very much involved, but rather with the ultimately-non surviving Cinema Integraph.[73]

This statement reveals a difference between engineers' conception of historical causality and that of the present study. For Hazen, the influential context for his servo work was the project in which it was immediately applied. Here, however, we are concerned with the institutional environment in which he was raised and trained, and the type of problems he and his colleagues faced. Concerns about feedback, stability, and even servos themselves consistently shaped the atmosphere of simulation and calculation which led to Hazen's papers.

Some see the Differential Analyzer solely as the first practical means of machine computation, a view which overlooks its institutional history and makes it merely a "point" in the progress of computers, and not a component of power system engineering or engineering culture.

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[71] Gordon S. Brown Interview. See, for example, John H. Howard, "Measurement and Analysis of Errors in the Cinema Integraph," (S.M. thesis, MIT, 1939), and Walter R. Hedeman, "Numerical Solutions of Integral Equations on the Cinema Integraph," (Sc.D. thesis, MIT, 1939).

[72] Gordon S. Brown, "The Cinema Integraph: A Machine for Evaluating a Parametric Product Integral," *JFI* 230 (1&2, August, 1940), 33.

[73] Harold Hazen to Stuart Bennett, October 22, 1975.

Going further, Larry Owens has integrated the Differential Analyzer into its educational environment, writing of the machine as a graphical language, making differential equations concrete for pedagogical purposes.[74] He cites a passage where Bush describes the ability of the Differential Analyzer to provide "the man who studies it a grasp of the innate meaning of the differential equation." Bush recalls how the machinist in his laboratory learned from the machine without formal mathematical training:

> I never consciously taught this many any part of the subject of differential equations; but in building that machine, managing it, he learned what differential equations were himself....he could discuss the problem with the user and very often find out what was wrong. It was very interesting to discuss this subject with him because he had learned the calculus in mechanical terms — a strange approach, and yet he understood it..[75]

We can see the Differential Analyzer, however, as still more fully embedded in the rich context of academic engineering of the period.[76] Together, the Differential Analyzer and Network Analyzer represent two distinct but similar ways of attacking a complex problem. In fact, in the introduction to his major paper on the Differential Analyzer, Bush himself noted that the calculator provided only one class of solutions, and that it needed to be complemented by the simulation machine, "not any one machine, nor even any one program of development, can meet these [computational] needs."[77] By this time Bush was clearly interested in mechanical calculation for its own sake. Still, the origins of the Differential Analyzer are apparent in his list of published work that employed the machine. Seven out of ten references include the word "stability" in their title or concern issues of transients in power networks.

Both projects, the Network Analyzer and the Differential Analyzer, typified the state of engineering science in the late twenties and early thirties, initiated by Karl Comptons' "Technology Plan:" solving problems of large-scale interconnected networks, abstracting them

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[74] Larry Owens, "Vannevar Bush and the Differential Analyzer: The Text and Context of an Early Computer," *Technology and Culture* 27 (no.1 1986), 87.

[75] Vannevar Bush, Pieces of the Action (New York: Morrow, 1970), 262.

[76] Owens gives a detailed but standard account of the development of the differential analyzer, and a more informative account of the MIT's computing "facilities" and organization, especially of the later Rockefeller Differential Analyzer. His main argument, however, that the Differential Analyzer was a "text" for teaching "graphic language" to engineering students, that it "embodied an engineering culture belonging to the first decades of our century," relates only to the "graphic language" of mechanical drawing and not to the "graphic" nature of the power system stability computations nor the simulations of the Network Analyzer.

[77] Bush, "A New Machine for Solving Differential Equations," 448.

into systems, and working in close connection with industrial laboratories.[78] The simulator attracted more interest from the industrial world, whereas the calculator applied more to academic and scientific problems (only one of the first five of copies of this machine was built by an industrial firm). Both the Differential Analyzer and the Network Analyzer employed machinery as a representation, as an analog of something else.

In 1932, Hazen wrote a history of mechanical calculating machines and explained the importance of the approach he and Bush had developed. Sometimes simulation may be the preferred approach: "in specific cases, where a physical problem is involved, models or analogies may replace the need for the solution of algebraic equations as such....A significant example...is the alternating current MIT Network Analyzer." Recall his aptly-titled paper "Artificial representation of power systems."[79] For Hazen, the atmosphere of simulation and calculation tightened the connection between machinery and theory, sometimes without the intervening stage of mathematics. This perspective enabled him to understand the behavior of the servo as an independent entity, divorced from the difficult and often distracting contexts of steering ships, stabilizing aircraft, or regulating prime movers.

It was the servomechanism, Hazen writes, with its power amplification, that distinguished the MIT machines from previous generations of mechanical calculators. With characteristic clarity, he relates the need for amplifiers in calculating machines to the innovative "carry" mechanism in Pascal's calculator:

> Incidentally, this problem of carrying a figure from one denomination to the next higher is one of the most critical in the design of such machines. This may be appreciated by noticing what must happen when the number one is added to the number 99,999,999. Any appreciable friction in the mechanism will result in a locked machine.[80]

Lord Kelvin understood the potential of mechanical integrators, but could not incorporate them into complete systems, or useful calculating devices: "A most serious limitation which Kelvin and earlier men faced was the discrepancy between the energy available from a delicate, accurate

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[78] David Noble, America By Design (New York: Oxford University Press, 1977). See also Carlson, "Academic Entrepreneurship and Engineering Education," and Alex Soojung-Kim Pang, "Edward Bowles and radio engineering at MIT, 1920-1940," *Hist. Stud. Phys. Bio. Sciences* 20 (no. 2, 199),313-337, Larry Owens, "MIT and the Federal 'Angel:' Academic R&D and Federal-Private Cooperation Before World War II," *Isis* 81 (1990), 188-213.

[79] Harold Hazen, "Working Mathematics by Machinery," *The Technology Review* 34 (May 1932), 326. Spencer and Hazen, "Artificial Representation of Power Systems."

[80] Hazen, "Working Mathematics by Machinery," 325.

calculating mechanism and that required to operate dependent apparatus."[81] Hazen placed the MIT machines into this tradition of calculating machines, including the Cinema Integraph, "At present this line of attack is being further developed with the idea of obtaining a rapid, accurate evaluation of parametric integrals and an effective computational solution of certain physically important integral equations.[82] Hazen's work on servomechanisms was intimately related to the problem of power amplification in a calculating machine, and hence took a place in his own construction of the history of mechanical calculation.

In light of Hazen's servomechanisms work, then, the Differential Analyzer was not only a calculating machine; *it was also a control system*. It had all the critical elements of the governor. Human operators input data, providing perception and observation. Mechanical and electrical calculations integrated the data (mathematically) and the components (systemically). A mechanism of articulation expressed the output, a literary technology which used a servomechanism to move a pen to create an output graph. Thus the experience of MIT researchers applied to control systems in general, and they began to broaden their scope. Hazen used the Network Analyzer to model the currents in the Boston city water system and he built a machine to simulate water flows in the Cape Cod Canal.

Despite this wide ranging application of servo theory, it remained confined to the world of mechanisms and did not extend into electronics. Hazen and Bush, despite analogies of their mechanical systems to electronic amplifier, did not equate servomechanisms with feedback amplifiers. Hazen's transient analysis retained the time-domain legacy of power system engineering, and not the frequency-domain techniques characteristic of telephone engineering. In 1938, for example, Hazen commented on a paper which described a negative-feedback electronic amplifier designed to make sensitive measurements on a network analyzer. He discussed the amplifier feedback in detail but made no mention of his servomechanism theory. In the mid-thirties, however, an MIT undergraduate began to change the situation and to merge the two approaches. John Taplin studied mathematics under Norbert Wiener, electronics under Ernest Guillemin, and power systems under Murray Gardner. Taplin read Black and Nyquist's articles on

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[81] *Ibid.*, 345, see also Harold Hazen Interview, 12. Lord Kelvin had also been involved in the genesis of the fire control systems for Dreadnought-era battleships, and was on the board of directors of the company of Arthur Hungerford Pollen, who designed those systems. See John Testuro Sumida, In Defence of Naval Supremacy: Finance, Technology, and British Naval Policy 1889-1914 (London: Routledge 1989), 78.

feedback and recognized the similarity to Hazen's work. As he recalled in a recent interview, "They were all studying the same thing but they called it by different names." Taplin consulted with Nyquist on the telephone, and designed a servomechanism using frequency domain instead of the MIT transient analysis techniques.[83] Taplin himself left MIT for an industrial career when he graduated in 1935 but beginning with him, slowly and hesitatingly, theories of servomechanisms and negative feedback amplifiers began to merge.

The Rockefeller Differential Analyzer

While servo theory began to generalize in the late thirties, MIT's calculating machines also extended their reach. The Differential Analyzer, despite its success as facility and its flexibility for diverse problems, had a critical limitation. Every time the machine ran a new problem, it had to be disassembled and rearranged according to the new equations, a cumbersome, time consuming, and failure prone-task. Each new problem meant building a new machine. In 1935, Bush initiated a project funded by the Rockefeller Foundation to automate tese rearrangements. Instead of rotating shafts to interconnect the calculating units, this new machine would transmit its data electrically. Calculating units, such as gear ratios for division and multiplication could also be set up "by remote control." A central "switchboard" interconnected all the units, which could then be rearranged simply by resetting the switches, just like in naval fire control.

This new machine, known as the "Rockefeller Differential Analyzer," proved both more complex and more versatile than the earlier machines. It took more than five years to build, and did not go into service until 1942. It had 18 integrators, could be expanded to accommodate thirty, and worked to an accuracy of one part in ten thousand. The integrators, similar in structure to Hazen's earlier wheel and disk type, now used a glass disk for better accuracy. The mathematical units connected together through a compact, single-unit servomechanism, which used Hazen's recent work to implement much higher performance than the earlier servos (and the proportional-integral-derivative type control first proposed by Minorsky). Viscous dampers in the system improved stability. The central switching function was implemented by a crossbar switch, borrowed from the telephone network, "A trunking system similar to telephone practice is used in

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[82] Hazen, "Working Mathematics by Machinery," 345.

[83] John Taplin, interview with author, August 10, 1995, Wellesley, Mass. Notes in author's posession. Also see Bennett, A History of Control Engineering, 1930-1955, 90. Taplin is mentioned in James, Nichols, and Phillips, Theory of Servomechanisms, 16.

order to provide paths by which any [data] transmitter can reach any receiver. Bell Labs, in fact, donated their prototype crossbar (which connected any input to any other output) when they completed their development of the device."[84] The Rockefeller Differential analyzer, with its combination of servos and telephone switches, combined control and communication.

A user could set up any mathematical problem merely by selectively opening and closing the switches. That selection was determined by a punched paper tape, which contained "a four digit code," determining the relay switch closures and hence the configuration of the machine. This paper tape represented the program of the analog computer. The "automated" Rockefeller Differential Analyzer translated the process of setting up the machine from physical rearrangement to punching the right codes in the paper tape. The machine produced its output as numerical printouts on an IBM electrical typewriter which "'reads' the storage relays and writes down the result." A centralized "supervisory control panel," could run the whole thing by "remote control." [*Figure 5-10: Rockefeller Differential Analyzer]

With its switched routing of analog signals, The Rockefeller Differential analyzer was a hybrid analog/digital machine. It mirrored hybrid systems in the other institutions examined in previous chapters. Naval fire control routed analog information from instruments of perception through similar banks of switches, reconfiguring the system for different situations. The Bell System learned to manipulate conversations, television pictures, and telegraph messages as abstract signals, routed by "intelligent," banks of relays. And MIT's calculators manipulated their mathematical quantities through a "trunking system" according to a generalized program punched on paper tape. Each of these systems derived, directly or indirectly, from the basic governor structure of perception, integration, and articulation. And each spawned, through institutional, intellectual, and personal connections, the transformation of control, computing, and communications that would come with world war.

One further MIT student articulated the potential of the hybrid system. Claude Elwood Shannon had come to the institute as a research assistant on the Differential Analyzer in 1936 after earning dual bachelor's degrees in mathematics and electrical engineering. Shannon wrote on the mathematical theory of the Differential Analyzer, but he also became interested in the relays themselves and their potential for computation. Shannon's 1937 Master's thesis, "A Symbolic

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[84] Bush, "A New Type of Differential Analyzer."

Analysis of Relay and Switching Circuits," examined the logical structure and synthesis of relay circuits "in automatic telephone exchanges, industrial motor-control equipment, and in almost any circuits designed to perform complex operations automatically."[85] Building on the structure and notation of electrical network theory, Shannon applied Boolean Algebra to systems of relays, and showed they could be analyzed and synthesized with binary arithmetic. These relay circuits were one source from which Shannon's work would evolve into information theory, via the route of fire control.

Blocked Out by the Fog of War: Naval Control Systems at MIT

It would take a war, however, to solidify these continuities, connections, and analogies. The wartime transformation began for Harold Hazen and control at MIT in 1936. The Bureau of Ordnance, recognizing the importance of Hazen's work, asked him to develop a course on servomechanisms. The request flowed from a minor but continuous connection between the Navy and the institute. Bush had long served as an officer in the Naval reserve, and, as Hazen later recalled, "through his [Bush's] Navy connections, he knew that the solving of the differential equations for trajectories of projectiles underlying the production of range tables for artillery could be handled by this device [the differential analyzer]."[86] Indeed, in the twenties Bush did reserve duty on the battleship Texas, which tested the first Ford Rangekeeper prototype in 1916. Bush and Hannibal Ford never met, but in his memoirs Bush acknowledged Ford's machines could do nearly all of what the differential analyzer could do but many years earlier. Hazen too joined the Naval Reserve, and spent his only time on active duty in 1936 working with the Bureau of Ordnance learning about fire control. His memoirs give no flavor the experience, probably owing to secrecy.[87] But BuOrd was having trouble with the stability of turret servos connected to rangekeepers, particularly in the Mark 37 director. The bureau wished to send four officers per year to MIT to learn about the new servomechanism theory and study its application to fire control problems.

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[85] Claude E. Shannon, "A Symbolic Analysis of Relay Switching Circuits," *Trans. AIEE 57* (1938). "Mathematical Theory of the Differential Analyzer," *Jour. Math. and Phys.* 20 (no. 4, December, 1941). Both reprinted in N.J.A. Sloane and Aaron D. Wyner, ed., Claude Elwood Shannon: Collected Papers (New York: IEEE Press, 1993).

[86] Harold Hazen Interview, 17, 54.

[87] Harold Hazen, Memoirs. Vannevar Bush, Pieces of the Action (New York: Morrow, 1970), 183

In 1938, Hazen began planning a special course in controls but soon handed the work over to Gordon Brown, who had just joined the faculty.[88] Hazen's 1934 papers marked the end of his direct involvement in research; his withdrawal from teaching in 1938 resulted from the parallel technical and institutional progress of control at MIT. That year Bush, who had been Vice President and dean of the engineering school at MIT since 1932, was named president of the Carnegie Institution in Washington D.C. He relinquished his MIT post in the beginning of 1939, initiating an administrative reshuffling. Edward Moreland, then head of the department of electrical engineering, replaced Bush as dean of engineering, and Hazen replaced Moreland as head of electrical engineering, a post he was to hold until 1952.

With Hazen as department head, then, Gordon Brown took over the nascent control course and research in control at this critical time. Brown began teaching control systems to four naval fire control officers from the Bureau of Ordnance and a two students from Charles Stark Draper's lab in the fall of 1939. The four Lieutenants, Edwin Hooper, Lloyd Mustin, Alfred Ward, and Horacio Rivero, sometimes called the "four horsemen," stood at the intersection of two major pre-war threads of control systems: naval fire control and servomechanism theory. Partly because of their fortuitous arrival at this intersection, and partly because of how they applied what they learned at MIT during World War II, all four became admirals. They had graduated from the Naval Academy in 1931 or '32 and served as gunnery officers in the fleet for several tours. At the time, gunnery represented the high-profile career for bright young officers, "before the real surge of glamour of naval aviation," Hooper recalled. He had been accepted to MIT in Mechanical Engineering but went to Annapolis instead. Rivero had rejected Rhodes Scholarship in favor of his commission. He struggled to get into gunnery from a career in communications, because "Ordnance was *the* thing in the navy, the exciting thing, especially when you go to war and fight."[89] During their tours, they had intimate contact with technical details "company representatives spent time on board ironing bugs out of the new fire control, the remote control of guns, and other new devices." All four had been sent to postgraduate school in

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[88] Gordon Brown, interview with author, August 27, 1994.

[89] Edwin B. Hooper, Oral History Interview by Richard T. Glascow and Nelson Wood, August 22, 1978, and Interview by A.B. Christman, Feburary, 1971, Edwin B. Hooper Papers, Oral Histories Folder and Box 10, Library of Congress. Horacio Rivero Oral History Interview by John T. Mason Jr., May 20, 1975, Naval Operational Archives. Also see admiral's biographies for Hooper, Rivero, Mustin, and Ward, Naval Operational Archives and Edwin Hooper Oral History Interview by John T. Mason Jr., June, 23-26 1970 in the Hooper biography folder.

gunnery at the Naval Academy in 1938, a typical stop for a rising career. After a year of "PG" work at the academy, the head of the ordnance group at the postgraduate school selected them for an atypical stop, the new course, "Fire Control," at MIT. They arrived in September, 1939, unsure of what to expect.

MIT had numerous naval officers as students, but mostly in aviation and construction. The Bureau of Navigation and not BuOrd ran the postgraduate program at MIT, so when the four horsemen arrived at MIT, they didn't quite fit in. The university did not think they had enough time, in two semesters, to get Master's degrees, and the navy captain in charge at MIT agreed. But the four Lieutenants insisted, causing some friction with their superior. They soon found they had taken on a bit more than they could handle. Although the previous year at Annapolis had been spent preparing for the MIT course, it proved inadequate; Hooper, Mustin, Ward and Rivero had to do remedial work in mathematics to keep up, making for a grueling schedule. They took Gardner's course on transients in linear systems and Samuel Caldwell's course on numerical analysis. What really excited them, however, was Charles Stark Draper's new work on gyroscopes, which seemed to have applications in fire control. Halfway through the year, Draper agreed to teach them about gyroscopes instead of the planned lectures on aviation instruments, and credited them for the original course without informing the navy.[90] Gordon Brown taught the four horsemen and several MIT students what may have been the first course ever in servomechanisms and control theory. They studied Minorsky and Hazen's papers and applied the principles to naval fire control. As Brown later recalled, the existing "Ford [fire control] machines used up enough energy they could practically drive the battleship" so servomechanisms would be useful as amplifiers, unburdening computational elements to drive large machinery, just as the servos did in the MIT mathematical machines. In the spring semester, Brown and his students began setting up a laboratory, partly with equipment borrowed from Sperry Gyroscope.[91]

Hooper and Ward wrote their Master's thesis from this course, on controlling large turrets with small electric signals. Where voltages commanded the position of large guns (as in the General Electric systems), Hooper and Ward realized they could apply Hazen's conception of the

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[90] This account comes primarily from Rivero's Oral History with additions from Hooper Oral History in the Naval Operational Archives.

[91] Gordon Brown, interview with author, August 27, 1994. Also see the manuscript version of Wildes and Lindgren, <u>A Century of Electrical Engineering</u> in the Wildes Papers, MIT archives, 5-10 to 5-15.

servomechanism as an amplifier. Hazen's 1934 papers were the first two citations in their thesis, which described fire control as a "pyramidal system" with several different levels of signals, several different sources of power, and several layers of feedback. They designed a servo to amplify a signal from 1/200th of a horsepower to eight horsepower, but pointed out it could be used up to a hundred horsepower. The design employed a variable-speed hydraulic drive produced by Sperry subsidiary Waturbury Tool Company. Hooper and Ward borrowed much of their electronics from existing MIT machines, noting the "electrical amplifier is essentially the same as that used for the motor drive in the electrostatic servo...used in the new Differential Analyzer."[92] This thesis addresses what we might call the "classical" problem of Naval fire control, how to direct a ship's guns at long range against a target, taking into account the ships pitch, roll, and velocity, as well as the range and velocity of the target. This was the problem that the Navy had originally intended to work on when it first approached MIT in 1936. Hooper recalled Gordon Brown asked him to reword the acknowledgment of his involvement in the project so Brown could use the results in his own work.[93]

The other two students in the Navy course examined a still newer problem which was rapidly becoming urgent. By the start of the fall semester in 1939 war had begun in Europe, and the British Navy was beginning to realize that its ships were vulnerable to German aircraft, whose speed made them difficult to hit with antiaircraft fire. Lloyd Mustin, a pistol shot expert, had worked in anti-aircraft before coming to MIT. For their thesis, Mustin and Rivero analyzed ships under attack by short-range, high speed airplanes, especially dive-bombers, strafers, and torpedo planes. They brought to their analysis the systematic, transient approach of Bush, Hazen, and Brown. Mustin and Rivero wrote, "as far as is known, no control device for the short-range problem has been developed anywhere which pretends to solve the three-dimensional problem involved." [94] Antiaircraft fire control was replacing long range fire control and power system stability as the primary driver for control system technology.

Mustin and Rivero's thesis focuses on light antiaircraft machine guns which can follow rapidly moving targets. It clearly shows the influence of Hazen's emphasis on dynamic performance and transient analysis:

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[92] E.B. Hooper and A.G. Ward, "Control of an Electro-Hydraulic Servo Unit." Master's thesis, MIT, 1940.
[93] Hooper oral history, Naval Operational Archives.
[94] H. Rivero and L.M. Mustin, "A Servo Mechanism for a Rate Follow-up System." Master's thesis, MIT, 1940, 2.

> There can be no compromises as to speed; the solution must be delivered at the point of application, and in its 'steady state' within a fraction of a second after the device has gotten on its target...a final requirement is that the solution be produced at a power level sufficiently high for it to be applied automatically and directly to the point of use.[95]

Because of the vibration and smoke produced by these guns, the controllers should be located at some distance from the guns themselves, hence the guns should work under "remote control." Mustin and Rivero analyzed how a gyroscopic device, based on the commercially-available Pioneer Turn Indicator for aircraft, might predict the path of an oncoming airplane. The basic problem was to derive the "rate" or angular velocity of the target and then to calculate the lead. But differentiating a function is a difficult task, highly susceptible to error. In an integrator, extraneous noise, like the errors due to human operators in tracking the curves on the Bush machines, gets averaged out. A pure differentiator amplifies noise but a gyroscope could be rigged with a spring to calculate lead angles in a smooth, stable, and accurate measurement. When Mustin and Rivero went to Draper for help on the analysis of this problem, however, the professor "froze," and "shut up like a clam."[96] Mustin and Rivero had stumbled into another of the pre-war threads of control systems, also coming to MIT for help: Sperry Gyroscope.

For several years, Draper had been consulting for Sperry Gyroscope on aircraft instruments, including turn indicators, blind flying apparatus, and engine instrumentation. While Hazen and Brown were defining the new discipline of control, Draper created his own field, aircraft instrumentation, embodied in his Instrument Laboratory. Draper's work, like that in control, was characterized by an emphasis on transient phenomena, models and analogs of physical systems, and graphical solutions. It was also characterized by industrial relationships. Before coming to MIT, Draper had worked at Sperry Gyroscope, and he retained his contacts there, especially with chief engineer Preston Bassett, president Reginald Gillmor, and Director of Research Hugo Willis. In the mid-thirties, Sperry began supporting and funding Draper's work, commercializing the products of his research, and hiring graduates of his laboratory. During the fall of 1939, as war broke out in Europe, Draper thought to apply a gyroscopic turn indicator he had developed to an instrument for computing the lead angles for guns on tanks. It was this project he was working on when Mustin and Rivero brought their idea for a lead computing sight

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[95] *Ibid*, 10.

[96] Rivero oral history. Gordon Brown interview.

for antiaircraft guns.[97] No evidence documents what caused Draper to turn from the tank-sight to an antiaircraft sight, but it may well have been Mustin and Rivero's thesis. It is clear, however, that in June of 1940, with a contract from Sperry Gyroscope, Draper turned his attention to antiaircraft fire control.

In the spring of 1940, the four horsemen stressfully completed their degrees and returned to the navy. MIT's work in control from the preceding decade thus began to diffuse into the military — through the dual conduits of industrial relations with Sperry Gyroscope and military liaison with the Bureau of Ordnance. At that point, servo theory, its usefulness for fire control established, disappeared behind a veil of military secrecy — and remained invisible until 1945. The Mustin and Rivero thesis, given the vague and deliberately uninformative title "A Servo Mechanism for a Rate Follow-up System," was classified when written, and remained so until 1972. Brown recalled "by 1940 the development of rigorous methods of analysis and synthesis had reached the stage of adolescence, when suddenly the work was blocked out by the fog of military security."[98] That year Brown wrote a paper incorporating his control research and teaching experience. "Transient Behavior and Design of Servomechanisms," presented a general summary of the field to date, introduced its basic principles, discussed transient response and analysis and presented design examples. Its second footnote cited Hazen's two 1934 papers. Brown also discussed Black and Nyquist's work on feedback amplifiers.[99] Brown planned to present this paper at the annual meeting of the American Society of Mechanical Engineers in 1940.

As it happened, Brown did not present to the ASME. The paper, in fact, would not see publication for five years. In July, of 1940, a few months before he planned to present it, Brown greeted an important visitor and explained to him the current state of servo research. He showed

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[97] Lloyd Mustin, Memorandum introducing S.M. thesis upon declassification, 1971. Mustin recalled that "Though Dr. Draper did not suggest any gun control applications at that time, he later acknowledged the contribution of this thesis to the development of his own concepts." For a detailed account of Draper's relationship to Sperry in the thirties, see Michael Dennis, "A Change of State: The political cultures of technical practice at the MIT Instrumentation Laboratory and the Johns Hopkins Applied Physics Laboaratory, 193-45," (Ph.D. dissertation, Johns Hopkins University, 1991), Chapter 2, and Chapter 4 for the lead-computing gunsight. For an example of the collaborative MIT/Sperry research, see C.S. Draper, G.P. Bentley, and H.H. Willis, "The M.I.T.-Sperry Apparatus for Measuring Vibration," *J. Aeronautical Sciences* 4 (no. 7, May, 1937), 281-85.

[98] Gordon S. Brown and Donald P. Campbell, Principles of Servo Mechanisms: Dynamics and Synthesis of Closed Loop Control Systems (New York: John Wiley & Sons, 1948), 9.

[99] Gordon S. Brown, "Transient Behavior and Design of Servomechanisms,"

the visitor the differential analyzer and the center for analysis. Brown explained the previous year's fire control course and the contributions the four naval gunnery officers had made. He complained his work would move more quickly but for problems of personnel and equipment. The visitor had been sent by Vannevar Bush, who had just formed the National Defense Research Committee (NDRC). His name was Warren Weaver, and just a few weeks before he had been asked to setup a special NDRC division devoted to fire control. When the committee met in 1940, it quickly classified Brown's paper and issued it as a restricted report. With that news, however, came a contract for Brown to extend his research and found the Servomechanisms Laboratory.

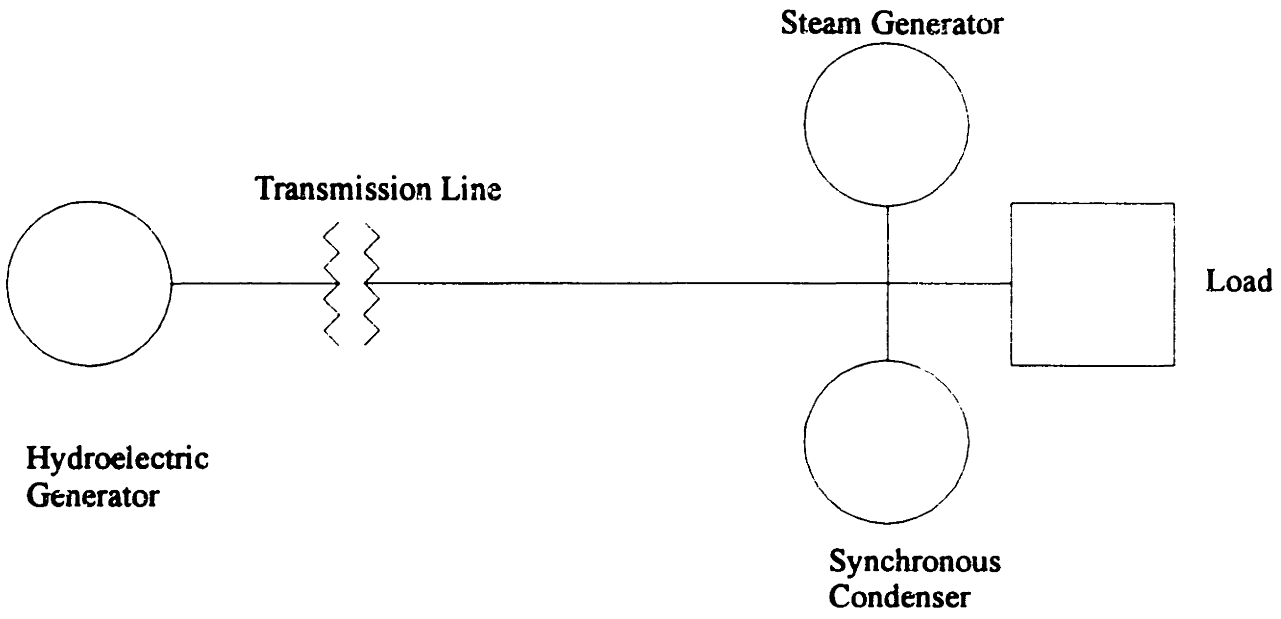


Figure 5-1: Simplified system layout for stability problem (redrawn from Bush and Booth, "Transmission Line Transients," *AIEE Transactions* 44 (1925), 236).

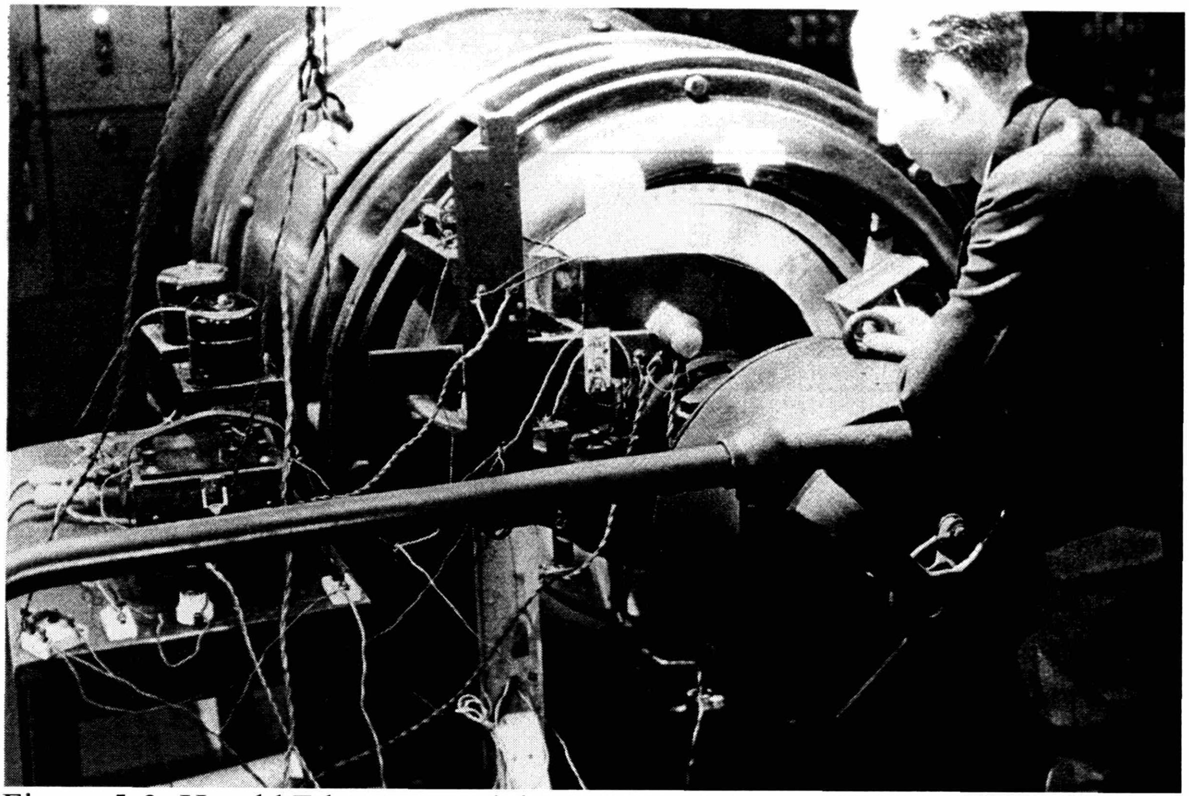
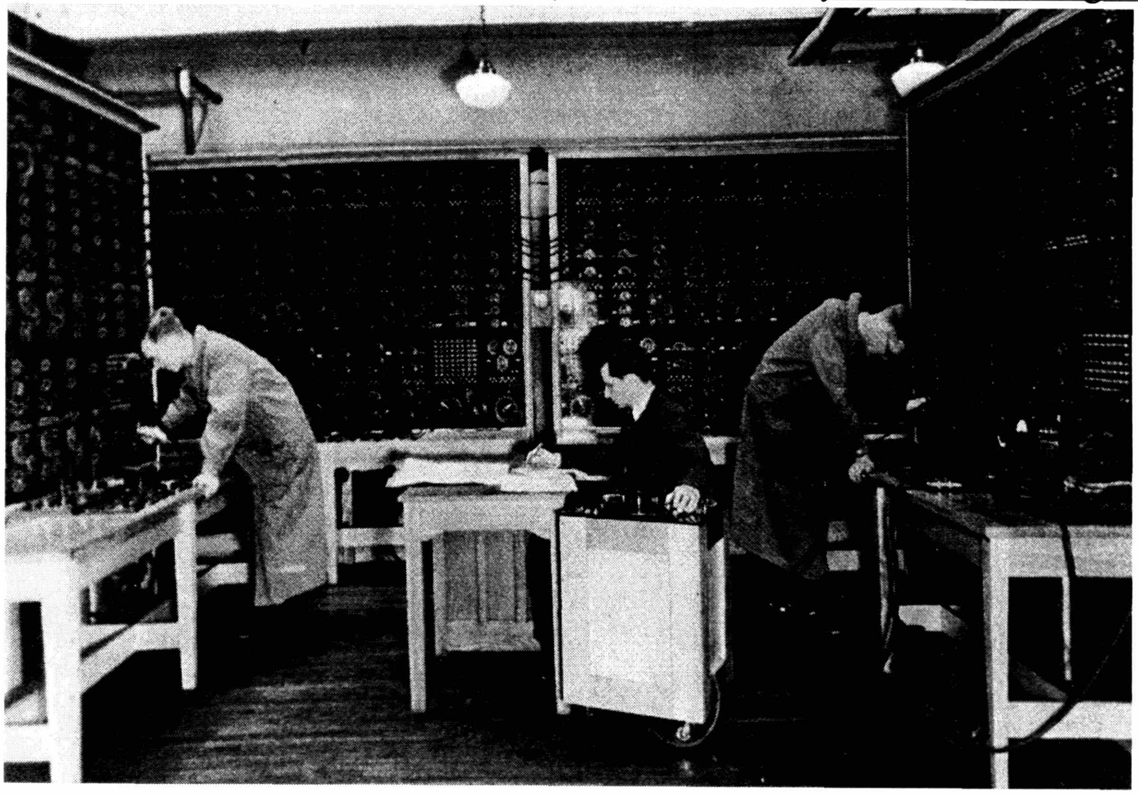


Figure 5-2: Harold Edgerton studying power system stability with a stroboscope. Note "frozen," N and S poles on generator (Wildes, A Century of Electrical Engineering, 146).

Figure 5-3: Harold Hazen (seated) and Network Analyzer. On left is Samuel Caldwell, on right Sidney Caldwell (Wildes, A Century of Electrical Engineering, 102).



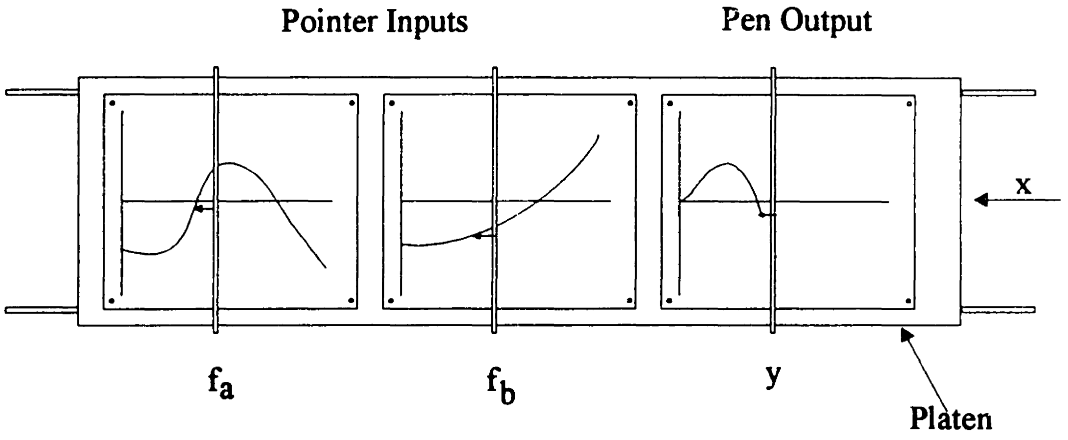


Figure 5-4: First Product Integraph Functional Layout

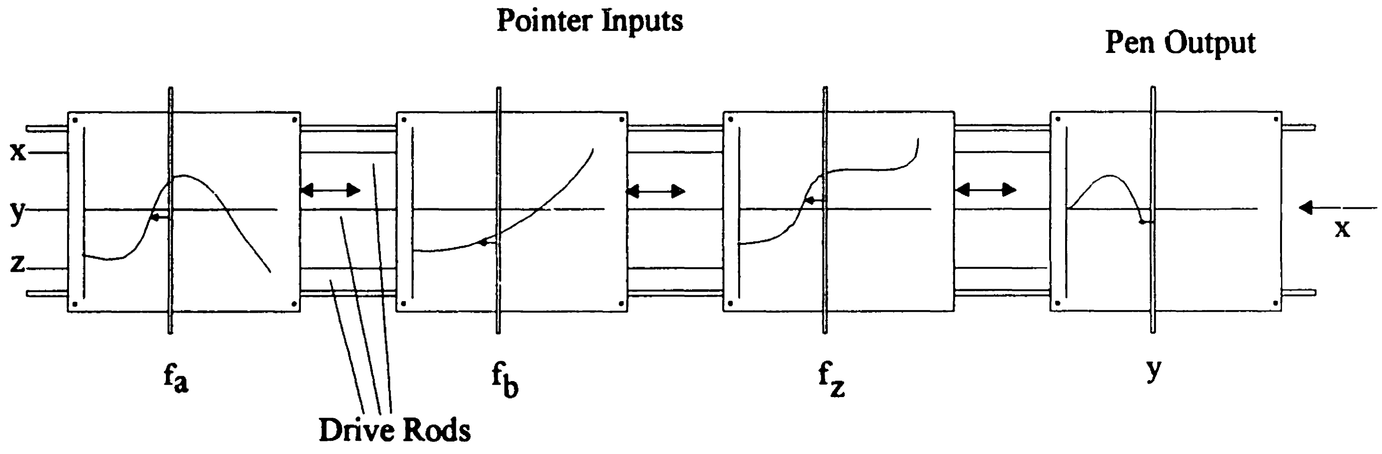


Figure 5-7: Second Product Integraph Graphic Layout

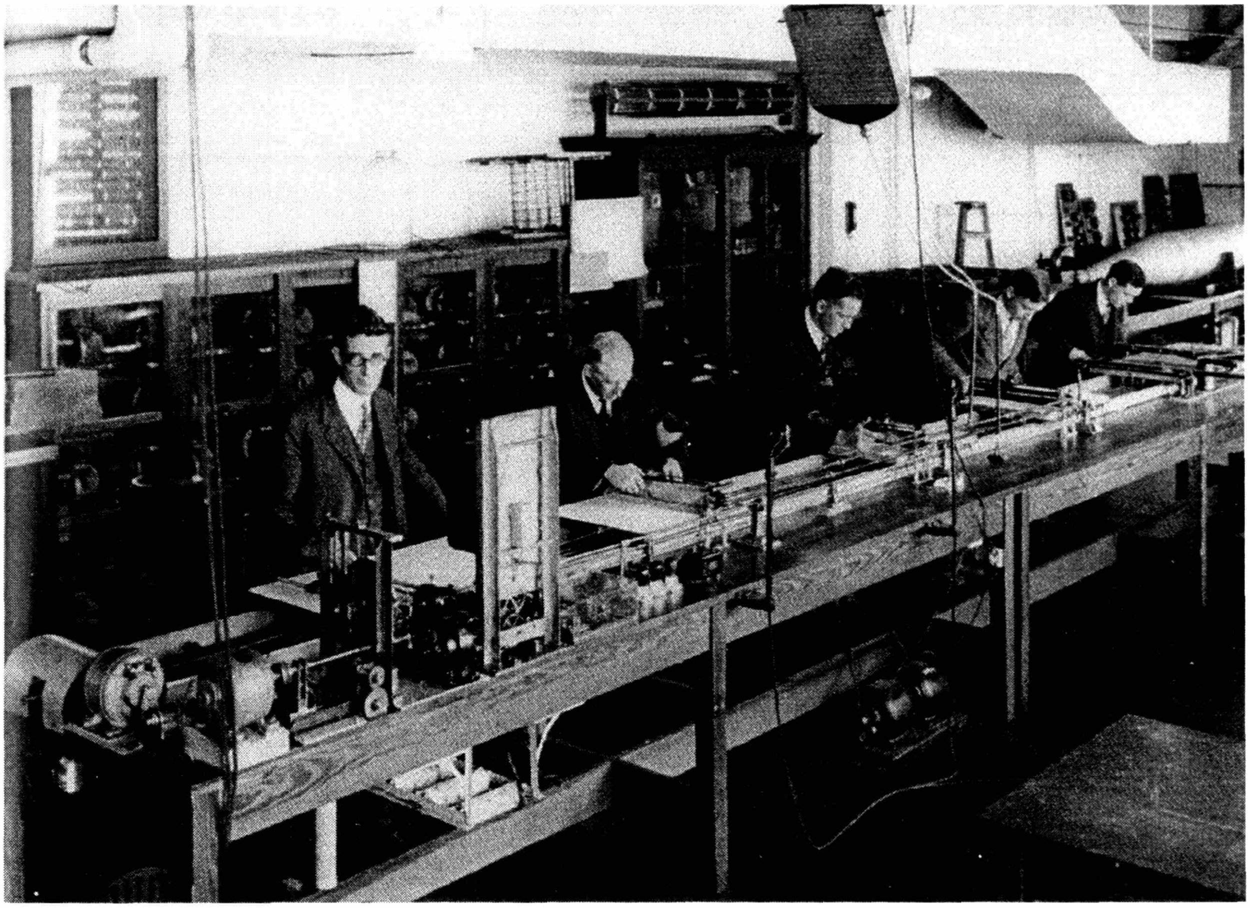


Figure 5-5: Vannevar Bush (left) and the Product Integraph, late 1920s. Harold Hazen is second from right. Note electric motors driving plotting tables, vertical boards containing servomechanisms, and automobile radiator hanging above to cool precision resistance instruments (Wildes and Lindgren, A Century of Electrical Engineering, 79).

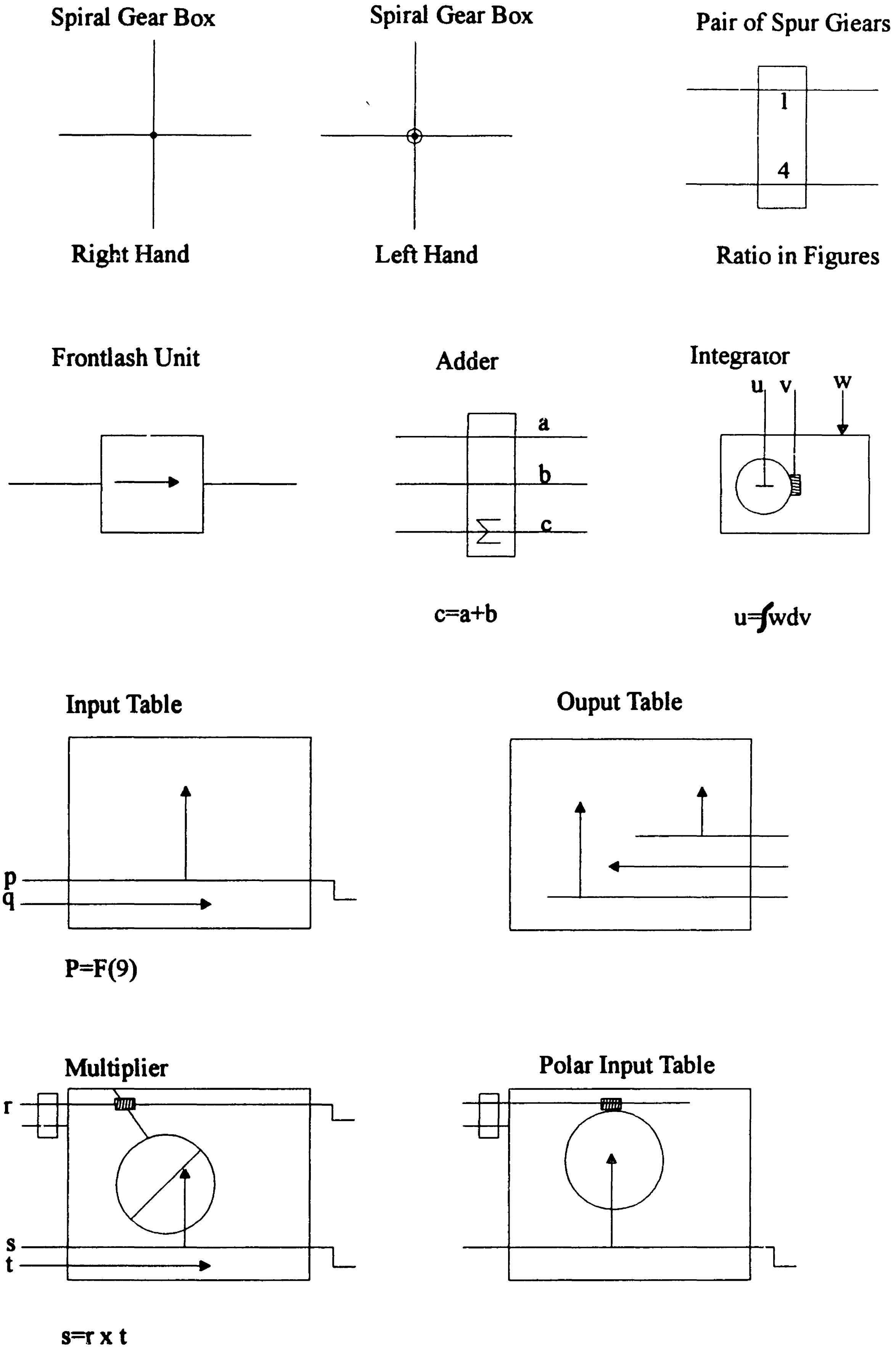


Figure 5-8: Standard Symbols for Mathematical Function Units in Differential Analyzer

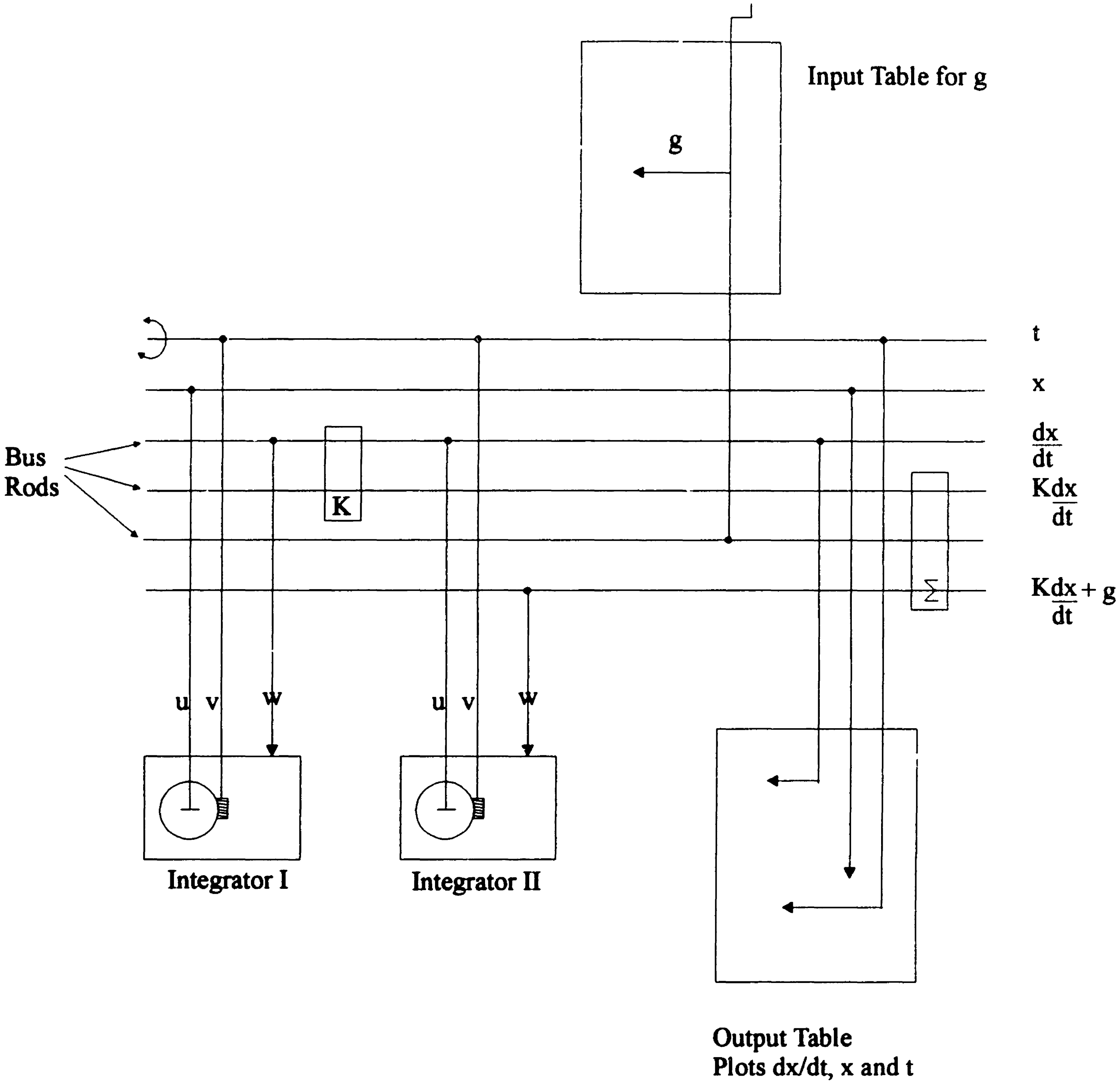


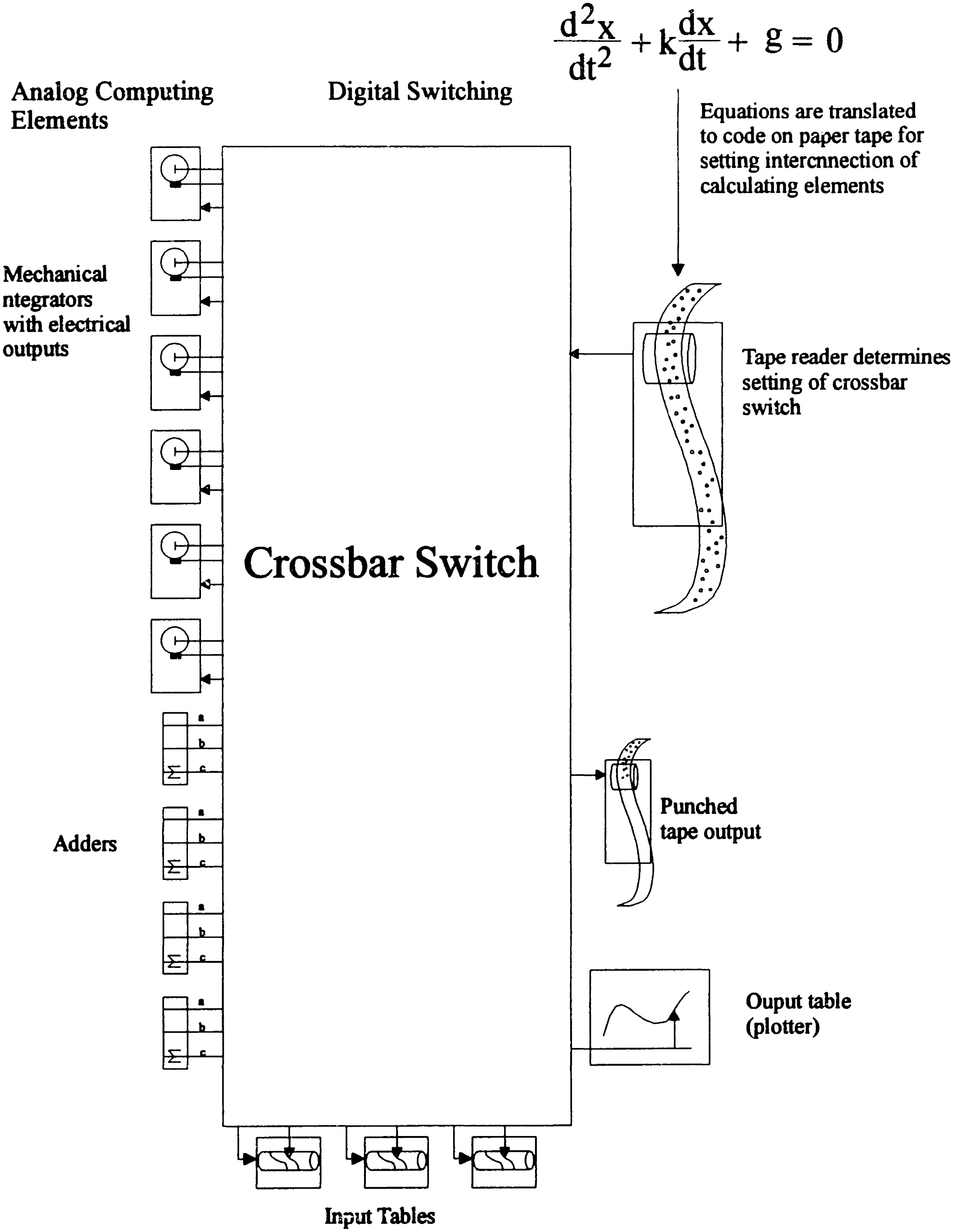
Figure 5-9: Differential Analyzer set up for a particular problem. Note that bus rods rotate and transmit data from one unit to another. For this problem, no real-time inputs are needed, all data is input as initial conditions. Input table is provided so value for gravity can be easily changed.

Setup shown is for solving a basic falling body problem:

$$\frac{d^2x}{dt^2} + k\frac{dx}{dt} + g = 0$$

(V. Bush, "The Differential Analyzer. A New Machine for Solving Differential Equations," *Journal of the Franklin Institute* 212 (no. 4, 1931), 457).

Figure 5-10: Rockefeller Differential Analyzer, principle of operation



Chapter 6

Acquiring Control:

The Fire Control Divisions of the NDRC

In the spring of 1940, as the school year ended, Mustin, Rivero, Hooper and Ward finished their theses at MIT. The four officers then embarked on a "cooks tour," visiting industrial, military, and research laboratories working on fire control. In September, the navy, mobilizing in response to events in Europe, cut short the tour and sent the four horsemen to their new assignments: Ward joined the Naval Inspector's office at the Ford Instrument Company, Hooper did the same at G.E., Mustin went to the Naval Gun Factory, and Rivero to BuOrd's Fire Control Section.

These men were at the fore of a broad convergence as World War II brought together the four traditions, centered on problems of fire control and a new research agency. Sperry and its gun directors spawned ambitious efforts in fire control, electronic computing, and integrated systems. MIT's work in servomechanisms led to power controls which harnessed guns to human operators and computers without the danger of instability. Bell Labs' feedback amplifiers brought the fire control problem to an entirely new level of sophistication and merged it with telephone engineering. The Bureau of Ordnance and its contractors provided a mature industrial base for control systems, as well as a pressing demand to defend vulnerable ships against attacking aircraft.

Wartime Research and The Anti-Aircraft Problem

During World War II, a broad research program brought engineers, ideas, and machines together in new combinations. The resulting synergy produced military devices and spawned derivative threads in control engineering, systems engineering, information theory, and cybernetics. The fire control divisions of the National Defense Resource Committee (NDRC), formulated the program, divided it up into separate projects, and oversaw the research. From guns and bombs to signals and systems, military technology required precision and power. Innovations in technologies of control paralleled inventions in the control of technology. Historical work on wartime science, however, tends to focus on the war as origin: of nuclear standoff, of the information age, of science policy.[1] Science plays the leading role. Rarely have historians examined the continuity of wartime research with what came before, rarely have they looked at wartime technological change as anything but subsidiary to scientific change, and rarely have they examined the practitioners who forged and experienced these changes. Dominant book-length works on wartime research remain official histories written in the forties.[2]

Recently, historians have begun to revisit the origin myths and to open the black box of wartime technology. A. Hunter Dupree placed the NDRC within a much longer history of science and the federal government. He identified "the great instauration of 1940," as the key period, eighteen months before Pearl Harbor, when the scattered military science of the thirties coalesced into Vannevar Bush's new agency, the National Defense Research Committee (NDRC). Dupree pointed to the significance of the research contract in shaping the universities' role, and to importance of the NDRC division chiefs and their staffs, who personally connected scientific ideas to the military. He also emphasized the NDRC's focus on "the total system of radar and radio connected to weapons."[3] In a similar vein, scholars have argued the 1940 "watershed" was as conservative as it was revolutionary.[4] The NDRC conducted, by dollar value, only a fraction of wartime research. Bush had to fight numerous boundary battles to maintain the autonomy of his

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[1] As examples, see: Daniel J. Kevles, The Physicists: The History of A Scientific Community in Modern America (Cambridge, Mass.: Harvard University Press, 1971). Richard Rhodes, The Making of the Atomic Bomb (New York: Simon and Schuster, 1988). Herman H. Goldstine, The Computer: From Pascal to von Neumann (Princeton: Princeton University Press, 1973). For discussions of wartime research in the context of modern science policy, see Paul Forman "Behind Quantum Electronics: National Security as a Basis for Physical Research in the United States, 1940-1960," Hist. Stud. Phys. Bio. Sciences 18 (no. 1, 1987), 149-229. Andy Pickering, "Cyborg History and the World War II Regime," Perspectives on Science 3 (no. 1, 1993) 1-48. These works, particularly Kevles, Rhodes, and Forman, do make significant contributions to our understanding of wartime physics and postwar science policy and practice, but they do not help us understand wartime technology on its own terms.

[2] James Phinney Baxter, Scientists Against Time (Boston: Little Brown and Co., 1946). Irvin Stewart, Organizing Scientific Research for War: The Administrative History of the Office of Scientific Research and Development (Boston: Little Brown and Co., 1948), 322. Baxter's book was the "short version," of the official history. The long version was published as separate volumes; the most relevant to this chapter is Joseph C. Boyce, New Weapons For Air Warfare: Fire-Control Equipment, Proximity Fuzes, and Guided Missiles (Boston: Little Brown and Company, 1947). Henry Guerlac, Radar in WWII (New York: American Institute of Physics, 1987).

[3] A. Hunter Dupree "The *Great Instauration* of 1940: The Organization of Scientific Research for War," in Gerald Holton ed., The Twentieth-Century Sciences: Studies in the Biography of Ideas (New York: W.W. Norton and Co., 1970), 459.

[4] Nathan Reingold, "Vannevar Bush's New Deal for Research: or the triumph of the old order," Hist. Stud. in the Phys. and Bio. Sciences 17 (1987), 299-344.

agency.[5] And the NDRC/OSRD may have been "counterproductive" to Bush's vision of post-war research.[6]

Most important, historians are slowly recognizing the technology component of the OSRD, the role of engineers in addition to scientists. Bush himself created some of the bias, as he used the mantle of science to distinguish his organization from companies like Sperry and Ford,

> When I came to work closely with the army and the navy I found it essential to introduce all of my people as scientists, for the word engineer to them meant to often the sales engineer coming from one of their contractors. This finally went to the point where every man in my organization got called a scientists, although it was fortunately well permeated with engineers.[7]

As Larry Owens puts it, in the OSRD "engineering was often more important than science, practice more important than theory, and the ability to mediate, to move comfortably among university, government, military, and industry....most important of all."[8] Owens's recent overview of the OSRD provides a framework for closer analysis. Michael Dennis puts a piece in that frame with his comparative study of two wartime laboratories. Both Owens and Dennis make extensive use of the remarkably complete official records of the OSRD.[9] That the records were declassified less than twenty years ago helps explain the silence.

Owens and Dennis begin to open up the internal workings of wartime technology beyond the Manhattan Project, Science the Endless Frontier, and the ENIAC. The following close examination of the OSRD's work on control systems attempts to answer further questions: When, and how, did wartime research acquire the expertise of its predecessors? How did the OSRD relate to the military services? To industry? How could it innovate where others had failed? Where did it fail? What was the social fabric, the organizational culture, which allowed the OSRD to oversee these interactions? Starting in 1940, how did the government control technology?

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[5] Carroll Pursell, "Science Agencies in World War II: The OSRD and Its Challengers," in Nathan Reingold ed. The Sciences in the American Context: New Perspectives (Washington: Smithsonian Institution Press, 1979), 360-78.
[6] Larry Owens, "The Counterproductive Management of Science in the Second World War: Vannevar Bush and the Office of Scientific Research and Development," Business History Review 68 (Winter 1994), 515-576.
[7] Bush to Hoover, April 27, 1945. Bush papers, Library of Congress, Box 51 Folder 1261.
[8] Owens, "The Counterproductive Management of Science," 537n.
[9] Michael Dennis, , "A Change of State: The political cultures of technical practice at the MIT Instrumentation Laboratory and the Johns Hopkins Applied Physics Laboratory, 1930-45," (Ph.D. dissertation, Johns Hopkins University, 1991), 357.

Organizing Research and "The Antiaircraft Problem"

The original impetus for the NDRC arose, at least in part, from what Vannevar Bush called "the anti-aircraft problem." He left MIT for Washington in 1938 to assume one of the central positions in American science, head of the Carnegie Institution. From this lofty vantage point he could survey the landscape of scientific research with unusual breadth. An additional position as Chairman of the National Advisory Committee on Aeronautics (NACA) focused Bush's attention on the dramatic strides in military aircraft and their fearsome implications. In the spring of 1939, while Europe was still at peace, he grew alarmed about "the anti-aircraft problem." Bush wrote to his hero, the retired engineer-president Herbert Hoover, for help. As Chairman of the NACA, Bush wrote, he saw the rapid progress aircraft were making toward higher speeds and greater altitudes. He also understood that such progress made airplanes difficult, if not impossible, to hit with traditional gunnery. High-frequency radiation research at MIT and Stanford (early radar, partially supported by Sperry Gyroscope) held promise as a way to detect and locate aircraft, he continued, but no one was coordinating the connection of such equipment into systems which could direct "the precise and rapid control of guns."[10]

Hoover had no advice for Bush, but he found support from other colleagues closer to home. He wrote to Frank Jewett, then president of Bell Labs, that his interest in national defense arose from both NACA work and "a private conviction that antiaircraft is not receiving the attention it should have."[11] With the outbreak of war in Europe in September, 1939, the *blitzkrieg* dramatically demonstrated the airplane's central importance in modern warfare. In 1940, Bush proposed his idea for a council to coordinate defense research, much as the NACA coordinated aeronautics research. He wrote to President Roosevelt that, while the NACA "correlates military and civil research activities on aeronautical devices, no similar agency exists for other important fields, notably anti-aircraft devices."[12]

On June 27, 1940, as the four horsemen turned in their theses at MIT, President Roosevelt approved an order establishing the NDRC and directing it to fund scientific research into military problems. The committee consisted of leaders in American science and engineering: Bush, Jewett

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[10] Bush to Hoover, April 10, 1939 and April 29, 1939. Bush Papers, Library of Congress.

[11] Bush to Jewett, March 23, 1939. Jewett folder, Bush file, Carnegie Institution of Washington records quoted in Carroll Pursell, "Science Agencies in World War II: The OSRD and Its Challengers," in Nathan Reingold ed. The Sciences in the American Context: New Perspectives (Washington: Smithsonian Institution Press, 1979), 360.

[12] Draft Memorandum, n.d., OSRD, Central Classified File.

(now also President of the National Academy of Sciences); James Conant, President of Harvard; Karl Taylor Compton, President of MIT; Conway P. Coe, Commissioner of Patents, Richard C. Tolman of Caltech, and one liaison each from the War and Navy departments, initially Major General G.B. Strong and Rear Admiral Harold G. Bowen (MIT graduate and post-war founder of the Office of Naval Research) . The group tilted toward academia (even Jewett's Bell Labs had a decidedly academic flavor), and NDRC work overall would heavily favor MIT. This bias would be simultaneously the NDRC's strength and its weakness. Ph.D. scientists and engineers brought fresh ideas and a vigorous quantitative approach to military problems. Many professors and researchers, however, were novices in fields in which others had already built careers. The NDRC's eagerness could rapidly shade into arrogance, both intellectual and organizational: army and navy laboratories, industrial contractors, and any number of government agencies would seek to restrict their influence.[13] "There were those who protested that the action of setting up NDRC was an end run," Bush wrote in his memoirs, "a grab by which a small company of scientists and engineers, acting outside established channels, got hold of the authority and money for the program of developing new weapons. That, in fact, is exactly what it was."[14]

D-2 and Division 7

To structure his organization, Bush surveyed the armed services for pressing problems and set up the NDRC in four divisions: Division A, Armor and ordnance under Tolman; Division B, bombs, fuels, gases, and chemistry under Conant; Division C, communications and transportation under Jewett; and Division D, radar, fire control, and instruments under Compton.[15] Bush also included a "Uranium Committee" which would later transfer to the army and become the Manhattan Project. Division D divided into four sections:

D-1, Detection and radar
D-2, Fire Control
D-3, Instruments
D-4, Heat Radiation.

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[13] Pursell, "Science Agencies in World War II."
[14] Vannevar Bush, Pieces of the Action (New York: Morrow, 197), 31-32, quoted in Owens, "The Counterproductive Management of Science," 522.
[15] OSRD7, Office files of Karl Taylor Compton, folder NDRC Misc., has several early organization charts for the NDRC which do not include any section devoted to fire control, although Owens reprints a chart Bush shared informally with colleagues in June, 1940, before Roosevelt's executive order, which includes a division for fire control, "The Counterproductive Management of Science," 523.

This chapter outlines the work of D-2 and its successor, Division 7, which had NDRC responsiblity for control and let eighty research contracts.[16]The following three chapters examine key aspects of this research in servomechanisms, radar and systems engineering, and computing and information. D-2 and Division 7 sought to fuse the four pre-war lines of control systems: the Navy's fire control, Sperry's feedback controls, Bell Labs' electronic amplifiers, and MIT's servomechanisms. They fostered the convergence by funding projects in fundamental and applied research — using changing definitions of "fundamental" research to define their organizational role. Serving as a kind of central technology bureau, the NDRC transferred information between groups, set standards, and charted new directions for investigation. The members of D-2 and Division 7 had to craft this role carefully, however, employing a combination of research contracts, technical authority, and political power. They did not always succeed.

For fire control in particular, the NDRC records paint a detailed picture of the technology and its politics as they unfolded. Every time committee members visited a facility, attended a meeting, had an important phone call, or even made a relevant observation, they wrote up a "diary" entry and distributed it to the rest of the group. Harold Hazen (who headed Division 7) recalled, "These diaries, automatically circulated to all division members, were entirely highly classified internal documents, and hence gave free scope to uninhibited expression by creative individualists. They were often brilliant, salty, and very flavorable."[17] Division members did not hesitate to discuss sensitive or unpleasant personal and technical matters in these classified and credible memos.

Technical reports, produced in the normal course of research as well as at the end of the war, illuminate the technical aspect of the work. These reports and all NDRC documents were secret when produced, a fact which produces unforeseen benefits or the historian. The NDRC carefully tracked its classified documents, so the process of technology diffusion can be recreated in detail; Knowledge was carefully tracked as it moved through organizations and across

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[16] For the official history of D-2 and Division 7, see Joseph C. Boyce, New Weapons For Air Warfare, Chapters III-IX. The sources for these chapters are the personal histories written by the section members for Boyce in National Archives Record Group 227, Division 7 (hereafter referred to as OSRD7), General Project Files (hereafter referred to as GP), History File. Boyce published these accounts virtually intact, but he edited out much of the most interesting material on institutional friction, conflict, and competition. I will therefore cite the original members' histories wherever possible, although only for general observations. For the actual chronologies of committee activities, I will work from the original committee documentation, on which the members' histories are all clearly based.

institutional boundaries. Gordon Brown's 1940 paper on servomechanisms, for example, culminated MIT's work in control theory during the 1930s. When the NDRC began its work in 1940, it immediately classified the paper and, under controlled distribution, used it to bring researchers who were new to the field quickly up to date. Brown's document defined the boundaries of the technical community. Chapter 8 traces where it went, when, how it was received by those who read it, and how they incorporated it into their work.

Making fire control a science

Getting Started: Weaver Assembles the Section

To head section D-2, Bush chose his colleague Warren Weaver. Weaver like Frank Jewett, had been a student of physicist Robert Millikan. He spent the twenties and early thirties on the Mathematics faculty at the University of Wisconsin. In 1932, Weaver moved to New York to become director of the Natural Sciences Division of the Rockefeller Foundation, a job of central importance in the interwar scientific community.[18] It was here, in 1935, that Bush met and befriended Weaver, and when he began sponsoring Bush's computing machine, the Rockefeller Differential Analyzer. When Bush moved to the Carnegie Institution in 1938, the men became peers. Weaver, a talented teacher and administrator, brought a mid-western pragmatism to foundation science patronage. Despite the frustrations of the depression, he crafted a role as a "manager of science" at the Rockefeller characterized by interdisciplinary programs and project-oriented grants. As an administrator, Weaver worked as "an active partner in setting research agendas."[19] He brought these practices to the NDRC, producing a style of federal patronage, at least in the critical early months, which confirms Nathan Reingold's assessment of Bush's "instauration," as "the triumph of the old order."

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[17] Harold Hazen, Memoirs, MIT Archives, 3-34.

[18] Warren Weaver, Scene of Change: A Lifetime in American Science (New York, Charles Sribner's Sons, 1970), 45.

[19] Robert E. Kohler, Partners in Science: Foundations and Natural Scientists 1900-1945 (Chicago: University of Chicago Press, 1991), Chapter Ten, "Warren Weaver and his Program," is a detailed exploration of Weaver's patronage at the Rockefeller foundation in the 1930s. Kohler writes "The system of federal patronage which evolved mainly out of military research programs of World War II, differed in important ways from prewar foundation programs," (404) which is undoubtedly true, but Kohler does not examine Weaver's wartime work, which establishes a significant continuity between prewar-private and wartime-federal patronage styles. Reingold, "Bush's New Deal," (325) comments that Bush's proposal for NSF contracts derived from a combination of Weaver's Rockefeller strategy and the OSRD contract, but without explicitly noting Weaver's role in the two.

Indeed Bush's selection of Weaver underscores the NDRC's initial continuity with the pre-war world. Bush selected men of his own status or higher — the scientific elite. While intellectually equipped to deal with technical matters of fire control, Weaver had no experience with either the military aspects of the problem or with the previous years' work in feedback controls and theory (he had done some work on gyroscopic stabilization during the first world war). At first, the government's agents were to be gentleman scientists, skilled in the arts of private patronage and applying those skills to the distribution of federal dollars.

Weaver assumed chairmanship of D-2 in early July, 1940 and immediately began assembling experts. He invited Thornton C. Fry and Samuel H. Caldwell to join as members of D-2. Fry, Bell Labs' mathematical research director, had been a colleague of Weaver's in the Math department at Wisconsin. Caldwell, head of MIT's Center for Analysis, had been Bush's graduate student and had collaborated with him on the differential analyzer. Edward J. Poitras joined D-2 as Chief Technical Aide. Poiras a former student of Bush and Hazen, went from MIT to the Ford Instrument Company, at Bush's suggestion, to design controls for the Mount Palomar Telescope.[20] Security clearances for these men took some time, so Weaver spent the summer on his own and with the main NDRC committee gathering information.

On July 9, Weaver met with Bush, who briefed him on the history of fire control. The first directors had been built for heavy naval guns before World War I, Bush told him, by Hannibal Ford, and the first antiaircraft fire control had been built about fifteen years ago. Weaver began thinking about setting up committees under his auspices; he divided the field into Electronics, Optics, Mechanical Design, and Mathematics. This early plan included no separate effort for servomechanisms, feedback, or theory.[21] The following week, Weaver met with Colonial Taylor, head of the army's Anti-Aircraft Artillery Board. Taylor described the Sperry directors and relayed the army's disappointment with "human servomechanisms:"

> At several points in the [fire control] process, operators are necessary to match dials. This is true of the actual laying of each gun in altitude and azimuth, of the fuse [sic] setting, and apparently at several other points. Much, if not all, of this could obviously be eliminated.[22]

Taylor pinpointed the difficult problems with the Sperry machines. Power controls for moving the guns, Taylor explained, were a particular weakness.

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[20] Ronald Florence, The Perfect Machine: Building the Palomar Telescope (Harper Collins, 1994) 320-21.

[21] WW diary, July 9, 1940. OSRD7 GP.

To explore the academic side of things, Weaver visited MIT and met with Gordon Brown and Sam Caldwell. Caldwell and Weaver discussed the differential analyzer work and the Center for Analysis — an official entity which incorporated MIT's computing facilities (Caldwell was supervising the new differential analyzer which Weaver, through Rockefeller, was funding). Brown briefed Weaver about MIT's program in control, including the the four fire control officers.[23] The day he returned from his MIT visit, Weaver came up with a tentative agenda for the first meeting of section D-2. The topics he listed as "pressing problems" show how his thinking had progressed in four weeks:

> 1) Developement of much more rapid, reasonably accurate, automatic controls for lighter AA guns
> 2) Possible improviement of existing fire control equipment for heavier guns
> 3) Increased accuracy by (a) simplification of equipment or procedure (b) by combining units (c) by substituting automatic for manual controls
> 4) Consideration of any specialy problems referred to us by the Fire control groups of the Army or Navy (e.g. automatic fuze setting, improvment of rangefinders)
> 5) Theoretical analysis by (a) overall analysis of errors (b) analysis of complex systems of servomechanisms, particularly a determination of most effective and simplest type of intercoupled damping to sercure stability when several servomechanisms are connected in series (c) analysis of function of computer, including higher order derivatives (d) probability analysis of risks involved in various dispositions of equipment
> 6) Basic program of development of servomechanisms (MIT group, Brown & Caldwell)

Here Weaver already understands much of the pre-war work and moves toward a conception of fire control as a system. He includes the mechanism for D-2 to respond to problems raised by the services, and, unlike his listing a few weeks before, he sees the need for special attention to theory. "Intercoupled damping" and connecting servos in series referred to stability problems with the Mark-37 antiaircraft director. Bush had chosen the right man; Weaver quickly grasped the salient problems of this complex field — demonstrating the competence as a "science manager" he had developed at Rockefeller. While somewhat self-fulfilling, this early memo predicts the important work of the next five years.

D-2 as a formal committee or "section" held its first meeting in Hanover, New Hampshire in September, 1940, when the members were in town for a meeting of the American Mathematical Society. This meeting had a special session on "War preparedness among mathematicians," and connected academics with military research. Also at the meeting, George Stibitz of Bell Labs

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[22] WW diary, July 18. 1940. OSRD7 GP.
[23] WW diary, July 29, 1940. OSRD7 GP.

demonstrated the remote operation of his "Complex Number Computer" or "Model I Relay Computer" over a telephone line to New York, credited as the first instance of computing through the network. Stibitz had been experimenting with telephone relay calculators since 1937, and his research director (now D-2 member) Thornton Fry urged him to build a complex number calculator to aid electronic filter design at Bell Labs. Those who attended Stibitz's demonstration included Norbert Wiener and John Mauchly (later designer of the ENIAC), among others, probably including the members of section D-2.[24]

The following week, Weaver made one more visit without his committee, to the army's Aberdeen proving ground in Maryland with Bush, Compton, and the NDRC's army and navy liaisons. Alfred L. Loomis also attended, a wealthy New York lawyer with an interest in microwave radio who now headed section D-1 on radio detection. The group saw the Sperry M-4 director, which was at Aberdeen for testing with new electro-hydraulic power gun controls. Weaver observed the problems with the Sperry servos, "there is no hunting, but the motion is frequently jerky and the rates are slow." The navy representative, noted Weaver, "who is, of course, familiar with the Navy automatic control, takes WW to one side and agrees that this is a pretty unsatisfactory device."[25] The day at Aberdeen demonstrated the limitations of Sperry's interwar antiaircraft program, and a naval officer was only too eager to point out weaknesses in the army's technology.

In later months Bush, Compton and the members of the NDRC proper would not be so directly involved as this day at Aberdeen. At this early stage, however, the remained small enough that a few men could attend to the whole thing, particularly as Bush was concerned about antiaircraft from the start. Also, Weaver would need unprecedented access to military and government facilities for his upcoming work. Bush, with Roosevelt's executive order behind him, helped pave the way.

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[24] Brian Randell, The Origins of Digital Computers: Selected Papers (New York: Springer-Verlag, 1982), 241-45. Stibitz's paper presented at this conference does not survive, but Randell's volume reprints a similar paper from 1940, 247-52. George Stibitz's memoir has reprints the program of this meeting, The Zeroth Generation: A scientists recollections (1937-1955) from the early Binary Relay Digital Computers at Bell Telephone Laboratory and OSRD to a fledgling Minicomputer at the Barber Coleman Company (unpublished MS, 1993), appendix I-5 to I-6. Courtesy Paul Ceruzzi.

[25] WW diary, September 18, 1940. OSRD7 GP.

Learning the Field

Because of the navy's clear advantage in fire control, Weaver and D-2 initially concentrated on the army's problems of land-based antiaircraft fire, especially for heavy antiaircraft artillery. Service attitudes influenced the choice; Naval fire control had a well-earned reputation as a closed technical community. The army, however, unhappy with its equipment, extended a welcoming hand to the NDRC. Until late in the war, in fact, BuOrd and navy fire control would remain outside of the NDRC's domain. It would cost them their lead in the technology (see Chapter 8).

Colonel William S. Bowen, president of the army's Coastal Artillery Board (CAB) agressively recruited the NDRC help on fire control (no relation to Admiral Harold Bowen, NDRC member and later founder of the Office of Naval research). On October 3, Weaver and the committee (Fry, Caldwell, and Poitras) visited Bowen at Fort Monroe in Virginia. Bowen explained the "dissatisfaction" in the service with the current M-4 director, and especially with Sperry Gyroscope as the sole manufacturer of such devices. Directors incorporating electrical rather than mechanical techniques had been proposed by Bell Labs, added Bowen, but such equipment would need to be very rugged to be useful. Microwave detection techniques had also been proposed, but their present accuracy was not suitable. Bowen listed limitations in the accuracy of existing fire control. Tracking of targets with handwheels, for example, was not smooth enough. Weaver noted Bowen had strong opinions about the automation, and lack of it, implemented by Sperry in its directors:

> The use of personnel for the matching of gun dials is quite undesirable, large errors ocurring under conditions of firing...They [CAB] feel that mechanical loading of the guns is a step in the right direction to minimize and make more constant dead time.

Once again, Weaver and D-2 saw that for all its strides, Sperry's replacement of human operators by servomechanisms had still not gone far enough. Bowen was also concerned about the reliability of automated machines:

> Again it was emphasized [Weaver noted] that servos are to be used wherever possible in place of manual matching of dials. *The saving of manpower in this way is not important but the accuracy is of great importance.* It was pointed out that with the increased use of the automatic equipment, the number of men required for servicing such equipment tends to offset the saving in operating personnel.[26]

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[26] WW diary, October 3, 1940. OSRD7 GP. Emphasis added.

They could not have had a more succinct statement of the problems of military automation.

And, of course, D-2 visited Sperry. They met with Reginald Gillmor, Sperry Gyroscope president Preston R. Bassett, Director of Research Dr. Hugo Willis (now a member of NDRC section D-1on radar) and Director of Fire Control Development Earl Chafee. Chafee explained the details of Sperry's fire control computers and the advantages of their "plan prediction method." Sperry was currently modifying its M4 director, Chafee added, to incorporate the suggestively-named "aided laying," which partially automated tracking. They were also increasing the M-4's range and including provision for microwave tracking inputs.[27] D-2 thus examined the results of Sperry's pre-war program with the army which, despite its flaws, served as a baseline against which to compare new approaches. D-2 designed their research program accordingly.

Weaver and D-2 did not see naval fire control as pressing as the army's problem, but D-2 did everything it could to incorporate information from that tradition of control. At the Naval Gun Factory they examined directors, computers, and rangekeepers, and were given copies of the manuals for these machines.[28] They went aboard the USS Quincy, interviewed its gunnery officer and examined its Ford Rangekeeper, its thyratron servos, and its antiaircraft directors.[29] At RCA in Camden, New Jersey, D-2 discussed the company's work on electronic computing for BuOrd gun directors.[30] When the committee visited General Electric in Schenectady, they met Edwin Hooper, one of Brown's four navy students, now with the Naval Inspector's Office at the company. D-2 observed G.E.'s switchboards, rangefinder stabilizers, electronic computers, and a wide variety of servos.[31] At Ford Instrument in Long Island City, the section saw the company's delicate rangekeepers and a machine for making ballistic cams.[32] Caldwell visited Edwin Land at Polaroid to discuss the optics of rangefinders.[33]

Finally, D-2 learned about electronics, feedback amplifiers, and communications engineering. They visited Bell Labs in New Jersey, which was already at work building an

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[27] WW diary, November 6, 1940. OSRD7 GP.

[28] WW diary, October 9, 1940. OSRD7 GP.

[29] D-2 Diary, November 10, 1940. OSRD7 GP Box 70 collected diaries volume 1. The Quincy sank in the Battle of Savo Island in Guadalcanal in 1942. In 1992 the author was part of an expedition which located and photographed the wreck.

[30] WW diary, October 23, 1940. OSRD7 GP.

[31] WW diary, November 8, 1940. OSRD7 GP.

[32] WW diary, November 7, 1940. OSRD7 GP.

[33] SHC diary, November 1, 1940. OSRD7 GP Box 70. collected diaries volume 1.

electronic director incorporating feedback amplifiers (see Chapter 8).[34] In New Jersey, at Fort Monmouth, the Signal Corps research lab was supporting the Bell Labs work, and hoping to integrate it with their "microwave detection" sets, of which they had 450 on order.[35] The army still had a cultural bias against electronic equipment, which they believed too unreliable for field service. The prevalence of radio, however, and the anticipated importance of radar were forcing greater acceptance of electronics.[36]

D-2's busy first months demarcated the landscape, both geographic and technical, of fire control as it existed at the beginning of the war: industrial firms, military sites, and one university. The bulk of the NDRC's fire control work (and the bulk of NDRC's contracts overall) would occur within the confines of the industrial region bounded by Virginia on the South (Ft. Monroe), Massachusetts in the North (MIT), concentrated in New York and New Jersey. Central technical problems were those Sperry had failed to solve, those which could be taken from the navy and applied to other fields, and those which arose from new technology in other fields, especially electronics. In this initial investigation, D-2 tapped the knowledge of the four pre-war traditions of control systems. Military users, academic scientists, and industrial engineers explained the problems of fire control and how scientific and engineering research might contribute solutions. D-2 absorbed this diverse technical knowledge into its own fledgling organization.

With key problem areas identified, and a budding core of expertise, D-2 began to define its program. At an October meeting Weaver outlined critical areas and assigned responsibilities. Fry would coordinate systems, statistical analyses of errors, and research in servomechanisms. Caldwell and Poitras would investigate electrical analogs for mechanical computers and work on servomechanisms. The whole committee would look at problems of optical rangefinders, evaluate rangefinder operators, perform efficiency studies of manual procedures in loading guns, and improve instruction books for antiaircraft systems. Also, a standardized graphical language for mechanical computers (similar to the one Bush designed for the differential analyzer) would allow more consistent notation across projects. Claude Shannon, now a post-doc at Princeton was suggested to create that language, as he had previously created similar notation for MIT's

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[34] WW diary, October 24, 1940. OSRD7 GP.

[35] WW diary, October 25, 1940. OSRD7 GP.

[36] Caldwell, "A History of Section D-2, NDRC," February 21, 1946, 7. OSRD7 Office Files of Harold Hazen, Box #6.

computers and for relay circuits. In a similar vein, the committee suggested adopting a standard nomenclature for the antiaircraft problem itself, and compiling a table of equivalent symbols used by the army and the navy.[37] Weaver soon circulated a memo by Thornton Fry with suggestions for this new language of control.[38]

In November 1940, after this short but intensive two months of study, section D-2 began letting contracts for research in control. Before examining these projects, however, and D-2 and Division 7's management of the technology, it is worth considering the remarkable novelty of these arrangements. How could a group of university professors and industrial researchers direct one of the military's most secret and most complex technologies? How did the government acquire and mobilize this expertise? What resistance did this new approach meet from established groups? In short: who controlled the technologies of control?

D-2's Fresh Approach

During these early months, D-2, a civilian group, acquired proficiency in a highly-technical, highly-specialized, and highly-classified military technology. For at least ten years, Sperry, the nation's leading control systems company, had tremendous difficulty making progress in this complex field. D-2, in contrast, identified the pressing problems in less than three months and began directing research toward their solution. The members of D-2 had at least three advantages which enabled them to tap new sources of innovation. First, they were either Ph.D.s or academically-trained engineers. Weaver and Fry were among the country's top minds in applying mathematics to practical problems, and Caldwell among the most experienced in applying calculating machines to science. Even Poitras, although an engineer with a master's degree who worked at an industrial firm, had emerged from the engineering science world of MIT, still an unusual pedigree for engineers. This academic background enabled D-2 to use mathematics and theory which were largely absent from previous military and industrial approaches to the problem.

The section's second advantage stemmed from the highly secret and compartmentalized nature of fire control. Backed by a presidential order and holdng the strings on a large purse, these men had unprecedented access. Even in their first two months, D-2 members achieved an

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[37] Diary of Section D-2 meeting, October 16, 1940. OSRD7 GP.

[38] WW to D-2, November 4, 1940. OSRD7 GP.

overall view of fire control that few, if any, had previously enjoyed. They were shocked to find almost no communication, indeed outright hostility, between army and navy fire control designers (section D-1, the radar committee, had a similar experience, noting the army and the navy were unaware of each other's programs).[39] Third and finally, Weaver, Fry, Poitras, and Caldwell were new to the problem, as yet unencumbered by institutions or traditions. They brought not only fresh perspectives, as Harold Hazen later recalled, but also "a range and breadth of experience over a variety of fields that could see relations between fire control and many varied fields of endeavor that, superficially viewed, are unrelated to it."[40] Such seemingly-unrelated fields included telephone engineering, mechanical computing, and applied psychology. These men began to see fire control as a particular case of a general problem of control: a feedback problem, a computer problem, and a stability problem.

The NDRC Reorganizes

Within a year of its founding, the NDRC spent more than $6 million (it would spend more than $500 million between 1941-46), and had grown to such a size and complexity that it needed reorganization.[41] In June 1941, an executive order created the Office of Scientific Research and Development (OSRD), which incorporated the NDRC along with a number of other committees, including medical research. NDRC's responsibilities expanded to include more design, pilot production in some cases, and less fundamental research.

In December, 1942, the NDRC itself reorganized into a more bureaucratic form.[42] The earlier four divisions now became seventeen, numbered instead of lettered, D-2 now became Division 7, still responsible for fire control. Other divisions included ballistics, missiles, subsurface warfare, and electrical communication. Harold Hazen headed the new Division 7. Possibly because of friction at D-2 (see Chapter 7), Warren Weaver moved to head the newly-created

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[39] Guerlac, Radar in World War II, 249 and 252, note 20.

[40] Harold Hazen, Summary Techical Report of Division 7, NDRC Volume I: Gunfire Control (Washington: Office of Scientific Research and Development, National Defense Research Committee, 1946), 5.

[41] Irvin Stewart, Organizing Scientific Research for War: The Administrative History of the Office of Scientific Research and Development (Boston: Little Brown and Co., 1948), 322. Owens, "The Counterproductive Management of Science," has comprehensive statistics for OSRD funding.

[42] Early in 1942 Weaver became afraid the growing size of the NDRC would mean "it will spend a great deal of its energy solving problems which it itself creates." He lamented to Bush his longing for the days of July, 1940, when "the NDRC was small, indefinitely flexible, mobile, and unafraid." WW Diary, March 19, 1942. OSRD7 GP Box 70, collected diaries volume 3.

Applied Mathematics Panel (AMP), which collected mathematicians to provide analysis services to the divisions. Weaver remained a member of Division 7 as an advisor and as liaison with AMP.

In late 1942, Weaver and Hazen planned the transition of leadership. All of D-2's projects were transferred to the new Division 7, except for several "of an essentially mathematical character."[43] These went to the Applied Mathematics Panel, which Weaver headed along with Thornton Fry. The AMP embodied of Fry's vision for the Mathematics Department at Bell Labs, of the industrial mathematician as "a consultant, not a project man."[44] Weaver's new position was no doubt more suited than the messy industrial world of military control systems contracting to his preference for fundamental research and his talents as a science manager.[45]

Division 7 became more established, more bureaucratic, and more procedural than D-2. Hazen, the engineer, administrator, and department head replaced Weaver the science manager. Division 7 meetings became more budgetary and contractual than the mix of administration and engineering which characterized D-2. The division funded no "fundamental" research which did not show immediate promise of contributing to the war effort, and indeed canceled several significant projects.[46]

The character of the contracts reflected the shift away from fundamental work: Division 7's research became more industrial than D-2's. Of fifty-two control contracts let before 1943, fourty-four percent went to academic institutions, the remainder to companies (including industrial labs). Under Division 7, only eighteen percent went to universities. Committee logistics, and hence committee culture, continued the trend toward industry. Under Weaver, D-2 had met fifty-five floors above New York City in the lush Rockefeller Center headquarters of the Rockefeller Foundation. Under Hazen about half the meetings remained in New York but the other half rotated between the industrial organizations on which the division depended, including Sperry Gyroscope, General Electric, the Franklin Institute, the army's Aberdeen Proving Ground, and MIT's Radiation Laboratory. Usually such gatherings lasted two days, including laboratory

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[43] WW diary, November 5, 1942, and November 12, 1942. OSRD7 GP Box 72, collected diaries volume 5. WW to Heads of Divisions 4,5,6,7,14, December 10, 1942. OSRD Applied Mathematics Panel General Records, E-151, Box 16.

[44] Thornton C. Fry, "Industrial Mathematics," *BSTJ* 20 (no. 3, July 1941), 258.

[45] Larry Owens, "Mathematicians at War: Warren Weaver and the Applied Mathematics Panel, 1942-1945," in David E. Rowe and John McCleary eds., The History of Modern Mathematics Volume II: Institutions and Applications (Boston: Academic Press, 1989) 287-305. Also see Warren Weaver, Scene of Change: A Lifetime in American Science (New York: Charles Scribner's Sons, 1970), 87.

tours and equipment demonstrations. Despite its different style, Division 7 carried on D-2's work, retaining its contracts and letting new ones. The easygoing Hazen recalled Division 7 meetings as "'family affairs'...among friends in which the discussion was often brutally frank and in which no punches were pulled."[47]

Division 7 divided into a number of subsections. [*Figure 6-1: Division 7 photo] Their organization indicates the growth in complexity and variety of fire control problems in the two years since Weaver's initial assignments:

7.1 Ground-based antiaircraft fire control.
    Chief: Duncan Stewart, President of the Barber-Coleman Company
7.2 Airborne fire control systems
    Chief: Samuel H. Caldwell, MIT
7.3 Servomechanisms and data transmission
    Chief: Edward J. Poitras, Ford Instrument Company
7.4 Optical range finders
    Chief: Thornton Fry (replaced by Preston C. Bassett, President of Sperry Gyroscope)
7.5 Fire control analysis (administrative connection to Applied Mathematics Panel)
    Chief: Warren Weaver
7.6 Navy fire control with radar (added in 1944 as liaison with the Radiation Laboratory).
    Chief: Ivan A. Getting, MIT Radiation Laboratory

Other Division 7 members and technical aides included J.R. Ragazzini of Columbia, George Valley, Karl Wildes and Charles Stark Draper of MIT, George Stibitz of Bell Laboratories, George Philbrick of the Foxboro Company, Walter MacNair of Bell Labs, John Taplin (who had identified the similarity of feedback amplifiers and servomechanisms), and John D. Tear, Director of Research at the Ford Instrument Company.[48]

Management Style

For five years, D-2 and Division 7 supervised the research and development of control systems applied to wartime problems. During their tenure, D-2 and Division 7 let eighty contracts

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[46] Division 7 Meeting minutes, December 18, 1942. OSRD7 GP, Box 72, Division 7 Meetings Folder.

[47] Harold Hazen, to Irvin Stewart, January 31, 1946, OSRD7 E-82, Office Files of Harold Hazen Box 6. This memo contains Hazen's personal observations on the operation and management of Division 7 and is the basis of the chart in Stewart, Organizing Scientific Research For War, 12.

[48] For a full listing of Division 7 members, consultants, and technical aides, see United States Office of Scientific Research and Development National Defense Research Committee, Summary Technical Report of Division 7, NDRC Volume I: Gunfire Control, (Washington, DC, 1946), 168-169. Hannibal Ford had been mentioned as a possible member of D-2 and Division 7, but he played only a peripheral role. At least three Ford Instrument Company employees, however, Ed Poitras, J.D. Tear, and R.E. Crooke were officially associated with the fire control division.

totaling a bit more than ten million dollars.[49] [*Table 6-1: Division 7 Contracts] These contracts formed the core of control systems work in the United States during the war, and incorporated the four pre-war threads into a synthesis of control, communications, systems, and computing. Projects originated in several different ways. Sometimes the services requested work on a difficult problem. Sometimes the army or the navy turned over existing research projects for the NDRC to administer. Others arose from committee discussions which pointed to a promising or neglected path of inquiry. Sometimes contractors made proposals of their own. Often ideas came up informally, with preliminary arrangements made through members' personal contacts.[50]

The "research contract" itself represented a significant institutional invention. Traditional government procurement practice dictated the delivery of some physical equipment or even piece of paper. Sperry Gyroscope financed its development of antiaircraft devices in the 1930s by selling pilot production lots to the government. "Instead, the research contract assumed that the end item was research and development itself." NDRC contracts freed wartime research from the strictures of procurement and assured a free and flexible control of money. To safeguard this separate sphere, Bush consistently resisted requests from the military for the NDRC to produce the machines it designed (except for small, temporary, and urgent runs). Preferably NDRC contractors (companies or universities) would turn production blueprints over to another organization when research contracts finished. These arrangements also allowed scientists and engineers to remain in the employ of universities or companies rather than become military personnel.[51]More important, the government would pay the *full cost of research*, which included not only equipment and salaries, but also indirect costs, the now-famous factor of *overhead*.

Division 7's standard questionnaire, used to review the status of projects, conveys the NDRC's goals. Even fundamental research was expected to lead to military applications:

> 1) Date of completion of First Phase:
> 2) Date of transition to development:
> 3) Date at which Mr. Gordon's office becomes involved: [This was the engineering transition office, charged with "few quick" pilot production]
> 4) Date of first field trials:

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[49] General information on Division 7 contracts comes from OSRD7, General Project Files. Financial information is from the NDRC index card file, National Archives Record Group 227, Index to Contracts.

[50] *Ibid.*

[51] Dupree "The *Great Instauration* of 1940" in Gerald Holton ed., The Twentieth-Century Sciences: Studies in the Biography of Ideas (New York: W.W. Norton and Co., 1970), 459. See also Stewart, Organizing Scientific Research for War, Chapter XIII and Owens, "The Counterproductive Management of Science," 521, 525-6.

5) Date of first effect on military or naval action (few quick in action):
6) Date of extensive use (effect of mass production):
7) What is the status of procurement of devices or equipment the new device will supplant?
8) If you had more money and men, what time schedule would result?
9) What is the section estimate of the miltiary significance of the work?
10) Is there a shortage of personnel, equipment, or materials in the research or contemplated program?[52]

D-2 and Division 7 members, however, served as more than contract administrators. The group developed its own methods of operating distinct from those of other divisions. Several created central laboratories for their work. Division T, the effort to produce a proximity fuze, set up a lab at Johns Hopkins, which later became that university's Applied Physics Laboratory. Similarly, Division 14 (radar), concentrated all its resources in a single institution, the MIT Radiation Lab (the most expensive NDRC project). The members of Division 14 served primarily as contract administrators; the technical work occurred exclusively at MIT (or its subcontractors).[53]

D-2 and Division 7 took a hands-on approach, acting in Hazen's words as "a closely knit group of experts...studying, analyzing, and formulating service needs in terms of possible projects, then obtaining and directing contractors in the carrying out of such projects."[54] Hazen ran the division from a special office at MIT, Poitras managed the main Washington office. Members took to the road supervising contracts, observing demonstrations, and meeting with military services. Every month or so the division would meet to discuss projects, report progress, solve problems, and discuss technical direction. This arrangement embodied a more multiple and flexible approach than the other efforts, but the lack of a centralized laboratory also had disadvantages. Outside of its small group of members, D-2 and Division 7 could not build up an institutional culture, a potential source of both stagnation and synergy.

Like Bees Pollinating Flowers — Diffusion and Standards

Still, D-2 and Division 7 were intensely involved with a wide array of contracts, industries, and services. They sponsored the technology not only as a funding source or technical consultant. "Like bees pollinating flowers" the members transferred information, techniques and equipment

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[52] Edward J. Poitras, "Tentative Calendar of Instrumental Developments Section D-2," April 27, 1942. OSRD7 Office Files of Warren Weaver, Index Folder.
[53] See Dennis, "A Change of State," and Guerlac, Radar in World War II.

between the contractors, the services, and the other research groups who had not previously been in contact.[55]

D-2 clearly articulated this role at a meeting in July of 1941 that the committee held with consultants to define its relationship to existing industrial contractors. Ed Poitras and Warren Weaver met with Hannibal Ford and R.E. Crooke of Ford Instrument, William L. Maxson, owner of another military contractor, Preston Bassett, President of Sperry Gyroscope, and Al Ruiz of General electric. Poitras told the consultants "NDRC can make contracts which are decidedly long shots which the [military] services can hardly do." He offered to help the companies make university contacts for mathematical studies andhelp them plan and finance test programs. To Bassett, the most valuable thing the NDRC could do would be to standardize testing procedures for gun directors. Hannibal Ford emphasized the need for "coordinated designs of directors with microwave and/or optical rangefinders." Weaver agreed, stating "the NDRC might serve either as an equivalent to the Bureau of Standards or as Consumer's Research; those present had in mind a working arrangement comparable to the Bureau of Standards." Industry envisioned the agency as an information bureau, providing intellectual infrastructure.

To build this infrastructure, D-2 and Division 7 standardized symbols and vocabulary, creating a common language of fire control (though not one uniformly adopted by contractors). More important, the NDRC developed a means of testing fire control devices, creating a standard measure for new machines. The NDRC's broad view of the secret activity in a number of laboratories, industrial, academic, and military, provided a potent source of technology transfer, innovation, and synthesis.

Yet the wartime climate constantly opposed knowledge diffusion. D-2 and Division 7 confronted military secrecy, proprietary industrial information, and lack of cooperation between the army and the navy. These struggles could get rather heated: despite their large budgets and frenetic activity, Division 7 and the NDRC controlled only a portion of wartime research.

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[54] Hazen to Stewart, January 31, 1946. National Archives RG 227, Office files of Harold Hazen. This letter was Hazen's personal history of Division 7 for Stewart's <u>Organizing Scientific Research for War</u>.

[55] I borrow this phrase from Merritt Roe Smith, who used it to describe how skilled workers and supervisors transferred knowledge about precision arms manufacturing among government arsenals, private arms makers, and other industrial firms in the mid-nineteenth century, "The Military Roots of Mass Production: Firearms and American Industrialization, 1815-1913," unpublished manuscript, 1995. Dennis has written the OSRD resembled "a large scale consulting agency," serving as an advisory liaison between industry and military. But that view is too narrow, because it is based only on a study of the atypical Section T (which developed the proximity fuze).

Government laboratories and industrial firms carried on their own relationships with the services. Firms and universities worked together as well; turf battles often ensued. Some companies had much to lose from the new agency: for several decades they held a monopoly on expertise in fire control. Sperry, for example, made no contracts with Division 7. The company already had a relationship with the army and was funding research at MIT (by Charles Stark Draper and Gordon Brown) under a navy project (see Chapter 8). Similarly, Ford Instrument had no NDRC contracts, continuing instead its decades-old relationship with BuORd.

The Contracts

When D-2 began letting contracts in November of 1940, it was still more than a year before Pearl Harbor, but the country's scientists and engineers were mobilizing. The shock of December, 7 1941 is barely visible in the NDRC's working documents: by that point its members had been on a wartime footing for many months. Pearl Harbor surely strengthened the case for advanced control systems. Before the Japanese strike, few questioned the need for antiaircraft defenses, but few also had found it urgent. Afterward, the fear of air attack was etched into American consciousness.

The character and distribution of D-2 and Division 7's eighty contracts map the world of control systems. Twenty-nine contracts went to academic institutions, the remaining fifty-one to industrial firms or laboratories. The largest contract cost $1,273,000 (Bell Labs' gun director work) and the smallest $2,000 (for Norbert Wiener and his assistant), the average was about $145,000. The longest lasted nearly five years, the shortest four months, and the average about two years. More than half of Division 7's contracts went to institutions along the east coast of the United States, the remainder mostly concentrated in the Midwest and California. Most of the contracting organizations remain familiar today: Western Electric/Bell Labs, MIT, Caltech, Princeton, the Franklin Institute, Eastman Kodak, Polaroid, Foxboro, RCA, Bausch and Lomb, Bristol, and Leeds and Northrup, to name but a few.

A Systems Approach

"One must always remember that a fire-control system is more than the sum of component parts," wrote Harold Hazen at the end of the war. "It is an integrated whole with interrelated functioning of all its parts and one is safe in considering the parts separately only if one always keeps in mind their relation to the whole."[56] Before the war an engineering vision of control as a general principle had been taking shape, and the NDRC completed the formulation, taking a "systems approach" to organization, contracts, and engineering.

When the NDRC began operations, Sperry Gyroscope had defined the components of antiaircraft fire control. [*Figure 6-2: AA System]. Input devices, in the form of optical rangefinders and tracking telescopes, provided range, bearing and elevation of the target. As the war progressed, radar took over these functions, at first just for rangefinding and later for tracking. A central computer or gun director integrated these data with settings for wind, terrain, and predetermined ballistics, which depended on the particular gun and shell. The director predicted thc future location of the target based on its speed and direction and calculated an output azimuth and elevation for aiming the guns as well as a fuze setting (the time after firing when the shell would explode). These data were transmitted to the guns, which pointed automatically with hydraulic or electric power controls or manually based on "follow-the-pointer" indicators.

The NDRC came to see this system through the lens of the general feedback device or governor: instruments of perception, integration, and articulation. Of the eighty projects D-2 and Division 7 funded, more than sixty addressed one of these components of the land-based antiaircraft system. Some built individual elements, some worked on interconnection, some studied the human operator, and some worked out theory. Most projects attacked the army version of the problem; some added speed, pitch, and roll sensors for the navy. The remaining projects concerned gun controls for airplanes, torpedo and rocket directors, regulators and governors, and bombing and bombsights (a bombsight is really the reciprocal of an antiaircraft computer), and guided bombs.

Surveying the Contracts

Division 7 contractors developed several new gun directors, building on the Sperry systems or taking entirely new approaches (Project numbers 2,30) (see Chapter 9). A combined project with Division 7, the Radiation Lab, and General Electric built an integrated control system

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[56] Harold Hazen, "Fire Control Activities of Division 7, NDRC," in Summary Technical Report of Division 7, NDRC Volume I: Gunfire Control, 4. Stuart Bennett has noted the "systems approach" in his comparison of British and American fire control work during the war in A History of Control Engineering: 1930-1960 (London: Peter Peregrinus, 1993), 125.

for naval fire control (71,79,85,86) (see Chapter 8). One project modified Sperry directors, adding features and integrating them with radars for which they were not designed (51). The Bristol Company designed plotting boards to lay out the geometry of an engagement on paper, similar to those in naval gunnery (64). Several projects addressed fundamental or theoretical aspects of the fire control problem, including new types of prediction (4,11,12,78), simplified mechanisms (68), or controllers for new types of guns. Many studied or improved optical range finders for various types of directors and gunsights. The Barber Coleman Company modified the British M5 or "Kerrison," director for easier production and put together conversion kits to update the units in the field for higher performance (31). Western Electric similarly modified the Sperry M7 (51).

D-2 and Division 7 put a great deal of effort into instruments of perception, improving classical optical ranging and tracking. Polaroid developed a "short base" rangefinder for use with small guns or aboard an airplane (32). Barber Coleman combined tracking telescopes and rangefinders into a single unit (52). Eastman Kodak and Bausch and Lomb studied improved optics and geometries for ranging devices. They found a major source of error to be optical distortions caused by temperature differential of air within the sight itself. Filling the devices with helium markedly improved their accuracy; the American Gas Association provided its testing lab for this work and designed seals and pressure relief valves for containing the gas (41). Other sources of error included haze, camouflage on targets, low light levels, and misalignment and bad calibration. Some studies considered optical design, reticule patterns, illuminated reticules, and eyepieces (44,58).

"Lead computing sights" moved the gunner's reticule to automatically lead the target for gunners defending against close in attack (61,73). The McMath-Hulbert Observatory at the University of Michigan studied pneumatic controls for these sights (40), as did Eastman Kodak. The Bristol Company designed an antiaircraft rocket director (38), Bausch and Lomb and Barber Coleman antitank sights (59,66), and a stabilizer for an aerial camera, and General Electric a torpedo director (72).

Even at the start of the war, microwave ranging techniques (later called radar) showed the potential to automate perception and replace optical tracking. Still, the technology remained in its infancy, and many feared the enemy would develop suitable countermeasures and render microwave detection useless. D-2 let a contract to Bell Labs for a "radio ranging device" which would replace the most unreliable input to a director. The result of that project, the SCR-547 radar, was nicknamed "Mickey" because its separate parabolic antennas for send and receive gave it the look of mouse ears. This device determined range only, and needed to accompany telescope tracking (14). Westinghouse built a radar which could measure the velocity of a shell as it left the muzzle of a gun (65,83), but most radar work was taken over by the MIT Radiation Laboratory under NDRC Division 14.

Division 7 did not have a central laboratory of its own, but two Division 7 contractors had large laboratories for certain types of fire control problems. Eastman Kodak completed a broad range of work under a single contract including rangefinder improvements, lighting studies, and pneumatic controls (17). The Franklin Institute was became a central laboratory for airborne fire control and conducted studies in torpedoing, bombing, gunnery, rocketry, and integrated systems (33).

Testing posed a major problem for all types of antiaircraft devices. At the start of the war, no quantitative comparisons could be made of the relative performance of new technologies. The Barber Coleman Company built the "Dynamic Tester" which generated "perfect" inputs for gun directors and compared their outputs to ideal solutions (25). George Stibitz at Bell Labs made three digital relay computers for testing, using easily-changed paper tapes as the sources for target aircraft trajectories (60, 63,54) (see Chapter 9). The University of Texas built a simulation facility for airborne devices (50), another lab developed a means for measuring the smoothness of a turret's motion (75) for manufacturing testing.

Division 7's most lasting research concerned the integration component of the control system, particularly in the areas of mathematics and computation (see Chapter 9). Norbert Wiener of MIT studied a statistical method for predicting the future trajectory of an airplane based on its past performance (6,29). As a part of the testing program, George Stibitz built computers that interpolated intermediate points into trajectories and calculated the ideal output for a fire control system (70,74). RCA studied the feasibility of electronic computing methods (48). Engineers at the Moore School of the University of Pennsylvania continued to improve their Vannevar Bush-style Differential Analyzer (62). Differential analyzers at the Ballistics Research Lab, at MIT, and at Penn did computations for a variety of studies, under a contract with the Franklin Institute (39).

Division 7 also referred a few projects to the NDRC's applied mathematics panel such as Columbia University's work on bombing statistics (23) and one for the general analysis of aerial combat (47).

Division 7 also funded the articulation component of control, letting sixteen contracts for investigations in servomechanisms. The MIT Servomechanisms Lab studied fundamental theory and designed a number of servos (1,35) several of which were put into large-scale production (46) (see Chapter 8). Barber Coleman did research in clutch-type servos (27). The United Shoe Machinery Corporation developed boosters to aid gunners in moving machine guns aboard bombers (15), and did fundamental research into hydraulic servos for the gun mounts (16). Other projects, at Leeds and Northrop, for example, developed motor regulators for use aboard aircraft (81). Two projects extended the methods of antiaircraft gunnery to coastal defense, where the units of the control system were separated by long distances and required devices to transmit data back to a base station (20, 34).

Combining perception, integration, and articulation led to an overall view of the system. Harold Hazen suggested studying the human operator "as an integral component of an automatic control system" during the development and design process.[57] Fire control spawned seminal studies on what today we would call "human factors in automation." Seven contracts studied a broad array of psychological and physiological factors in rangefinding and tracking performance (10,43,45,37) (see Chapter 10). All except fatigue produced negative results, showing no effects on ranging or tracking (36,42). Other work sought standards for selection of rangefinder operators including height, vision, intelligence, mechanical ability, interpupillary distance, and coordination. This work sought to put what had previously been an *ad hoc* informal process, namely matching the capabilities of the human to the characteristics of the machine, onto a scientific, psychological, and physiological foundation.

Beyond the Contracts

This summary of the D-2 and Division 7 projects outlines the scope of the research program and conveys a sense of the problems it attacked. As a unit of analysis, however, the

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[57] Harold Hazen memorandum to Warren Weaver, "The Human Being as a Fundamental Link in Automatic Control Systems," May 13, 1941. OSRD7, Office files of Warren Weaver.

contract can be misleading; they were far from equal. A number produced significant advances and were consistently extended. Others showed no promise and were unceremoniously terminated. Many successfully completed their initial assignments and ended. A few created important machines that went into production and into combat. Typically, contracts produced prototypes, pilot studies, and reports. Some projects had been initiated redundantly as "insurance" against the failure of larger, more central efforts. When the primary approaches succeeded, the backup designs were not needed.

Together, the eighty contracts presented a technical and administrative challenge to the members of the sections and their aides. This view alone, however, paints an incomplete picture. Many contracts were small, short, and insignificant. Some were so broad and lengthy that they encompassed many smaller projects. To understand more completely how the wartime work in control systems, we must examine some D-2 and Division 7 projects in more detail. Institutional cultures, individual engineers, and international events all shaped the technologies of control. The military services, the contractors, and the NDRC — both in tension and in synergy — all sought the control of technology.

Chapter 7 narrates the NDRC's evolving relationship to MIT and Sperry which generated friction over ownership of new technologies. Sperry, the pre-war industrial leader in control systems, had a tense attitude toward the NDRC as the new organization usurped the company's expertise in fire control. Furthermore, Sperry had established relationships with the services themselves and didn't need to rely on D-2 and Division 7 for military projects.

D-2 and Division 7 oversaw a number of projects to integrate new radar tracking devices with gun directors to make automatic "blind firing" systems. Chapter 8 compares the two most successful of these efforts, the M-9, produced by Bell Labs for the army, and the Mk 56, produced by General Electric for the Navy Bureau of Ordnance. Both included radars from the MIT Radiation Lab, and both eventually became fully operational systems. The two evolved rather differently, however, due to their differing institutional origins and the technical and organizational worlds they entered. The M-9 was the first control system produced by the telephone company, as well as the first electric fire control device. The Mk 56, in contrast, was built by the established naval fire control industrial base. The Radiation Lab and Division 7's, through liaison Ivan Getting learned how to manage large, integrated technology projects in a way that would later be called "system engineering."

To improve the performance and accuracy of these fire control systems, engineers and mathematicians at MIT and Bell Labs began to study the flow data throughout the system as a problem of communications. Chapter 9 shows how this stance, that the true "signal" could somehow be separated from extraneous "noise," (even when the noise was generated by the human operator), began to unify problems of machine control, communications electronics, and the manipulation of information. This evolution, although incomplete at the end of the war, brought many different technologies, from electric generators to hydraulic power drives to indicating dials, under the control of a single set of theoretical tools. Division 7 endeavored, with varying success, to to understand fire control as a complete system including the human operator, mechanical linkages, electronics, servomechanisms, and computation.

TABLE 6-1: NDRC D-2 & Division 7 Contracts [1]

#	Title	Contractor	Investigator	Supervisor	Start	End	Cost
1	Servomechanisms	MIT	Hazen	EJP	11/1/40	9/1/41	$6,721
2	Electrical Director	WE/BTL	Fletcher	DJS	11/6/40	9/30/43	$224,468
3	Methods of Improving Optical Rangefinders	Cal. Tech.	Bowen	TCF	12/1/40	1/1/43	$127,500
4	Geometrical Predictor	Cal. Tech.	Bowen	DJS	12/1/40	2/1/42	
5							
6	General Mathematical Theory of Prediction and Applications	MIT	Wiener	WW	12/1/40	1/31/43	$28,209
7	Mathematical Studies Relating to Fire Control	Princeton	Shannon	WW	12/1/40	10/1/41	$3,044
8	Studies of Fire Control Equipment and Personnel	Princeton	Flood	TCF	12/1/40	1/31/43	$271,509
9	Mathematical Stuides	U. of Wisconsin	Skolnikoff	WW	2/1/41	8/15/41	$11,730
10	Psychological and Physiological Factors of Importance in Fire Control	Tufts	Carmichael	TCF	3/1/41	6/30/42	$89,586
11	Fundamental Director Studies	WE/BTL	Fletcher	DJS	2/10/41	11/30/45	$166,062
12	Prediction Devices	Iowa State College	Atanasoff	TCF	3/1/41	11/2/42	$28,168
13	Height Finder (Mihalyi)	Eastman Kodak	Bishop	TCF	3/1/41	5/1/43	$39,909
14	Optically Tracked Radio Range Finder (Mickev)	We/BTL	Bown	SHC	4/4/41	4/4/42	$38,324
15	Hydraulic Controls for Small Caliber Guns	USMC	Roberts	EJP	2/1/41	3/31/42	$50,000
16	Hydraulic Servos	USMC	Roberts	EJP	3/1/41	3/1/42	$24,388
17	Fire Control Research	EK	Bishop	TCF/ DJS	6/1/41	11/30/45	$24,364
18							$1,195,604
19							
20	Data Transmission System (Seacoast)	WE/BTL	Clark	EJP	5/1/41	2/1/42	$56,137
21							
22							
23	Statistics of Train Bombing	Princeton	Williams	WW	1/1/43	8/31/44	$123,503
	Bombradiers Calculator	Columbia	Williams				
		U. of Calif.	Nevman				
24							
25	Dynamic Tester	BC	Lilja	HLH	8/10/41	8/31/45	$64,779

#	Title	Contractor	Investigator	Supervisor	Start	End	Cost
26	Simplified Electrical Predictor (Elec. multiplier for T-21, used in M5A2)	GM Labs	McMaster	DJS	9/26/41	2/3/42	$4,113
27	Servoemechanisms	BC	Lilja	EJP	4/1/43	12/31/44	$9,546
28	Intermediate Director	BC					
29	**Extrapolation, Interpolation and Smoothing of Stationary Time Series**	**MIT**	**Wiener**	**WW**	**2/1/42**	**10/31/42**	**$2,000**
30	Electrical Director (BTL II)	WE/BTL	Fletcher	DJS	11/10/41	11/30/45	$600,183
31	Simplified Director (Type BC)	BC	Lilja	HLH	10/15/41	31-Oct	$87,000
32	Short Base Range Finder	Polaroid	Land	TCF/ DJS	12/1/41	8/31/44	$140,000
33	Air-Borne Fire Control Equipment	Franklin Inst.	McClarren	SHC	2/1/42	10/31/45	$1,070,000
34	Pilot Model, Data-Transmission System	Leeds & Northrup	Quereau	EJP	2/1/42	6/30/43	$29,521
35	**Improvement of Servo for 37 and 40mm Guns**	**MIT**	**Brown**	**EJP**	**2/1/42**	**4/30/43**	**$41,973**
36	Effects of Fatigue on Space Perception	Dartmouth	Pearson	TCF	2/1/42	3/31/43	$13,500
37	**Effectiveness of Controls and Data Presentation**	**Foxboro**	**Bristol**	**TCF**	**3/10/42**	**8/31/45**	**$247,112**
38	Rocket Director Development	Bristol	Bristol	DJS	4/15/42	6/30/44	$50,387
39	Computations	Franklin Inst.	Allen	WW	3/27/42	8/31/44	$24,701
40	Gyrscopic Director	U. of Michigan	McMath	EJP	5/15/42	11/30/45	$75,000
41	Helium Retentivity	AGA Testing Lab	Conner	TCF/ PRB	5/1/42	8/31/45	$64,391
42	Relation Between Fatigue and Tracking	Tufts College	Carmichael	TCF	7/1/42	7/31/43	$204,000
43	Acuitites in Telescopic Vision	Harvard U.	Holway	TCF/ PRB	7/1/42	12/31/45	$142,994
44	Emotion in Military Performance Reticle Design	Brown U.	Graham	TCF/ PRB	7/1/43	8/31/45	$38,999
45	Stereoscopi Acuity	Ohio State U.	Bridgman	TCF	7/1/42	11/30/43	$14,767
46	Servos for Medium-Caliber Guns	Westinghouse	Wolfert	EJP	5/25/42	2/29/44	$81,438
47	Air Warfare Analysis	Columbia U.	Hotelling	WW	7/1/42	8/31/44	$513,000
48	**Electronic Computing Devices for Predictors**	**RCA**	**Zworykin**	**DJS**	**7/1/42**	**12/31/42**	**$20,000**
49	Fire Control Analysis Device	Stanolind Oil & Gas	Silverman	SHC	7/25/42	4/30/43	$10,127
50	Testing Plane to Plane Fire Control Equipment	U. of Texas	LaCoste	SHC	9/1/42	11/30/45	$935,000

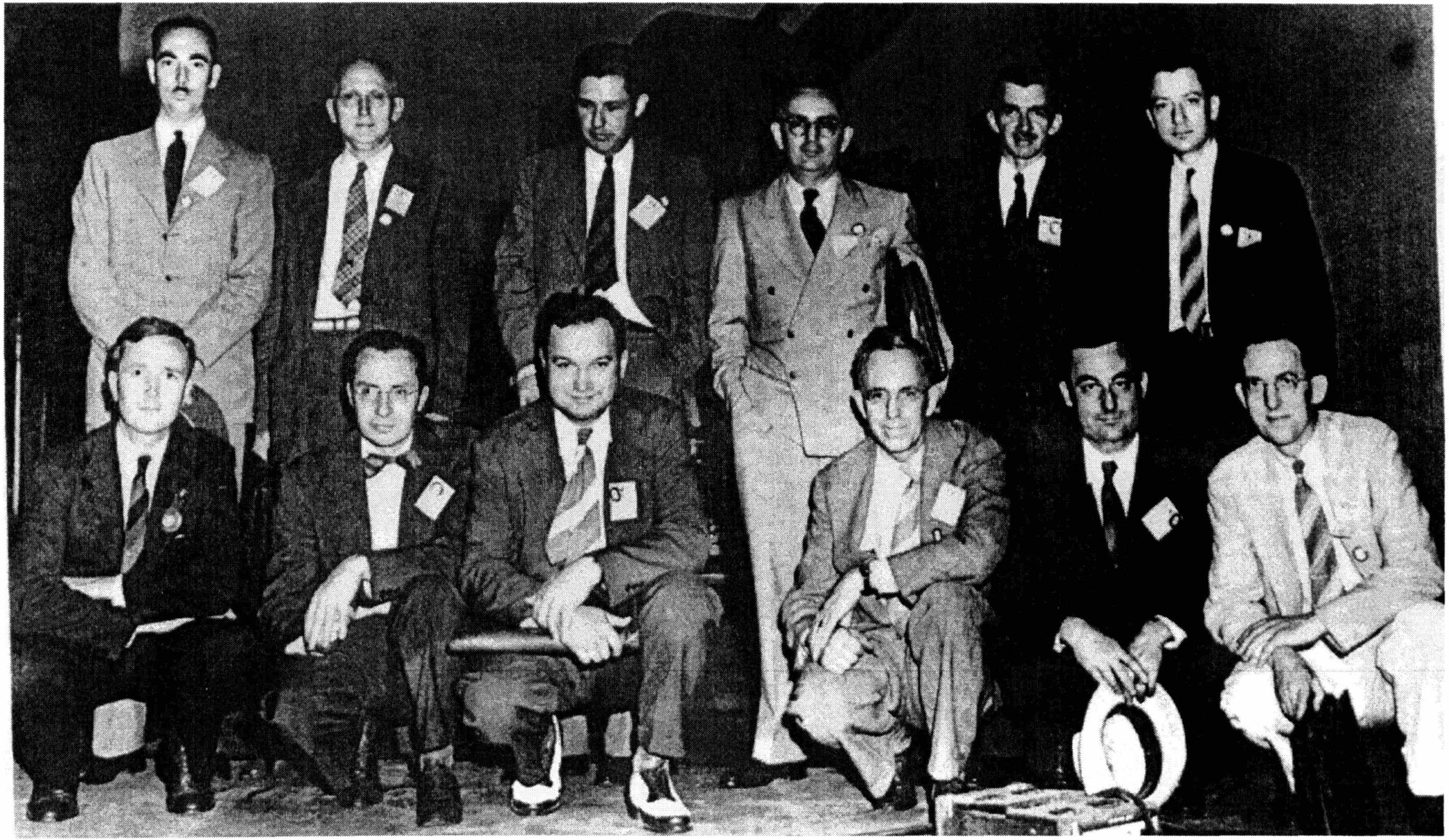
#	Title	Contractor	Investigator	Supervisor	Start	End	Cost
51	**Modification of M-7 Director for Field Conversion**	**WE/BTL**	**Fletcher**	**DJS**	**9/1/42**	**2/1/43**	**$29,945**
52	Combined Tracking and Rangefinding Devices	BC	Lilja	HLH	9/1/42	8/31/44	$12,978
53	Torpedo Director	IBM	Daly	SHC	9/1/42	3/1/43	$34,954
54	Anti-Aircraft Fire Control Testing	U of N. Carolina	Ruark/ Shearin	DJS	3/10/42	10/31/45	$124,880
55	Gyroscopic Computer (pneumatic)	Wilcolator Co.	Taplin	EJP	10/15/42	6/30/43	$4,954
56	Invar Bar for M-2 Height Finder	Bausch & Lomb	Bausch	TCF	10/1/42	5/1/43	$2,392
57	Air-Borne Gunnery Computers	GE	Bowman	SHC	11/1/42	9/30/45	$165,000
58	Range Finder Redsign	EK B&L Keuffel & Esser	Bishop Bausch Keuffel	TCF/ TD	2/1/43	3/31/45	$222,143
59	Anti-Tank Director	BC	Peterson	EJP	11/1/42	2/28/45	$21,174
60	**Punched Tape Dynamic Tester**	**WE/BTL**	**Seibel**	**DJS**	**11/10/42**	**10/31/45**	**$376,094**
61	Anti-Aircraft Computing Sight	Pitney-Bowes	Bernart	DJS	12/1/42	5/31/44	$25,000
62	**Improvement of Differential Analyzers**	**U. of Penn.**	**Brainerd**	**WW**	**12/1/42**	**8/31/44**	**$18,500**
63	Data Recorder	WE/BTL	Dow	DJS	11/1/42	12/31/44	$179,800
64	Chart Type Data Smoother & Retransmitter	Bristol	Waidelich	DJS	1/10/43	8/31/44	$57,305
65	Muzzle Velocity Instrument	Westinghouse	Hanna	IAG	1/1/43	10/31/45	$31,634
66	Tank Fire Control	Bausch & Lomb	Bausch	TCF	2/1/43	8/31/44	$11,622
67	Vector Gun Sight & Assesing Camera	Jam Handy Org.	Campbell	SHC	4/1/43	9/30/45	$115,820
68	Mechanical Director	Byrant Chucking	Rose	DJS	7/1/43	10/31/45	$63,874
69	Steering Mechanism for Torpedoes	Foxboro	Howe	EJP	8/1/43	9/30/45	$50,000
70	**Relay Interpolator**	**WE/BTL**	**Dow**	**WW**	**7/1/43**	**8/31/44**	**$22,075**
71	Gyro Unit for M56 Director	GE	Coutant	IAG	8/1/43	8/31/44	$48,639
72	Torpedo Director	GE	Coutant	IAG	7/1/43	9/30/45	$39,227
73	Course Invariant Sights	Baker Mfg.	Baker	DJS	8/1/43	11/30/45	$80,000
74	**AAA Board Computer**	**WE/BTL**	**Dow**	**WW**	**9/1/43**	**9/30/44**	**$108,220**
75	Mechanism to Measure the Smoothness of Control of Aircraft Turrets	Waugh Labs	Roy	EJP	11/20/43	8/31/45	$16,117
76	Fire Control Electronics	Columbia U.	Ragazzini	SHC	11/15/43	9/30/45	$85,000
77	Redesign of Gun Directors Mk 49	MIT	Edwards	ALR	11/1/43	8/31/44	$8,773
78	Second Derivative Curvature Attachment for M9 Director (T17)	WE/BTL	Fletcher	DJS	12/1/43	12/31/45	$21,179
79	**Gun Director Mk 56**	**GE**	**Leveen**	**IAG**	**1/1/44**	**10/31/45**	**$1,273,532**

#	Title	Contractor	Investigator	Supervisor	Start	End	Cost
80	Aircraft Fire Control Analysis - Patuxent	Northwestern	Calvert	SHC	2/15/44	10/31/45	$485,000
81	Speed Regulator for Motors and Motor Generators	Leeds & Northrup	Lane	EJP	2/1/44	8/31/45	$12,760
82	Control Elements for Fire Control Applications(pneumatic)	Lawrance Aeronautical	Young	EJP	6/1/44	9/30/45	
83	Chronograph T4	Westinghouse	Osbon	IAG	7/1/44	10/31/45	
84	Components for Pilot-Operated Sights	Bristol	Mabey	SHC	7/1/44	9/30/45	
85	Computer for Mk 56 (???)	Librascope		IAG	9/30/44	10/31/45	$348,247
86	???	Armour		IAG			
						Total	**$11,090,595**

[1] *Source: NDRC Index Card File, RG-227, National Archives, Index to Contracts.*
NOTE: Some contract numbers not used

Figure 6-1: Division 7, Fire Control, of the National Defense Research Committee, 1943 (Getting, All in a Lifetime,200).

George Philbrick, Foxboro Co.
Karl Wildes, MIT
Duncan Stewart, Barber Coleman
Warren Weaver, Rockefeller Found.
Preston Bassett, Sperry Co.
Harold Hazen, MIT



Al Ruiz, G.E.
George Stibitz, Bell Labs
Ivan Getting, MIT/ Rad Lab
Lawson McKenzie, U.S. Navy
Thornton Fry, Bell Labs
Samuel Caldwell, MIT

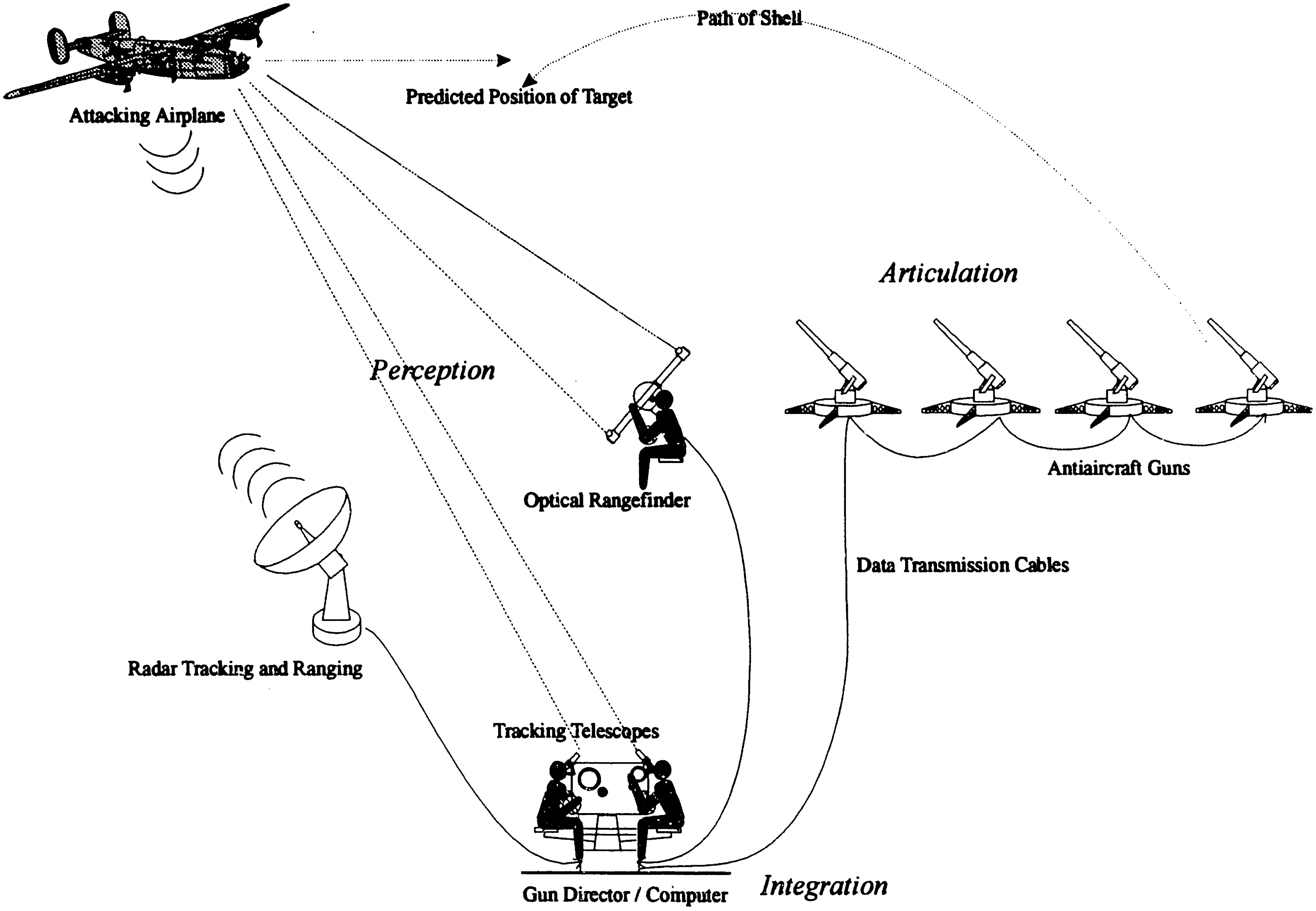


Figure 6-2: Schematic Representation of Antiaircraft System Elements

Chapter 7

"Fire Control for the Masses" and the Servomechanisms Laboratory

The NDRC's research program in control systems, though extensive and far-ranging, did not cover the entire field. D-2 and Division 7, in fact, supplemented more than appropriated the existing technological landscape. They brought in institutions, researchers, and technologies that had not previously been involved in fire control, contracting, for example, with MIT and Bell Labs, not Sperry Gyroscope or Ford Instrument. The prior infrastructure did not wither or evaporate. In fact, the Bureau of Ordnance radically reorganized its research and development, partly seizing control of the technology from its clique of contractors, themselves scrambling to meet the production demands of a wartime boom. With the vast resources of wartime ordnance procurement to back it up, BuOrd's new research division posed a formidable rival to the immature NDRC.

BuOrd would only rival the NDRC however, if they competed for the same resources. Money was not the issue, there was plenty, if not too much, for all. Nor were materials; research, even with its need for special tools and machinery, made modest demands compared to production. Manpower, however, especially scientific manpower, proved the bottleneck over which competing wartime research agendas clashed. Individual scientists and engineers retained a degree of choice, hence they faced local competition and made personal decisions. The contested terrain, however, proved to be where scientific manpower meets material resources: the laboratory. In control, one laboratory stretched in more directions, pulled by more actors, than any other. It was founded by D-2's very first contract, developed close relations with Sperry Gyroscope, and joined servomechanisms with telephone engineering: MIT's Servomechanisms Laboratory, founded by Gordon Brown. Brown simultaneously defined a new laboratory, a professional specialty, and an academic subject.

Through the Servomechanisms Laboratory (Servo Lab for short), changes in the control of technology bore on technologies of control. When the NDRC classified and appropriated Brown's 1940 paper, it plucked servomechanism theory out of the civilian sector and deposited it into the secret world of military research. This action privileged the theoretical work that had been done at his institute during the 1930s over an existing industrial culture of regulators and governors. As Brown's paper made its way through the military and its contractors, it recruited researchers and engineers into the new field. We can reconstruct its path in detail because the NDRC itself carefully tracked the classified document.

The paper, however, accompanied a laboratory, preceding and representing it. When they classified the paper, D-2 granted Brown funds to found the Servo Lab. Through his laboratory, Brown created a space within the university for a vision of control as a general, abstract principle, possibly the first of its type. "Fundamental studies," of servomechanisms began to merge the servo theory developed at MIT by Hazen, Brown, and their colleagues with the feedback theory developed at Bell Labs. This combination entailed a shift from time-domain analysis, which still retained the legacy of power system transient studies, to the frequency domain, characteristic of amplifier design. Nevertheless, throughout the war, "fundamental," remained an unstable category — from both the labs' and the NDRC's perspective.

General, fundamental studies and the seemingly pure space of a lab devoted entirely to servomechanisms could not exist on their own. Their very generality and seeming purity, in fact, depended on an extensive supporting infrastructure of military applications, commercial interests, and pressing wartime emergencies. The navy had failed to prepare itself adequately for defending its capital ships against aircraft, and it eagerly put into production any technology which could aid that defense. BuOrd reorganized its own research and development, one result of which produced a fire control research section akin to the NDRC's own. The new organization, staffed with professional fire control officers, claimed ownership of fire control technology — a claim which inevitably clashed with the NDRC. Such contests characterized the Servo Lab in the first years of its existence as it struggled to find a foothold on the shifting ground of control.

Defining the Outside: The Professional Culture of Control

At D-2's second meeting in October 1940, the committee reviewed the recent work at MIT on servomechanisms. It decided to obtain a copy of Mustin and Rivero's thesis on fire control against short-range attacks by high-speed aircraft, written in Brown's course.[1] The day after the meeting, Caldwell wrote to Brown requesting that his paper, "Behavior and Design of

Servomechanisms," not be published for "open circulation" by the ASME. The NDRC, instead, will "undertake limited publication at its own expense."[2] Thus the curtain of military secrecy descended on servomechanisms, or at least MIT's version of it, just as Brown was about to present it to his professional peers.

What was the state of control engineering at the time, and how the NDRC reconfigure it? The main professional group in America, to which Brown belonged, was the ASME's Committee on Industrial Instruments and Regulators (the American Institute of Electrical Engineers dealt with automatic control only through its committee on automatic substations). As the title suggests, the group related primarily to industrial controls, factory instrumentation, and the various pressure, temperature, and flow regulators those arenas employed. Despite, and perhaps because of, Brown's withdrawal from their 1940 meeting, the ASME became aware of the NDRC's activities. Its president, Edward S. Smith (who founded the group in 1936), wrote to D-2 in early 1941 offering the services of the organization for the wartime effort. He sent a membership list and an evaluation of the members (rating each one with an A, B, or C) which outlined the character of the organization at the time. Of the eighty-nine members, forty-one were associated with companies, seventeen with universities, and three with government agencies. Twenty-six listed no affiliation, being either independent consultants or maintaining professional membership as individuals and not through their employers.

Unaffiliated members may also have worked for fire control companies who did not wish to be associated with feedback. None were listed. Those companies which did appear tended to be either large process-oriented firms like chemical producers (Dow Chemical, Gulf, Monsanto, Standard Oil) or smaller firms which made instruments for those processes (Bristol, Taylor Instrument, Foxboro, Leeds and Northrup). Although Elmer Sperry Jr. belonged to the group, neither he nor any other member listed as an affiliation Sperry Gyroscope, Ford Instrument, Arma, or any of the Sperry Company subsidiaries (one member was from General Electric). The university members included Gordon Brown, Harold Hazen, Charles Stark Draper, and Professor Trinks from the Carnegie Institute of Technology. Government representatives came from the

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[1] Diary of Section D-2 meeting, October 16, 1940. OSRD7 GP.

[2] Caldwell to Brown, October 17, 1940. OSRD7 GP, Project #1, Box 1. Gordon Brown, "Behavior and Design of Servomechanisms," OSRD 39, Report to the Services 2, The Massachusetts Institute of Technology, November, 1940.

National Bureau of Standards, the U.S. Patent Office, and the U.S. Navy Postgraduate School at Annapolis (the only member with a potential fire control connection). Overall, Smith's list shows that in 1940, control engineering as a professional group was dominated by industrial process control. In addition, Smith supplied the NDRC with a bibliography of relevant literature which reflected a similar bent. Nearly all the twenty six entries relate to process controls, except for two of Draper's papers on aviation instruments. The bibliography did not include papers by Black, Nyquist, Bode, or either of Hazen's papers on the theory of feedback.[3]

What would Brown's paper, "Behavior and Design of Servomechanisms," have brought to this community? It represented the high performance, transient, and analytic approach which originated in MIT's power systems studies, which Hazen had applied to servomechanisms, and which Brown and his navy students had developed. It would have brought *control* to a community still defined by *regulation*. But it was not to be; D-2 classified the paper and kept it from public view. The ASME group and their companies, with the exception of the university researchers, would play almost no role in the NDRC's control systems work over the next five years. In 1943, for example, Weaver and Hazen killed a proposal to hold a session on servomechanisms at an AIEE meeting, declaring even that "it is undesirable to use the words servomechanisms or even automatic control in the announcement or program of the symposium."[4] D-2's action defined the community of control negatively: it was still not clear who would take up feedback as a wartime cause, but it would not be this established, professional, industrial, civilian group.

Defining the Inside: Brown's Initiating Text

With this outside defined, the inside began to take shape behind a curtain of secrecy. Brown's paper became the initiating text for a new community of military control systems. When an administrator, researcher, military officer, or company first became associated with section D-2, they were sent a copy of Brown's paper. Controlled distribution meant this textual process

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[3] E.S. Smith to T.C. Fry, February 10, 1941, and attached "Membership List: Process Industries Division, Committee on Industrial Instruments and Regulators," and D-2 Diary, "Conference with Mr. E.S. Smith, Chairman, A.S.M.E. Committee on Industrial Instruments and Regulators," February 14, 1941. OSRD collected diaries, Box 70. Also see Stuart Bennett, "The Emergence of a Discipline: Automatic Control, 1940-1960," Automatica 12 (1976), 115, although Bennett mostly covers the period after 1945.

carefully documented itself. A list in the NDRC archives of who signed out Brown's paper tracks the growth of this new community of control in detail from 1940 to 1945.[5] Of course, all classified papers were similarly tracked, but no trail in D-2's files displays the care and breadth of this paper's path. It was the first, the foundation, the initiation.

Consider the first few entries, for the latter half of 1940 when D-2 was organizing and building its network. The first person to sign out the paper was "Dr. Weaver," followed by "Mr. Poitras." The next six all went to "G.B. Davis, Bureau of Ordnance," demonstrating the early interest of naval fire control in Brown's work. The next eight papers went to Brown's BuOrd students from MIT (two each): Hooper, Ward, Mustin, and Rivero. Soon thereafter telephone engineering applied its methodology to fire control; number seventeen went to Donald Parkinson and Thornton Fry of Bell Labs (see Chapter 8). Papers nineteen through forty-three went to Gordon Brown himself. Before the end of 1940, recipients of the paper included Brig. General Somers of Army Ordnance, Ray Stearns of General Electric, Thomas Doe, President of Ford Instrument Company, Arthur Davis, founder and President of Arma, Harry Vickers of Sperry subsidiary the Waturbury Tool Company, Theodore von Karman of Cal Tech, Sam Caldwell, Carroll Wilson, and "Dr. Bush." The distribution of Brown's paper traces the diffusion of control technology, at least in the flavor developed at MIT in the 1930s, as it covered and defined the secret wartime landscape.

Solidifying the Paper's Absence: The Servomechanisms Laboratory

This canonization of Brown's paper helped spread his ideas and enhance his reputation, but it also stripped his control of the process. Brown no longer possessed the academic's primary tool for selling an intellectual program, publication. But he needed to sell, for in early 1940 Brown and his navy students had begun to set up a laboratory, with equipment donated by Sperry Gyroscope. When his students graduated, Brown soon grew frustrated over a lack of manpower and equipment; without official wartime duties he would be unable to get either. Warren Weaver visited MIT on his initial tour in July, and Brown seized the opportunity, giving him a proposal for the NDRC to fund projects in servomechanisms.

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[4] WW diary, October 8, 1943, WW to Prof. Cromwell, New York University, October 7, 1943, and Hazen to Edward Moreland, October 11, 1943. OSRD E-151 Applied Mathematics Panel general records, Box 5 Servomechanisms Folder.

[5] "Distribution List: 'Behavior and Design of Servomechanisms," OSRD7 GP Project #1, Box 1.

Brown's proposal, academic to the point of pedantry, discussed highly technical aspects of control and circuit theory, and made no mention of military applications. He proposed, "a broad exploration of the properties of bridges, tuned circuits, non-linear tubes, reactors, materials, frequency modulation, frequency proportional to signal systems, television principles, from the viewpoint of their possible usefulness in establishing error, error-derivative, and error-integral signals for actuating control devices in servomechanisms."[6] His language held little interest for those not immersed in the detailed problems of servos. It would have been hardly comprehensible to Weaver at the time, and certainly of little note to anyone in the military. This summer, before D-2 classified his paper, Brown remained a civil scientist. Weaver nonetheless saw the worth of MIT's control systems expertise, and Brown learned quickly. He submitted another, rewritten proposal which displayed considerably more acumen in trying to attract government support. Brown began,

> There now exists at the institute [MIT] a background of experience which has come first, as a result of the work conducted here during the past decade on calculating machines and associated mechanisms, and second, as a result of a formal program of teaching and graduate research on servomechanisms inaugurated a year ago in connection with a program of graduate training for U.S. Naval Fire Control Officers.[7]

Invoking ten years of experience at the institute, Brown emphasized facilities, personnel, experience, and direct military relevance: "thus the foundation on which we could build a program of research on fundamental problems in Fire Control in a relatively short time, and the talent available for consultation and guidance of such a program are really appreciable." Brown's phrase "fundamental problems in Fire Control," today reads like an oxymoron — basic research into an applied problem — but Brown was cannily defining his boundaries. He reached outward and inward, connecting his work to military problems, and addressing it to the essence of feedback.

Almost as a consolation for classifying the paper, Weaver recommended an appropriation for Brown's laboratory. MIT would pursue five projects in "fundamental" studies of servomechanisms. First, a study of relay servomechanisms would attempt to quantify this most simple and standard type of servo. Second, Brown would study "the problem of the control of an hydraulic gear used as a follow-up system for military purposes." This would extend the earlier servo theory to hydraulic controls, as well as extending Hooper and Ward's thesis on gun turrets.

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[6] Gordon Brown, "Proposal," July 15, 1940. OSRD7 GP Box 1, Project #1, Box 1,.

The third project would develop a servo for "for the automatic guiding of an ultra-short-wave target locating apparatus," that is, control a radar antenna to track a moving target. Fourth, and perhaps most difficult, Brown's lab would conduct,

> an investigation of means for measuring and indicating time rates of change of error in a servomechanism when the error is indicated by some electrical quantity such as direct or alternating voltage or by a mechanical quantity such as angle or velocity of a shaft. This problem might be stated as the general problem of developing anticipation networks.

The "rate follow up" in the earlier Sperry directors had first raised this problem. When tracking a target, how do you determine how fast it is going? It sounds simple, except that a tracking signal is jerky and noisy, and noise gets differentiated into very large errors. Studying "anticipation networks," relates to this problem, as a good velocity signal is needed for the prediction in fire control (Norbert Wiener would seriously examine this question). Fifth, the MIT lab would investigate high-power continuous control servos, up to 500 watts.[8] On November 1, 1940, the NDRC allocated $24,500 for this work: D-2's first contract, Project #1. On November 9, Poitras visited Brown at MIT and picked up his paper on servomechanisms to take it back to Washington for restricted publication.[9]

Brown proposed a program not in distributed or integrated control systems, nor even in mechanical computing, but one exclusively concerned with servomechanisms themselves: devices to control the position of heavy machinery by precise, low-power signals. A servomechanism is not a computer or a system or an idea, but a thing, akin to a motor. Brown's program did not explicitly continue MIT's previous experience in computing and information systems, but it did inherit the common threads of rotating machinery, transient phenomena, and engineering science. At this point things should have carried on smoothly. Brown had achieved official recognition, backed up by funding, of his servo work in the previous years. If D-2 had been looking for a central laboratory, they found the ideal candidate. The Servo Lab, in effect, was set up as the nation's primary facility for "fundamental" studies of servos, control systems, and control theory. But Brown was a practical man, he loved the immediacy of machinery; "fundamental studies," by

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[7] Gordon Brown, "A Preamble to a List of Projects for an M.I.T. Group Working on Servomechanisms," n.d. (about August-September, 1940). OSRD7 GP, Project #1, Box 1.

[8] Warren Weaver, "Recommendation for Appropriation," November 1, 1940, OSRD7 GP, Project #1, Box 1. The actual contract was not signed until June 10, 1941, but this was typical procedure for the NDRC at the time. Most project specifications are laid out in these informal "Recommendation for Appropriation" memoranda.

[9] EJP Diary, November 9, 1940. OSRD7 GP Project #1, Box 1.

themselves would be a dry contribution to the war effort. He had other suitors besides the government, and much to the irritation of the NDRC, he did not drop everything for fundamental research.

Besides appealing to Weaver for support, Brown had also pursued an industrial avenue: Sperry Gyroscope. Sperry had become interested in Brown's work through Draper, and the company aggressively courted him when Brown expressed interest in working with General Electric. The summer of 1940, when Weaver visited MIT, Brown consulted at Sperry and Ford Instrument. He returned to MIT with the trunk of his car full of hydraulics and servo equipment. Industry gave him what he needed: equipment, pressing problems, relevance. Brown's original NDRC proposal, in fact, closely follows Sperry Gyroscope's own research agenda at the time. The "automatic guiding of an ultra-short-wave target locating apparatus," for example, matched Sperry's work on radar. Similarly, Brown's proposed study of a "means for measuring and indicating time rates of change of error," had much in common with Draper's work on a "rate gyroscope" to detect rates of change of motion for lead computation in a gunsight, already in process at MIT under Sperry direction. In fact, Brown probably originally prepared his proposal not for Weaver but for the Sperry Company. Brown credits Sperry's Willis with sparking his interest and giving him the term "fresh fundamental approach" to servomechanisms.[10]

Sperry Borrows Brown

Sperry had a project underway with the British Merchant Marine to design defenses for ships traveling up the coast of Norway to the Russian port of Murmansk. Norway was by then in German hands and threatened the British with air attack.[11] As part of this project, in the summer of 1940, Draper was hard at work for Sperry to apply his "rate of turn gyro" to a lead-computing sight for light, naval antiaircraft guns. Draper called it "a disturbed line of sight" device. When calculating the lead, the device would offset the reticule, so all the operator had to do was keep the target in the crosshairs and the lead would be applied automatically [*Figure 7-1: Mark 14

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[10] Gordon S. Brown, Computers at MIT oral history interview with Alex Pang, July 24, 1985, MIT Archives, 50-52, 65. In a meeting with Karl Compton in October, 1941, Weaver observed that Brown's original proposal was made for Sperry, although it cannot be taken as authoritative since Weaver and Brown were at the time at each other's throats. Still, Weaver's observation matches the correlation of Brown's proposed projects with Sperry's interests and Brown's own recollections. See WW diary, October 30, 1941. OSRD7 GP Box 70, collected diaries volume 2.

[11] Gordon Brown, interview with author, August 27, 1994. Brown also tells this story in his Computers at MIT oral history, 66.

Diagram]. The sight mounted on a "dummy" platform, and its motions (still controlled by a human tracker) transmitted via a selsyn to a gun (or guns) which was some distance away. Servomechanisms then "slaved" the gun to the dummy platform with the sight. By the fall of 1940, Draper completed theoretical work on this idea and Sperry undertook a full development program.

Because of Sperry's experience with Gordon Brown that summer and his clear credentials in control, Sperry research director Hugh Willis wanted Brown's help with the servos for moving the remote gun. Brown's servomechanisms paper, which Weaver had sent to Sperry in November, made it still clearer that Brown's analytical skills with servomechanisms would be useful to Sperry.[12] In December. Sperry and MIT's Division of Industrial Cooperation signed a contract, just like the NDRC's, for "fresh, fundamental research" into servomechanisms.[13] Brown was already obligated to the NDRC, so Willis and Draper proposed to D-2 that Sperry "borrow" Gordon Brown for help with Draper's sight. They argued Brown was having good luck assembling engineers for his Servo Lab, but the work was proceeding slowly due to lack of equipment, "without extensive aid from the services or industrial concerns, progress with Institute projects will be very slow."[14] Indeed, by this time Brown had recruited a respectable staff: Albert C. Hall, a graduate student from MIT's measurements laboratory, Donald P. Campbell, a new graduate student from Union College, and George Newton, an undergraduate.[15] Eventually Brown added Jay Forrester, Robert Everett and William Pease to the list. But Brown seemed to be having difficulty making the military and industrial connections necessary to acquire the unusual equipment required for his work. The infant laboratory was growing as a group of people, but it was stillborn as a set of instruments.

Weaver, responding to Sperry's request, asked MIT President and NDRC member Karl Compton that Sperry be allowed to borrow Brown from his NDRC work for three months. Because of the difficulty acquiring the appropriate equipment, Weaver wrote, "Professor Brown's program is not going forward efficiently at this time...The Sperry Company, on the other hand, is

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[12] Gillmor to Weaver, November 19, 1940. OSRD7 GP Project #1, Box 1. This letter thanks Weaver for receipt of the paper, although Gillmor doesn't appear on the distribution list.

[13] Manuscript version of Wildes and Lindgren, A Century of Electrical Engineering. Karl Wildes Papers, MIT archives, Chapter 5, 10-15. For an overview of the Servo Lab and Draper's lab, see John Burchard, Q.E.D.: MIT in World War II (New York: John Wiley & Sons, 1948) Chapter 9, "To Make the Guns Behave."

[14] Diary of visit of TCF, SHC, and EJP at MIT, December 5, 1940. OSRD7 GP Project #1, Box 1.

in a position to furnish Professor Brown immediately with all of the equipment necessary to undertake the important job which they propose." Weaver predicated his willingness to release Brown from NDRC contracts on his continuing to teach navy students, and on the entire lab returning to its NDRC servo work after three months. During that period, the NDRC would try to procure the necessary equipment on loan. In Brown's absence, Harold Hazen would run the Servo Lab, which would focus on building an automatic fuze setting machine.[16] Weaver then wrote to Bush, releasing Brown of his NDRC responsibilities for three months.[17] But Brown's attentions, as it turned out, could not be so cleanly divided.

Sperry wanted Brown's help driving guns from Draper's new sight. The company produced power drives for the Army's 90mm gun to go with its antiaircraft director, and was now working on servos for the army's 37mm antiaircraft gun. The 90mm drives were having terrible problems in production, and the company wanted to avoid similar troubles with the new device.[18] In the fall of 1940, the army's Watertown Arsenal provided a 37mm mount for Brown's laboratory, and he installed it in a confined basement room at MIT.

Designing a fast, powerful, and yet stable servo posed a considerable challenge. Existing devices suffered from a number of performance defects, key among them "velocity lag." This error occurred when the gun tracked a target and was commanded with continuous motion. While it moved, actual gun position would fall a fixed amount behind commanded or desired position, in a "steady-state error." Sperry's practical feedback artists could not solve this problem, but it fell within the range of the more theoretical MIT engineers. Here was a chance to apply Hazen's theory and its fruits to a practical military and industrial problem. Sperry had little expertise in servo theory, but it understood where to find help.

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[15] Wildes A Century of Electrical Engineering manuscript, Chapter 5, 16.

[16] Sperry's development program of the 1930s had produced directors that were accurate enough that the biggest source of error was the "dead time" between when the firing solution was completed and when the shell was loaded into the gun. Not only was this a source of unnecessary delay, but with manual shell loading it varied from shell to shell. Shortening and standardizing, i.e. mechanizing, the setting and loading (or "ramming") the fuze was thus the surest way to reduce this uncertainty and improve accuracy.

[17] Weaver to Compton, December 12, 1940. OSRD7 GP Project #1, Box 1. Weaver to Bush, December 12, 1940. OSRD7 GP Project #1, Box 1.

[18] Sperry Company Report, "Power Controls," February 7, 1944. SGC, Box 40. Sperry's devices were designated M2 (for the 4.7" gun), M3&M4 (for the 90mm gun) and T9 (for the 105mm gun). The controls the Army eventually asked Brown for help with were M1 and M5 for the 37mm and 40mm guns, respectively.

Brown and a student, Jay Forrester, solved the problem of velocity lag with a special correction mechanism. Forrester, raised on a cattle ranch in Nebraska, came to the Servo Lab in January, 1941 after a year at MIT's High Voltage Laboratory. With the help of Sperry's Patent Attorney, Forrester and Brown patented their controller and assigned it to the Sperry Gyroscope Company.[19] In the spring of 1941, they tested it with the Army 37mm gun. At Fort Heath in Massachusetts, a Draper sight, connected to a selsyn data transmission, drove a gun which fired live ammunition over Massachusetts Bay. An operator directed the sight by hand to track and airplane and the gun mount moved remotely to follow the sight. The tests succeeded. The precise gyroscopic instrument rapidly directed the large gun several meters away, and the servos held up to the shock and vibration of the gunfire. The delicate sights, physically offset from the guns, were also immune to the gun's effects.[20] The Sperry/Draper/Brown arrangement had paid off: Draper's instrument of perception drove Brown's articulated gun, through a human operator who integrated the system. Precision harnessed power.

Smug Attitudes and Practical Experience

As Sperry's three-month "borrow" of Brown neared its end early in 1941, Weaver grew concerned that Brown was ignoring his NDRC contracts. He did not profit financially from his work for Sperry, earning only a token consulting fee of one dollar, but his commercial work did overlap his contract for D-2. Sperry treated Brown's results as proprietary information and would not allow Brown to release them to Weaver's group. This withholding directly threatened D-2's self definition as a disinterested clearinghouse for information and controller of secret knowledge about fire control. Weaver wanted to use any and all products of Servo Lab work for the NDRC's purposes, which naturally alarmed the Sperry company. Just because the company supported some work at MIT, they argued, didn't mean all their technology should belong to the government. Weaver appealed again to Karl Compton, who declared all members of Section D-2 should have complete access to all research and development work done at MIT for Sperry by Draper and Brown.[21]

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[19] Gordon S. Brown and Jay W. Forrester, U.S. Patent 2,409,190, "Remote Control System." The patent lawyer signing the document was Herbert Thompson, Sperry's patent lawyer of many years.

[20] Wildes manuscript, <u>A Century of Electrical Engineering</u> Chapter 5, 15-18 quotes Brown's accounts of these tests.

[21] KTC to GSB, CSD, SHC, WW, TCF and PRB. March 1, 1941. OSRD7 GP Project #1, Box 1.

Compton's dictum upset Sperry, which tried to protect itself from the "capture" of its technology by the NDRC.[22] After significant wrangling, Caldwell, Compton, and Brown finally agreed to cancel those parts of Brown's NDRC contract which overlapped with Sperry. Brown consulted with his mentor Harold Hazen (temporarily in charge of the Servo Lab) and agreed the more "fundamental" aspects of servo control, and "reducing to practice" ideas already developed, should remain under MIT auspices, with the more applied work going wholly to Sperry.[23] Brown, who liked the immediacy of the industrial work, dived deeper and deeper into Sperry projects and continued to avoid his NDRC contracts. The original MIT servomechanisms project was slated to terminate in September of 1941. While many of D-2 and Division 7's other contracts were continually extended, Project #1 terminated as scheduled on September 1, 1941. The contract was rewritten to include only the work already done; Brown had spent only about one-quarter of the budgeted funds.[24]

This friction between Weaver, Brown, and Sperry illustrates some of the difficulties D-2 faced in beginning its development program. The new organization could work as a clearinghouse between mutually agreeable organizations with common interests, but established military contractors had little to gain from the NDRC. To Sperry, information exchange meant loss of ownership. From their point of view the NDRC knew little about the company's technology, and would only appropriate it and give it to others. Sperry made its reputation developing advanced and proprietary technology. Whether classified or not, the company placed heavy emphasis on trade secrets, local knowledge, and patents. They were not about to let the technology out of their control just because the government had a new agency for research.

Sperry must have been particularly sensitive in the area of fire control, in which they had put so much effort and had so little to show. In these months, from mid-1940 to mid-1941, Sperry's dominance (indeed its monopoly) in land-based antiaircraft fire control steadily slipped away. In the company's eyes, this erosion stemmed largely from the efforts of the NDRC and its fire control committee. Studies under D-2 contracts showed Sperry's solutions to be inadequate,

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[22] SHC diary, March 8, 1941. OSRD 7 Project #1, Box 1.

[23] SHC diary, "Conference with G.S. Brown," March 5, 1941, "Conference of Dr. Compton, G.S. Brown, and SHC," March 5, 1941, "Conference with H.L. Hazen and G.S. Brown," March 6, 1941. OSRD7 GP collected diaries, Box 70.

[24] NDRC index card file, OSRD7, Index to Contracts. For a brief summary of this episode from the MIT point of view, see Wildes manuscript, A Century of Electrical Engineering Chapter 5, 15-16.

if not plain wrong. Several D-2 contracts explicitly analyzed and corrected flaws in Sperry antiaircraft directors. D-2 was funding an electronic version of the director at Bell Labs which would end Sperry's business in the area altogether (see Chapter 8). Already in 1941 Sperry Gyroscope was transferring plans for its M-4 antiaircraft directors, the pride of its pre-war development program, to Ford Motor Company for quantity production. Company president Preston Bassett lamented, "Sperry gets nothing out of this deal."[25] While Sperry had begun promising projects in radar in the late thirties, the NDRC, with its newly-established Radiation Lab, threatened to usurp that technology as well. Sperry's relationship to D-2 and Division 7, while not strictly one of competition, began in tension. Despite Sperry's leading position in control systems before the war, and even despite their continued involvement during the war, it made no contracts with D-2 or Division 7.[26]

Experienced industry hands saw NDRC members as novices in the complex (and not entirely rational) world of fire control contracting. The scientists, for their part, saw this world as bureaucratic and inefficient. Weaver had the impression that Brown's project for Sperry was replicating work done several years before by the Ford Instrument company. In describing the situation to Compton, he articulated the NDRC's tense relationship to the secret politics of military contracting during this early period of 1940-41:

> It is somewhat peculiar that the relations between the Sperry Company and the Ford Instrument Company are such that the Sperry Company needs to go to an outside man to get a job done which could have been done (and very probably better done) by engineers in the Ford Instrument Company...To the best of my knowledge, two factors have brought about this situation. First, and most important, the Ford Company is a Navy Contractor and the Sperry Company an Army contractor, and they have always been instructed that information was to be kept secret between the two companies. But it is also true, I am let to believe, that the Sperry Company has frequently taken a somewhat smug attitude that they had a great deal of practical experience and that there was very little necessity for

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[25] Bassett explained his anxieties to Caldwell during a visit to the company. SHC diary, September 5-6, 1941. OSRD7 GP collected diaries volume 2, Box 70. Also see WW to KTC, May 1, 1942 which reports a meeting when "the younger Sperry men talked rather frankly concerning the relationship between D-2 and the Sperry company with respect to [antiaircraft] developments." They felt the NDRC was "more or less ducking the Sperry Company." OSRD7 Office Files of Warren Weaver, Box 4, Sperry Gyroscope Folder.

[26] The company and the committee did, however, reconcile. Weaver noted in May of 1942, "Mr. Bassett of the Sperry Company appeared at the door with flowers in his hand, love in his heart, and kisses on his lips." At that point Sperry officially acknowledge and supported the NDRC's efforts in fire control. WW Diary, May 21, 1942, OSRD7 GP collected diaries volume 3, Box 70. This memo records Sperry and Earl Chafee's experience licensing production of its mechanical gun director to other manufacturers. When Hazen took over as head of Division 7, he immediately appointed Bassett as a "part-time" member, formalizing the peace. HLH to PRB, December 1 and December 8, 1942, OSRD7 Office Files of Preston Bassett, Box 53.

> them to learn from other sources. This later point, I believe, has some bearing on their opinion of Section D-2.

Weaver asked Compton, whom he later described as "a special expert in the Sperry attitude toward the NDRC," to try to break down the barrier between the "information and experience" at Ford and the "definite need for material on the part of Sperry Company."[27] Nearly ten years after their integration under a single company, Sperry Gyroscope and Ford Instrument remained separate universes, largely due to their different military sponsors. When Ed Poitras raised this issue with the company, Preston Bassett, President of Sperry Gyroscope, pointed to old wounds, "due to the work of the Sperry Company being of a commercial nature the Bureau of Ordnance had always been reluctant to do business with the company...he suggests that if anything is to be done regarding interchange of information it should be done through the Bureau of Ordnance."[28] As a vehicle for the "exchange of information," then, the NDRC was directly replacing BuOrd's strict controls; the contractors reacted with caution. Furthermore, the lack of communication between Ford and Sperry mirrored that between the navy and the army. The NDRC, acting as a clearinghouse, was trying to bridge both these gaps at the same time. To the engineering administrators, efficiency dictated these walls should break down, but tradition and commercial interests conspired to keep them up.

While Weaver's perception of Sperry's "smug attitude" because of "practical experience" in control systems was probably accurate, they had to have been well aware of the problems with their control systems. They were having trouble producing stable servos for their power drives. On a typical visit, in May of 1941, Bassett, Chafee, and Willis told Ed Poitras of their problems stabilizing servos. They "avoid the cascading of servos. If three servos in cascade are required in an instrument they plan to make the intermediate one a human servo."[29] The company, especially Willis, recognized academic control research could help Sperry out of these stability problems. Draper and Brown held the keys to Sperry's regaining dominance in fire control — hence its willingness to push the NDRC and demand Brown's time.[30] Ultimately, both the NDRC and

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[27] WW to KTC, March 3, 1941. OSRD7 GP. For the "special expert" comment, see WW to KTC, May 1, 1942, OSRD7 Office Files of Warren Weaver, Box 4, Sperry Gyroscope Folder.

[28] EJP diary, Visit to Sperry May 14, 1941. OSRD7 GP collected diaries volume 1, Box 70.

[29] *Ibid.*

[30] Draper seems not to have experienced the friction Brown encountered, perhaps because Draper's field, aeronautics, was still controlled by NACA, a more established organization (recall that the NDRC's charter excluded all NACA terrain). On Draper's relationship to the NDRC, with whom he had no contracts, see Michael

Sperry wanted Brown to solve the same problem: the earlier inadequacy of mechanical gun directors. If the NDRC owned the solution, it could go to any contractor for production. If Sperry owned it, they might regain the favor of their old patron, the Bureau of Ordnance, grown rich with emergency.

BuOrd's Antiaircraft Revolution

While the NDRC had to confront existing interests in fire control, those interests themselves were hardly static. In fact, in the year before Pearl Harbor BuOrd changed radically — responding both to internal shuffling and to wartime events. William Furlong had been chief of BuOrd since 1937. He headed the Fire Control Section after World War I, and introduced General Electric and synchronous electric systems to fire control, with the corresponding ouster of Sperry (see Chapter 2). Despite his earlier innovative role, twenty years later Furlong represented the conservatism of an established technology and its organizational structure. In 1940 BuOrd could boast of its fine systems for main battery control and heavy antiaircraft directors. But it had no similar technologies for directing machine guns, no fire control radar, no antiaircraft directors for small ships. Cause and symptom of these problems, BuOrd relied entirely on its captive contractors; it had no development or test facilities of its own. The intense, frightening first year of European and Asian war brought home the threat the airplane posed to the navy. Germany's invasion of Poland and the Battle of Britain dramatically demonstrated the role of the airplane in modern warfare. Pearl Harbor and the sinking of the British battleship Prince of Wales and heavy cruiser Repulse showed that role might mean the death of the capital ship so beloved by the navy.

The navy defended its ships with guns, technology, and administration. In 1940 a new chief radically altered BuOrd's policies for fire control, for antiaircraft, and for research and development. William H. P. Blandy a had been raised among the technologies Furlong himself helped introduce. A 1913 Naval Academy graduate, Blandy was a certified member of the "gun club." He served as gunnery officer on the battleship New Mexico, which had one of the original Ford Rangekeepers, and also aboard West Virginia, which had a new General Electric system. In these posts Blandy pushed automation and computers as a replacement for manual plotting,

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Dennis, "A Change of State: The political cultures of technical practice at the MIT Instrumentation Laboratory and the Johns Hopkins Applied Physics Laboratory, 1930-45," (Ph.D. dissertation, Johns Hopkins University, winning his ships numerous gunnery trophies.[31] Ironically, in 1938 Blandy saw the future of naval warfare while commander of one of the oldest battleships in the fleet. The Utah had been converted into a floating antiaircraft gunnery school and a target for aerial bombing practice. Sitting on the bridge as the passive recipient of simulated air attacks instilled in Blandy a passion for new defenses against dive bombers and torpedo planes. He came to BuOrd in 1940 to coordinate antiaircraft work and to expedite antiaircraft gun production.

Blandy's personal mission became a top priority for the Navy. An Antiaircraft Defense Board, headed by Rear Admiral Ernest J. King reported in December, 1940 that "the lack of adequate close range antiaircraft gun defense of existing ships of the Fleet constitutes the most serious weakness in the readiness of the Navy for war."[32] In February, 1941, Blandy, the navy's antiaircraft expert, was promoted over a hundred senior officers to head BuOrd; at age fifty, Blandy was the youngest line Admiral in the navy. Antiaircraft was to define BuOrd's mission during the war: the U.S. Navy underwent a veritable antiaircraft revolution. BuOrd spent $4 billion on antiaircraft defenses during World War II, its largest single expenditure. Ships began to bristle with antiaircraft guns. [*Figure 7-3: AA Refit diagram] At the center of this revolution were light, close-range antiaircraft guns, not only because they could fend off dive-bombers and torpedo planes, but also because they could be added *ad hoc* to existing ships. In contrast, large, centralized directors, no matter how accurate, could be installed only during new construction or major refits.

Blandy was frustrated by the conservatism of the fire control clique and found the companies disconnected from practical problems. One historian's observation of gun technology in the thirties applies to fire control as well, "Gunnery lost the vision to succeed. Instead of leading, it went on the technological defensive."[33] Ford Instrument, G.E., and Arma all had machine gun director projects underway, but they produced ponderous, impracticable solutions.

1991), Chapter 4.

[31] When Sperry was having production troubles in 1917, Van Auken offered to Elmer Sperry to detail Blandy to the company and help smooth out production. Van Auken to Elmer A. Sperry, November 10, 1917. Blandy Papers, Library of Congress, Box 1, personal correspondence folder. For biographical information on Blandy, see "The Navy's Gun Man," Sunday Star, Magazine Section, Washington DC, April 19, 1942. Blandy Papers, Box 1, Biographical and Genealogical Folder, and a host of other clippings in that folder.

[32] Quoted in Buford Rowland and William B. Boyd, U.S. Navy Bureau of Ordnance in World War II (Washington, DC: Bureau of Ordnance, Department of the Navy, U.S. Government Printing Office, 1953), 220.

[33] W.J. Jurens, "The Evolution of Battleship Gunnery in the U.S. Navy, 1920-1945," *Warship International* (no.3, 1991), 265.

The main antiaircraft director in the fleet, the Mark 37, was having bad problems with stability of its servos.[34] Decades of secrecy, isolation, and peacetime had divorced the contractors from the changing tactical threat, and the pressures of production had frozen complex designs in an obsolete or inadequate state. Blandy wrote to a colleague,

> When I arrived here I found the Fire Control Section and all of the civilian engineers of the commercial companies could think only in terms of the complete solution, namely: to make the 1.1 [inch gun] capable of bringing down any plane on any bearing and any position angle...Well, as you can imagine, such a director involves enough gyros, cams, potentiometers, etc. to make your head swim, plus a great deal of weight, cost, and time to deliver.

In peacetime, the contractors produced elegant, complete solutions under expensive contracts. Blandy agreed on the need for an ultimate solution, but immediate circumstances required compromises to make antiaircraft directors small, light, and capable of mass production. "I want something in a hurry" he pressed, "which will take care of the much simpler problem of repelling a dive bombing attack on a ship."[35]

BuOrd Reorganizes

Toward this goal, Blandy reorganized the bureau. Before the war, BuOrd remained fairly small, consisting of only forty seven officers at the start of 1940, and vertically organized: one section for fire control, another for mines, another for each type of weapon. Each section oversaw research, development, test, production, distribution, and maintenance for its particular technology. Each reported directly to the chief of the bureau. But as the bureau grew (to 309 officers by Pearl Harbor, more than 600 in 1942), the advantages of this concentrated responsibility dissipated — especially with numerous new and inexperienced personnel. The section heads were weak; too much responsibility burdened the chief. In April, 1941, then, following the example of the Bureau of Ships, Blandy imposed a vertical organization and delegated more authority to senior officers. Divisions now had functional responsibility such as

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[34] These became gun directors Mark 44, 45, 46, and 47. Most were discontinued during development, some advance models entered the fleet but were quickly removed. See Friedman, US Naval Weapons (London: Conway Maritime Press, 1983), 243. Friedman's table is largely derived from United States Navy, The U.S. Navy in World War II, Volume 79, Fire Control (Except Radar). For a good summary of the state of BuOrd's work at the start of the war, and its problems, see Ed Poitras' notes on after meeting with Comdr. France of BuOrd, February 27, 1941. collected diaries volume 1, OSRD7 GP Box 70. Although Poitras might have favored his employer, Ford Instrument, his assessment was still bleak.

[35] Blandy to J.R. Palmer, Commander, USS Utah, November 10, 1940. Blandy Papers, Library of Congress, Box 1 Personal Correspondence Folder.

production, fleet maintenance, administration, and research and development. The Research and Development division (designated "Re") conducted fundamental studies, design, and also production engineering, so it employed all the bureau's engineers.[36] "Re" divided into a number of groups, with Re14 responsible for fire control design and Re4 responsible for fire control. To initiate him into the new world of scientific control, Comdr. M. Emerson Murphy, in charge of Re4, immediately received five copies of Gordon Brown's paper on servomechanisms. Thus BuOrd permanently supplanted the expertise of the fire control clique. In Murphy's words, it ended, "the condition where we are totally dependent on a few fire control companies, such as Ford, Arma, Sperry, and General Electric, for fire control development."[37] BuOrd created a single, specialized organization in charge of all fire control research and development; the navy now had its own version of the NDRC's D-2.

Still, Re4 had important differences from its civilian counterpart. As a military organization, it had much closer contact with the line operations it sought to improve. A number of its reserve officers had been engineers as civilians, and they often went to sea to gain experience with problems and equipment, experience which made them, in Murphy's words, "view things from a much more practical angle."[38] Unlike D-2 and Division 7, BuOrd had its own research laboratories, seventeen in all. BuOrd also contracted for private research, spending about $34 million on research at 162 industrial organizations during the war. It also spent almost $700,000 in educational and research institutions, much of it at MIT.[39] BuOrd did work with the OSRD which Murphy viewed it as a device "to put the laboratories and scientific agencies of the country at the disposal of the Army and Navy." Section T, in fact, which developed the proximity

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[36] The reorganization had actually begun before Blandy was named chief of BuOrd, but it was his plan that his predecessor Furlong began to execute. Julius Augustus Furer, Administration of the Navy Department in World War II (Washington: United States Navy, 1959), 319. Blandy describes the reorganization in detail, as well as much of the Bureau's work during his tenure as chief, in "Final Report of present Chief of Bureau of Ordnance," December 9, 1943, Blandy Papers, Library of Congress, Box 1 Official Correspondence Folder. For the official history, see United States Navy, The U.S. Navy in World War II, Volume 73, Research and Development, 6-10. For a list of BuOrd officers at the start of the war, see Directory, Officers on Duty and Civilian Personnel, Bureau of Ordnance. W.E. Furlong Papers, Library of Congress, Box 4, General Correspondence, Military File.

[37] M. E. Murphy, "Memorandum: Report of Fire Control Section (Re4), Summary of Activities and Accomplishments, and Recommendations for the Future." Reprinted as Appendix A of United States Navy, The U.S. Navy in World War II, Volume 79, Fire Control (Except Radar), 330.

[38] *Ibid.*

[39] United States Navy, U.S. Navy in World War II, Volume 73, 10-41 contains a remarkably frank discussion of the organization of research and its problems; 153-157, "Administration of Research Activities and Contracts," has contracting procedures and numbers.

fuze (BuOrd's primary cooperative project with the OSRD) became a virtual extension of BuOrd. Not all projects, however, were as clearly delineated, technically or organizationally as the fuze. Part of BuOrd's antiaircraft revolution required new guns. Because they defended against faster, closer, and more agile targets than existing guns, new guns needed new controls.

Fire Control for the Masses

Blandy pushed the procurement of two new antiaircraft guns which would cover American warships for the duration of the war: the Swedish 40mm Bofors, and the Swiss 20mm Oerlikon. At the start of the war, the navy used 1.1 inch and 30 and 50 caliber guns to defend against close-in aircraft. The first was just not a good gun, the latter two too weak. Both were aimed by tracer bullets, which made the gunners feel good but whose seeming accuracy proved illusory. In comparison, the 40mm Bofors canon was powerful and fast, firing a 2lb projectile at 160 rounds per minute — but it needed a director to be accurate against moving targets. [*Figure 7-4: 20mm Oerlikon] [*Figure 7-7: 40mm Bofors] It became known for its ruggedness and reliability, mounted in single, double, and quadruple mounts. The Oerlikon, more like a heavy machine gun, fired 450 rounds per minute and was light, easily maintained, and required no external power so it could be bolted down anywhere on a ship. A man could freely swing the gun in all directions with his own muscle power. Blandy recommended adopting both the Oerlikon and the Bofors while still head of antiaircraft, and in November of 1940 Furlong concurred.[40] The Oerlikon began entering service in late 1941; nearly 150,000 were produced during the war. The Bofors, though it was adopted by the Army as well, faced difficult production problems, and entered the fleet in mid-1942, with nearly 40,000 produced during the war.[41] These guns put antiaircraft defense in the hands of the common sailor. Now Murphy needed "fire control for the masses."[42]

Here BuOrd's investment in an MIT connection began to pay off. To help with production, Blandy assigned gunnery officer and former MIT student Lloyd Mustin.[43] Blandy also set up a special anti-aircraft section which reported directly to him, under the direction of ballistics expert Captain E. E. Herman. And at the head of Blandy's new "Radar Desk," was Mustin's

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[40] Norman Friedman, US Naval Weapons, 75-81.

[41] See Rowland and Boyd, Bureau of Ordnance in World War II, Chapter 11, for a detailed discussion of the production of these guns.

[42] Murphy, "Memorandum: Report of Fire Control Section (Re4)," 311.

[43] Blandy to Palmer, January 6, 1941. Blandy Papers, Library of Congress, Box 1, personal correspondence folder.

master's degree partner, Horacio Rivero. Rivero mentioned to Herman that Professor Draper at MIT had been working on a gyroscopic sight, which might be small enough to fit the new 20mm Oerlikon. Murphy and Rivero went to see the sight in May of 1941, and followed soon after with Herman and other Navy officials.[44] They were favorably impressed and supported Sperry and Draper's continued development of the device, both as a gunsight and as a small director for a remotely controlled gun. In June, twelve pilot models of the sights were ordered, eight for the individual 20mm mounts, and four for remote 40mm mounts.[45] In October, 1941, BuOrd ordered 2,500 of the sights for its 20mm Oerlikon guns, and officially designated it the Mark 14 Sight, also known as the Sperry-Draper sight (more than 85,000 were eventually produced). When the device operated the remote gun, it became the Mark 51 director (about 14,000 were produced).[46] [*Figure 7-2: Mark 14 Sight] [*Figure 7-5, 7-6: Mark 51 Director] [Figure 8-11: Mark 51 Director]

The Mark 14 succeeded not because of the quality or precision of its computations, but rather because of its compromises. Range was the most significant shortcut. Rather than using a bulky and slow rangefinder, the operator merely estimated range by eye and then dialed it in by hand — a rough approximation when the range was changing rapidly, as it inevitably would with an attacking airplane. But such errors diminished in significance as the target got closer (in contrast, in pre-war directors close ranges exacerbated errors). The Mark 14 sight hit the right combination of precision, ease of use, and simplicity in the tactical situation for which it was designed. It represented the return of Sperry Gyroscope to naval fire control, and also the triumph of the company's simple tight coupling of operator and machine over the complex, integrated systems produced by the fire control clique (actually, Sperry and Draper had originally proposed a "barber chair" setup where the operator literally sat inside the director, but the navy rejected the option).

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[44] Horacio Rivero, oral history interview, 113. Admiral's biographies, Naval Operational Archives.

[45] Friedman, US Naval Weapons, 86.

[46] A number of accounts of this project survive. The most contemporary is M. E. Murphy, "Memorandum: Report of Fire Control Section (Re4)," 312-14. For the view from Sperry Gyroscope, see Thomas A. Morgan, "The Navy's Mark 14 Gyro Gun Sight," *Sperryscope* 10 (no. 8, August, 1945) 15-17. See also Robert Ward, "Gunsight Mark 14 and Gun Director Mark 51," House Report, January 20, 1944. SGC Box 40. Michael Dennis narrates MIT's role in the transition from instrument to production, "A Change of State," Chapter 4. For the Mark 14 in the context of BuOrd fire control, see United States Navy, The U.S. Navy in World War II, Volume 79, Fire Control (Except Radar), 160-68. Also see Wildes and Lindgren, A Century of Electrical Engineering, 214-15. Friedman, US Naval Weapons, 86.

These compromises, and the innovative coupling of operator and machine, expressed in solid form the combination of industrial, university, and military technology: the Mark 14 embodied relationships between Sperry, MIT, and BuOrd. These relationships existed entirely outside of the auspices of the NDRC and its Fire Control Committee. BuOrd, with its new R&D organization, its recent MIT connections, and a private contractor, remained the cutting edge of naval fire control. You could see it in the gunner's hands.

The Servo Lab's Continuing Work, Oilgear Servos

While BuOrd's ties with university researchers blossomed, Brown's relationship with Warren Weaver soured. The two simply did not get along. D-2 and Division 7's records show no pattern of disagreement as consistent as that between these men. Weaver thought Brown "acted like a baby," and was difficult to work with. But in the fall of 1941, Brown still needed legitimacy for his new lab, and the NDRC could provide it. He still wished to do servo work for D-2, and may even have been tiring of his relationship with Sperry.[47] Weaver distrusted Brown's interest in fundamental research, "it being WW's opinion," he wrote, "that B. will never be satisfied, having once tasted blood [i.e. industrial work], to deal exclusively with a patient long-time academic general program."[48] Hazen proposed extending its contract to include some funding for graduate student work in Brown's absence, but Weaver blocked the move.[49]

Despite this personal friction, Brown's work was going well, and he was learning a great deal from his Sperry experience. He took small, high power electro-hydraulic motors Sperry was producing and included them in servos for tanks and aircraft turrets.[50] He recognized such a lightweight, portable servo could be deployed in large numbers in war machinery. Rather than propose a project to his nemesis Weaver, however, Brown brought it up with Compton. Brown

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[47] Brown called Caldwell in September of 1941 to "do a little unofficial weeping." Caldwell noted "he is not at all happy about the way things are going with Sperry and himself, but SHC did not press for details." Despite the unofficial nature of Brown's weeping, Caldwell wrote it up as a memo and distributed it to the committee. SHC diary, September 8, 1941. OSRD7 GP, Box #1, Project File #1.

[48] WW diary, October 30, 1941. OSRD7 GP Box 70, collected diaries volume 2.

[49] HLH to WW, August 15, 1941, WW to HLH, September 5, 1941, OSRD 7 GP Project #1, Box 1. Meanwhile, the original project (agreed to have continued in Brown's absence) to build an automatic fuze setter did near completion under the leadership of Donald P. Campbell (and the oversight of Harold Hazen), Donald P. Campbell, "Report on a Relay Controller to Provide Proper Fuze Time on the Fuze Setter, M8, Corresponding to Director's Fuze Range," OSRD7 GP Project #1, Box 1.

[50] For a summary of the Servo Lab's work in late 1941, see EJP diary, December 8, 1941. OSRD7 GP Box 70, collected diaries volume 2. And Gordon Brown quoted in Wildes manuscript, A Century of Electrical Engineering Chapter 5, 16.

predicted the Army and Navy Air Corps might need as many as a quarter to a half a million units; he displayed a notably non-academic, non-"fundamental" interest in production issues. The Vickers company of Detroit made a good hydraulic device, but it was not amenable to quantity production — unless a new producer was found, the device could not contribute to the war effort. "It seems impossible," wrote Brown, "for me to do anything more than bring this matter to the attention of someone who might be able to bring a question of this kind into the open."[51] Brown noted that Ed Poitras of D-2 shared this opinion and had suggested writing the note. Compton read between the lines — Brown wished to do more NDRC work but he could not propose it directly to Weaver. Two days later Compton passed the message to Weaver.[52] He also gave Brown's letter to Vannevar Bush, with support from the Army Air Corps and Ordnance Department.

Brown's proposal matched an Army interest. In early 1941, they adopted the British fire control system built around the "Kerrison Predictor," named after its inventor, along with power drive for the gun. The system drove a 40mm Bofors antiaircraft gun, the same one the navy had adopted. The Army standardized the Kerrison as the M5 Director and Brown witnessed a demonstration of the system in the summer of 1941. Sperry, already under contract to do pilot production, was assisting Singer Sewing Machine and Delco to go into full production.[53] But the army was concerned about manufacturing the hydraulic pump and motor which drove the guns. Firestone Tire and Rubber had a model in production which barely worked at all. It had problems with velocity lag, and it lost power at low speeds. Brown could solve this problem; he and the Servo Lab were working with a servo designed by the Oilgear Company of Milwaukee which might replace the troubled British design. Hazen proposed an NDRC project for the Servo Lab to study the problem, Division 7 let contract #35 to Gordon Brown and MIT to begin on February 1,

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[51] It's worth noting that Brown's proposal involved placing the servo in production in competition with the Vickers company, a Sperry subsidiary, giving weight to the argument that Brown's relationship with Sperry had soured. WW to KTC, December 18, 1941. OSRD7 GP Project #35.

[52] KTC to WW, December 20, 1941. OSRD7 GP Project #1, Box 1. Two months previously, Weaver had written to Joseph Boyce at MIT, asking him to report on Brown's work. WW to Boyce, October 14, 1941. OSRD7 GP Project #1, Box 1.

[53] Sperry Gyroscope Company, "Memo: M5 and M6 Director," Box 33, Fire Control Folder, Hagley Museum and Library. For the Kerrison predictor in England, see General Sir Fredrick Pile, Ack-Ack, Britain's Defence Against Air Attack During the Second World War (London: Harrap, 1949), 246-7, and 240 for a picture of the predictor in action. For a diagram of the internals of the Kerrison predictor, see Allan G. Bromley, "Analog Computing Devices," in William Aspray ed. Computing Before Computers (Ames, Iowa: Iowa State University Press, 1990), 188-89.

1942, "For studies and investigations looking toward the immediate improvement of the British Oilgear servo of the 40mm gun and the design of a speed gear servo for application to the 37 and 40mm gun mounts" — a long way from the "fresh, fundamental research," of the previous contract. Brown's group redesigned the Oilgear servo to eliminate velocity lag and made it interchangeable with previous systems. Five months later, the Oilgear servo was successfully tested in the laboratory, at the Aberdeen Proving Ground and at the Antiaircraft Artillery Board, in North Carolina.[54]

The NDRC then contracted with Westinghouse to complete a production design, but the company ran into trouble (Project #46). Brown wanted to help smooth out the problem. Weaver adamantly believed the NDRC should stay out of production — to him Brown remained an NDRC researcher. Army Ordnance also felt the NDRC had no appropriate contribution to make to production design or manufacturing, and wanted them out of the project.[55] The tension between the two men, which had been stewing for nearly two years, came to a head in the summer of 1942. Weaver, by his own account, "unfortunately loses his temper and tells Gordon Brown several things which should have been made clear for him by his mother long ago...This has to be charged over against war nerves." Weaver concluded of the project, "this whole business has been completely messed up by the fact that...Gordon Brown is constitutionally incapable of collaborating in a sensible or adult manner with anyone."[56] Weaver recommended the project be discontinued, and Poitras then refused a request by Brown for further funds.[57]

Brown's lab continued to help Westinghouse under the direction of Army Ordnance. The company, it turned out, had built the pistons in the pumps to too close a tolerance. One of

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[54] HLH to EJP, January 24, 1942. OSRD7 GP, Box 30, Project #35. See also Gordon Brown Computers at MIT oral history, 59-61. Memorandum of Agreement, February 1, 1942. OSRD7 GP, Box 30, Project #35. This wording is slightly inaccurate, because the Oilgear servo was not British. Hazen, Summary Technical Report, 40. GSB to EJP July 3, 1942. OSRD7 GP Project #35. For the official NDRC history of the project, see Lawton M. McKenzie and Ed Poitras, "History, Section 7.3," 4-7, March 22, 1946, OSRD7 E-82, Office Files of Harold Hazen, Box 6. For the Servo Lab's history of this project, as well as the final report on the servo, see "Report of Studies on Remote Control Systems M-1 and M-5," MIT Servo Lab, Division Of Industrial Cooperation Project 6047, November 1942, Servomechanisms Laboratory Papers MIT Archives AC-151 (hereafter referred to as Servo Lab. These papers were the office files of Robert Everett, and are fairly incomplete as documentation of the Servo Lab). Box 2 Folder 8. The Servo Lab devised separate controls for the 40mm gun to be driven by a computer from a data transmission system, and by a human operator with a "handlebar input." See Servo Lab reports in Folder 6 and 7, respectively.

[55] WW to Alan Waterman (Vice Chairman, Division D), August 28, 1942. OSRD7 GP Box 30 Project #35.

[56] WW diary, August 20, 1942, OSRD7 GP.

[57] GSB to EJP, August 22, 1942 and EJP to GSB, August 26, 1942. OSRD7 GP Box 30 Project #35.

Brown's students showed that oil leaking around the piston was equivalent to a damping term in the servomechanism, which actually improved stability, so making the pistons to a wider tolerance actually improved performance. For Brown, finding this problem was the ultimate contribution of an academic lab to the war effort, and of theoretical study to practical problems of servos, "If there hadn't been any MIT [people] leaning over their shoulders, if there hadn't been people looking at these equations long enough and [who] had some insight into all of the factors that contribute to the instability" the device never would have worked.[58]

That such similar tensions flared between Weaver and Brown on two successive projects, suggests something more than personal conflict. The two men simply had different ideas of how the NDRC, and the research it sponsored, should contribute to the war effort. Brown saw little distinction between his role as a consultant and as a professor. In wartime, he would do whatever he could to make automatic control useful in the field — even if it meant working on a factory floor. Weaver, in contrast, held a more traditional view, consistent with his background as a "science manager" before the war. Brown's work proved effective at the level of the individual researcher but his seemingly casual crossing of institutional boundaries was unacceptable as NDRC policy. With institutional threats coming from every corner, not least from BuOrd's new research division, D-2 would have to carefully define its role to avoid becoming dissipated as yet another wartime industrial bureaucracy. Bush called his engineers "scientists," so the military would distinguish them from "the sales engineer coming from one of their contractors." Brown, Weaver believed, needed to behave more like a "scientists," and less like an engineer.

Larger forces intervened to help resolve these differing philosophies. In the NDRC reorganization in December 1942, Weaver left D-2 to head the Applied Mathematics Panel. D-2 became Division 7, headed by a man who could not have been more friendly to Brown and his program: his mentor and department head, Harold Hazen. No evidence suggests a causal link between Weaver's departure from leading D-2 and his disagreements with Brown. But Karl Compton, NDRC member and president of Brown's home institution, often mediated between Weaver and Brown. Compton certainly had Bush's ear, so a causal link is within the realm of possibility. Still, Weaver remained a consultant to Division 7 and continued to voice his opinions

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[58] Gordon Brown, Computers at MIT oral history, 63.

on Brown. It remains a remarkable coincidence that in a time of intense conflict, for the powerful position of sponsor, Brown's harshest critic should be replaced by his closest friend.

What Was the Servo Lab?

When D-2 stabilized as Division 7 in late 1942, the Servomechanisms Laboratory's identity stabilized as well. Brown had spent two years carving an institutional and conceptual space for servomechanisms, servo engineers, and the Servo Lab. "We were seeking an identity," Brown recalled of the period.[59] Until late in the war, the identity he found did not encompass broad notions of systems or computers, but only the servos themselves: powerful motors, harnessed by feedback to move with precise elegance.

What was the Servomechanisms Laboratory? Despite the capital letters, it had no formal status within MIT. It was a place, at first in a basement lab and then in a much larger building on the MIT campus. Servo Lab was also a label for about a hundred people, including engineers, students, machinists, administrators, and a handful of professors. Under Brown's direction the lab maintained an educational mission; young engineering students managed major projects. The Servo Lab was equipment: gun mounts, electric and hydraulic servos, measuring instruments, and a rolling platform to simulate a ship at sea. "There was no formal announcement," recalled Brown of the Servo Lab, "it just grew, because people had a kind of competence that fitted the bounds of a particular application." And during the war, this competence acquired a reputation with the army, the navy and industrial firms.

No company related to the Servo Lab like its original sponsor. Sperry Gyroscope's association did not end with the Mark 14 gunsight. The company and the lab remained intimately connected throughout the war. Sperry personnel worked full-time in Brown's laboratory, where they were treated "almost...as part of the family." For Brown's part, "we appreciated, I know, the chances and privileges of going down to Sperry [in Long Island]."[60] With Sperry as a "catalyst," the Servo Lab also retained a close tie to Draper's "Confidential Instruments Laboratory." Partly as a result of the Mark 14 and Mark 51 projects, Brown's group were seen as the "servo arm" of Draper's gunsight work, building the power drives that responded to Draper's delicate calculating

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[59] Gordon S. Brown, Computers at MIT oral history, 75-76. For one description of the Servo Lab's operating environment, see Kent C. Redmond and Thomas M. Smith, Project Whirlwind: The History of A Pioneer Computer (Bedford, Mass.: DEC Press, 1980), 10-12.

[60] Gordon S. Brown, Computers at MIT oral history, 50, 65..

mechanisms — a position Brown looked on with favor. Draper already had reputation and prestige, Brown was building it, and the association could only benefit his new laboratory. Brown explicitly imitated Draper's laboratory procedures, especially the practice of building the pilot models of new devices.[61] In contrast, the Radiation lab built up its own servo group; while it let some small contracts to Brown for servo design, the devices were not used in practice (See Chapter 8).

Harris and Hall: Servos as Feedback Amplifiers

The Servo Lab's growth paralleled an intellectual transformation, from transient analysis to the frequency domain. For Brown, lab and ideas were intimately connected; he considered frequency response techniques "very important to the growth of the Servo Lab and the development of servo theory."[62] Initially, Brown and his engineers used transient analysis, derived from his and Hazen's work. They worked directly from the differential equations that specified a system, adjusting design parameters to get the desired transient response. Brown and Forrester's early work for Sperry on the 40mm power drives utilized transient techniques exclusively, as did nearly all Servo Lab work until the end of 1942.[63] Typically, engineers would get an existing servo as a sample from a company or military service and test it for transient response to a step input, or a constant velocity input. It was quite difficult to translate these responses into meaningful design criteria, especially when working with an existing system. The inadequacy of the transient approach, then, related to the Servo Lab's institutional position: it derived from the lab's practice of closing feedback loops around existing actuators.

Radar also stressed the limits of the transient approach. One of the original projects defined by the Servo Lab was to make a radar drive a servo to move the antenna and track a target automatically. But radar produced unruly electrical signals with noise from a number of sources. A servo would try to translate this noise into motion, producing grinding gears, jerky motions, and possibly instability. Servo Lab member Albert C. Hall remembered simultaneously the intrusion of noise and the attack on Pearl Harbor,

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[61] Gordon S. Brown, Computers at MIT oral history, 65-67, 71.
[62] Gordon Brown, Computers at MIT oral history, 97.
[63] See, for example, "Description and Operating Instructions of the Sperry MIT Automatic Remote Control System for the T-36 Gun Carriage," Division of Industrial Cooperation Project 6041. August 11, 1942, and several similar reports in MIT Servomechanisms Laboratory,. MIT Archives, AC-151, Box 2 Folder 4. This report was not declassified until 1962.

> I well remember an instance in which MIT and Sperry were cooperating on a control for an air-borne radar, one of the first such systems to be developed. Two of us had worked all day in the [Sperry] Garden City laboratories on Sunday, December 7, 1941, and consequently did not learn of the attack on Pearl Harbor until late in the evening. It had been a discouraging day for us because while we had designed a fine experimental system for test, we had missed completely the importance of noise, with the result that the system's performance was characterized by large amounts of jitter and was entirely unsatisfactory.[64]

Control engineers then, needed a new way to conceptualize feedback. Signals were the key. Hall noted "the advent of radar required the controls engineer to design equipment to operate well in the presence of signals that he could not even describe in terms then in general use."[65] The advent of radar, with its close affinities to communications electronics, required that designers understand control systems in the frequency domain.

Herbert Harris explicitly drew the analogy between servomechanisms and feedback amplifiers. The NDRC published Harris's "The Analysis and Design of Servomechanisms," like Brown's original paper, as a restricted report.[66] It applied the notion of frequency response, "used in the radio and telephone arts," to a servomechanism. Building on the work of Taplin and Brown, Harris brought together MIT's servomechanisms with Bell Labs' feedback amplifiers, "The recognition of the similarity between servomechanisms and feed-back amplifiers makes available to the automatic control engineer many valuable analytical tools developed by communications engineers." Harris proposed a general vision of control, based on the functions of a system's blocks and not on its physical structure. In servo design, frequency response provided "a powerful aid in thinking about the various factors that can produce stability or instability in a system." He employed Black's characteristic equation, Nyquist's stability criterion, and Bode's magnitude-phase relationship to discuss, probably for the first time in print, a mechanical system with the terms and methods of the communications engineer. When he left MIT in the spring of 1942, Harris went to work for Sperry Gyroscope.

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[64] Albert C. Hall, "Early History of the Frequency Response Field," reprinted in Ralph Oldenberger ed., Frequency Response (New York: MacMillan, 1956) 4-5. Much of this volume is a reprint of a special edition of *ASME Trans.* 76 (no. 8, 1954). The other Servo Lab engineer with Hall that day was George Newton. See Wildes manuscript, A Century of Electrical Engineering Chapter 5, 19.

[65] Hall, "Early History," 4.

[66] Herbert Harris Jr., "The Analysis and Design of Servomechanisms," OSRD 454 Report to the Services 23, The Massachusetts Institute of Technology. This paper was revised and published as "The Frequency Response of Automatic Control Systems," *AIEE Trans.* 65 (1946), 539-46.

Following Harris, Albert C. Hall explored in detail the implications of a frequency response approach to servos. His 1943 dissertation, "The Analysis and Synthesis of Linear Servomechanisms," formulated "a servomechanism design procedure based primarily on an analysis of the system response to sinusoidal inputs of various frequencies" (the language "analysis and synthesis" alludes to classical electrical network theory).[67] Unlike Harris, Hall did not build his analysis on the analogy between servos and feedback amplifiers. He acknowledged the similarities, but also some important differences: servo designers are concerned with precision, amplifiers designers are not, and servos work in a much lower frequency range than feedback amplifiers, so the electronics are much easier. Hall recognized a complex of tradeoffs between transient and sinusoidal representations. For example, frequency domain data was easier to obtain, but servo performance was ultimately specified in the time domain, as transient response.

Hall's analysis reflects the realities of working in an academic lab tied to industrial concerns. He does not offer a rigidly defined design technique, but rather a set of guiding principles for the servo designer, "It is not possible to set up a formal, well-defined system of servo design because of the individual nature of specific applications." He developed a number of graphical and analytical techniques which the designer could use as tools including a "transfer locus" plot, compensating networks, lead controllers, and integral controllers. Reflecting the Servo Lab's abiding interest in reducing the velocity lag of tracking servos, Hall devoted two significant sections to "minimum velocity error servos."[68] More than most academic writing, Hall's work conveys the limitations of theory in a practical environment.

Harris and Hall's work fundamentally changed the practice of control system engineering at the Servo lab. Using frequency response, Servo Lab engineers injected sine waves of varying frequencies into servos under study and plotted the magnitude and phase of the response. Until

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[67] Albert C. Hall, The Analysis and Synthesis of Linear Servomechanisms, (Cambridge: The Technology Press, MIT, 1943). Hall's thesis was published as a restricted report in 1943, and then reprinted in 1947 when it was declassified. For a technical discussion of Hall's paper, see Bennett, A History of Control Engineering, 1930-1960 (London: Peter Pegrinus, 1993) 140-43.

[68] In a version of his thesis published as a paper in 1946, Hall overcame his hesitation at retaining the distinction between servos and feedback amplifiers, and acknowledged his "transfer locus," to be identical to the Nyquist diagram. He even changed his title to "Application of Circuit Theory to the Design of Servomechanisms," further eroding the boundary between electronics and servomechanisms. Albert C. Hall, "Application of Circuit Theory to the Design of Servomechanisms," *JFI* 242 (no. 4, 1946) 279-307. A close comparison of this paper with Hall's thesis, though beyond the scope of this chapter, would detail the further merging of electronics and servo theory during 1943-46.

1943, Servo Lab reports presented only transient analysis. After Harris and Hall, the reports all include frequency response plots and Nyquist diagrams.[69] Ironically, control engineers at MIT used a graphical technique borrowed from another discipline to define their own technological practice.

Conclusion

In addition to building servos themselves, the Servomechanisms Laboratory defined servos as a field, both intellectual and institutional. It was born through exclusion, as the NDRC appropriated the theory of feedback controls from the civilian engineering world. During the course of the war, the NDRC distributed two hundred ninety four copies of Brown's paper to the industrial, academic, and military organizations, establishing and defining a new landscape of control. Recipients included:

- The United Shoe Machinery Corporation, which manufactured power drives for antennas and guns
- Chrysler Corporation, which made Sperry antiaircraft directors and tracking radars for the Radiation Lab
- The US Navy Postgraduate School at Annapolis, for instruction in fire control
- John B. Russell and J.R. Ragazzini, NDRC researchers at Columbia University
- The Franklin Institute, which studied airborne fire control for the NDRC
- The Navy Department's Coordinator of Research and Development
- English and Canadian control systems researchers
- John G. Brainerd of the Moore School of Engineering at the University of Pennsylvania
- The Manhattan Engineering District.

The MIT Servo Lab, then, became the founding member of a new control engineering, defined by war, and characterized by collaboration of academic engineers and industrial concerns on high-performance, fast-acting mechanisms. No longer were distant battleships the primary targets, no longer were fire control computers large, centralized machines, no longer was servo behavior studied as a purely transient phenomenon, and no longer did the navy's fire control

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[69] See, for example, Stephen H. Dodd Jr., "Design and Test of a Hydraulic Transmission," MIT Servo Lab, 1945. Servo Lab Files Box 1 Folder 2; "Automatic Control Characteristics of a 0.682 cubic-inch per revolution Oilgear Hydraulic Transmission," June, 1943, Servo Lab Files Box 2 Folder 2; both of these reports present frequency domain analysis. The only Servo Lab report in the archives which uses frequency response before 1943 is a project for stabilizing a radar antenna on a ship done under contract to Raytheon. The project report lists no author, but it was likely done by Hall or Harris, since this was the type of project which led them to their frequency response work. Servo Lab Files Box 1 Folder 15.

clique have a monopoly on the field's technical secrets. The Servo Lab rode broad and converging trends in the technology of control and the control of technology.

Initially, the Servomechanisms Laboratory understood servos as manifestations of the classical governor. The lab built drives which represented only a single component of a larger control system: articulation. Pumps and motors articulated the output from perception and integration instruments designed by the Servo Lab's collaborators, Sperry and Draper. The laboratory's vision remained local, tied to particular artifacts which embodied feedback control as powerful, precise motors. Harris and Hall began to expand this local vision with their frequency domain approach. Now the mechanisms themselves became processors of signals, just like any other component in a larger system. This vision drew heavily on telephone engineering, and it was telephone engineers who explored the utility of feedback control for designing overall systems rather than just individual servos or amplifiers.

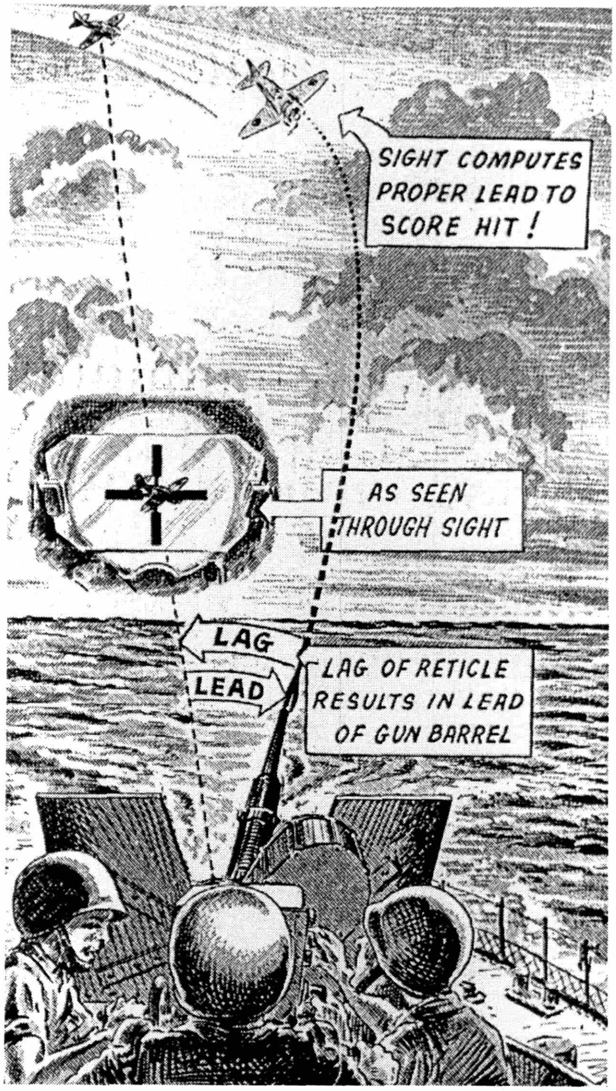
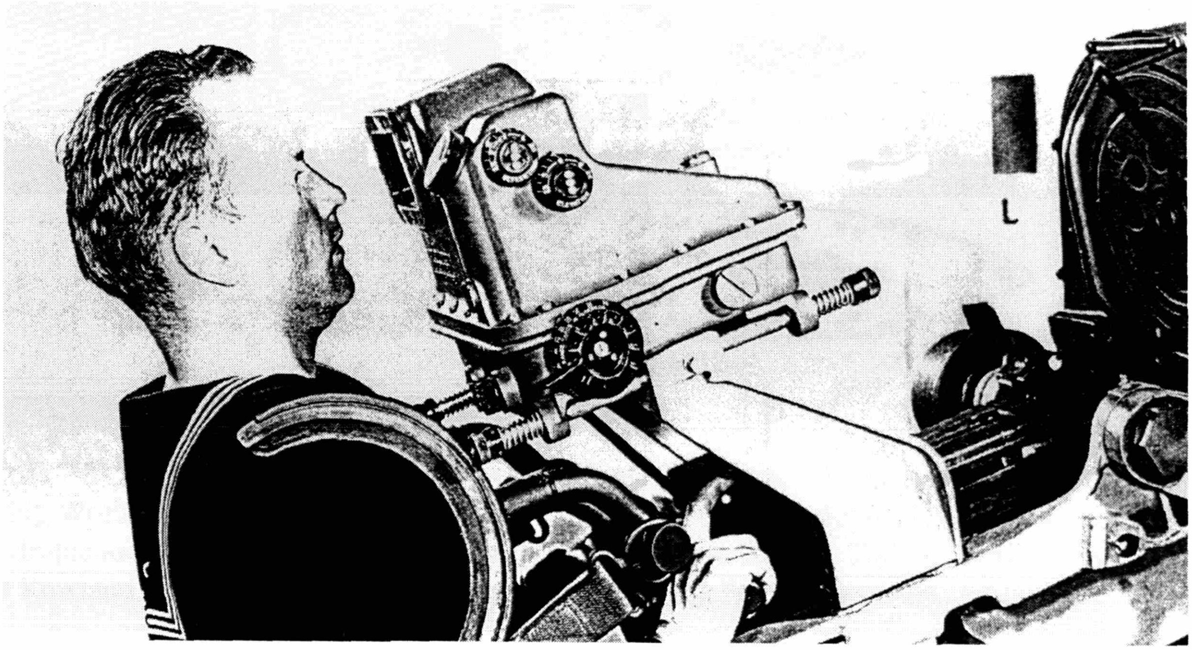


Figure 7-1: Theory of operation of Sperry/Draper Mark 14 lead computing gunsight. ("Gun Sight Mark 14, Gunner's Operating Bulletin," United States Fleet, Headquarters of the Commander in Chief, Sperry Gyroscope Company Papers Box 20, Hagley Museum and Library).

Figure 7-2: Mark 14 lead computing gunsight connected to Oerlikon 20mm antiaircraft gun (Hagley Museum & Library)



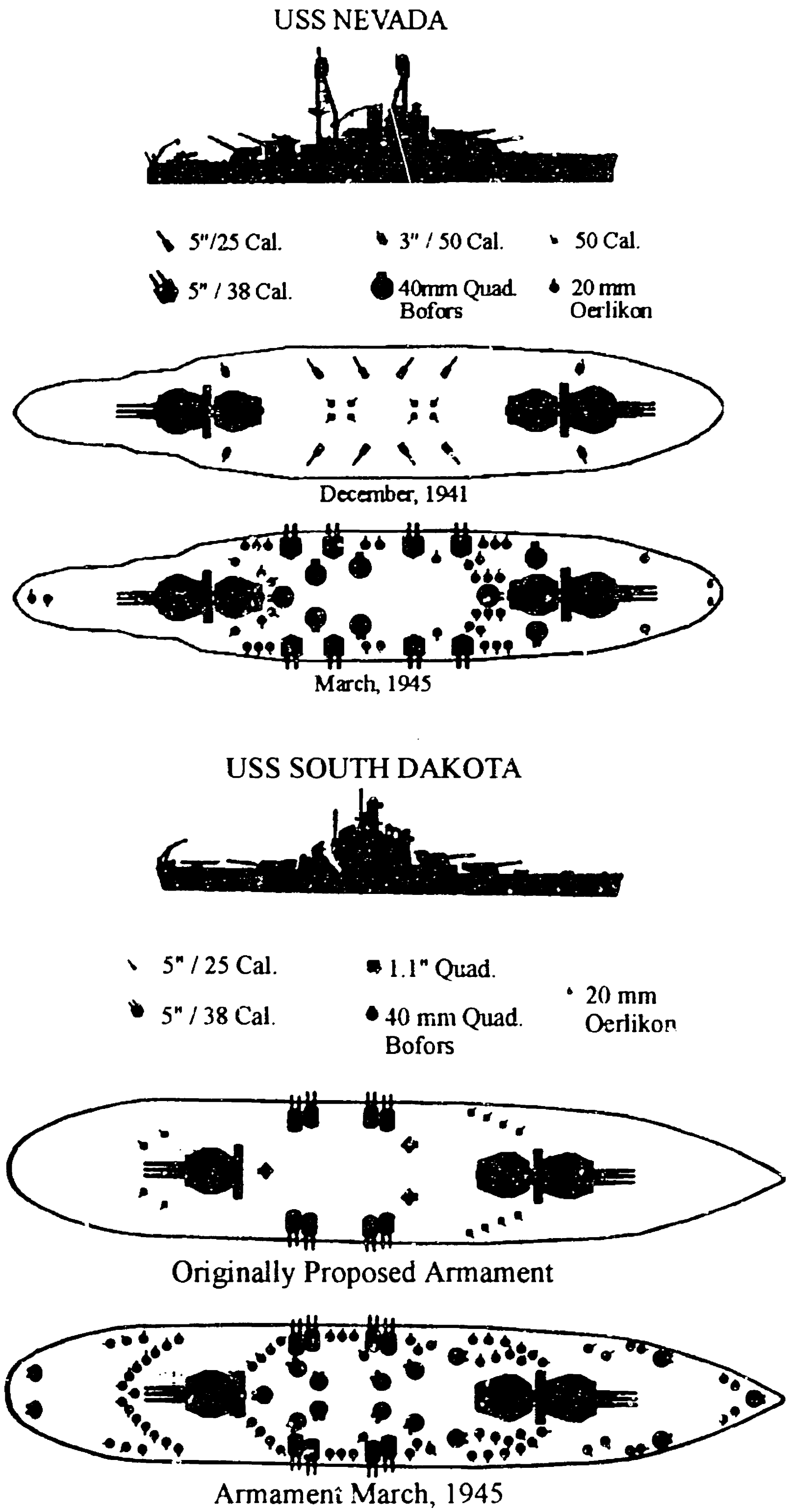
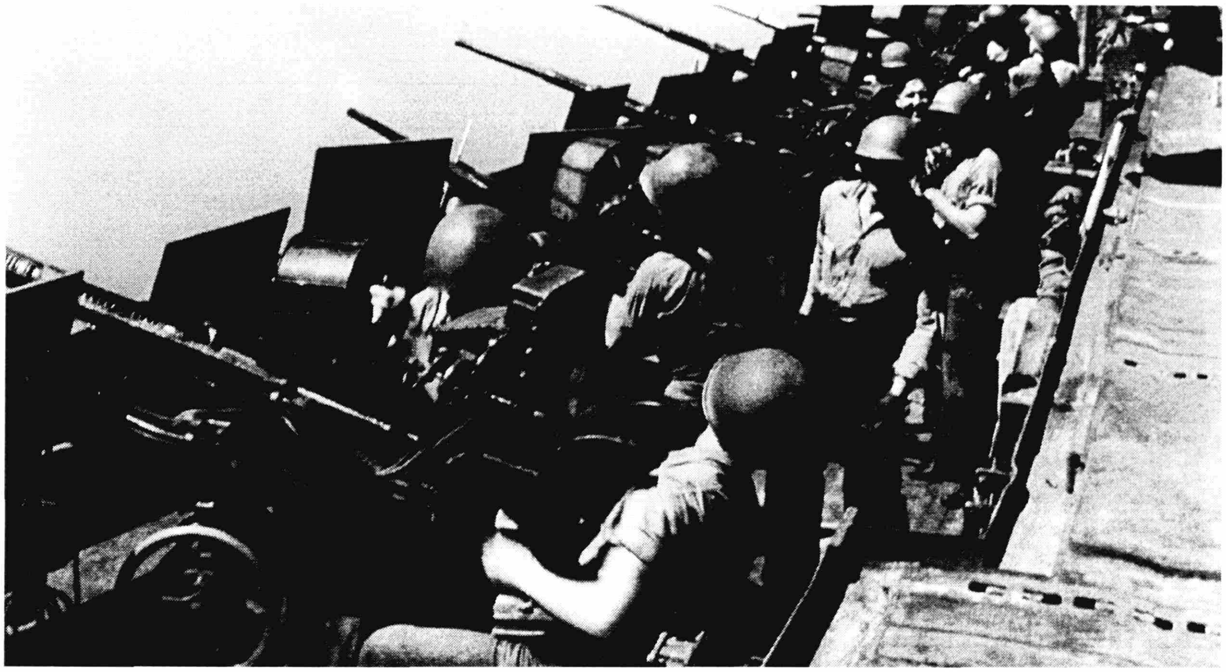


Figure 7-3: The Blandy Antiaircraft Program. Growth in antiaircraft weapons on typical battleships during World War II. Note the removal of 5"/25 and 3"/50 guns and the addition of 40mm Bofors quadruple mounts, dual 5"/38 mounts, and 20mm Oerlikons (Blandy Papers, Library of Congress, and Rowland and Boyd, US Bureau of Ordnance in World War II, 243, 246).

Figure 7-4: Gunners with 20mm Oerlikon guns and Mark 14 sights defending the USS Hornet, February, 1945. (Charles Kerlee, from Christopher Phillips, Steichen at War (New York: Harry Abrams, 1981, 95).



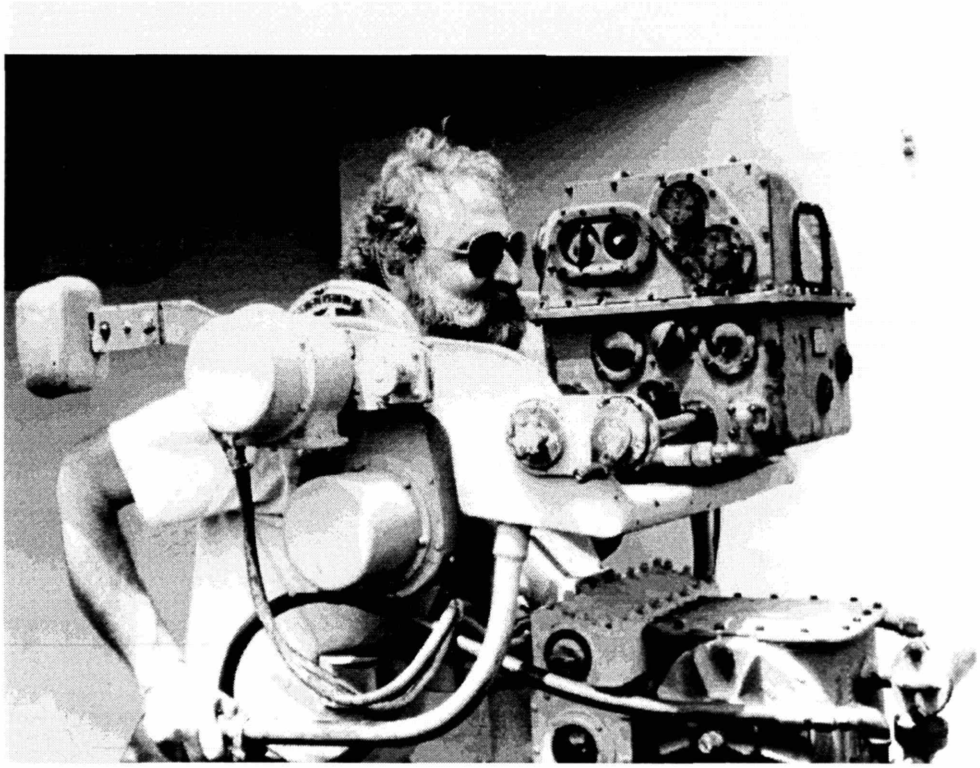


Figure 7-5: Mark 15 sight (modified Mark 14 for greater range) integrated into Mark 52 director, with handlebar controls. Operator follows target with director which remotely drives 40mm or 5-inch gun mount. (Photograph by the author aboard USS Massachusetts, Fall River, Mass.)

Figure 7-6: Gunners operating a Mark 51 director, connected to a 40mm Bofors gun off to the right (from Christopher Phillips, Steichen at War , New York: Harry Abrams, 1981).

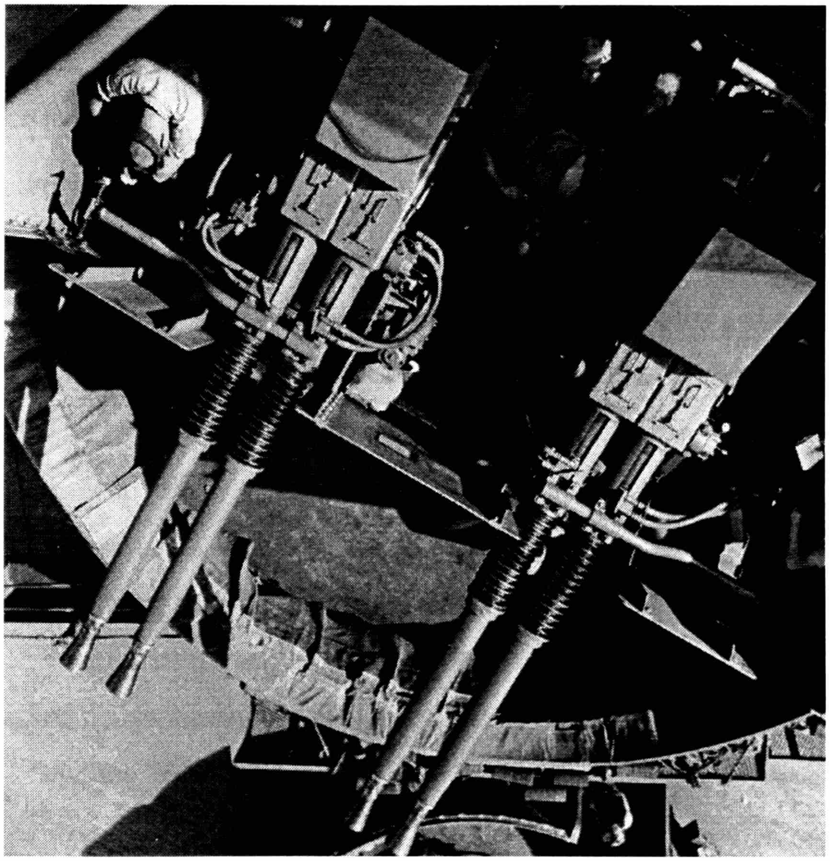


Figure 7-7: 40mm Bofors antiaircraft gun, operated remotely from the Mark 51 director. (from Christopher Phillips, Steichen at War , New York: Harry Abrams, 1981).

Chapter 8

Automation's Finest Hour:

Radar And System Integration

> It is nearly as hard for practitioners in the servo art to agree on the definition of a servo as it is for a group of theologians to agree on sin.
>
> Ivan Getting, 1945

> At first thought it may seem curious that it was a Bell Telephone Laboratories group which came forward with new ideas and techniques to apply to the AA problems. But for two reasons this was natural. First, this group not only had long and highly expert experience with a wide variety of electrical techniques... Second, there are surprisingly close and valid analogies between the fire control prediction problem and certain basic problems in communications engineering.
>
> Warren Weaver, 1945[1]

Engineers at the Servo Lab refined the articulation component of control; at Bell Telephone Laboratories they merged electronic messaging with system integration. They extended the convergence Frank Jewett envisioned before the war, where the general notion of "signal" represents the characteristics of all machines. Under D-2's Project #2, Bell Labs designed and built a gun director which employed electronic circuits and servomechanisms for calculation. The device replicated and replaced the Sperry mechanical directors. Bell Labs engineers used feedback to describe not only the amplifiers in their machine, not only its servos, but the behavior of the system overall.

A new instrument of perception, radar, gave added impetus to this new approach. Integrating gun directors with radar raised problems of the system's response to noise, the dynamics of radar tracking, and jittery echoes. Bell engineers then, in conjunction with their rivals and collaborators at the Radiation Laboratory, learned to engineer the entire system's behavior from the beginning, rather than just connecting individual, separately-designed components.

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[1] Ivan Getting, "Introduction," in Hubert M. James, Nathaniel B. Nichols, and Ralph S. Phillips Theory of Servomechanisms (New York: McGraw Hill, 1947) Radiation Laboratory Series #25. Warren Weaver, foreword to "Final Report: D-2 Project #2, Study of Errors in T-10 Gun Director," OSRD7 Office Files Of Warren Weaver, 3.

This new system logic reflected institutional relationships and evolved to suit their shifts. To the Radiation Lab it meant the designing the system around its most critical and sensitive component — the radar — and not the director, computer, or gun. By the end of the war then, the Radiation Laboratory, in competition with a number of other research labs, assumed control of system design. The Rad Lab ran the war's only successful project to design a fully automatic radar-controlled fire control system, the Mark 56 Gun Fire Control System. Still, the existing tangle of arrangements between the Rad Lab, Division 7, and the Bureau of Ordnance did not give the Rad Lab the responsibility it sought. Ivan Getting, director of the Mark 56 project, redefined his organizational role and invented the job of system integrator, a technical, institutional, and epistemological position.

The Western Electric/Bell Labs Gun Director

The Myth of Origin

The Bell Labs project has its own mythology of origin, starting with the dream of a staff member, physicist Donald B. Parkinson. In the spring of 1940, Parkinson was working on a device to record the logarithm of an applied voltage on a strip of paper. It solved a simple equation, y=log(v), by a logarithmically shaped card wound with wire. An electrical wiper swept across the card and developed an electrical resistance which varied with the function on the card (it was thus a "logarithmic potentiometer"). Parkinson's circuit connected to the pen, and "to all intents and purposes this small potentiometer could be said to control the motion of the pen."[2]

"I had been working on the level recorder for several weeks," Parkinson recalled,

> when one night I had the most vivid and peculiar dream...I found myself in a gun pit or revetment with an anti-aircraft gun crew...There was gun there...it was firing occasionally, and the impressive thing was that *every shot brought down an airplane!* After three or four shots one of the men in the crew smiled at me and beckoned me to come closer to the gun. When I drew near he pointed to the exposed end of the left trunnion. Mounted there was the control potentiometer of my level recorder! There was no mistaking it—it was the identical item...It didn't take long to make the necessary translation—if the potentiometer could control the high-speed motion of a recording pen with great accuracy, why couldn't a suitably engineered device do the same thing for an anti-aircraft gun!

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[2] For Parkinson biographical info, see *Wisconsin Bell Magazine*, n.d., AT&T Archives Box 60 04 01. D.B. Parkinson wrote an account of his dream on January 5, 1975, which is in the AT&T archives and partially reprinted in M.D. Fagan ed., A History of Engineering and Science in the Bell System: National Service in War and Peace (1925-1975) (Whippany, New Jersey: Bell Telephone Laboratories, 1978), 135-36.

To Parkinson, an antiaircraft director was a laboratory instrument engineered for the field. About June 1, 1940, he proposed the idea to his superior, Clarence A. Lovell. Parkinson outlined three BTL technologies which could contribute to an "electrical predictor for automatic control, calculation, and pointing of a small anti-aircraft gun or machine gun:"

> 1) A coil winding machine which can wind potentiometers on any shaped card thus giving a rotation which was a rational function of the voltage applied.
>
> 2) An electrical differentiator proposed and tested for another job... capable of measurement of extremely small angular velocities.
>
> 3) We have designed extremely high-acceleration electrical servos, [based] on electrical feedback circuits which operate at high speeds and are critically damped. It should be possible to extrapolate them to larger size and make them swing the gun around automatically.[3]

With no prior experience in fire control, Parkinson had quickly grasped the essence of the problem. It required a means of solving equations electrically (#1), a means of deriving rate for prediction (#2), and a means of moving the guns in response to firing solutions (#3).

Lovell liked Parkinson's idea, and proposed to his boss, Mervin J. Kelley, then Director of Research of BTL (later, as president of BTL, Kelley would direct the project which led to the transistor). Kelley, in turn, presented the proposal to Frank Jewett, now at the National Academy of Sciences, who brought it to the Army Signal Corps.[4] Later in June, Parkinson, Lovell, Kelley, and several other BTL engineers met with representatives from the Signal Corps at Fort Monmouth, New Jersey, which at that time was working on radio detection of aircraft. There the BTL group inspected a Sperry M4 director and other fire control equipment, and received manuals and books on antiaircraft and fire control.[5] The Bell engineers also presented their ideas to the Navy, which, content with its own directors, had no interest in the project.[6] Colonel Roger

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[3] D.B. Parkinson, Notebook #16413, Project File 23140, ATT. For other notebooks on this project, see B.T. Weber, #16042, K.D. Swartzel, Jr., #17512 & 16312, C.A. Lovell, #17665 & 15627, D.B. Parkinson 16413, and B.T. Weber, 18009. The control system also required an instrument of perception, and here Parkinson repeated a mistake made by both Arthur Pollen and Sperry in their first forays into fire control. He had an idea for a long-baseline rangefinder, where two widely-separate operators tracked a target and a data transmission system connected them. With more than one target present, however, it proved nearly impossible for both trackers to stay on the same one (this idea was soon dropped).

[4] For detailed chronology of this project, see "Check list for use in connection with record of laboratories work on N.D.R.C. and O.S.R.D. contracts, no. NDCrc-127." Project File 23140, ATT.

[5] E.C. Wente diary, July 3, 1940. Project file 23140, ATT. For another chronology of these events, see R.B. Colton to M.J. Kelley, October 6, 1944, OSRD7, Project #2 Folder, Office Files of Warren Weaver.

[6] These two meetings were on June 27, 1940 and August 5, 1940. *Ibid.*

Colton of the Signal Corps, however, strongly endorsed the BTL gun director to the army's Chief Signal Officer.[7]

During their initial period of exploration, Parkinson and Lovell put together a group of BTL engineers for preliminary analysis. Their study, "Electrical Mathematics," examined the mathematical functions required for fire control equations: addition, subtraction, multiplication, division, integration, differentiation, and looking up tabulated data. Lovell recorded in his notebook an idea for a machine based on electrical feedback mechanisms.[8] He described how "servomechanisms may be used directly in making transformation from one coordinate system to another without the necessity for setting up scale models having to be considered." He picked up a general knowledge of the Sperry directors at Fort Monmouth a few weeks before.

Sperry systems incorporated servos in their calculating units (replacing the follow-the-pointer operators), but only to transmit information between stages. Lovell's servos actually calculated, with a mathematical element directly in the feedback loop. Servos "solved" equations by their tendency to reduce the difference between their two inputs, the error, to zero. This application echoed BTL's use of telephone feedback amplifiers as equalizers to invert the distortion of a transmission line (see Chapter 4). Bell engineers commonly referred to it as "electronic," but they acknowledged it was really "electro-mechanical" — the servomotor turned a potentiometer, whose output voltage was a function of the angular position. Lovell noted his innovation, modeling mathematics with servomechanisms, could make not only a gun director but general calculator. He saw his computing elements as analogous to the mechanical ones used in earlier computers,

> the availability of accurate differentiators and servo-mechanisms make possible the solution of differential equations... machines of the same character as the Differential Analyzer of Bush and Caldwell can be made to operate electrically by the use of the means at our disposal and that a machine can be built to solve systems of simultaneous differential equations in particular multi-mesh network equations.[9]

In his notebook, Lovell sketched an equivalent of the Differential Analyzer, made entirely out of servo-mechanical computing devices.

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[7] Colton to Chief Signal Officer, September 5, 1940. OSRD7 GP Box 67, Antiaircraft Artillery Board, Project 1214, Test of Electric Antiaircraft Director T-10 folder.

[8] C.A. Lovell, June 18 1940, Notebook #15627, Project File 23140, ATT.

[9] C.A. Lovell, April 14, 1941. Notebook #15627, Project File 23140, ATT. C.A. Lovell, July 17, 1940, Notebook #15627, Project File 23140, ATT.

Parkinson worked on similar problems. While his original idea included a wire-wound potentiometer for solving equations, he had intended it only for mathematical functions, such as sines and cosines. He soon realized the potentiometer could look up data in ballistics tables,

> So far we have not given a great deal of thought to any method of building into the director the characteristics of the particular gun with which it is to be used other than to consider the use of either a ballistic cam or a space potentiometer as suggested by KDS [BTL Engineer K.D. Swartzel].[10]

Like Lovell, Parkinson displayed growing understanding of fire control and computing. He suggested a variation on Sperry's ballistic cam, a "space potentiometer" which would solve functions of two variables rather than the single variable of his "logarithmic potentiometer." Bruce Weber, another BTL engineer, examined Lovell and Parkinson's ideas from the standpoint of feedback amplifier and stability theory so familiar at Bell Labs.[11]

D-2 Funds the BTL Director

During this time, while the Bell Labs group sketched ideas, Warren Weaver was assembling Section D-2. The committee learned of the BTL project from the army; soon D-2 visited Bell Labs and met with Kelley, Lovell, Parkinson, Harvey Fletcher, and other Bell engineers. The group explained electrical computing, showed schematics of their circuits, demonstrated a machine for winding potentiometer cards of any shape, and displayed a sinusoidal potentiometer.[12]

Weaver and D-2 liked BTL's proposal. An electronic machine would provide a needed alternative to Sperry's directors, whose shortcomings were becoming clearer every day. Bell engineers argued that electronics worked with greater accuracy and speed and at lower cost than mechanical computing. The NDRC, however, was interested because an electronic fire control computer would be easier to reconfigure and correct in case of errors in its solutions (equivalent components could be rewired) than a mechanical computer, whose algorithm tightly connected to its physical structure. Furthermore, an electronic director could be built by the vast manufacturing capacity of Western Electric, which was at the time underutilized for war production. Sperry's resources, as well as those of many precision mechanical manufacturers, were already stretching

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[10] D.B. Parkinson, July 23, 1940. Notebook #16413, Project File 23140, ATT.

[11] Bruce T. Weber, August 7-October 25, 1940. Notebook #16042, Project File 23140, ATT.

[12] WW diary, October 24, 1940. OSRD7 GP, Project #2.

thin.[13] In addition, unskilled workers could build an electronic director with existing components, whereas Sperry ballistic cams required complex machining. The idea was also being proposed by scientists from a successful laboratory with a good reputation, an organization familiar to the members of D-2. BTL's founder and former president, Frank Jewett, was a director of the NDRC.

The army, although willing to fund the BTL project, proposed to Weaver that the NDRC take it over, "during the development stage, when flexibility of contract is important." Weaver agreed. The Fire Control Design Section of the Frankford Arsenal (which had directed Sperry's work in the thirties) would act as liaison.[14] D-2 let a contract, Project #2, effective November 6, 1940 (until then, Bell Labs had funded the project internally).[15] BTL would construct an electrical gun director, designated T-10, to drive a 90mm gun via (Sperry) hydraulic power controls. An optical rangefinder would provide altitude input, but the machine would include the provision for radar inputs. It would also keep the constant altitude assumption of previous directors, and include the "aided laying" feature of the M5 Kerrison director.

Over the next few months, BTL engineers continued gathering information. They studied army training courses, director operating manuals, and ballistics tables. Lovell visited the army's training schools for antiaircraft gunners and the arsenals responsible for technology development. He requested samples of their telescopes, synchronous transmitters, receivers and other equipment.[16] Frankford Arsenal sent him blueprints for the tracking mechanisms in the Sperry M4 director, and drawings of the M2 director.[17] Ed Poitras of D-2 sent Parkinson copies of Gordon Brown's paper "Behavior and Design of Servomechanisms," (numbers seventeen and eighteen) thus admitting BTL to the secret world of wartime control engineering (behind only Warren Weaver, Poitras, the Bureau of Ordnance, and Brown's four navy students).[18] In less than six

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[13] See WW to General Somers, October 24, 1940. OSRD7 GP, Box 67, AAB Project 1214, Test of Electric AA Director T-10 folder.
[14] WW diary of phone call to Somers, November 6, 1940. OSRD7 GP, Project #2.
[15] WW to Kelley, November 9, 1940, and Memorandum of Agreement between NDRC and Bell Telephone Laboratories, May 19, 1941. OSRD7 GP, Project #2.
[16] C.A. Lovell to Ordnance Dept., January 15, 1941. Project file 23140, ATT. See also Lovell and Parkinson, "An Electrical Director," August 30, 1940. OSRD7 GP Box 67, AAB Project 1214, Test of Electric AA Director T-10 folder.
[17] C.A. Lovell diary, December 21, 1940. Project file 23140, ATT.
[18] EJP to Parkinson, November 27, 1940. Project file 23140, ATT.

months, Bell Labs electronic director had transformed from an individual's dream to one of the leading control systems projects in the country.

During most of 1941, Lovell, Parkinson, and their colleagues designed and built the T-10 director, with help from the Mathematical Research group under Hendrik Bode. Throughout, they conceived and described the problem in the language of communications. As one engineer put it, "A servo, in general, involves a carrier, and a means for modulating that carrier according to some function," using terms radio and telephony.[19] Since the mathematical quantities in the T-10 were all represented by DC voltages, the amplifiers and servos needed precision and stability to variations in temperature, age, moisture, or any other number of factors — just like the requirements for telephone repeaters Harold Black had addressed nearly fifteen years earlier.

Inside the T-10

Figure 8-2 shows the block diagram for the T-10 computer. [*Figure 8-2: T-10 block diagram] The basic algorithm and data flow closely resemble that of the mechanical Sperry directors. Warren Weaver, in a foreword to the final report on the T-10 project, explained the similarity,

> When this project was first undertaken the notion of an electrical predictor was necessarily and properly subject to some doubts. It therefore seemed sensible to construct a predictor which would be a rather close electrical counterpart of the mechanical predictor which was the army's then standard for heavy AA. In this way one would get the most direct and easily interpretable comparison between the mechanical and electrical ways of going at the problem.[20]

Just like the Sperry schemes, the director takes three inputs: azimuth (α), elevation (ε) and range (r). It produces three outputs for the guns, azimuth (α_p), elevation (ε_p), and the fuze setting/time of flight (ΔT). Box I converts the slant-range input to a voltage, and box II combines slant-range with elevation to derive its height component. Box III combines the target height with azimuth to derive the target position in rectangular coordinates (x,y, and v for vertical height). Box IV performs the actual prediction, deriving the target velocities (i.e. differentiating the position components with respect to time), multiplying the velocities by the time of flight (ΔT) and adding them to the original positions. As in the Sperry system, the time of flight parameter closes a feedback loop around the prediction calculation. The output of Box IV, then is the predicted

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[19] K.D. Swartzel, Jr., April 9, 1941. April - June, 1941. Notebook #17512, Project file 23140, ATT.

position of the target, x_p, y_p, and v_p. Blocks V, VI, and VII then convert this set of three voltages representing rectangular voltages back to polar coordinates, represented now by angular shaft positions. The servomotors do both angular conversion (multiplying by a sine or cosine) and electrical to mechanical translation.[*Figure 8-1: Lovell feedback servos]

The T-10 had four servo motors, each with a selsyn transmitter for sending firing data to the gun, thirty DC amplifiers, five power supplies, and a host of voltage regulators, adjustment panels, and controls. The entire unit weighed 1,600 pounds. The human operators sat on a small, rotating "tracking head," mounted the telescopes, which transmitted its data to the "computer," a rack of electronics on a separate trailer. [*Figure 8-3: M-9 Tracker] Overall, the system was "ballistcally complete," i.e. it included all known factors into the ballistic calculation, and "approaches the ideal of completely automatic operation. The only manual processes involved in its operation are the tracking functions for deriving suitable input data."[21]

Bell engineers envisioned the T-10 director as a feedback system at every level: from amplifiers to servos to the computer as a whole. As engineers at the Servo Lab used frequency response methods to study their servos, so BTL engineers eroded the distinction between servos and feedback amplifiers, "servo performance is readily studied by the highly developed method of feedback analysis. That a servo is a feedback system becomes apparent from a comparison of its action and that of the feedback summing amplifier."[22] A section in the T-10 final report, "The Computer as a Servo" explains the feedback in the prediction loop. Were it not for the many corrections and firing data within that loop, the report notes, the entire prediction could be performed by a single servo. Overall, "the system has a structural resemblance to a feedback amplifier with multiple loop feedback, and may be analyzed by the usual feedback methods....the whole system is stable whenever there is a physical solution, provided the individual servo loops

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[20] Warren Weaver, foreword to "Final Report: D-2 Project #2, Study of Errors in T-10 Gun Director," OSRD7 Office Files Of Warren Weaver, 3.

[21] *Ibid.*, 129. Constructing this electrical computer proved no easy task. Among the many difficulties, none proved as challenging as the shaped potentiometers. The wire that wrapped them needed uniform resistance all down its length, and to maintain consistency despite temperature changes. The oddly shaped potentiometers which stored the firing tables required new equipment to smoothly wind their wire. Fletcher to R.R. Williams, May 21, 1941. Project File 23140, ATT. For a more detailed discussion of the wire winding machine, see Fagan ed., A History of Engineering and Science in the Bell System, 144-45.

[22] "Final Report: D-2 Project #2, Study of Errors in T-10 Gun Director," OSRD7 GP, Project #2, 37.

are stable."[23] Feedback methods developed for the T-10 became "operational amplifiers," among the most common building blocks in modern electronics.[24]

"A Rather Devastating Device:" The Dynamic Tester

The T-10 posed a problem for D-2: they had no way to judge it. With antiaircraft directors, a considerable amount of debate surrounded any new device. Did it work better than older machines? Did it work at all? What did "better" mean? In the words of Sam Caldwell, "It was literally impossible to make a decision regarding any fire-control equipment from an appreciation of realistic, quantitative data."[25] Before D-2 or Bell Labs could evaluate the T-10 prototype, they needed a way to compare, and hence define, performance. One method, live firing tests, towed a sleeve or sock from an airplane as a target. Gunners fired at the target and theodolites and cameras observed the smoke from shell explosions. In this chaotic and uncontrolled scheme, numerous parameters changed from test to test and even from moment to moment. Different operators produced different test results, as did the same operator on different days. Standardized testing became the ideal contribution for the new NDRC group. The function matched industry's wish for a kind of military National Bureau of Standards. Measurement also fit the self-image of the largely-academic D-2.

A solution came in 1941, the "Dynamic Tester." Duncan Stewart, President of the Barber Coleman Company of Rockford Illinois, wanted to work in fire control. Barber Coleman, a medium-sized manufacturer (1800 employees) of machine tools, textile machinery, small rotary tools, and air-conditioning accessories, had experience with temperature controls, follow-ups, and other types of closed-loop devices. Stewart, whom Harold Hazen described as "a very shrewd savvy Scot" designed an antiaircraft director and presented it to the army. Unimpressed, they put him in touch with D-2 and Warren Weaver, who had known Stewart as an instructor at the University of Wisconsin. Weaver, though equally unimpressed with the director design, thought

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[23] *Ibid.*, Appendix II, "Stability Considerations."

[24] The 1947 paper by Ragazzini, Randall, and Russell (also NDRC researchers) which coined the term "operational amplifier" acknowledges the authors drew inspiration from "the circuits employed in the Western Electric M-IX antiaircraft gun director [the operational version of the T-10]." John R. Ragazzini, Robert H. Randall, and Frederick A. Russell, "Analysis of Problems in Dynamics by Electronic Circuits," *Proc. I.R.E.* 35 (May, 1947), 444. Also see C.A. Lovell, "Continuous Electrical Computation," *Bell Laboratories Record* 24 (no. 3) March, 1947.

[25] Caldwell, "A History of Section D-2, NDRC," February 21, 1946, 10. OSRD7 Office Files of Harold Hazen, Box #6.

Stewart and the company had promise.[26] With the T-10 director nearing completion, D-2 needed a means for evaluation. In August, 1941, they let a contract to Barber Coleman for a fire control testing machine, known as the Barber Coleman Dynamic Tester (Project #25).

The Dynamic Tester simulated the inputs to an antiaircraft director by mimicking the hand motions of a "perfect" human operator. Servos turned the handwheels and cranks on the input of the director. The Dynamic Tester generated the movements from predetermined test flight profiles for an imaginary target airplane, "programmed" with a set of machined cams. Three cams determined a particular flight profile in azimuth, elevation, and range. Programmed courses included dive bombing, level flight, and close-in attack. The output of the director then fed back into the Dynamic Tester, which compared it with an ideal output calculated from another set of three cams, deriving the "error" for the director and recording it on a chart. Different flight profiles could be simulated by changing the cams in the tester, which also had a "perturb" switch to add noise to the data. The machine could also measure the director's response to transient inputs or sine waves of different frequencies. Thus researchers could characterize the director's "transfer function," both in the time and frequency domains, to determine its mathematical behavior as a control system. The machine was not intended to replace live-firing tests, but rather to measure ideal performance. [*Figure 8-4: Dynamic Tester]

The Dynamic Tester became a fixture at Fort Monroe; groups involved in fire control brought their machines to be evaluated by the device. D-2 and its contractors could now quantitatively compare control systems under laboratory conditions. The Dynamic Tester allowed them to make fine distinctions between new techniques. Caldwell recalled, "The Dynamic Tester was a rather devastating device. It had no respect for the opinions of experts, including those within section D-2, and it gave no credit for lucky hits."[27] Barber Coleman built a several copies of their machine and distributed them to contractors, including Sperry Gyroscope and Ford Instrument.

The NDRC, through the Dynamic Tester, brought fire control into the laboratory and under the control of scientist-engineers. It redefined fire control as a feedback system, a black box

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[26] WW diary, December 5, 1940. D-2 diary of visit to Barber Coleman Co., January 3, 1941. OSRD7 collected diaries, Box 70. TCF to General R.H. Somers, March 10, 1941 gives a brief history of the initiation of this project. OSRD7 GP, Project #25.

with inputs that could be simulated and outputs that could be measured. D-2 redefined successful performance: not hitting practice targets, but achieving measurable transient and dynamic performance (manifest as traces on paper) in a controlled setting. D-2 acquired the authority to compare new technologies and determine their veracity. With this machine, the NDRC literally built its expertise in fire control. The Dynamic Tester's paper tapes, however, did not yet persuade the army.

Making it Work — Delivery and Testing of the T-10

The first T-10 prototype, after several months delay, shipped to Fort Monroe for testing the day before Pearl Harbor.[28] Tests showed the T-10 performed about as well, or perhaps a bit worse, than the Sperry directors, which were tested at Fort Monroe at the same time. Duncan Stewart, who had now joined D-2 as a member, oversaw the project with BTL. To him the test data were inconclusive, and "there was little to choose between any of these [Sperry or BTL directors] on the basis of results."[29] George Stibitz of Bell Labs, now a member of D-2 as well, shared Stewart's reservations. He believed "the mechanical inaccuracies in T-10 are completely swamped by poor use of data," and that a "smoothing network" or other method of eliminating noise and jitters would improve performance. In Stibitz's view, the army, overly impressed with Bell Labs and the new machine, was foolishly rushing into production, "I cannot emphasize too strongly my own feeling that, since at least $2.5 million will be spent on the first few directors, every effort should be made to improve this part of the predictor, and this effort should be made as promptly as possible."[30]

The Antiaircraft Artillery board reported the Fort Monroe tests as showing the T-10 to be about equal to the mechanical directors. D-2 agreed and argued the device should not go into full production but rather that a pilot production lot be made quickly for field trials. The army, however, did not believe in the tester's paper tapes; it accorded little authority to D-2's

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[27] Caldwell, "A History of Section D-2," 23. For Hazen's comments on Stewart, see Harold Hazen, <u>Memoirs: An Informal Story of My Life and Work</u> (Unpublished manuscript, MIT Archives, 1976), 3-38.

[28] Duncan Stewart wrote "It is important to bear in mind that the Bell Telephone Laboratories, with unselfish and patriotic motives, has undertaken the development and construction of this instrument in accordance with a program which not only would be foolish under normal circumstances but is entirely at variance with the Bell Telephone Laboratories ordinary development procedure." DJS diary, September 20, 1941. OSRD7 GP, Project #2. Kelley to WW, December 17, 1941. OSRD7 GP, Project #2.

[29] DJS to HLH, WW, EJP, GRS, December 31, 1942. OSRD7 GP, Project #2. This memo summarizes Stewart's numerous objections to the BTL project.

quantitative test results. Advantages of production and procurement outweighed deficiencies in performance. In November, of 1941, the army announced an order for 200 of the directors. The army did not wait for Bell Labs to complete its own tests; the army did not care about D-2's approval; the army did not do any of its own testing.[31] They told Weaver "if a good supply of instruments [the T-10] were available which were not even as good as the Sperry M-4, [Army] Ordnance would still feel compelled to purchase this supply."[32] In mid-February, 1942, the army standardized the T-10 as the M-9 director. In these tense weeks after Pearl Harbor, the army needed quick action on new technology. D-2's instrumental approach to fire control could not counter army procurement imperatives in a national emergency.

The army's decision to buy without test results threatened D-2's status as experts. "There is some unhappy evidence," Weaver reported, "that the higher BTL authorities, although perhaps motivated by patriotic convictions that further delays are not warranted, nevertheless seem to be more interested in production than they are in improvement."[33] To remedy the situation, D-2 acted the only way it could, within its own domain. It extended BTL's contract (named Project #2c) for improvements in smoothing and error reduction in the T-10 before production. This work, in the spring of 1942, achieved its intended results, in two ways. It brought the T-10's performance to a level that satisfied D-2 and it allowed D-2 to sign off on the device without losing face. Duncan Stewart, for his part, remained concerned that the M-9 would have "prohibitive" field troubles.[34] Recall, however, that Stewart had proposed a director of his own; his animus toward the BTL machine contains a hint of residual resentment.

In October of 1942, the M-9 first came off Western Electric production lines, including components from subcontractors Ford Instrument and International Harvester.[35] [*Figure 8-5: M-9 System] During the war, Western Electric produced more than 1500 of the M-9 director and its derivative models modified for different ballistics (M-8, M-10, M-12, M-13 and M-14). The following year, BTL held a public demonstration of the automatic director at its site in Murray Hill for a group of army brass, BTL management, and 1,500 BTL employees. The chief of Army

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[30] GRS diary, December 25, 1941. OSRD7 GP, Project #2.
[31] WW to KTC, November 11, 1941. OSRD7 GP, Project #2.
[32] WW to Fletcher, October 31, 1941. OSRD7 GP, Project #2.
[33] WW diary, February 25, 1942. OSRD7 GP, Project #2.
[34] DJS to HLH, WW, EJP, GRS, December 31, 1942. OSRD7 GP, Project #2

Ordnance told the crowd, "The M9 director, electrically operated, is, we feel in Ordnance, one of the greatest advances in the art of fire control made during this war," and he cited its combat successes in the Pacific.[36] The T-10/M-9 director was the primary NDRC fire control technology to become operational during the war.

The T-15 Director

The T-10, the result of a rush project to design an electrical director and get it into production as quickly as possible, introduced no innovations in computation. It used the same algorithm as the Sperry M-7, but with electrical components. The Sperry algorithm, however, had basic problems. Most important, the "Plan Prediction Method," derived the target's velocity directly from its position, by differentiating. Observed position data, however, unavoidably contained roughness, due either to the jerky nature of human tracking, or to electrical noise in a radar signal. Thus the instantaneous rate or velocity derived from this signal fluctuated wildly. Smoothing could average out the errors, but only over some time period, introducing delays which sent stale data to the predictor.

To overcome these problems, in February, 1941, only months after the T-10 began production, D-2 suggested an electrical director based on new algorithms. This became Project #11 with Bell Labs, "Fundamental Director Studies." BTL designed a new machine, the T-15, under the direction of Walter MacNair. Hendrik Bode, as part of McNair's team, applied his experience with electrical networks and feedback amplifiers to smoothing networks for the T-15, which used AC and not DC electronics. Instead of the Plan Prediction Method, the T-15 worked entirely in polar coordinates and employed a "memory point method" for deriving rates. The director stored an initial data point for the target in a mechanical "memory." For any future time, it derived the target's velocity by subtracting the initial from the current position, and the dividing the difference by time. The memory point method required no differentiation and inherently smoothed out perturbations. It would prove about as good as Norbert Wiener's "optimal" design

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[35] WW diary, January 23, 1942. OSRD7 GP, Project #2. See also DJS diary, October 5, 1942. OSRD7 GP Box 71, collected diaries volume 4.

[36] For BTL accounts, see "Development of the Electrical Director," *Bell Laboratories Record* (January, 1944) 225-240, "Electrical Gun Director Demonstrated," *BLR* (December, 1943)157-67, "Blow Hot — Blow Cold: The M-9 never failed," *BLR* (December, 1946) 454-6. For production numbers, see William J. Wuest, "History of Heavy AA Fire Control and Materiel," (Ft. Bliss, Texas: U.S. Army, The Artillery School, Antiaircraft and Guided Missiles Branch), 1951. In ATT folder 84 05 02 03.

(see Chapter 9). Because it used the difference between the current position and the predicted position (D-2 came to call this the "one plus" method), it dealt with quantities of relatively small magnitudes, which required less accurate computing mechanisms.

When the T-15 design was completed in November, 1941, D-2 gave BTL a contract (Project #30), to build the device. Completed about a year later, test results showed the T-15 to be more accurate by about a factor of two than the T-10, and to settle on a solution two to three times more quickly. T-10/M-9 was already in production, however. The army never adopted the T-15. Still, the T-15 project produced some useful results. It used the same assumption of constant course and altitude as the Sperry and the T-10 director, but T-15 engineers began to consider the possibilities of predicting the position of airplanes taking evasive action, or "curved flight prediction." D-2 let further contracts to BTL to study this problem. And the T-15 did advance electrical computing and analytical understanding of the fire control problem.[37]

Radar and System Integration

With the T-10/M-9, the director, as a system integrator, exceeded the accuracy of its instruments of perception. Thus attention turned toward improving and automating perception. During the thirties, the Army Signal Corps tried to incorporate new "radio ranging" devices into existing mechanical gun directors. In 1937, this work produced the SCR-268 radar (which Western Electric began producing in 1940), designed to supply fire control data to Sperry's M-4 director.[38] [*Figure 8-7: SCR-268] The SCR-268, although deployed in large numbers, imperfectly matched the M-4, which was designed for optical equipment. These early radar sets performed similar to the old sound ranging equipment they replaced: useful for detecting incoming aircraft and providing an idea where they were, but not as precision inputs to fire

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[37] National Defense Research Committee, "Antiaircraft Director T-15," Report to the Services No. 62 (Contractor's Report on OEMsr-353), August, 1943. See also Fagan ed., A History of Engineering and Science in the Bell System, 151-155; Fagan's account is account based on this NDRC report and on C.A. Lovell, memo to M.D. Fagan, January 3, 1974, Folder 84 05 02 03 ATT, and on M.D. Fagan, "The War Years," manuscript in the same folder.

[38] Roger B. Colton, "Radar in the United States Army: History and Early Development at the Signal Corps Laboratories, Fort Monmouth, N.J.," *Proc. I.R.E.* (November, 1945), 740-53.

control systems. The SCR-268, however, worked much better than acoustic devices, and could direct searchlights to track a target.[39]

The SCR-268's poor accuracy derived in part from its relatively low-frequency, long-wavelength of 1.5 meters. Existing vacuum tubes could not generate higher frequency (shorter-wavelength) signals at high enough powers for aircraft detection. So in 1940, shorter wavelengths, or "microwaves," were part of Vannevar Bush's solution to the "antiaircraft problem." When the NDRC began operations in 1940 it included microwave research, under section D-1, the "Microwave Committee." During the summer of 1940, when Weaver and D-2 toured the field and learned about fire control, the Microwave Committee did the same for radar. Like the fire control group, the committee realized neither the army or the navy were aware of each other's work. They found very little research on tubes capable of producing waves below one meter, and none for "microwaves," with wavelengths below ten centimeters.[40]

American radar radically changed in September of 1940, when a British technical mission, the "Tizard Mission" came to the United States and met with the NDRC. In a remarkable act of technology transfer, the Tizard Mission revealed the "cavity magnetron" to the Microwave Committee. The device could produce ten kilowatts of microwave power at a wavelength of ten centimeters. Not only did high frequencies produce more accurate echoes, but their small antennas could be carried aboard aircraft. The Tizard Mission intended for Bell Labs and Western Electric to begin research and production in magnetrons. Vannevar Bush and the NDRC, however, continuing their "end run," set up a central laboratory for microwave research at MIT, the "Radiation Laboratory," or Rad Lab. It become the NDRC's largest project.

Of the Rad Lab's initial three projects, Project II sought automatic fire control. Louis Ridenour headed the project and recruited Harvard physicist Kenneth T. Bainbridge. Bainbridge brought a junior fellow from his laboratory, a young physicist named Ivan Getting. Getting, the son of Czeckoslovakian diplomats, had grown up in Europe and Washington, DC. He attended to MIT on scholarship and did an undergraduate thesis in physics under Karl Compton in 1934.[41]

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[39] Guerlac, Radar in World War II (New York: Tomash Publishers / American Institute of Physics), 103-110. A similar device developed by the army, the SCR-270, with a wavelength of 2.5 meters, was designed as an early warning and search system. It was deployed in Hawaii in August, of 1940, and detected the attack on Pearl Harbor at a distance of over a hundred miles.

[40] Guerlac, Radar in World War II, 243-250.

[41] Ivan Getting, All in a Lifetime: Science in the Defense of Democracy (New York: Vantage Press, 1989), 37.

After completing graduate work in physics as a Rhodes Scholar at Oxford, he returned to the United States as a member of the Harvard Society of Fellows. In November, 1940 Getting joined Project II, "to demonstrate automatic tracking of aircraft by microwave radar of accuracy sufficient to provide data input to gunnery computers for effective fire control of ninety-millimeter guns."[42] Getting was put in charge of the "synchronizer" the master timing device "which tied the system's operation together."[43] The group also included electrical engineers Henry Abajian and George Harris, and physicists Lee Davenport and Leo Sullivan.

At this time, tracking targets with radar remained a manual activity; it required "pip matching," the radar equivalent of follow-the-pointer. The operator viewed radar return signals on an oscilloscope screen and used a handwheel-controlled blip to select which radar echo was indeed the target. Then the blip or "pip" and not the actual radar signal went on as the valid range. The operators worked like the "human servomechanisms" in Sperry's directors: distinguishing signals from noise. Bowles and Loomis, aware of MIT's strength in automatic control, suggested Project II mechanize this task for "automatic tracking." If the radar signal itself could drive servos to move the antenna, the radar would follow the target as it moved. Project II set out to automate the work of the radar operator.

To solve this problem, the Rad Lab developed "conical scan," which rotated an off-center beam thirty times per second to make an precise "pencil-beam" for tracking. If the target was "off axis," i.e. off the centerline of the beam, a feedback loop moved the antenna to return the target "on axis" to the center of the beam. If the target was moving, like an airplane, the antenna would thus track its motion. The Rad Lab obtained a machine-gun mount from General Electric to move the antenna, and G.E. engineer Sidney Godet to design the selsyn servos for tracking. They first tested conical scanning at the end of May, 1941 on the roof the Walker Memorial building at MIT.[44] By February, 1942 the Rad Lab built a prototype, the XT-1; they bought a truck and modified the radar to fit inside.

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[42] Getting, All in a Lifetime, 107.

[43] Ivan Getting, "SCR-584 Radar and the Mark 56 Naval Gun Fire Control System," *IEEE Trans. Aerospace and Electronic Systems* AES-11, (no. 5, September, 1975), 924.

[44] Getting, "SCR-584 Radar." For a technical discussion of the 584 servos, see Hubert M. James, Nathaniel B. Nichols, and Ralph S. Phillips Theory of Servomechanisms (New York: McGraw Hill, 1947) Radiation Laboratory Series #25, 212-224. Stuart Bennett, A History of Control Engineering: 1930-1955 (London: Peter Peregrinus, 1993), 143-6, which includes Godet's servo.

The truck added more than mobility; it added enclosure. Earlier army radar sets (the SCR-268), mounted displays and operators directly on the rotating antenna platform, much as the Sperry directors had in the thirties. [*Figure 8-7: SCR-268] This arrangement reflected the army's conception of the radar operators: they were soldiers on the battlefield operating a piece of equipment like a radio. To the Rad Lab, it seemed foolish; the operators' eyes could not adjust to see the cathode-ray displays in bright sunlight; exposed to rain and snow, their hands got too cold to precisely tune the equipment.[45] Getting and his engineers saw the operators as technicians, reading and manipulating representations of the world. The XT-1 truck brought the operators inside a darkened, air-conditioned trailer: a control room, a laboratory.

Enclosure allowed their eyes to adjust to the delicate blips on the CRT; it freed their hands from cold; it isolated their ears from the sounds of battle. Glowing radar screens presented a captivating simulacra of the world outside. Earlier oscilloscope (or "J-scope") displays showed a single horizontal trace of the radar echo over time. These were replaced with a "plan position indicator" or PPI: a round tube displaying a rotating beam tracing out a virtual map of the area being scanned. Now radar operators and their commanders could perceive and manipulate the field of battle as a map and not as electrical reflections. Radar created an analog of the world, collecting data from a broad area and representing it in compressed form. These systems were among the first in which an operator controlled a machine based on visual input from a cathode-ray tube — an act akin to our own interaction with computers.

After testing the army reported, "The Radio Set XT-1 is superior to any radio direction finding equipment yet tested by the Coast Artillery or Anti-aircraft Artillery Boards for the purpose of furnishing present position data to an anti-aircraft director."[46] In April of 1942 the XT-1 was standardized and went into production as the SCR-584 radar system; the army ordered more than a thousand units from General Electric, Westinghouse, and Chrysler. [*Figure 8-6: SCR-584] [*Figure 8-8: SCR-584 control] As an "early warning system" it could scan the skies

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[45] Getting oral history interview by Frederik Nebeker, June 11, 1991. IEEE Center for the History of Electrical Engineering, Radiation Lab Oral Histories, available on the World Wide Web at htttp://www.ieee.org:80/history_center/oral_histories/oh_rad_lab_menu.html

[46] "Report of A.A.B. Test on XT-1 at Fort Monroe, Virginia, February, 1942," Radiation Laboratory Report no. 359. For first-hand accounts of the XT-1/SCR-584 development and its field deployment, see Henry Abajian oral history interview by Frederik Nebeker, June 11, 1991. Lee Davenport oral history interview by John Bryant June 12, 1991. Leo Sullivan oral history interview by Frekerik Nebeker, June 14, 1991. IEEE Center for the History of Electrical Engineering, Radiation Lab Oral Histories.

up to 90,000 yards and then track an aircraft to one-twentieth of a degree to a range of 32,000 yards. It provided output signals for azimuth, elevation, and range which could feed into the Sperry M-4 or M-7 directors, or the BTL M-9 director. The SCR-584 became the most successful ground radar of the war, with nearly 1700 units eventually produced.[47]

The SCR-584 by itself was a remarkable device, "the answer to the antiaircraft artilleryman's prayer."[48] Rad Lab Project II, however, aimed at more than a tracking radar: it sought automatic fire control. Marching toward that goal, however, tread on D-2's terrain. Early on, Warren Weaver recognized the potential for overlap. He wrote to Loomis of his desire for "a reasonably definite understanding of the location of the fence between our two regions of activity...a wire fence, through which both sides can look and a fence with convenient and frequent gates." Weaver proposed the relationship between the organizations mirror that of radar to a computer, of perception to integration, "The boundary between the activities between the two sections I would suppose to be fairly well defined by saying that your output (three parameters obtained from microwave equipment) was our input (input to a computer or

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[47] For a summary of SCR-584 projects, including a number of modifications, see National Defense Research Committee, NDRC Division 14 Final Project Report, MIT Archives, 2-41 to 2-68.

[48] The SCR-584 proved no simple devices to manufacture. It required 140 tubes, a host of specialized electronics parts, weighed ten-tons total, and cost about $100,000. It did not go into full production until mid-1943. For the difficulties of producing the SCR-584 see George Raynor Thompson, Dixie R. Harris, Pauline M. Oakes, and Dulany Terrett, The United States Army in World War II: The Technical Services, The Signal Corps: The Test (December, 1941 to July 1943) (Washington, D.C.: Office of the Chief of Military History, United States Army, 1957), 265-274; Getting, All In A Lifetime, 121-127; Guerlac, Radar in World War II, 481-83. Getting, Harris, Abajian, Davenport oral histories. For the operational history of the SCR-584, see George Raynor Thompson and Dixie R. Harris, The United States Army in World War II: The Technical Services, The Signal Corps: The Outcome (Mid-1943 Through 1945) (Washington, D.C.: Office of the Chief of Military History, United States Army, 1966), 474-477. Guerlac, Radar in World War II 480-496, 853-862, 882-897, 1018-1025. Rugged and versatile, field commanders employed the SCR-584 for numerous uses beyond the one originally envisioned. It could track mortar shells back to their source, so army units could attack mortar positions. It tracked V-2 trajectories, so American bombers could go after their launch facilities. In combination with automatic plotting boards, it enabled air controllers to "talk" fighter planes to their targets — prefiguring the automated air defense systems of the Cold War and the air traffic control systems of today. During testing at Fort Monroe, it tracked shell fired from the army's 90mm guns and led to the discovery of a significant error their firing tables. The firing table had been calculated on a Bush differential analyzer, but its operator had setup its gearing incorrectly. These errors had then been built into all the Sperry M-7 directors, but, since the T-10 was still in development, it could be properly corrected. The army used it during the battle of the bulge for tracking enemy vehicles as well. It was also used to track remote controlled planes for automated bombing attacks (like the one in which Joe Kennedy was killed). A number were given to the Soviet Union, and for many years Soviet Radars incorporated many of the SCR-584's design features. Getting, All in a Lifetime, 130-35. Also see Abajian, Davenport, Harris, Getting oral histories.

predictor)."[49] Karl Compton, in charge of division D, agreed and set up a special committee, known as D-1.5 to represent its liaison between D-1 (radar) and D-2 (fire control). It consisted of Bowles of D-1, Ridenour and Getting of the Rad Lab, and Caldwell and Fry of D-2. This group, only in existence for about a year, conducted a comprehensive survey of all radar development in the U.S. and Canada.

Where did Sperry fit into this new domain? With its background in fire control the company should have been the obvious choice to build new integrated systems. Its work with the Varian brothers on klystron tubes gave it an advantage in radar as well. The army, however, just beginning to fully understand the shortcomings of the Sperry mechanical directors, distrusted the company's ability to develop a new system. It requested Sperry, then, only to integrate its existing M4 director with the SCR-268 radar, both of which the army already possessed in large numbers.[50] The army and the NDRC, however, drew on Sperry corporate knowledge in another way. Sperry's fire control director, Earl Chafee, joined the Ordnance Department and was assigned to survey existing technology and propose "the best all-around fire control system which could be put together out of equipment on which the basic research is now completed." Chafee was to work with D-2 and examine not only individual components but "The emphasis is to be placed on the over-all aspects of the system... on the role which radar should play in such a unified system."[51] The so-called "Chafee Inquiry," did not lead to a new development program but it clarified the problems involved in automating traditional instruments of perception with microwave radar, problems Bell Labs and the Radiation Lab already faced. [52]

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[49] WW diary, December 5, 1940, meeting with Loomis. WW to Loomis, December 10, 1940. WW diary, December 13, 1940. OSRD7 GP Box 70 collected diaries Volume 1.

[50] See TCF diary of meeting with Col. Bowen, July 3, 1941, OSRD7 GP Box 70 collected diaries volume 2. And Earl W. Chafee, "Memorandum of Conference in Fire Control Department," September 24, 1942. OSRD7, E-83 Office Files of Warren Weaver Box 4, Sperry Gyroscope Folder.

[51] Underline original, WW diary, November 12, 1942. ORSD7 GP Box 72, collected diaries volume 5. WW to Lovell, November 23, 1942. WW to Chafee, December 1, 1942. OSRD7 E-82 Office Files of Harold Hazen, Box 9, Rad Lab folder, see other correspondence to Weaver from Fry, Hazen, and Caldwell as input for Chafee's report, many of which are more informative on issues of "coordination" between system elements than the report itself.

[52] Earl W. Chafee, "Study of the Requirements for a Satisfactory Antiaircraft Fire Control System," Febraury 15, 1943. Sperry Gyroscope Company Papers, Box 33, Hagley Museum and Library. The report can also be found in OSRD7 E-82 Office Files of Harold Hazen, Box 9, Rad Lab folder. The Chafee Report includes the most comprehensive history of Sperry's pre-war antiaircraft development program in the historical record. A meeting held at Sperry Gyroscope in February, 1943 covers similar issues, with input from Rad Lab officials (Ridnaur, Griggs), the Ford Instrument Company (Tear, Jahn), Sperry (Draper, Bassett, Holschuh, Willis, White). John B. Russell diary, OSRD7 GP Box 70 collected diaries volume 3.

Ivan Getting, who had learned of the Bell Labs director project during the D-1.5 survey, began working with BTL to connect his XT-1 tracking radar to the T-10. Weaver's "wire fence" worked fairly well in this case. The T-10 and XT-1 designs proceeded together, and throughout BTL stayed in touch with the MIT group. Ridenour and Getting of the Rad Lab and Stibitz and Lovell of BTL visited back and forth, exchanging information and discussing interfaces between the machines. Getting was particularly interested in "time constants," measures of how quickly the T-10 could respond to inputs. When designing his antenna and tracking unit, he had to know how fast the T-10 could keep up with incoming data — its frequency response.[53] The T-10 final report touted the value of coordinated work, "Close liaison should be maintained between director designers and designers of radars and other tracking equipment. The specifications on each unit should be written with full consideration of the features and capabilities of the other."[54] During this project, the idea emerged that a system might be more than the sum of its parts; the added element was noise.

What difficulties did the Rad Lab and BTL face in trying to connect their instruments? Just as Albert Hall had found at the Servo Lab, noise posed the biggest problem. Servos worked fine as calculators when input data was smooth and ideal. Errors in tracking, however, "would produce prediction errors of dominating proportions;" differentiating the prediction signal tended to emphasize high-frequency noise.[55] Radar signals had several sources of noise, making the problem especially bad. For example, as a radar beam reflected off an airplane, it would shift from one part of the plane to another (analogous to the airplane "twinkling" in the sun). A data smoother could eliminate short, high-frequency perturbations from the input data, but with tradeoffs. Smoothers introduced time lag, so the smoothed data was no longer current when sent into the predictor.

How could one determine the optimal smoothing versus time lag for a network? Could one reduce the time lag for a given network? How did the smoother distinguish proper tracking data from erroneous inputs? What effect did the time lag of a smoother have on the dynamics of a

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[53] WW to Fletcher, February 28, 1941. Project file 23140, ATT. Ridenour to Lovell, September 24, 1941. Project file 23140, ATT. GRS diary, May 21, 1941. Ridenour to Lovell, August 6, 1941. Lovell to Ridenour, September 23, 1941. OSRD7 GP, Project #2.

[54] "Final Report: D-2 Project #2, Study of Errors in T-10 Gun Director."

[55] "Study of Errors in T-10 Gun Director," 72. For a Rad Lab study of jitter in a tracking servo from radar data, see "Data Smoothing," Radiation Laboratory Report no. 673.

feedback loop? Would smoothing avoid or induce instability? These questions resembled those telephone engineers had been asking for at least a decade. As Nyquist and Bode had shown, and as Harris and Hall were applying to servos, the answers depended on the frequency response of the system's components. Warren Weaver put it best when he observed that building radar controlled systems raised, "certain basic problems in communications engineering.... if one applies the term *signal* to the variables which describe the actual true motion of the target; and the term *noise* to the inevitable tracking errors, then the purpose of a smoothing circuit (just as in communications engineering) is to minimize the noise and at the same time distort the signal as little as possible."[56]At BTL and the Rad Lab, just as at the Servo Lab, building control systems meant rethinking the nature of electronic information. Using radar to close a feedback loop required paying attention to connections as well as to components. With radar, control engineering became a practice of transmission, of signals, of communications.

Neither the Bell Labs director nor the Rad Lab's radar had been designed from the first with such a practice of "systems engineering." Rather, two groups tried to connect two separate machines, neither having formal responsibility for coordination. Still, the cooperation paid off. In the fall of 1942, the army held a competitive test of radar-controlled "blind firing."[57] The XT-1 was matched against two other radars, all connected to a T-10 director and Sperry power drives on a 90mm gun. The XT-1 performed best and competing programs were canceled. Although problems remained, particularly extraneous electrical noise in the cables, the system demonstrated that a radar-controlled director could track a target, figure a firing solution, and aim the guns (although it still required human input for target selection, pip matching, and a number of other tasks). By 1944, the M-9/SCR-584 combination entered service in the European theater as an automatic antiaircraft fire control system.

The T-10/XT-1 program gave Getting new ideas for engineering systems. Technical success brought him new responsibility and the opportunity to articulate his vision: the Radiation Lab reorganized, dividing into a number of divisions for components, support, research, and

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[56] Warren Weaver, foreword to "Final Report: D-2 Project #2, Study of Errors in T-10 Gun Director," OSRD7 Office Files Of Warren Weaver, 3.

[57] The competitors were a similar Bell Labs radar, the SCR-545 (which was produced in limited numbers), and the Canadian GL-III-C, which had been designed in response to Tizard's initial assignment for gunlaying. The SCR-545 was the closest rival to the Radiation Lab set, and included a long-wave search radar along with its microwave tracker. SHC diary, March 26, 1942, and J.B. Ridenour Diary, April 4, 1942. OSRD7 GP, Project #2.

"systems." Ivan Getting took charge of Division 8, responsible for all army ground radar and naval fire control. Ralph Phillips headed a special subsection for mathematics and servos which included on its staff Walter Pitts and Paul Samuelson. While this group seemed to violate Weaver's cordial fence between division between D-1 and D-2, Getting believed system design orbited around radar; under his direction the Rad Lab would become the center of gravity for integrated systems.

The Difficult Stepchild: Radar and Fire Control in the Navy

The source of that gravity, however, would not be the army but the old fire control expert, the Bureau of Ordnance. In 1943, the M-9/SCR-584 combination gave the army the most automated fire control system in the war, leapfrogging the navy with help from the NDRC. BuOrd, for its part, had done little work with D-2, Division 7, or the Radiation lab. Still, the navy was pushing radar; automated perception radically altered naval fire control. Naval control systems, especially for heavier guns, changed more slowly than equivalent army technology, however, because they depended on modifying ships instead of just sending systems into the field on trucks. This momentum, combined with the conservatism of BuOrd and its contractors and their failure to take immediate advantage of the NDRC, meant that in 1943 the bureau came to Division 7 and the Rad Lab for help designing a new automated system. Before examining Getting's handling of this project, however, and hence his definition of system engineering, we must understand BuOrd's difficult cultivation of fire control radar.

The Naval Research Lab had done some of the earliest work with radio ranging in the 1930s; it produced sets and installed them in the fleet in 1940. But these devices, intended for search and navigation, came under the cognizance of the Bureau of Ships, and like the early army systems, used long wavelengths too inaccurate for fire control. BuOrd, to add radar to its control systems, had to pry the technology away from BuShips; only in the summer of 1941did BuOrd get complete and official cognizance over fire control radar.[58] By that time, however, BuOrd had *de facto* control of the technology. It had the only officers in the navy with academic training in control: Gordon Brown's four former students. One, Horacio Rivero, brought radar into naval fire control.

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[58] Rowland and Boyd, The U.S. Navy Bureau of Ordnance, 415-6.

When Rivero came to BuOrd in the fall of 1940, he was assigned to examine radio ranging research at the Naval Research Lab (NRL) to determine if it had utility for fire control. He immediately recognized the device's potential as input for a fire control system. No one in BuOrd understood the technology, so Blandy gave the young lieutenant a free hand. Rivero recalled, "I was then the first man in charge of Radar in the Bureau of Ordnance." Along with Samuel Tucker, head of BuOrd's antiaircraft section and Jim Smith, an engineer on loan from Bell Labs, Rivero initiated a major program in fire control radar research, design, and production (Tucker is credited with coining the term "radar"). Their work got underway in the fall of 1940, just as the NDRC was organizing, and as the British Tizard mission brought the cavity magnetron to the U.S. Rivero immediately began directing BTL to build radars for fire control (they were working on a search radar for the Bureau of Engineering).[59] Before BTL even had a prototype, Rivero ordered production to begin, much as the army had ordered the T-10 from BTL before testing. As each set came off the assembly line at Western Electric, Rivero assigned it to the fleet and had it urgently shipped for installation.[60] In these early years of the war, responsibility for radar roughly divided along service lines: army radar came out of the Rad Lab, navy sets from Bell Labs and Western Electric.

The first fire control radars entered the fleet in July, 1941. In the hands of skilled operators they fundamentally changed fire control. Suddenly naval gunnery became a truly closed loop system: the new instrument of perception could track targets, follow shells along their trajectories, and display shell splashes for spotting. Spotting aircraft were soon removed from battleships.[61] Edwin Hooper, another of MIT's four horsemen, exemplified the early application

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[59] This became the CXAS, or FA when applied to fire control. For prewar development of fire control radar, see L.S. Howeth, History of Communications-Electronics in the United States Navy (Washington: US Navy Bureau of Ships and Office of Naval History, 1963) 463-7. W.C. Tinus and W.H.C. Higgins, "Early Fire-Control Radars for Naval Vessels," *BSTJ* 25 (no.1, January, 1946), 18. For operational experience and differentiation between model and mod numbers, see "Resume of Shipboard Fire Control Radar," *CIC Magazine*, August, 1944, World War II Command File, CNO, Naval Operational Archives. Also see Fagan ed., A History of Engineering and Science in the Bell System Chapter 2, radar. The table of radar development programs on pages 68-69 shows the first antiaircraft fire control radar initiated in October, 1940, a Rivero project. For a comprehensive list of naval radars and Mark numbers, see Norman Friedman, Naval Radar (Annapolis: Naval Institute Press, 1981), 145-182. Rowland and Boyd, The U.S. Navy Bureau of Ordnance, Chapter 17, "Fire Control Radar."

[60] Horacio Rivero, oral history, Admiral's Biographies, Naval Operational Archives. Early in 1942, Rivero left BuOrd for duty in the fleet, and he brought a new radar set to his ship the San Juan. With this set, he watched night battle of Savo Island in Guadalcanal the following August.

[61] W.J. Jurens, "The Evolution of Battleship Gunnery in the U.S. Navy, 1920-1945," *Warship International* (no.3, 1991), 255.

of radar in the fleet. Hooper joined the gunnery staff of the battleship Washington, which soon received the first two main battery fire control radars and four of the first five secondary battery (antiaircraft) sets. Hooper adapted it to the user and the ship: "You had not only to organize things...but even formulated your own procedures to handle this new equipment."[62] The radar display was designed to go into the director tower; Hooper moved it down into the plotting room. The antenna was to be cranked by hand; Hooper designed a servo to drive it, "so that it will operate coupled in dynamically with the rangekeeper." He remembered an exciting, innovative time, "the greatest difficulty we had was in drilling through this Class A armored deck on the Washington."[63] Spotting, previously done by telescopes and rangefinders, now became a matter of matching the target blip to the blip from the shell splashes. The gunnery officer thus controlled a feedback loop, integrating perception from the radar into inputs for the rangekeepers.

For twenty years, the gun club had developed fire control in peacetime, but Hooper tested his system in combat. Off Guadalcanal in November of 1942, the Washington, with Hooper at the gun controls, sank the Japanese battleship Kirishima, from a range of 18,000 yards — the first surface victim of a U.S. battleship's guns since 1898. Throughout the encounter, Hooper recalled, he understood the behavior of the new feedback loop with concepts from "my studies at MIT in servo-mechanisms and in dynamics."[64]

But not all gunnery officers, few in fact, had Hooper's training and creativity. Furthermore, while radar easily transformed the comparatively slow (and mature) main battery fire control, antiaircraft stressed the technology to its limits. It could not track automatically or lock onto moving targets. Operators read off values from an oscilloscope and cranked them into existing directors or rangekeepers. While the technology vastly improved the navy's powers of perception, it would take considerable effort to make fire control automatic, turning automated perception into action at a distance.

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[62] Edwin Hooper oral history, Admiral's Biographies, Naval Operational Archives. Hooper later became Historian of the Navy. See, for example, United States Naval Power in a Changing World (New York: Praeger, 1988).

[63] Hooper oral history, 94-5.

[64] *Ibid.*, 81. After the battle, historian Samuel Eliot Morison came aboard the Washington and Hooper gave him the rangekeepers' plots of the battle. See also Ivan Musicant, Battleship at War: The Epic Story of The USS Washington (New York: Harcourt Brace Javonovich, 1986) Chapter 5, for an account of the battle from the Washington's perspective and Hooper's role. Erling Hustvedt gives a personal account of the gunnery room aboard the South Dakota during the same battle, "Battleship Gunfire Control," (unpublished manuscript, University of Maryland, March 15, 1990), Courtesy John Testuro Sumida.

Rivero's work added the Western Electric Mark 4 or 12 radar (or both) to the Mark 37 director, the most common antiaircraft system in the fleet. The combination was first tested aboard the USS Roe in September, 1941 with an audience of navy brass and scientists (including Rivero and D-2's Poitras and Caldwell). Caldwell recalled "The firing was entirely wild and was probably due to lack of training of the director crew. The gunnery officer thought that the trouble was in the failure to obtain a solution at the computer."[65] Nevertheless, this system served the navy successfully through much of the war (over 600 were eventually installed), and defended US Navy ships in the Pacific. Its limitations became crippling when the Japanese introduced the 600 mph jet-propelled suicide bomb, *Baka*, near the close of the war.[66]

Still, in the words of an official BuOrd history, radar was "a stepchild slow to win affection." Typically, as with the Mark 37, it augmented existing fire control equipment not designed for electronic inputs. During the war, BuOrd's tough love spawned twenty seven different fire control radar designs, only ten entered production, seven actually saw action, and only three (Marks 3, 4, and 8) became widely available.[67] They had problems with reliability, maintenance, short ranges, and target discrimination. Only intense human mediation — similar to the old "human servomechanisms" — could produce high-quality electronic inputs for rangekeepers. Operators needed to "pip match" to eliminate noise, and to manually follow the target with the antenna, much as with traditional optical rangefinders and telescopes. They routinely switched between optical and radar tracking, and the combination threatened to overload their attention. Optical tracking remained necessary because tracking radars frequently jittered between closely-spaced targets; they had particular trouble locking onto airplanes attacking low across the water — a weakness Japanese pilots used to tactical advantage. Radar underscored the navy's problems with antiaircraft fire control in general; it worked fairly well against high, straight targets, but broke down when confronting fast, maneuverable, close-in attacks. Still the navy dreamed about fully automatic "blind firing," which could accurately shoot at night or through overcast (the anthropomorphic "blind firing" echoes the early use of radar for "blind landing" of airplanes).

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[65] SHC diary, September 30, 1941. OSRD7 GP Box 70 collected diaries volume 2. EJP diary, September 30, 1941, OSRD7 GP Box 1, Project file #1.

[66] Rowland and Boyd, The US Navy Bureau of Ordnance, 377-8. Fagan ed., A History of Engineering and Science in the Bell System, 67-72. Friedman, US Naval Weapons, 83-84, 243.

Several projects tried to adapt existing control systems for blind firing. In 1941, the Bureau supported the Rad Lab's development of a radar (Mark 9) to work with a director (Mark 45), then under development at the Ford Instrument Company. The Mark 9 became the first Radiation Laboratory set to go into production, but BuOrd soon canceled the program when it dropped the inadequate Mark 45. Similar fates befell other projects at Ford Instrument, G.E., and Arma (Marks 49, 46, and 50, respectively). The Rad Lab, working with NDRC Section T, added radars to the Sperry/Draper gyroscopic sight and its derivatives, but for range-only, manually-aided tracking. This project produced an operational director, the Mark 63, where an operator moved the director manually, aided by a red circle in the sight corresponding to the target's location.[68] Still radar played the frustrating stepchild — BuOrd, with its established contractors, simply could not produce a director and a radar at the same time. "Blind firing" remained an elusive goal.

Ivan Getting believed he could bring the stepchild into the family and build a blind firing system. He redefined the system: no longer a set of separate components connected together, but a single, dynamic entity. Signals, dynamics, time constants and feedback needed to be specified first — this was the system. The physical equipment and mechanical components merely solidified these relations. As with Brown's Servo Lab, Getting's vision entailed a new role for his laboratory. BuOrd's earlier attempts at blind firing had failed, he argued, because they lacked a central, coordinating technical body which could oversee the integration of the system:

> 1) There was no attempt made to integrate the radar and the computer into a functioning whole
> 2) The gross engineering was done by the Bureau of Ordnance, whereas the detailed engineering was done by the company who was not informed of the problem as a whole.[69]

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[67] Rowland and Boyd, The US Navy Bureau of Ordnance, 421, 429.

[68] For a detailed of each of these projects, see Administrative History of the U.S. Navy in World War II, Volume 79, Fire Control, Chapter IV, "Antiaircraft Fire Control." M. E. Murphy, "Memorandum: Report of Fire Control Section (Re4) Summary of Activities and Accomplishments, and Recommendations for the Future." Reprinted as Appendix A of United States Navy, The U.S. Navy in World War II, Volume 79, Fire Control (Except Radar). Ivan Getting gives the most pessimistic assessment of the situation in, "Draft History, Section 7.6," 1946, OSRD7 E-82 Box 6 Office Files of Harold Hazen. Getting provides the perspective of a BuOrd outsider but also of an interested party frustrated with the Bureau. The discussion in Getting's memoir, All in a Lifetime, 165-7 is based on this account. For the Mark 9 radar, see Guerlac, Radar in World War II, 279-81. "Mark 151 Director," March, 1946, Radiation Laboratory Report no. S-75. Division 14 Final Report, 4-54.

[69] IAG to KTC, "U.S.N. AA Director Mk. 56," December 29, 1943. OSRD E-39, Office Files of Karl Taylor Compton, Box 51, Division 7 folder.

The fire control clique still saw the computer and the radar as comprising the "functioning whole." But to Getting they were subsidiary to a more abstract notion of the system. Similarly BuOrd, with its highly specified and compartmentalized contracting, still believed it could break the fire control problem into component parts, technically and contractually ("gross engineering," and "detailed engineering"). Getting wanted to redefine the boundaries between components and between organizations in, "a totally integrated effort starting from basic principles."[70]

Getting's vision found willing allies in the NDRC and BuOrd. When the NDRC reorganized in the end of 1942, Harold Hazen, head of the new Division 7, recognized the value of coordinating radar and fire control design (he had, after all, grappled with related systems problems ten years before with the Differential Analyzer). Among Division 7's priorities, Hazen announced, would be "the overall design of fire control systems and the optimum use of radar on navy directors."[71] To smooth relations with the Rad Lab, he invited Getting to join. Soon thereafter, Division 7 began discussing a blind firing director for the navy's 5" 38 guns with Emerson Murphy, head of fire control research at BuOrd.[72] Getting proposed "A joint project under Division 14 and Division 7...[for] compact blind firing director for heavy machine guns, 3-inch guns, and 5-inch guns for the U.S. Navy." Murphy, attending a Division 7 meeting, endorsed the idea. BuOrd chief Blandy concurred, designating the project Gun Fire Control System Mark 56.[73]

Now Getting could start from scratch, defining the machine and defining his position. The NDRC would go one step beyond its usual role of designing equipment, building prototypes, and preparing drawings. It would now oversee the selection and preparation of manufacturers, and oversee a production run This would allow the NDRC complete technical control of all phases of the project. But which part of the NDRC? A radar-driven fire control device fell within two domains: Division 14 (the Radiation Lab), and Division 7. Division 7 members argued the Radiation Laboratory didn't have sufficient experience with fire control, and that the project should use M-9 director technology developed for the army (BTL was then building for BuOrd

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[70] Getting, "The SCR 584 Radar," 932.

[71] Division 7 Meeting minutes, February 3, 1943. OSRD7 GP Box 72, Division 7 meetings folder.

[72] HLH diary, April 20 & 21, 1943. OSRD7 GP, Office Files of Harold Hazen, Box 70.

[73] Division 7 Meeting minutes, April 28, 1943. Guerlac mistakenly recounts these events as the Summer of 1942, in Radar in World War II, 490, based on a misunderstanding of Getting's letter to Compton of December 29, 1943.

the naval equivalent of its electrical director, an electronic Ford Rangekeeper).[74] Getting's idea for the new system, however, had radar at its core.

To connect radar and fire control, Hazen created a special section of Division 7, dubbed 7.6 "Navy Fire Control with Radar." Ivan Getting would head Section 7.6 as a member of both Division 7 and the Radiation Lab's systems division. He described the new section as "an attempt by Dr. H. L. Hazen to bring together the necessary elements which had been more or less artificially separated by organization, personality, and history."[75] Getting questioned the traditional lines between subunits: the NDRC's divisions dated from a time when fire control and radar were separate technologies. For earlier projects, such as the M-9/SCR-584 combination, the arrangement worked well, given a high degree of communication between Bell Labs and the Radiation Lab. From that experience, however, Getting learned the value of coordination at the design stages and all the way through production — and the value of controlling that coordination. Section 7.6 absorbed a few other Division 7 projects relating to navy fire control and undertook a number of small contracts, but the Mark 56 formed its major work. Getting called the project, "the first fully-integrated radar fire control system that was not restricted by history or by prejudices."[76]

Yet he took advantage of history. For the new section, and for the Mark 56, Getting tapped members of BuOrd's fire control clique. He included Vice Presidents from Ford Instrument and Arma, Al Ruiz of G.E, Charles Stark Draper, and Robert M. Page, who had done the early radar work at the Naval Research Lab.[77] The committee did not actually meet until January of 1944, by which time the Mark 56 project was well underway. Section 7.6's primary function then became "supplying a forum where communications between the principals, including the Bureau of Ordnance, could be provided openly."[78] By this date, most 7.6 members were already overloaded with other work, and those from industry were further constrained. They had

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[74] Getting, "History of Division 7.6," 7. See Fagan ed., History of Engineering and Science in the Bell System, 158-62. BTL built a prototype of this computer, designated Mark 8, which directly replaced the Ford Instrument Mark I, but it was never put into production.

[75] Getting, "History of Division 7.6," 7.

[76] Getting, Oral History Interview.

[77] The Complete 7.6 membership was: George Agins, Vice President, Arma Corporation; R.F. Cooke, VP, Ford Instrument Company; C.S. Draper, MIT; A.W. Horton, Bell Telephone Laboratories; R.M. Page, Naval Research Laboratory; E. I. Poitras, Division 7 (Ford Instrument Company); R.B. Roberts, Section T, OSRD; A.L. Ruiz, Division 7 (General Electric).

[78] Getting, All in a Lifetime, 201.

other contracts with BuOrd and could not discuss status or technical details. Nor did they wish to share such information in a forum in which their commercial competitors participated. The world of naval fire control, with its multi-layered secrecy and its seeming archaism, frustrated Getting, used to the heady and open world of microwave radar in its early days.[79]

And despite Getting's vision, nothing inherent in "coordinated design," dictated a radar group should capture and hold the terrain. He and Division 7 confronted not only BuOrd's fire control establishment, but also other centers of technical expertise. "Blind firing" became the high prestige project for BuOrd, and several groups vied for the technical spotlight. An argument could be made that Draper's gyro culture was best positioned for system engineering, or Bell Labs, where research shared a corporate umbrella with Western Electric's manufacturing. Getting bitterly opposed bringing in Western Electric even as a manufacturer; he disparaged his earlier work with the telephone company, "In fact the Radiation Laboratory and Bell Telephone Laboratories are not complimentary but rather the same type of laboratories," he wrote to Karl Compton and threatened to resign from the Mark 56 project if production contracts were given to Western Electric.[80] The contracts, instead, went to General Electric, with whom Getting had worked so successfully on the SCR-584.

The most serious threat to Division 7's hegemony in fire control, and hence Getting's systems vision, came from within the NDRC. Section T, named after its leader Merle Tuve, developed the proximity fuze which entered production in 1943. Tuve built the Johns Hopkins Applied Physics Laboratory in parallel with the fuze, and he sought to capitalize on the success. For Tuve and his staff, in Michael Dennis's words, "fire control was the future."[81] Section T's had little experience with control systems, but it did have an intimate and unique relationship with BuOrd. Tuve, in fact, reported to the bureau and not to Bush. By 1943 Section T resembled an R&D version of the Ford Instrument Company: it wished to become "the secret fire control design section of the US Navy," that Ford had been decades before.

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[79] Getting, "History of Division 7.6" 10.

[80] [80]IAG to KTC, "U.S.N. AA Director Mk. 56," December 29, 1943. OSRD7, E-39, Office Files of Karl Taylor Compton, Box 51, Division 7 folder.

[81] Michael Dennis, "A Change of State: The political cultures of technical practice at the MIT Instrumentation Laboratory and the Johns Hopkins Applied Physics Laboratory, 1930-45," (Ph.D. dissertation, Johns Hopkins University, 1991), 341. Getting had actually collaborated with Section T on these fire control projects, and did not share Division 7's animus toward him. Still, Getting needed Division 7 for his own project, so did nothing to resist their fight with Tuve.

Blandy requested Tuve's group, in conjunction with Draper and Sperry, to develop a blind firing director for 5" 38' guns. Bush asked Division 7 to aid Tuve, raising the committee's ire. They resented Section T's relationship with BuOrd, an intimacy neither D-2 nor Division 7 ever enjoyed. After heated discussion, Division 7 resolved, "We recommend that the apparently anomalous relationship of Section T to OSRD be discontinued and its status as a Naval agency be clearly recognized." Division 7 considered Tuve "an extraordinarily able man with a great deal of energy but is wild and irresponsible," and refused to work with him, but would "give any possible assistance," if his group were officially placed within the navy. The situation was, in Caldwell's words, "pretty sour," and concerned the Division throughout 1943.[82] Bush quieted the impasse by decreeing Section T should undertake a short-term solution, helping Draper put his Mark 52 director into production and attempting to modify it for blind firing (section T went on to design several radar-controlled directors, Marks 57, 59, 61, and 62). Meanwhile, Bush directed, Section 7.6 would "undertake the development of a new fully integrated radar fire control system of an 'ultimate,' type."[83] In this project, "The Radiation Laboratory under the direction of Dr. Getting would act as central integrated clearing point...Division 7 acting as consultants."[84] Getting's vision, of radar designers at the center of systems' design, thus survived a serious challenge, but by a narrow margin. The ambiguous division between long-term and short-term research blurred as the war drew to a close. Still, Getting won the ideological victory: Tuve's group would combine existing components, while Section 7.6, seeking the ultimate integrated system, would build from fundamentals.

Beginning in 1943 the Rad Lab undertook the Mark 56 program. [*Figure 8-9: Mark 56 Layout] Its conical-scan, X-band (3cm wavelength) radar could search broadly for targets, and then automatically track them, even at low-angles. A "line of sight gyro," in the Mark 56 established a reference as the line between gun and target. Radar operations took place below decks; two sailors in the director itself could acquire and track targets optically. For the computers, the Rad Lab did not defer to prior experience, over Division 7 objections. Instead, Czech exile and fire control expert Tony Svoboda in the Rad Lab designed a wholly new type of

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[82] Division 7 meeting minutes, April 9-10, 1943, July 7-8, 1943. See also Dennis, "A Change of State," 340-46 for how this dispute played out in Section T.
[83] Getting, "History of Division 7.6," 8.
[84] Division 7 meeting minutes, April 28, 1943.

mechanical computer, using innovative four-bar linkages. The MIT Servo Lab modified their Vickers servo to drive the director, but the devices were never used. In August, 1943 Division 7 let a contract with General Electric's Aero and Marine Division in Schenectady for the gyro assembly (Project #71). General Electric contracted to do production design on the radar based on a Rad Lab prototype (Project #79). The Librascope Corporation of California (chosen over a competing proposal from Ford Instrument) produced the ballistic computer (Project #85). The device was first tested on a specially-constructed rolling platform at Fort Heath north of Boston in the Spring of 1944. The first full-up test, including guns, took place the following December.[85]

The project's radical character adversely affected its timing. BuOrd, tuned for wartime production and deployment, allocated its priorities solely by anticipated delivery date. The long-term Mark 56 fell low on the list and its schedule suffered. Despite Bush's compromise with Section T, however, Getting saw his "ultimate" system as a crash program to get blind firing to the fleet as soon as possible. He lobbied Chief of Naval Operations Admiral King, who pushed BuOrd to let production contracts. But King voiced the fleet's frustration with previous automatic tracking radars and demanded the new system include optical as well as radar tracking — a further source of delay. When the war ended, Division 7 had five prototypes on order from General Electric, two of which neared completion. When the NDRC closed down, it transferred the contract to BuOrd in October, 1945, which ordered one hundred systems. Further problems, delays, and changes by the Bureau delayed Mark 56 production models from reaching the fleet until 1947. It did however, proliferate widely in the fleet and remained standard through the 1970s (never firing a shot in anger).

Throughout the Mark 56 project, Getting continued to redefine the work of building control systems. This entailed two parallel moves: transforming the Rad Lab from a radar group to a system integrator, and transforming the human operator into a dynamic component. For the first, Getting elaborated the Rad Lab's earlier position between the government and its contractors as a coordinating technical body. Earlier in the war, the urgency of the antiaircraft

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[85] For the design history of the Mark 56, see IAG Diary, "Conference on Mark 56 Director," June 10, 1943, "Mk 56 Radar Discussions at Bureau of Ordnance," July 15, 1943, "Mk 56," July 2, 1943, "Mk 56," July 26, 1943, OSRD7 GP, Box 72, IAG Diary folder. Division 7 "Minutes of Rochester Meeting," January 5, 1944, OSRD7 GP, Box 72, Division 7 Meetings folder. Getting, All in a Lifetime, 177-81. For an operating description of the system, see Naval Ordnance and Gunnery, Volume 2: Fire Control (U.S. Navy Bureau of Personnel, NavPers 10798), 318-

situation tended to smooth over political problems, and the NDRC's novelty provided a certain temporary authority. Furthermore, a new field like radar had no established expertise to resist the scientists' designs, so Getting had "complete technical control." Late in the war, however, as things became more established, routine, and industrial, they also became more complicated. Getting was used to dealing with the army, a low-tech service still awed by electronics; now he took on the Bureau of Ordnance, among the most technically sophisticated — and entrenched — groups in the services. Getting wanted to control not only engineering but production (a move parallel to Gordon Brown's). Otherwise the role of the Rad Lab would evaporate as the Mark 56 design neared completion. Toward this goal, Getting continued to cross established boundaries. He had joined Division 7, he had merged it with the Rad Lab (7.6), now he reached into the belly of the beast and sought to place a liaison within BuOrd. Warren Weaver, by now experienced at compromise with the services, thought the plans too ambitious, "discussed in over-pretentious terms," and suggested "the way to work with the BuOrd is, so to speak, to work with the BuOrd."[86] Still, Getting got his way and made *himself* liaison he desired. In March, 1945, Radiation Lab Director Loomis ordered that Getting be assigned to the Bureau of Ordnance, "to devote your time and efforts to technical problems on fire control and their application to radar."[87]

Within BuOrd Getting acquired the long-sought authority to delineate the role of the Radiation Lab. He formalized the Rad Lab's job of system integrator, which had previously been merely informal. Now the Rad Lab would,

1) Make all technical information available to GE and the navy
2) Check and criticize designs at all stages of development
3) Send skilled representatives to participate in conferences
4) Report to the BuOrd on the progress of the project
5) Participate in testing of prototypes
6) Test pre-production models
7) Assist in establishing test and alignment procedures for manufacturing and acceptance tests
8) Assist in training programs

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340. For project history, see Division 14 Final Report, 4-55 to 4-63. For Svoboda's relay computers, see "Eloge: Antonin Svoboda, 1907-1980," *Annals of the History of Computing* 2 (no. 4, October, 1980 284-92.

[86] WW to IAG, January 16, 1945. OSRD7, Office Files of Ivan Getting, Box 62.

[87] Loomis to IAG, March 9, 1945. OSRD7, Office Files of Ivan Getting, Box 62.

Engineering, production, testing, alignment, training: these activities comprised Getting's systems vision as much as time constants and signal spectra. To carry out these functions, the lab would have the following privileges

1) Receive copies of correspondence between the navy and contractors
2) To receive copies of drawings and specifications prepared by contractors
3) To be notified when significant tests are carried out so representatives of the Laboratory may participate
4) To be notified of technical conferences and conferences where technical decisions are to be made so that representatives of the Laboratory may be present
5) To be given the opportunity to examine and criticize production designs or models before final design specifications are frozen
6) To have access to the establishments of the contractor and subcontractor by appointment, to confer with engineers or to inspect equipment
7) To receive one of the first production models for test and study if directed by the Navy[88]

Correspondence, drawings, specification, tests, conferences, inspections: these embodied the relations between institutions. Getting needed to control them as much as the signal flows between components. These remarkable lists reflect the experience Getting had acquired in a few years of doing research and managing contracts for the NDRC. Each point seems to correspond to a particular episode where he lacked necessary authority: being excluded from meetings, not receiving correspondence, not having access to factory facilities. Getting redefined control engineering as an organizational as well as a technical task, and he vehemently argued BuOrd by itself was not up to it. Rather, Getting argued, the Radiation Laboratory had the best overall view of automatic control.

Where Getting appropriated authority from contractors, designers, and manufacturers, he also appropriated the work of human operator. Unlike system integrators who organized and collated different types of data, Getting's operators functioned purely mechanically, like "human servomechanisms." In 1945, while fighting for his project's priority, Getting wrote to Admiral Furer, the navy's Coordinator of Research and Development, connecting his ideas for designing new integrated systems with the principle of "automatic operation." Getting argued wartime experience had demonstrated the value of automation:

1) Human judgment introduced wrong guesses
2) Human operators succumbed to battle fever
3) The human mind reacts slowly compared to modern servo equipment

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[88] "Statement of Relationships between the Bureau of ordnance, U.S. Navy and the National Defense Research Committee, OSRD, on the Development and Production of the Gunfire Control System Mark 56," reprinted in Getting, All in a Lifetime, 186.

4) The intellectual processes were incapable of utilizing most efficiently all the observable data.[89]

Radar burdened rather than relieved the operator by radically increasing the amount of information he had to sort through. Radar brought such complexity to military control that it strained human attention to hold the system together. Getting's automation would rein in that human involvement — a strategy which resonated with plans for demobilization, when men left the services but the machines remained.

To make his point, Getting invoked the success of the army's automated antiaircraft fire control. The M-9/SCR-584 system had entered the field, and Getting used the authority he gained by its success to sharply criticize the Navy's lack of automation, "In short the Navy is an order of magnitude behind the army in heavy antiaircraft fire control and radar." The solution, of course, was to grant highest priority to Getting's Mark 56, "a wholly integrated operational system." But to what experience did he refer? How did automatic control perform in combat? What had been the experience of the human operators, whose behavior Getting now used to make his claim for automation? The M-9/SCR-584 combination did see service in the war. What were its successes? Where were its limitations?

Automatic Control's Finest Hour

As Getting promoted and composed his new project, the first automated antiaircraft system, the Radiation Lab's SCR-584 combined with Bell Labs' M-9 gun director, made its way off the production line and onto the battlefield. It was first successful at the beachhead in Anzio, Italy in March, 1944, when two of the radars and sixteen directors systems were deployed on the beach to cover the landing force. Together the SCR-584 and the M-9, combined with Sperry power drives to move the 90mm guns, shot down enemy aircraft which had been harassing the landings.[90] On D-day, thirty-nine systems landed in Normandy (floated ashore in waterproof boxes) to protect the invasion force against air attack.

The M-9 still maintained the "constant altitude assumption" of the pre-war Sperry directors. Rushed into production in 1942, it did not incorporate the latest results on predicting curved flight from work at BTL and MIT. The M-9 worked best, then, against attackers that flew straight and level — a tactic enemy bombers quickly learned to avoid. In June, 1944, however, a

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[89] Getting to Furer, April 26, 1945, reprinted in, All in a Lifetime, 182-85.

[90] Leo Sullivan from the Rad Lab accompanied the SCR-584 to Anzio. See Sullivan oral history.

new threat emerged from Nazi engineers which perfectly matched the constant altitude assumption, exactly because it had no human operator. This threat itself relied on an automatic control system to fly, and hence was the perfect target for the automatic antiaircraft gun: the first operational robot bomb, the V-1.

Germany unleashed the "V-1 Blitz" against London in mid-1944, and launched almost 7,500 "buzz bombs" against the English capital during the following eighty days. In the words of the British commander of the Antiaircraft Command, "It seemed to us that the obvious answer to the robot target of the flying bomb...was a robot defense."[91] Here the M-9/SCR-584 combination, to paraphrase Churchill, saw its finest hour. In anticipation of the V-1 blitz, and in response to a special request by Churchill, Radiation Lab engineers rushed systems out of production, on to ships and accompanied them to England. The original SCR-584 design group (Getting, Davenport, Abajian, and Harris) and other Rad Lab staff members traveled along the English coast from battery to battery, aligning equipment, training crews, and tuning the radars — conveying tacit laboratory knowledge to crews in the field.[92]

One other technology completed the system: the proximity fuze, developed by Merle Tuve's Division T before their foray into fire control. The proximity fuze (known as VT or variable-time fuze) placed a miniature radar in each shell which sensed when it neared the target airplane and set off the explosion.[93] Until then, antiaircraft, with all its feedbacks and controls, remained an open-loop system once the shell left the gun. The proximity fuze closed the loop — making each shell a one-dimensional guided missile, capable of reacting to its environment.

Buzz bombs posed no easy targets. Smaller than a typical airplane, they flew faster than bombers of the day (380 mph), and at low altitudes, averaging about 2,000 feet (indeed fast and low would become the classic radar-evading strategy). And they proved remarkably robust to

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[91] General Sir Fredrick Pile, Ack-Ack, Britain's Defence Against Air Attack During the Second World War (London: Harrap, 1949), 314-15. Also see Pile to George C. Marshall, quoted in Bush to Hazen, August 31, 1944. OSRD7 E-82 Office Files of Harold Hazen, Box 9 Rad Lab folder.

[92] Getting, Davenport, Abajian oral histories.

[93] "Antiaircraft Artillery Fire Control," Prepared by the Bell Telephone Laboratories for the Ordnance Department, U.S. Army in fulfillment of Contract W-30-069-Ord-1448, May 1, 1945. ATT, 14. Those manning the batteries were often slow to recognize the value of the fuze. If the VT fuzed shells didn't find a target, they exploded after some fixed time-out period due to a self-destruction mechanism. Because these explosions were likely to be far from the targets, the proximity fuze did not produce large numbers of explosions near the target like time fuzes did. Instead, gunners would see very few explosions near the target and many explosions far beyond it. "To those used to seeing large numbers of bursts around the target from time fuzed ammunition, this distribution of bursts makes the performance of the battery look very poor," despite much improved accuracy.

shellfire, sometimes taking several hits before falling. Still, between June 18 and July 17, 1944, the automated guns shot down 343 V-1's, or 10% of the total attack, and 22% of those shot down (the others were hit by aircraft, barrage balloons, and ships). During this period the AA batteries were deployed in a ring south of London; and their ability to fire was limited to avoid hitting fighters that also pursued the buzz bombs. The guns could fire only on positive identification of the target and if no fighter were in pursuit, giving aircraft the first chance to shoot down the missiles. In mid-July, the AA batteries moved to the coast where they could fire without limit over the channel. From July 17 to August 31, the automated guns accounted for 1286 V-1 kills, or 34% of the attack, 55% of those shot down (the improved success rate probably also reflects the effects of the Rad Lab members' assistance).[94] That October, the M-9/SCR-584/VT-Fuze combination defended Antwerp from the V-1 with similar success. In this tense confrontation of robot weapons, the automated battlefield, which even today remains a dream of military technologists, began to take shape.

Despite its success, the system had seams in its automation. Radar's new way of seeing did not immediately replace ocular vision. Throughout the war, automatic and manual perception had an uneasy coexistence — translating between the two proved difficult, error-prone, and fatiguing. A detailed assessment of these issues came not from Ivan Getting but from his rivals and former collaborators at Bell Labs. In July and August of 1944, a group of four army officers and two BTL employees, including Clarence A. Lovell (who headed the T-10/M-9 design team), traveled to Europe to tour antiaircraft batteries and observe their operation against the V-1s. This group's report set out requirements for future antiaircraft systems. Unsurprisingly, the BTL report criticized the Rad Lab radar because the SCR-584 could not search and track simultaneously (BTL's rival SCR-545 could).[95] BTL also reported the system demanded unreasonable concentration from its operators, "there are too many sources of present position data for the computer," because it allowed radar, optical trackers and a rangefinder, or a combination. Operators had to judge and juggle these alternate instruments. Manual tracking, for example, was

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[94] Guerlac, Radar in World War II, 859. For a personal account of the automatic system vs. the V-1, see Abajian Interview.

[95] "Antiaircraft Artillery Fire Control," 9.

still necessary because of interfering ground echoes (for targets low on the horizon), closely-spaced targets which a radar might not be able to distinguish, and the possibility of jamming.[96]

The M-9/SCR-584 was more a combination of two separate units (the BTL director and the Rad Lab radar) than an integrated system. Radar trackers sat inside a trailer while optical trackers and rangefinders (on the director) sat outside. BTL's report proposed adding a means for switching between radar and optical tracking. Ultimately, it argued, any new system should mount optical instruments right at the radar station so operators could "track either optically or by radar without changing their positions or the controls which they employ."[97] [*Figure 8-10: Proposed optical/radar station] BTL's report recommended combining tracking and computing in a single unit, similar to the integrated, blind-firing system Ivan Getting proposed to the navy in 1944.

Getting built that case on the success of the SCR-584/M-9 combination, and on the seeming inability of human operators to keep up with the data flow. Much of the trouble, of course, arose not from the limits of human performance, but from relationships between design organizations divided among perception, integration, and articulation. Getting's Mark 56, the "wholly integrated, operational system," proposed to overcome these difficulties by defining a new institutional role, the system integrator, supervising tighter coupling of radar and computer, design and production, operator and machine.

More Than the Sum of its Component Parts: Dynamic Systems and Military Contracting

Radar's new subtlety accompanied new expertise; the Radiation Lab staked out a role as a system integrator. Organizational relationships solidified as technical systems, at first the partially-integrated but combat-tested SCR-584 radar, and then the integrated Mark 56 Gun Fire Control System. The Rad Lab also embodied its claims as knowledge, among its most lasting contributions. After the war, the laboratory, with OSRD funding, published a twenty- seven-volume series on radar to distribute the results of their wartime work. Three of these twenty seven volumes emerged from the work of Getting and his associates: Louis Ridenour's <u>Radar System Engineering</u>, Tony Svoboda's <u>Computing Mechanisms and Linkages</u>, and <u>Theory of Servomechanisms</u> by physicist Hubert M. James, Rad Lab Division 8 servo engineer Nathaniel B. Nichols (who had come from the Taylor Instrument Company), and Division 8 mathematician

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[96] "Antiaircraft Artillery Fire Control," 10.

[97] *Ibid.*, 29.

Ralph S. Phillips.[98] Along with similar volumes from Bell Labs and the Servo Lab, "James, Nichols, and Phillips," became a canonical post-war text of control engineering — introducing a generation of engineers to newly constituted discipline.[99]

For the Rad Lab scientists and engineers, the boundaries of this knowledge derived from the boundaries of radar-driven fire control. The book opens, "The work on servomechanisms in the Radiation Laboratory grew out of its need for automatic-tracking radar systems." Ivan Getting's introduction reviews the basic definitions of servomechanisms and the history of design techniques. Noting the field's lack of stable epistemology, Getting observes, "It is nearly as hard for practitioners in the servo art to agree on the definition of a servo as it is for a group of theologians to agree on sin." Getting and his co-authors certainly acknowledged their predecessors; the twenty-page introduction cites Hazen, Bush, Minorsky, Nyquist, Harris, Brown, Hall, Wiener and Bode. Still, the book reflects Radiation Lab culture: design examples include the SCR-584 radar, numerous automatic and manual tracking schemes, filters for radar signals, and methods for dealing with noisy echoes. The Rad Lab volume, while stabilizing control systems as a coherent body of knowledge, defined that stability by the systems vision of radar scientists.

Their notion of the system as a dynamic entity, however, conflicted with the pre-war vision, which saw a system as a "sum of component parts." Once Harold Hazen defined the modular blocks of the differential analyzer, for example, he could be manipulate and recombine them ad infinitum. Hazen articulated the newer approach in his 1945 preface to Division 7's "Summary Technical Report,":

> One must always remember that a fire-control system is more than the sum of component parts. It is an integrated whole with interrelated functioning of all its parts and one is safe

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[98] Hubert M. James, Nathaniel B. Nichols, and Ralph S. Phillips Theory of Servomechanisms (New York: McGraw Hill, 1947) Radiation Laboratory Series #25. Antonin Svoboda, Computing Mechanisms and Linkages (New York: McGraw Hill, 1948) Radiation Laboratory Series #27. Louis B. Ridenour Radar System Engineering (New York: McGraw Hill, 1948) Radiation Laboratory Series #1.

[99] Gordon S. Brown and Donald P. Campbell, Principles of Servomechanisms (New York: Wiley, 1948). Leroy MacColl, Fundamental Theory of Servomechanisms (New York: Van Nostrand, 1945). See Chris Bissel, "Textbooks and Subtexts: A sideways look at the post-war control engineering textbooks, which appeared half a century ago," *IEEE Control Systems* 16 (no. 2, April, 1996), 71-8, for an account of the post-war publishing effort, and a comparative discussion of control textbooks. Comparing degrees of importance for these books is, of course, splitting hairs, although Bissel calls the Rad Lab volume "perhaps the most influential of all the American publications of the 1940s."

> in considering parts separately only if one always keeps in mind their relation to the whole.[100]

In a dynamic control system, each component affected the others. Computer design, for example, depended on the bandwidth of the radar, its noise spectrum, and the capabilities of the human operator. But the political economy of military technology was built on the older model where systems were decomposable. BuOrd divided up problems, assigned pieces to separate contractors, and assembled the pieces into systems. That approach only worked, however, if a system really was the sum of component parts; noise proved it was more. The NDRC's fire control division, and then the Radiation Lab's Ivan Getting, reconfigured the structure of contracting to suit a dynamic, noisy, error-prose model of a system. To embody their model in working systems, however, they needed a set of engineering techniques to complement institutional relationships. Those techniques began to emerge during the war as well, driven by similar problems of radar noise and feedback loops, gradually defining a general quantity to flow through integrated systems.

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[100] Harold Hazen, "Fire Control Activities of Division 7, NDRC," in Summary Technical Report of Division 7, NDRC Volume I: Gunfire Control, 4. Stuart Bennett has noted the "systems approach" in his comparison of British and American fire control work during the war in A History of Control Engineering: 1930-1955 , 125.

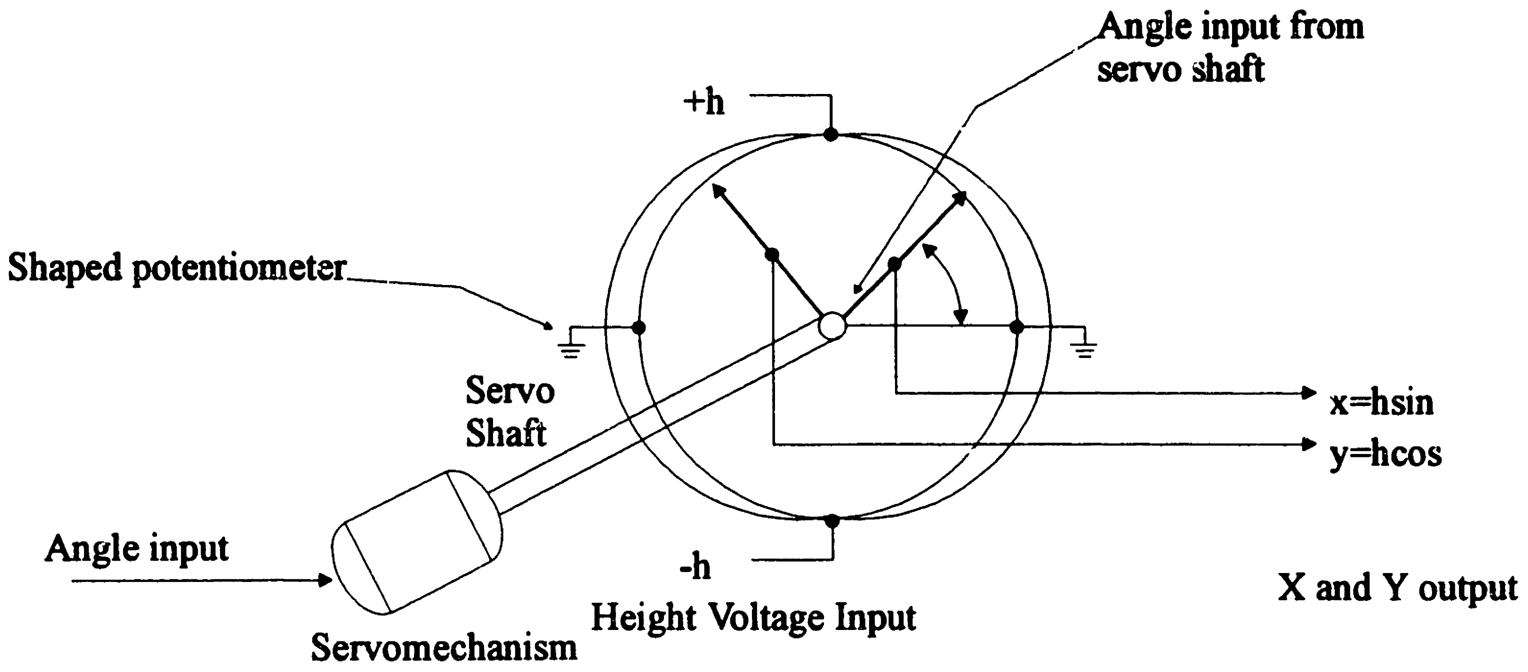


Figure 8-1. Coordinate conversion with a sinusoidial potentiometer driven by a servo shaft (from "Final Report: D-2 Project #2c, Study of Errors in T-10 Gun Director," OSRD7 GP).

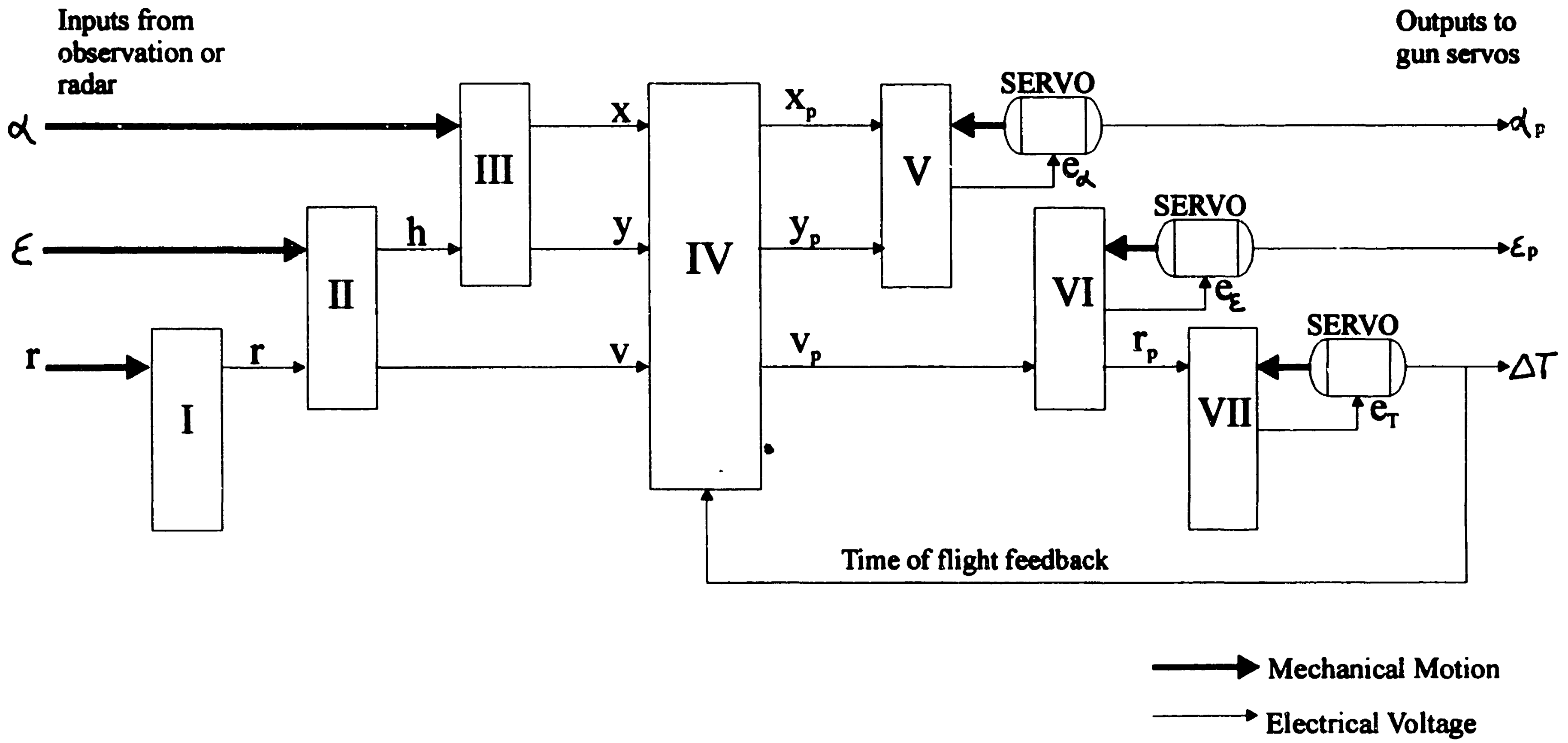


Figure 8-2. Simplified Block Diagram of T-10 Director "Computor Structure" (from "Final Report: D-2 Project #2c Study of Errors in T-10 Gun Director," OSRD7 GP).

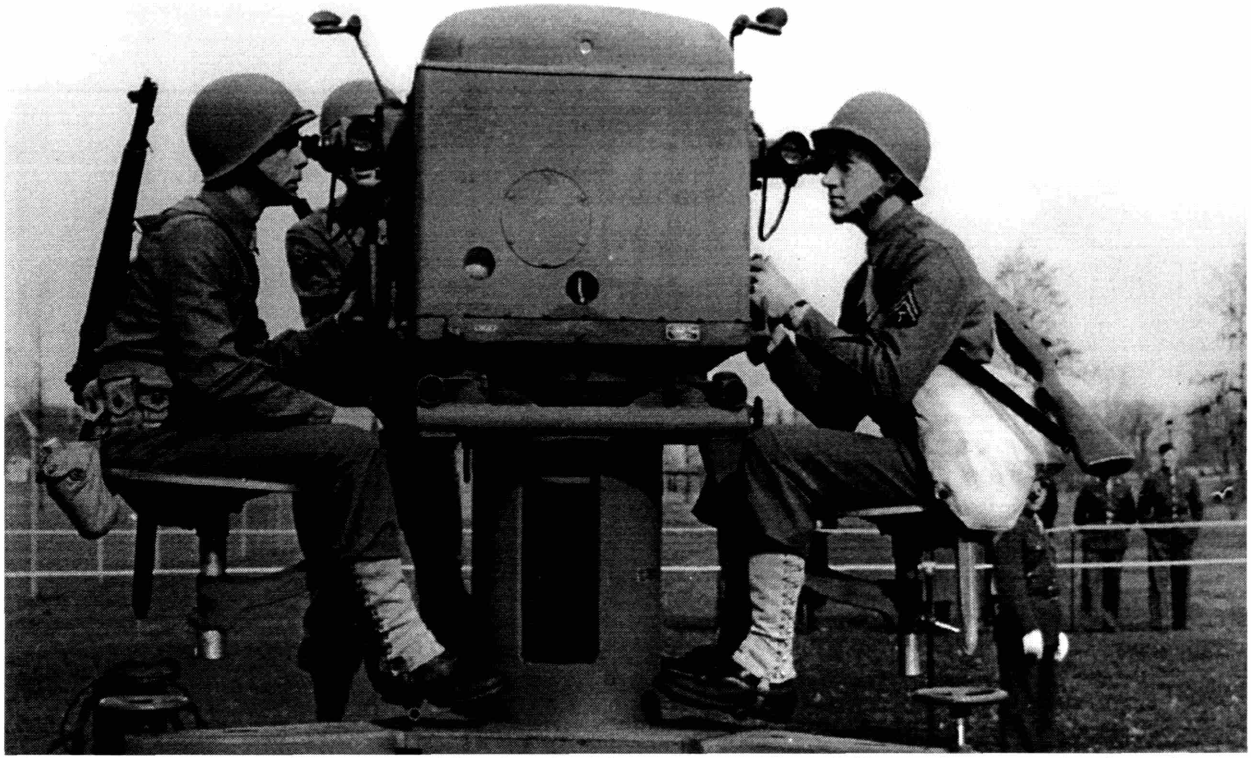
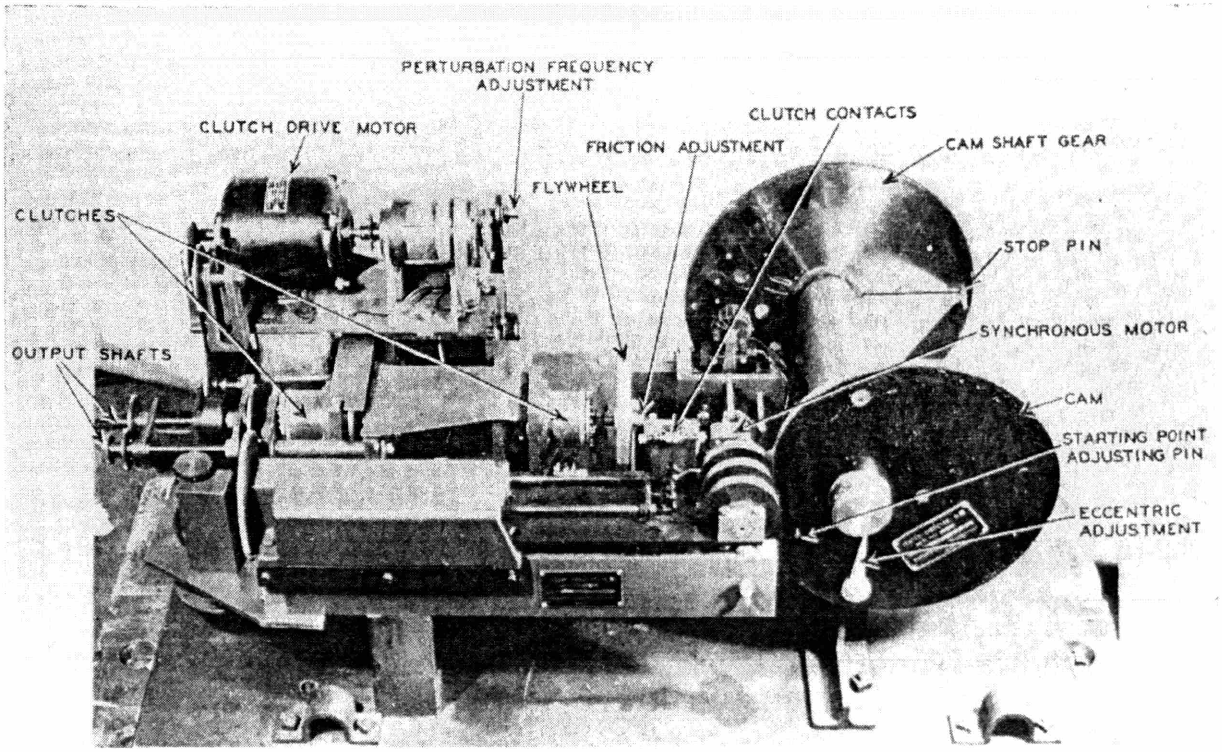


Figure 8-3: M-9 gun director, tracking head with operators. One follows the target in elevation, the other in azimuth. The unit and the operators rotate with azimuth tracking. (AT&T Archives)

Figure 8-4: Barber Coleman Dynamic Tester for antiaircraft directors. Specially-shaped cams at right provide data to drive director inputs with shafts at left (Summary Technical Report, Division 7, National Defense Research Committee, 1946).



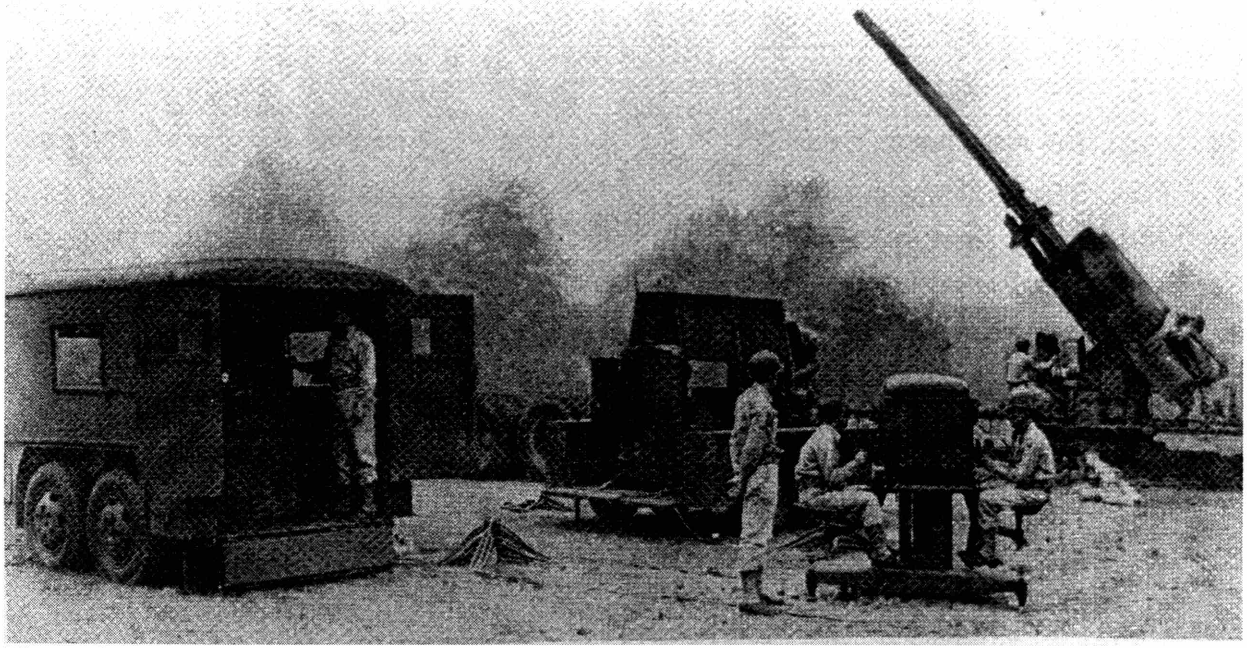
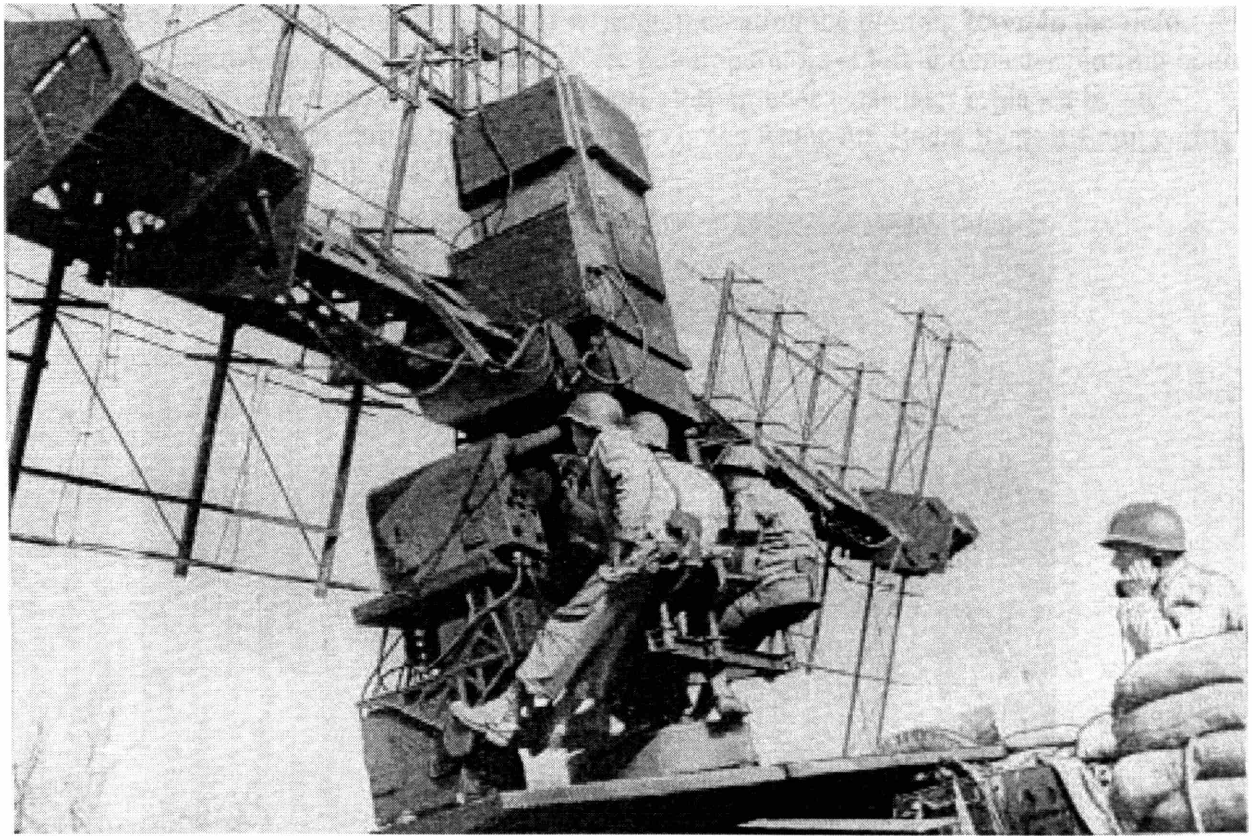


Figure 8-5: Automated system versus robot weapons. M-9 antiaircraft director with power supply, computer, tracking head, and servo-driven 90mm gun With the SCR-584 radar, this machine fought the V-1 (AT&T Archives).

Figure 8-7: Army SCR-268 Fire control radar (Louis Ridenour, Radar System Engineering (New York: McGraw Hill: 1947).



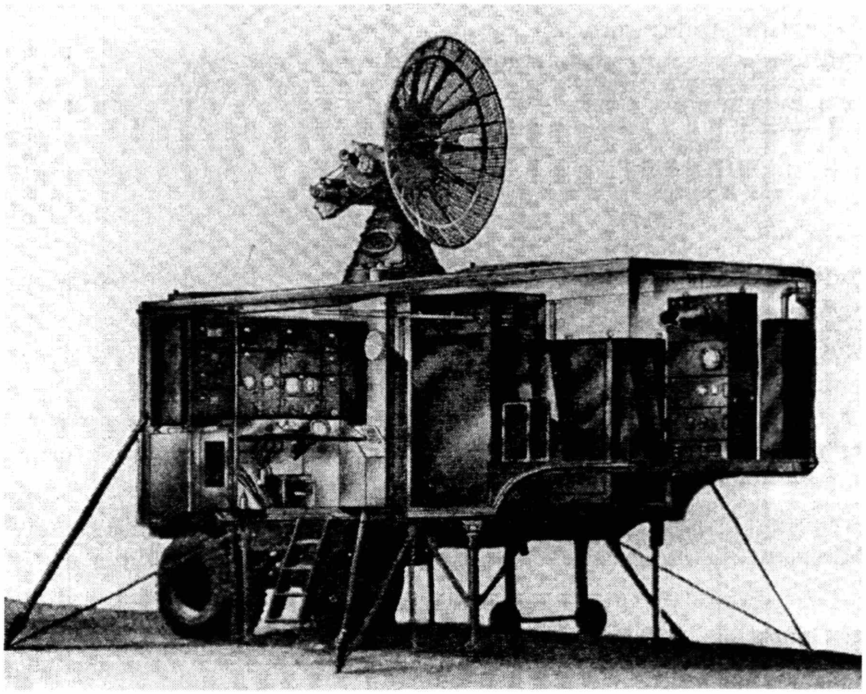
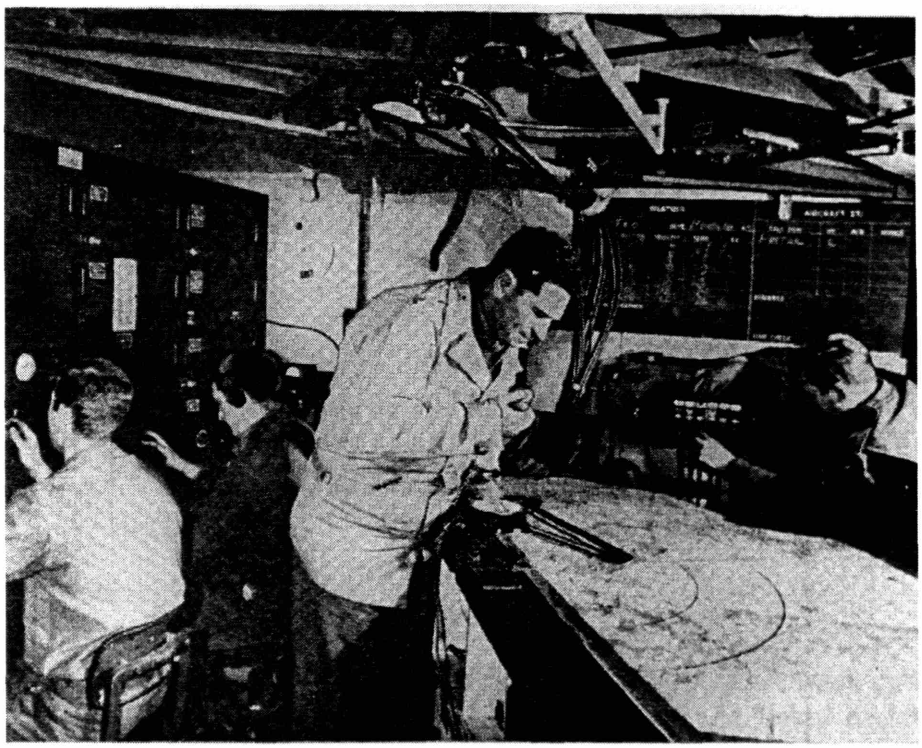


Figure 8-6: SCR-584 Fire control radar with control van. Note tracking operator's console at left in van, and range operator's console at right (Louis Ridenour, Radar System Engineering (New York: McGraw Hill: 1947), 209).

Figure 8-7: Traces on screen and traces of writing, recreating the plotting room in the field. Interior of control van for SCR-584 radar. Note radar operators at left and master plotting board for mapping successive radar tracks. The manual plotting board was later replaced by an automatic, servo-driven plotter built by Bell Labs (Louis Ridenour, Radar System Engineering (New York: McGraw Hill: 1947), 239).



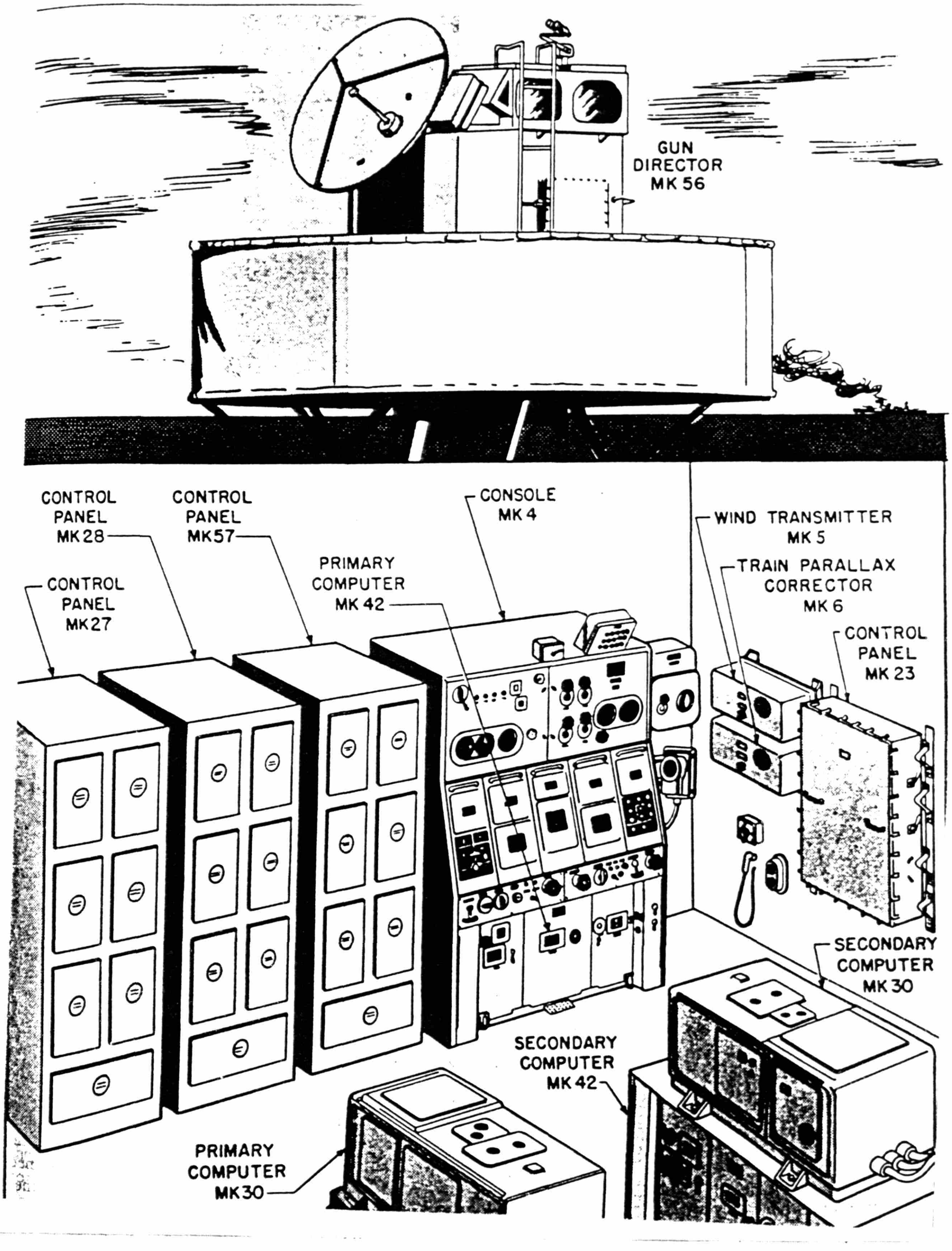


Figure 8-9: Layout of Mark 56 Gun Fire Control System. Two operators track optically from the deck positions, and two more work at the console in the control room below deck. (From Naval Ordnance and Gunnery: Volume 2, Fire Control (U.S. Navy, Bureau of Personnel, NavPers 10798, 1955, 319).

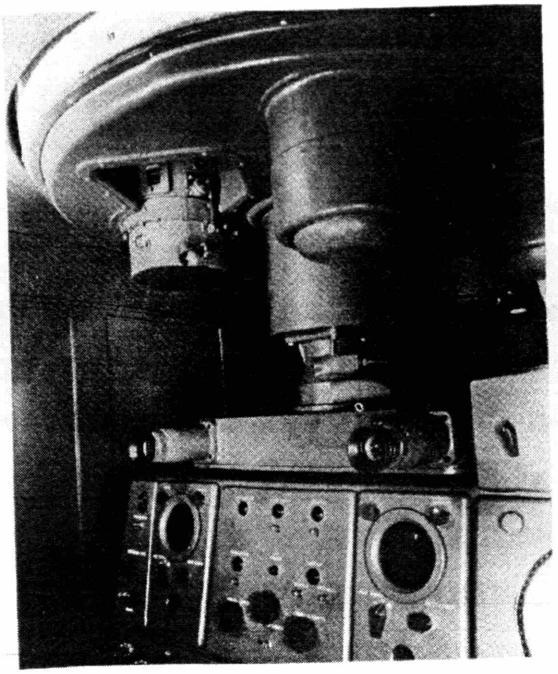


Figure 8-10 (left): Optical sights integrated into radar console in the M33 fire control system, proposed by Bell Labs at the end of the war but never produced. (from Fagan, ed., A History of Engineering and Science in the Bell System, 365).

Figure 8-11 (below): While automated fire control fought robot bombs in Europe, remote controls in the pacific faced another novel human/machine combination: Kamikaze attack, here on battleship *Missouri*. The quad-mounted 40mm gun at lower left (number 9) is under control of the Mark 51 director at center right (also 9). Similarly, the 40mm mount 11 is under control of the director at right (from Edward Steichen ed., US Navy War Photographs (New York: Crown Publishers, 1980), 89).

Chapter 9

The Turn to Information

Bell Labs, the Servo Lab the Radiation Lab, with their work in noise and system integration, brought together pre-war threads of servo theory and feedback engineering. The merge engendered more than combination. In crucible of wartime research, control and communication began to synthesize a new quantity: information. Technology reconfigured perception, integration, and articulation as distinguishing signals from background noise, processing them as representations, and transmitting them back into the world. The idea of the generalized information processor begat the general information machine, the "computer."

It has become commonplace, however, to say World War II merged "communication and control." Norbert Wiener, in his 1948 book Cybernetics: or Control and Communication in the Animal and the Machine articulated the marriage for a generation of engineers, systems theorists, and technical enthusiasts of varied stripes. Wiener declared the merger occurred instantly, obviously and completely in the course of his work on prediction devices. "I think that I can claim credit," Wiener wrote in his memoir, "for transferring the whole theory of the servomechanism bodily to communication engineering."[1] Recent historians, including Steve Joshua Heims and Peter Galison have revisited this account, exploring the genesis of his project, its roots in his earlier work, and its short-term failure and profound long-term effects.[2] But their views still center on Wiener: the academic, the intellectual, and the mathematician; they tend not to address his connection to a broader technical culture.

Before Wiener's cybernetics, technology was already suffused with what would later be called "cybernetic" ideas. The pre-war threads of control engineering, as well as wartime work on fire control, suggest a broader and more gradual convergence of communications and control than a "Wienerian" account. The culture of the NDRC, with its dual emphasis on Bell Labs and MIT, brought institutional pressure to bear on communications and control. Servo engineers turned to

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[1] Norbert Wiener, I Am a Mathematician: The Later Life of a Prodigy, (Cambridge: MIT Press, 1956), 265. Also see Cybernetics: or Control and Communication in the Animal and the Machine, (Cambridge: MIT Press, 1948), 8 for a similar account and a similar claim.

frequency response to characterize servomechanism behavior. Radar engineers adapted communications theory to deal with noise in tracking. And human operators were always necessary but problematic components of automatic control systems. These were but a few of the features of the technological terrain onto which Norbert Wiener stepped in 1940.

This terrain, particularly the field of control systems, was developing the technology, indeed the practical philosophy, that Wiener would articulate so effectively in his postwar writing. During the war, much of that philosophy coalesced around difficult problems of fire control: dynamic performance, mathematical precision, corrupted data, and the human operator. Research in data smoothing and prediction, including Wiener's, began to formalize the signals-based approach emerging at several institutions. Engineering practice coevolved with this theoretical work, and sometimes preceded it. Ballistics and firing tables stretched calculating machines, especially the differential analyzers, to their limits. Engineers at RCA and MIT sought to improve these "continuous" devices with "numerical" techniques, both for central computing facilities and for battlefield automation. Bell Labs built digital testers (indeed coined the word "digital,") out of telephone relays, maintaining and disseminating the NDRC's authority to evaluate fire control systems.

Tracing this broad outline of communications and control sets into relief the emergence of distinct ideas of "information," and "computers" out of the conjunction of communications and control. Neither "information" nor "computer" had a stable meaning during World War II, as each underwent struggles of definition. No episode illustrates these struggles better than Division 7's experience with the proposed electronic computer which eventually became "the first electronic digital computer," ENIAC. The NDRC fire control committee turned down a request to fund the machine. Why was Division 7, highly innovative in other respects, unwilling or unable to support forward-looking work in electronic computing? Instead of explanations pointing to Division 7's "limited vision," or "commitment to analog computing," we must understand Division 7's interest (or lack thereof) in electronic digital computing in the context of its overall research program in fire control. As a kind of scientific controversy, the NDRC's rejection of the ENIAC proposal

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[2] Steve Joshua Heism, John von Neumann and Norbert Wiener: From Mathematics to the Technologies of Life and Death (Cambridge: MIT Press, 1980). Peter Galison, "The Ontology of the Enemy: Norbert Wiener and the Cybernetic Vision," *Critical Inquiry* 21 (Autumn, 1994), 228-66.

highlights the epistemological and institutional stakes surrounding information and computers during World War II.

In contrast to their ambivalence about electronic computers, Division 7 supported fundamental intellectual work. Wiener and Claude Shannon, both under Division 7 sponsorship, shaped information science as much as any hardware innovation. In general — through feedback theory, frequency-response methods, smoothing and prediction theory, and system engineering — fire control established a mathematical and conceptual framework for post-war electrical and computer engineering. The legacy of Division 7 thus remains a richly charged paradox: failure to build the first "computers" combined with successful intellectual contributions. This paradox, however, is no coincidence. It derives from tensions in the NDRC's role as sponsor of "fundamental" research within a focused wartime environment. Technologies of control bore the imprint of the control of technology.

Noise, Out of Control, Predicting the Future

During the fall of 1940, when Warren Weaver and D-2 conducted their research survey, Ed Poitras visited MIT and met separately with Gordon Brown and Norbert Wiener. Brown was beginning his NDRC work on servos, and Wiener felt he might be able to contribute. He wanted to apply communications and network theory to servo problems. Poitras noted in his diary,

> [Wiener] wants to tackle the problem of solving for the controller of servos in terms of the input as the frequency spectrum...He believes that considerable of the present network theory could be applied to the servo problem.[3]

Wiener referred Brown's work then underway in his own institute; from Poitras's notes he seemed not yet familiar with fire control and expressed no interest in prediction. None of Wiener's correspondence mentions prediction before late 1940. He did, however, draw on long-standing interests in network theory and harmonic analysis. On and off for nearly ten years, Wiener had worked with a former Bush student, Yuk Wing Lee, at MIT and in China, reformulating network synthesis and even building an analog computer. While simultaneous with Bode's work on feedback networks, Wiener's work did not address feedback. He recalled later, "What was lacking in our work was a thorough understanding of the problems of designing an apparatus in

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[3] EJP diary, November 9, 1940. OSRD7 GP Box 70, collected diaries volume 1.

which part of the output motion is fed back again to the beginning of the process as a new input."[4] When Wiener proposed applying network theory to servo design to Poitras in 1940, then, he unknowingly sought to replicate Bode's work which was published that year.

Wiener and Bigelow: Prediction and Stability

Just where Wiener learned of the fire control problem and the importance of prediction is not clear from surviving documents. It may have been his peripheral involvement with Brown and Draper's work.[5] But by late 1940, Wiener applied his knowledge of networks to prediction in fire control — trying to circumvent the problem of differentiating the target's location to derive its velocity, an operation highly susceptible to noise. Working with Sam Caldwell, Wiener simulated a prediction network on MIT's Differential Analyzer, with encouraging results. Caldwell, who was then beginning as a member of D-2, submitted a proposal to the NDRC to build a network and an anticipator. D-2 let a contract, Project #6, on December 1, 1940 for "General Mathematical Theory of Prediction and Applications." Wiener then hired a research assistant, electrical engineer Julian Bigelow, who had had graduated from MIT in 1936 and worked for Sperry Gyroscope and IBM as an electronics engineer.[6] For the contract, Wiener and Bigelow would devise a theory to follow a given curve, chosen to represent the path of an airplane, and to estimate the value of that curve at some time in the future. During early 1941 Wiener and Bigelow designed and built a machine to simulate their ideas for prediction.[7]

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[4] Wiener, I Am a Mathematician, 190. Pesi R. Masani, Norbert Wiener 1894-1964 (Basel: Burkhäuser Verlag, 1990), 168-9.

[5] According to a letter Brown wrote to Nathaniel Sage of MIT's Division of Industrial Cooperation on December 9, 1940, Wiener had been invited into fire control work after a meeting at MIT between Fry and Brown that November. Brown wished to discriminate Wiener's "fundamental," work (which belonged to the NDRC) and his own practical work (which belonged to Sperry). Brown wanted the ability to freely apply Wiener's conclusions in the Servo Lab. Brown wrote to Sage, "Dr. Wiener is an authority on many aspects of the branch of mathematics that is related to this work. However, he is but meagerly informed on the techniques necessary to reduce to practice the matters which he can express mathematically. He is also but meagerly informed on the specific limits which must be met when the results of a mathematical investigation are reduced to practice." Brown to Sage, December 18, 1940. OSRD7 GP Box 4, Project #6.

[6] Steve J. Heims interview with Julian Bigelow, November 12, 1968, Princeton NJ. Steve J. Heims Papers, MIT Archives, MC-361 Box 1 Folder 5. It is unlikely Bigelow worked on fire control at Sperry, as his specialty was communications electronics; he probably worked in Sperry's nascent radar group.

[7] Several published accounts narrate of Wiener's work in prediction. Wiener, I Am a Mathematician, 242-56. Stuart Bennett, A History of Control Engineering, 1930-1955 (London: Peter Peregrinus, 1993), 170-79. *Idem.*, "Norbert Wiener and Control of Anti-Aircraft Guns," *IEEE Control Systems* (December, 1994), 58-62. Peter Galison, "The Ontology of the Enemy." P. Masani and R.S. Phillips, "Antiaircraft Fire Control and the Emergence of Cybernetics," in Norbert Wiener: Collected Works with Commentaries, ed. Masani, (Cambridge: MIT Press, 1985), Volume 4, 141-79. This article, 157-69, has a mathematical analysis of an antiaircraft director system based

They quickly ran into a stability problem: "the pieces of apparatus designed for best following a smooth curve were oversensitive and were driven into violent oscillation by a corner." In other words, like classic prediction methods, Wiener's network was highly sensitive, even unstable, in the presence of high frequency noise, "it became obvious that in any curve not precisely of the shape of a simple sinusoid or straight line, any attempt to use this method of prediction would lead to a failure because of lack of stability."[8] This was a cousin of the stability problem electric power had faced twenty years before — transient inputs caused high-frequency oscillations. Engineers at Sperry, and increasingly at BTL also, knew only too well that jerky-tracking and rapid maneuvering of the target would introduce high-frequency perturbations. Wiener quickly realized the problem was fundamental, "in the order of things," and would need a new approach (he compared it to Heisenberg's uncertainty principle). He and Bigelow now turned to statistics, designing a new predictor based on "a statistical analysis of the correlation between the past performance of a function of time and its present and future performance." The network calculated a future position of the target based on the statistical characteristics of its past performance (its autocorrelation). It then continually updated its own prediction as time passed, comparing the target's flight path with previous guesses. A feedback network converged on guesses which minimized this error.[9] In modern terms, this device might be described as a one-dimensional neural network, which learned about the world as it gathered new data.

By June of 1941, Wiener and Bigelow designed an electrical filter to perform this prediction and presented it to Bell Labs. Bode, Lovell, and their group were working on similar problems with their new electrical directors (the T-10 and T-15) and were favorably impressed.

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on Wiener's theory and on input from Ivan Getting. It describes a closed-loop system comprising a conically-scanned radar, along the lines of the SCR-584, a computing director, and a set of gun control servos. Its conclusion, however, that "All told, the results of the air war fought in the years 1942 to 1944 with AA directors designed and operated along the lines of the principles described in this section were impressive," is misleading. No directors working with conically-scanned radars, nor any electrical directors, were deployed until 1944. Even those devices, the M-9 and SCR-584 were both designed well before Wiener produced his result. (See Chapter 8). The statement "The methods of filtration used by workers after 1942 were all adaptations of the general filtration theory worked out by Wiener using the RMS error criterion," may have been true at the Radiation Laboratory, where Ralph Phillips himself took the lead in applying Wiener's ideas, but was not yet valid for the wider community of control engineers. Even close by at the Servo Lab, Wiener's ideas were not employed in servo design through most of the war.

[8] Norbert Wiener, Final Report on Section D2, Project #6, December 1, 1942, quoted in Masani and R. Phillips, "Antiaircraft Fire Control and the Emergence of Cybernetics," 152.

But while BTL sought a device for immediate application, MIT worked toward the longer-range goal of optimal prediction. Still, Wiener was cheered by the two groups' "similarity of approach" although he may have meant no more than "the identical concepts of realization by electrical [as opposed to mechanical] means."[10] Through the remainder of the year, Wiener worked out the theory behind his statistical approach in detail, scribbling on a blackboard as Bigelow took notes. Warren Weaver, the mathematician turned science manager, retained an active interest in the project and the two men got along well. Weaver noted Wiener's work "probably represents about the ultimate that could be accomplished in designing a predicting system which will take into account all ordinary geometric and dynamic factors, will do the best possible job in filtering out errors, and will take proper account of any statistical trends which may exist in aerial tactics and/or in the habits of aviators."[11]

Weaver let a D-2 contract (Project #29) for Wiener to write up his theoretical results. Wiener's report, The Extrapolation, Interpolation, and Smoothing of Stationary Time Series, was published by the NDRC for restricted circulation in early 1942. Here Wiener explicitly brought together statistics and communications theory and echoed Frank Jewett's pre-war aim of unifying communications theory along the spectrum,

> In that moment in which circuits of large power are used to transmit a pattern or to control the time behavior of a machine, power engineering differs from communication engineering only in the energy levels involved and in the particular apparatus used suitable for such energy levels, but is not in fact a separate branch of engineering from communications.[12]

Building on his own work in harmonic analysis and operational calculus, Wiener constructed a general theory of smoothing and predicting "time series," — any problem (including economic and policy questions) expressed as a discrete series of data. While he gestured at electric power and servo design as well as communications, Wiener did not explicitly address any previous work in feedback theory. Chapters included a general mathematical introduction, a treatment of linear

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[9] See Bennett, A History of Control Engineering, 174, and "Norbert Wiener and Control of Anti-Aircraft Guns," for a technical explanation of this approach. See also Thomas Kailath, "Norbert Wiener and the Development of Mathematical Engineering," (unpublished manuscript, Stanford University, 1996).

[10] "Meeting at BTL of Wiener and Bigelow (and SHC) and BTL Group," June 4, 1941. OSRD7 GP Box 70 collected diaries volume 2.

[11] "Summary of Project #6: Section D-2, NDRC," October 1, 1941. OSRD E-151 Applied Mathematics Panel General Records, Box 24.

[12] Norbert Wiener, The Extrapolation, Interpolation, and Smoothing of Stationary Time Series (Cambridge: MIT Press, 1949), 3. This is the published version of Wiener's original "Yellow Peril," report (so named because of its

prediction, an algorithm for minimizing the prediction error, a method for synthesizing filters to accomplish optimal prediction, and an extension of prediction to multiple time series. The final chapter detailed relevant examples, including the problem of deriving rates from noisy signals, so common to fire control and "of vital importance to all designers of servomechanisms."[13] Among the paper's numerous contributions was its demonstration that a feedback system could be made to optimize not only position or velocity, but an arbitrarily chosen measure of "goodness," in this case the statistically-defined "root mean square" (RMS) error. A number of NDRC researchers, including Ralph Phillips, George Stibitz, J.R. Ragazzini, and John Russell took up and expanded on Wiener's work. Wiener's frequently-cited paper formed the basis for post-war work of optimal estimation, smoothing and control (much of it intimately tied to military applications).[14] It was in response to a request for this paper in 1947 from a guided missile researcher that Wiener wrote his anti-military manifesto, "A Scientist Rebels."

Despite Weiner's formidable attack on prediction, Weaver and D-2 harbored doubts about its ultimate practicality. "It is not at all clear," Weaver wrote, "that this study will result in a design practicable for large scale production."[15] Fundamental and influential as the work would prove in later years, Wiener's scheme had insurmountable problems in practice. The algorithm assumed an infinite or very long period in the past on which to base its prediction. In reality, a target could be tracked for only a few seconds before the prediction was needed. Also, starting and stopping the system in a finite time interval introduced noise spikes at the ends of the time series, further corrupting the prediction. Furthermore, Wiener's scheme minimized RMS error, which gave progressively less value to a miss based on the square of its distance to the target. But RMS error does not accurately describe antiaircraft fire: if the shell does not explode within about ten meters

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yellow cover and difficult mathematics) "Extrapolation, Interpolation, and Smoothing of Stationary Time Series with Engineering Applications," NDRC Report to the Services 370, February 1, 1942.

[13] Wiener, Extrapolation, Interpolation, and Smoothing, 116. For a discussion of the technical significance of this paper, see Masani, Norbert Wiener, 182-87.

[14] For one example see Arthur Gelb, ed., Applied Optimal Estimation (Cambridge: MIT Press, 1974). Gelb cites Wiener's paper third in the introduction to the volume. Gelb also identifies the intimate similarity between smoothing, filtering, and prediction, all of which estimate the state of some system, differing only in that they work during, after, or before the data is available, respectively. Also see Kailath, "Norbert Wiener and the Development of Mathematical Engineering." L. A. Zadeh and J. R. Ragazzini, "An Extension of Wiener's Theory of Prediction," *J. Appl. Phys.* 21 (no. 7, July, 1950) reprinted in Richard Bellman, ed., Selected Papers on Mathematical Trends in Control Theory (New York: Dover, 1964), 150-62. For NDRC researchers involvement with Wiener's work, see Bennett, A History of Control Engineering 180-81.

[15] "Summary of Project #6: Section D-2, NDRC."

of the target, its worthless, no matter how far away it is ("a miss is as good as a mile"). The optimal predictor also required an extensive and complex network of electronics. George Stibitz of BTL, who now supervised the project for D-2, became increasingly skeptical, as, in fact, did Julian Bigelow himself. In July of 1942 Wiener and Bigelow demonstrated their predictor to Weaver, Poitras, Thornton Fry, and Stibitz. The D-2 members were impressed with the performance; Stibitz operated the device and recalled "It gave me the feeling of having my mind read." But questions remained, in Weaver's view, "Whether this is a useful miracle or a useless miracle."[16] Norbert Wiener, after all, was trying to build a machine for predicting the future — a goal with a definite alchemical tinge.

Wiener and Bigelow believed they were limited by statistical knowledge of pilot behavior and flight paths. They wanted to collect data on actual human tracking operators and pilots. The two set out then, on a tour of sites doing research in antiaircraft fire control. By this time Weaver became fed up with what he saw as Wiener's naïve faith in an ideal analytical solution: the project had been underway for nearly two years with no practical applications to show. During that time BTL, the Rad Lab, and the Servo Lab had radically transformed the practice, if not the theory, of fire control.

Weaver vented his frustration in a memo which conveys how poorly the idealistic mathematician fit into Weaver's new, secret world of control,

> [Wiener and Bigelow] have gaily started out on a series of visits to military establishments, without itinerary, without any authorizations, and without any knowledge as to whether the people they want to see (in case they know whom they want to see) are or are not available. WW [Weaver] is highly skeptical about this whole business, but inasmuch as the die has been cast that they are trying to do this job, the only alternative is to try to give them as complete an exposure as possible....Inside of twenty four hours my office begins to receive telegrams wanting to know where these two infants are. This item should be filed under "innocents abroad."[17]

Wiener and Bigelow visited military installations at the Aberdeen Proving Ground, the Frankford Arsenal, and the Anti-Aircraft Artillery board at Camp Davis, North Carolina. The two also visited Tufts, Princeton, Fort Monroe, and the Foxboro Company in Massachusetts, all of which

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[16] George Stibitz, <u>The Zeroth Generation: A Scientist's Recollection s(1937-1955) from the Early Binary Relay Digital Computers at Bell Telephone Laboratory and OSRD to a Fledgling Minicomputer at the Barber Coleman Company</u>, unpublished MS, 204. The author is indebted to Paul Ceruzzi for the loan of this rare book. WW diary, July 1, 1942. OSRD7 Box 71 collected diaries volume 4. Also see Bennett, <u>A History of Control Engineering</u>, 178-9.

[17] WW Diary, September 1, 1942. OSRD7 GP Bod 71, collected diaries volume 4.

were conducting studies of human operator performance under D-2 contracts. Wiener and Bigelow returned to MIT and prepared an experiment where a human operator would try to track a dot of light as it traveled along a random path on the wall — a means of collecting the statistics of human operators in the laboratory. All through the year Wiener had confidence in the program. Not immune to wartime ambition, he pressed for a larger organization, proposing the NDRC support a staff of six plus several mathematicians to help with servo and radar problems. As the project neared completion, he wrote to Weaver "we ourselves feel it has been carried out successfully and any further development will involve a very considerable expansion which we feel will be worthwhile."[18]

Where Wiener felt ambitious and inspired, Bigelow became discouraged. Discussing a report of their trip, Weaver recorded that Bigelow, "is now convinced that the Wiener statistical predicting method, taking into account the character of the present problem and the character of the associated equipment, has no practical application to fire control at this time," and the young engineer "seriously doubts that W[iener] will be able to bring himself to make this statement." Bigelow's pessimism was driven his observation of the highly subtle, non-linear nature of the human operators' performance.[19] This was late 1942, just as D-2 was transforming into Division 7. At the new division's first meeting, Weaver reported that Bode's work on "curved flight prediction" for the new T-15 director seemed more promising than Wiener's predictor. For predicting actual recorded target tracks, Wiener's "optimal" method proved only marginally more effective than Bode's far, far simpler design. At its next meeting, Division 7 decided to "terminate" Wiener's work; Project #6 ended in January, 1943 (Bigelow left to joint a statistical fire control group at Columbia).[20] By contrast, on this same occasion the committee initiated its work in system integration, naval fire control, and the optimal use of radar. The termination of Wiener's contracts just as D-2 transferred to Division 7, although somewhat coincidental, reflects the NDRC's turn away from "fundamental" studies toward more industrial, applied projects.

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[18] NW to WW, January 6, 1942 and June 18, 1942. Wiener Papers, Box 2 Folder 64.

[19] WW diary, November 10, 1942. OSRD7 GP Box 72, collected diaries volume 5.

[20] Division 7 Meeting Minutes, January 7-8, 1943 and February 3, 1943. OSRD7 GP Box 72 Division 7 Meetings folder. See also Galison, "The Ontology of the Enemy," 244-5 and Bigelow interview, 8. NW to WW, January 15, 1943 and January 28, 1943 are Wiener's last words on the project to the NDRC. Wiener recognized his predictor barely exceeded the performance of competing smoothers, but he believed there was too little data (only two courses for comparison) and that further work should continue to compare ten or a hundred courses.

Still, the shift away from fundamental research may not fully explain Wiener's termination. Something seems missing. Weaver and D-2 surely recognized the profound import of Wiener's ideas. In 1944 Weaver reported to Division 7 his belief that "when this war is over the theory and mechanization of smoothing will be one of the outstanding contributions of the NDRC fire control group."[21] Immediately after the war, Weaver and the Rockefeller Foundation supported Wiener's early cybernetics work. Wiener and Bigelow's two contracts cost just over $30,000, a paltry sum less than one third of one percent of D-2 and Division 7's total outlays (the $2,000 for Wiener's Extrapolation, Interpolation, and Smoothing report was the single smallest fire control contract). It remains odd that they terminated such important work that cost so little money, immediate application or no. Perhaps Wiener's inability to conform alienated him from the chummy culture of the NDRC. Perhaps the committee distrusted the left-leaning Jewish Professor with a disdain for secrecy. Existing documents do not confirm these speculations, but the evidence we have doesn't quite tell the full story.

Whatever the reasons, Wiener was disappointed by his failure to produce a practical device for the war effort; he plunged into elaborating on his work. The previous spring, Wiener and collaborators physician Arturo Rosenblueth and physiologist Walter Cannon, began addressing physiological and neurological feedback (Cannon's 1932 book, The Wisdom of the Body, had explored the feedback mechanism in biological homeostasis). In the Spring of 1942 Wiener first mentioned the idea of the human operator as a feedback element, an integral part of the system. He discussed the "behaviorist" implications his work in control, "the problem of examining the behavior of an instrument from this [behaviorist] point of view is fundamental in communication engineering."[22] "In order to obtain as complete a mathematical treatment as possible of the over-all control problem," he wrote in his memoirs, "it is necessary to assimilate the different parts of the system to a single basis, either human or mechanical."[23] This period, then, marked the conception of Wiener's "cybernetic vision," which would make him famous after the war. Wiener labeled this understanding of the servomechanical nature of the human-machine relationship as the

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[21] Division 7 Meeting Minutes, March 1, 1944. OSRD7 GP Box 72, Folder Division 7 Meetings.

[22] See, for example, Wiener to Haldane, June 22, 1942. Wiener Papers, Box 2 Folder 64. This letter is marked "NOT SENT." That May, Rosenblueth mentioned his conversations with Wiener and Bigelow in a presentation at a meeting on the physiology of the conditioned reflex, sponsored by the Macy Foundation. See Steve J. Heims, Constructing a Social Science for Postwar America: The Cybernetics Group: 1946-1953 (Cambridge: MIT Press, 1993), 14-15.

core of cybernetics and his research program sought to extend that understanding to biological, physiological, and social systems.

Did Wiener originate the idea of the human as part of a feedback loop? Or did he articulate and expand a "cybernetic vision" already taking shape within engineering practice? Our goal here is not to asses the significance of cybernetics, nor to trace its subsequent development and complicated legacy.[24] But since so much of cybernetics, even in today's colloquial sense, derives from the idea of the human as an integral part of the control system, these questions are worth asking. In light of the NDRC's research program in fire control, and, for that matter, in light of decades of pre-war control engineering, Wiener's syntheses, of communications and control, human and machine, were inspired articulations of broad patterns in control more than new configurations.

Consider, for example, a letter written by Harold Hazen to Warren Weaver, in May of 1941, after Hazen's own visit to the army's antiaircraft research facilities. He wrote "the idea struck me more and more forcefully that we should know as much as possible of the dynamic characteristics of the human being as a servo and therefore his effect on the dynamic performance of the entire control system." Hazen suggested studies of "the fundamental mechanical parameters of the human operator," and while he did not explicitly recommend a statistical approach, he did argue for knowing the frequency response of human reactions and "ranges of variation among individuals and for a given individual their variation with the various factors that influence human behavior." Psychologist Samuel L. Fernberger expanded on Hazen's ideas, suggesting emotional stability, group behavior of machine operators, and the effects of battle stress on human control as worthy, if difficult, objects of laboratory study.[25] Under Fernberger, D-2 initiated a program of research into the human being as an element in feedback loops.

By the time Wiener made his tour in late 1942, D-2 had its own program of "cybernetic" research. Psychologists at Brown University, Harvard, Ohio State, and Tufts and a number of other institutions studied the human element of fire control for Division 7, part of a larger pattern

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[23] Wiener, I Am a Mathematician, 190.

[24] For some of the complex evaluation of Cybernetics' legacy, see Heims, The Cybernetics Group. Michael Arbib, "Cybernetics After 25 Years: A Personal View of System Theory and Brain Theory," *IEEE Trans. Systems, Man, and Cybernetics* 5 (May, 1975), 359-365.

[25] HLH to WW, "The Human Being as a Fundamental Link in Automatic Control Systems," May 13, 1941. Fernberger to HLH, May 27, 1941. OSRD7 Office Files of Warren Weaver Box 3 MIT General Folder.

of social scientists contributing to the war effort.[26] Princeton University set up a special laboratory for man/machine interfaces at Fort Monroe in Virginia, which Wiener visited on his tour. Studies at the Foxboro Company, which Wiener also examined, looked at the effects of inertia, friction, and gear ratio on hand and foot controls, as well as the effectiveness of data displays. Computer innovator John Atanasoff conducted experiments at Iowa State College, for example, on tracking with small knobs instead of handwheels to achieve finer control with finger muscles than would be possible with coarser hand and back movements (Project #12). Another battery of tests tried to determine the effects of diverse factors on operator performance including gender, exercise, practice, stereo acuity, pupil size, startle, bells and loud noises, electric shocks, and drugs.

The strangest of these human performance studies gave new meaning to the concept of stability in a control system: psychologists searched for ways to determine if an individual would become "emotionally unstable" under fire. Division 7 brought five British seaman who operated fire control equipment to the Princeton Laboratory in Virginia. Two of these men had "broken up" in combat off Crete and the remainder had stayed at their positions. Without being told who was who, researchers tried to distinguish the "stable" and "excellent" men from the unreliable ones. Psychiatric evaluations, Rorschach Ink Blot Tests, opthamalogical exams, electric shocks, and a number of other scientific indignities all failed to detect which of the men had "broken."[27]

Communications engineers proved most rigorous in applying control theory to human performance. E.B. Ferrell of Bell Labs studied the stability of an antiaircraft system including the performance of the human operator. His May, 1942 memorandum, "Automatic Tracking as a Feedback Problem," used Bode plots of amplitude and phase relations to map the stability of a closed-loop tracking network,

> The difference in azimuth between the output shaft, as marked by the telescope cross-hairs, and the target azimuth is detected by a human eye and brain, amplified by human muscles, and passed through a handwheel and gear-train to the output shaft in such a polarity as to reduce the observed difference. This is a negative feedback system. If the higher frequency

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[26] For another social scientists in World War II, see Peter S. Buck, "Adjusting to Military Life: The Social Sciences Go to War 1941-1950" in Merritt Roe Smith, ed., Military Enterprise and Technological Change: Perspectives on the American Experience (Cambridge: MIT Press, 1987), 203-252.

[27] Report from Project 10 (Tufts College) to the NDRC, "Experiments with British Seamen," OSRD7, General Project Files. Also see Summary Technical Report of Division 7, NDRC Volume III: Rangefinders and Tracking, pp. 126-7.

> components are transmitted around the loop with improper phase relations then oscillations may occur and jerky tracking may result.[28]

This short survey of D-2 and Division 7 projects shows the idea of the human being as a servomechanism and the notion of the human-machine combination as a feedback system were maturing before Wiener did his work on fire control. He reacted to and built on an evolving understanding, pervasive among engineers and psychologists involved with fire control, that the boundary between humans and machines affected the performance of dynamic systems and was a fruitful area of research. Unlike Wiener, however, NDRC researchers remained bound by military secrecy at least until 1945 (many remained so after the war) and busy with contractual obligations. Wiener, by early 1943 was free to do and say as he pleased (he angrily resigned as consultant to the Radiation Lab in February, 1942), with no publication restrictions and no obligations to wartime research contracts.

Did Wiener's alienation from the NDRC induce his post-war estrangement from military engineering? Pesi Masani, Wiener's colleague and biographer, argues no, because his final report suggested further research into military prediction.[29] The wartime writings certainly contain none of the criticism which appeared after Hiroshima and Nagasaki. In the early forties, Wiener was anything but a pacifist; he suggested antiaircraft shells filled with flammable gasses to burn enemy planes from the sky and methods for fire bombing forested areas and grain crops.[30] Still, the disappointing NDRC project must have influenced Wiener's feelings about military research. His only substantive contact with what he later called "the tragic insolence of the military mind," occurred under NDRC auspices and ended in January, 1943.[31] George Stibitz, when submitting his final report on the NDRC project he supervised, added an addendum which read "Professor Wiener has asked that no mention of his name be mentioned in connection with any War work."[32]

For Norbert Wiener, in the midst of the technological war, cybernetics became a civilian enterprise. Working outside the massive wartime research effort, he had access only to civilian

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[28] E.B. Ferrell, "Automatic Tracking as a Feedback Problem," May 20, 1942, OSRD7 GP Box #2.

[29] Masani, Norbert Wiener, 190.

[30] Lt. Col. C. Thomas Sthole to NW, July 23, 1943. Wiener Papers, Box 1 Folder 57. NW to Bush, September 21, 1940. Box 2, Folder 58, Wiener Papers.

[31] Norbert Wiener, "A Scientist Rebels," Atlantic Monthly January, 1947, reprinted in Masani ed., Collected Works vol. 4 748. Note that in Masani, Norbert Wiener, the bibliography of Wiener's military work (p. 391) lists no contributions after January 15, 1943. Wiener did do some peicemeal consutling for the military through the early 1950s, and his attitude toward the military mellowed after the initial post-Atomic bomb bitterness.

resources. His 1943 paper, "Behavior, Purpose, and Teleology," written with Rosenblueth and Bigelow, allies servomechanisms with the "behavioristic approach" to organisms and classifies behavior by level of prediction.[33] The paper's philosophical tone and biological metaphors reflect not only Wiener's alliance with the life sciences but also the strictures of secrecy surrounding his prior work. Physiologists and biologists, like Wiener, were free of the war effort. Wiener acknowledged the role fire control and prediction played in his thinking, but beginning with "Behavior, Purpose, and Teleology," cybernetics recast military control in a civilian mold.

Most indicative of this alienation and reconstruction is Wiener's consistent failure to acknowledge the multiple traditions of feedback in engineering which preceded him. In all his writing on cybernetics, he *never* cited Elmer Sperry, Nicholas Minorsky, Harold Black, Harry Nyquist, Hendrik Bode, or Harold Hazen — all published on the theory of feedback before 1940; all were recognized as important to the field; all speculated on the human role in automatic control; some even wrote on the merger of communications and control and the epistemology of feedback. But Wiener only rarely cited *any* servo theory later than Maxwell's 1867 paper "On Governors."[34] Wiener called this paper as fundamental but, as Otto Mayr persuasively argued, it lacked the idea of a "closed feedback loop" so central to later conceptions of control.[35] The omissions are striking. Wiener must have been aware of these predecessors: he was closely involved in Vannevar Bush's research program in the 1930s including the work on servos; he worked with the MIT's Servomechanisms Lab and its Radiation Laboratory during the war; he was in touch with Hendrik Bode during the war. Still he wrote, *"I think that I can claim credit for*

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[32] George Stibitz, "Summary Report on Division 7.5, Relay Computers," OSRD7 Office Files of Harold Hazen, Box 6.

[33] Arturo Rosenblueth, Norbert Wiener, and Julian Bigelow, "Behavior, Purpose, and Teleology," *Philos. Sci.* 10 (1943) 18-24, reprinted in Masani ed., Collected Works Volume 4, 180-86.

[34] On page 7 Cybernetics, Wiener cites Leroy A. MacColl, Fundamental Theory of Servomechanisms (New York: Van Nostrand, 1946). This book synthesizes the Bell Labs approach to servos as developed for the T-10 director. In Wiener's Collected Works (Masani, ed.) on cybernetics, the only references to feedback theory other than Maxwell in about seventy-five papers: "Time Communication and the Nervous System," (220-242) a transcript of a speech Wiener gave to the New York Academy of Sciences meeting on "Teleological Mechanisms," in 1946, contains more references to feedback theory than the rest of Wiener's writing combined. It cites MacColl, James, Nichols, and Phillips, Arendt and Taplin, and Brown and Campbell, as bibliography entries, not footnotes. "Automazation," from the *St. Louis Dispatch*, December 5, 1954, was co-authored with Donald Cambpell, MIT engineering professor, Servo Lab engineer, and student of Gordon Brown. This article is clearly divided between Cambpell's predictions for industry and Wiener's warnings. "Muscular Clonus: Cybernetics and Physiology," (Rosenblueth, Wiener, and Garcia Ramos) 466-510 includes Nyquist diagrams (citing MacColl), but this paper seems to have been written by Garcia Ramos in 1985 from research notes.

transferring the whole theory of the servomechanism bodily to communication engineering." Wiener's chapter on "Cybernetics in history," from The Human Use of Human Beings, refers only to Leibniz, Pascal, Maxwell, and Gibbs as "anscestors," of the new discipline.[36] Wiener gave cybernetics an intellectual, scientific trajectory, divorced from the traditions of technical practice from which it sprang.

Improving the Analog Infrastructure

Wiener's relationship to other research in communications and control would be irrelevant were it not for their own subtle and significant histories. Because fire control involved ballistics, and because it required rapid, precision computation, D-2 and Division 7 drove research in computing machines. What we now see as the most advanced of these technologies — electronic digital computing — had a tense and ultimately untenable place within the NDRC. The agency's support and rejection of differential analyzers, "numerical" fire control, relay computers, digital testers, and electronic computers, traces control's hesitant contribution to computing.

At the start of the war, four Bush-style differential analyzers comprised the country's largest and primary mechanized calculating facilities. The original one, at MIT, had six integrators and spent the war doing ballistics full-time for the Naval Proving Ground at Dahlgren, Virginia. The Moore School of Engineering at the University of Pennsylvania built a copy of Bush's machine with fourteen integrators, and a six integrator machine for the Ballistics Research Laboratory (BRL) at the Army's Aberdeen Proving Ground in Maryland. The Penn machine, in addition to having the largest number of integrators (and hence the ability to solve the most complex equations), was also the most refined. Researchers there had embellished Bush's original design with new servos and automatic curve followers (improvements they also applied the to the BRL machine).[37] Both machines computed army ballistics tables during the war. General Electric built a differential analyzer, based on the Penn machine, with fourteen integrators, ready in the

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[35] Otto Mayr, "Maxwell and the Origins of Cybernetics" in Philosophers and Machines (New York: Science History Publications, 1976), 168-88.

[36] Norbert Wiener, The Human Use of Human Beings (Cambridge: MIT Press, 1950), Chapter I.

[37] Edward Moreland, OSRD, to Richard Taylor, MIT, August 18, 1942 and Richard Taylor to WW, August 29, 1942. OSRD7, Office Files of Harold Hazen, Differential Analyzer Folder. Taylor's letter contains a survey of computing facilities available to the NDRC in 1942.

1943.[38] The fourth computer, the Rockefeller Differential Analyzer at MIT, had eighteen integrators but was under construction until 1943. When operational, it ran fire control tables for BuOrd.

Even with these efforts, in 1942 firing tables became a bottleneck in the army's ability to field new ordnance. Pressure mounted, therefore, to increase capacity. In July of 1942, the Moore School, through the Aberdeen BRL, proposed the NDRC help improve its machine for faster throughput and greater accuracy. On December 1, 1942 Division 7 let contract #62 to Penn for "Improvement of Differential Analyzers." The project would add new types of torque amplifiers, modify the equations, and make a recording device to log several values simultaneously with regard to more than one variable. Penn would also improve input and output devices, study mechanical slip in the integrators, automate curve following and "provide for the possibility of automatically transferring recorded values for subsequent punch card operation." The Nieman torque amplifiers would be replaced with more advanced servos.[39] While Penn was attacking the practical improvement of the differential analyzers, Weaver suggested a theoretical project for Claude Shannon.[40]

Claude Shannon — Channel to Bell Labs

D-2's work on differential analyzers supported the machines as computing facilities; their ability to produce firing tables (and data for ballistic cams) made differential analyzers a legitimate part of fire control research. Another, possibly more important angle presented itself, however, through the technical similarity of differential analyzers and mechanical fire control computers. The analyzers used feedback loops (Hazen's "back coupling") to solve equations, just as the directors used feedback to solve the coupled problems of ballistics and prediction (Earl Chafee's "cumulative cycle of correction and recorrection"). Both made extensive use of differential gears, integrators, and servos — the standard building blocks of mechanical computing. D-2 sought to

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[38] H.P. Kuehni and H.A. Peterson, "A New Differential Analyzer," *Trans. IRE* 63 (May, 1944), 221-28 and discussion, 429-31 describes the G.E. machine, with commentary from C.N. Wygandt of Penn. Also see HLH diary, December 8, 1942, OSRD7 GP Box 72, HLH Diaries Folder.

[39] "Recommendation for Contract: Improvement of Differential Analyzers," December 23, 1942, OSRD7 GP Box 46 Project #62. Edward Moreland, OSRD, to Richard Taylor, MIT, August 18, 1942. Proposal from BRL to OSRD, July 27, 1942. OSRD7, Office Files of Harold Hazen, Differential Analyzer Folder. Also wee WW diary, October 28, 1942, meeting with J.G. Brainerd of Moore School, OSRD7 GP Box 71 Collected diaries volume 4.

[40] WW to Willy Feller, December 28, 1942. OSRD7 GP Box 46 Project #62.

apply the universities' experience with the machines, including the theory of feedback, to industrially-produced mechanisms. Claude Shannon made the connection.

Shannon left MIT for a post-doctoral year at Princeton, and he was there when the NDRC organized in 1940. On Thornton Fry's suggestion, D-2 let contract #7 to Shannon for "Mathematic Studies Relating to Fire Control," asking him to analyze Sperry's antiaircraft director and another designed by the Frankford Arsenal. Shannon produced five papers for this contract, mostly on improving the smoothness of tracking. He studied calculating mechanisms, especially the smoothing circuits, as feedback networks. Using feedback, stability, and transient response, Shannon identified both the director and the differential analyzer as dynamic, closed-loop systems. As he had done with telephone relays, he compared the devices to electric circuits, "I find the use of electrical analogues very useful in understanding these devices and have used throughout a notation which emphasizes this idea." In "The Theory and Design of Linear Differential Equation Machines," Shannon drew on network theory and his relay algebra to define "analysis and synthesis," for differential analyzer setups as though they were electrical circuits. This paper made explicit the equivalence between the mathematical machines created at universities and "fire control equipment as rate finders, predictors, smoothers, etc..."[41] [*Figure 9-2: Shannon Smoother]

In 1941 Shannon joined Thornton Fry's mathematics department at Bell Labs, where he had spent the previous summer working on relay switching circuits. At Fry's suggestion, he began to work on fire control. Shannon thus became an institutional and intellectual link between MIT's differential analyzers and BTL's new work in fire control. Until then, Shannon's work on mechanical smoothing analyzed and improved existing machines; it did not synthesize new devices except as add-ons. Differential analyzers and mechanical computers represented mature

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[41] Claude Shannon, "Theory and Design of Linear Differential Equation Machines," Report to the Services 20, Bell Telephone Laboratories, Inc., January, 1942, reprinted in N.J.A. Sloane and Aaron D. Wyner eds., Claude Elwood Shannon: Collected Papers, (New York, IEEE Press, 1993), 514-559. Also see Harold Hazen, Summary Technical Report of Division 7, NDRC Volume I: Gunfire Control (Washington: Office of Scientific Research and Development, National Defense Research Committee, 1946), 59-60. Shannon's other four papers were "A Height Data Smoothing Mechanism," "A Study of the Deflection Mechanism and some Results on Rate Finders," Some Experimental Results on the Deflection Mechanism," "Backlash in Overdamped Systems." Shannon Papers, MIT Archives. Also see Frankford Arsenal, Fire Control Design Section, "Description of Antiaircraft Director," November 12, 1940. OSRD7, Misc. Project Files, Box 68. D-2 meeting minutes, January 28, 1941. OSRD7 GP, collected diaries, Box 70

technology, the workhorse computers of the war. At Bell Labs Shannon was exposed to electrical computers and to the increasingly difficult problem of smoothing in electrical circuits.

Speed and Accuracy: Electronic Digital Computing

When Shannon arrived at Bell Labs, they were working on D-2's flagship project, the analog T-10 director. But D-2 was born with a digital spoon in its mouth. At section's first formal gathering at the American Mathematical Society meeting at Dartmouth in September, 1940, George Stibitz demonstrated his binary computer made out of telephone relays. The "Complex Number Computer," (or Model I) worked with the imaginary numbers familiar to communications engineers (and power engineers).[42] Thornton Fry, one of D-2's first members, was Stibitz's supervisor at Bell Labs and had encouraged his efforts. From this early date, relay computers had a high profile within the division, a profile which Stibitz used to great success.

MIT Background in Numerical Computing

In addition to Bell Labs, MIT, which became so prevalent in NDRC circles, pursued numerical projects as an outgrowth of differential analyzers. The Rockefeller Differential Analyzer, a hybrid analog/digital machine, was under construction in 1940. Vannevar Bush, before he left MIT, built on his computing experience and in 1937-38 circulated memoranda outlining a "rapid arithmetical machine" based entirely on electronic switching. This architecture included keyboard inputs, a control unit, an arithmetical unit, memory storage, and recording outputs. The machine would employ "cascaded electric counting rings" in electrical tubes. Sam Caldwell, an early D-2 member, pursued Bush's ideas after his departure, supervising research assistant William H. Radford under sponsorship of the National Cash Register Company (NCR). Caldwell, just finishing the drawn-out construction of the Rockefeller machine, warned Radford of the difficulty of building machines with large numbers of vacuum tubes. Thus he focused on components, trying to include an entire counter (the equivalent of about ten tubes) in a single tube to reduce complexity and improve reliability. In the fall of 1939, Radford began teaching and turned the project to Wilcox P. Overbeck, who had recently returned to MIT after a few years at Raytheon. Overbeck continued the work until early 1942 when he went to Chicago to work on the Manhattan project. Overbeck's departure, combined with Caldwell's commitment to the NDRC

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[42] George Stibitz, "Computer," 1940, reprinted in Randell, ed., The Origins of Digital Computers (Berlin: Springer-Verlag, 1982), 247-252.

marked the end of MIT's Rapid Arithmetical Machine project. No system-level hardware had been built; NCR collected the results.[43]

The Arithmetical Machine did leave a personal and institutional legacy in numerical computing. In 1940, MIT consolidated its computing research and calculating services into the Center for Analysis. Caldwell, as head of the center, was both invested in analog computing and familiar with the latest numerical techniques — his opinions would heavily influence D-2 and Division 7 policy on computing. While the Center for Analysis was primarily a calculating facility and not officially involved in problems of control, at least two of its research assistants found broad significance in the mathematical instruments. Claude Shannon, who had been a differential analyzer operator, wrote his 1937 master's thesis inspired by the relays in the Rockefeller Differential Analyzer. "A Symbolic Analysis of Relay Switching Circuits," brought rigorous mathematics to the design of switching circuits, based on Boolean Algebra (see Chapter 5).

When Shannon completed his doctorate and left MIT, he was replaced as operator of the differential analyzer by another Caldwell student, Perry O. Crawford. Crawford worked on the NCR Rapid Arithmetical Machine project, he read Shannon's thesis on relay circuits, and he became interested in numerical computing. Because the differential analyzers were heavily involved in ballistics work for BuOrd during the war, and because Caldwell became increasingly devoted to D-2, Crawford saw that numerical computing could be applied to fire control. His cryptically-titled 1942 Master's thesis, "Automatic Control by Arithmetic Calculation," sketched a design for a numerical fire control computer. It was not a complete system, only the mathematical architecture required for target prediction, with components borrowed from Radford and Overbeck's work.[44]

From the outset, then, D-2 had at its disposal burgeoning interest and expertise in numerical computing. Stibitz's relay computers had been successfully demonstrated before a

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[43] For a detailed history of these projects, see the manuscript version of Wildes and Lindgren, A Century of Electrical Engineering at MIT Karl Wildes Papers, MIT archives, Chapter 5, 101-112. Bush's original memoranda have been lost, but they are summarized in V. Bush, "Arithmetical Machine," March 7, 1940, reprinted in Brian Randell, ed., The Origins of Digital Computers, 337-43. Claude Shannon, "A Height Data Smoothing Mechanism," May 26, 1941, Shannon Papers, MIT Archives.

[44] Perry Crawford, "Automatic Control by Arithmetic Computation," (S.M. Thesis, MIT, 1942). Perry Crawford, interview with author, January 10, 1995 (notes in author's possession). While Crawford's ideas were never implemented in a machine, he did play a significant role in digital computing at MIT. Late in the war he joined the Navy Special Devieces Center (SDC), which built simulation and training devices. SDC sponsored an analog flight

sophisticated audience. MIT's work in electronic counters, while building on extensive experience with differential analyzers, had not produced any functional hardware. Most important, woven throughout these projects were engineers who saw the potential of applying this new type of computing to problems of control. D-2's task, then, was to determine how to deploy these intellectual resources and energies, in a proper balance of exploratory research and systems design, to its own goals in fire control.

Electronic Fire Control at RCA

MIT's was not the only project of its kind. One other investigated electronic numerical computing for fire control. At RCA, television pioneer Vladimir Zworykin and colleague Jan Rajchman were working under contract with the Bureau of Ordnance. For a new fire control system, they studied electronic computing, individual computing elements, coordinate systems, and "the manner in which these elements are coupled together." Zworykin and Rajchman had particular interest in "computing devices in which variables are represented by discrete impulses," i.e. numerical techniques. They concentrated their effort on "coders," or electromechanical analog-to-digital converters, and a "computron," a vacuum tube which incorporated elements for a ten-bit counter into a single tube (similar to the NCR-sponsored work at MIT).[45] Individual vacuum tubes failed regularly, so the computron, by reducing the number of tubes, would correspondingly improve the reliability of any numerical machine.

BuOrd, however, was not used to funding this kind of fundamental research. Bureau chief Blandy, pressed by the urgency of the antiaircraft situation, wanted an electronic gun director by the end of the contract period, April of 1942. He soon realized the bureau "was perhaps too sanguine in its hopes" for practical hardware.[46] Progress toward numerical computing would be slow and long-term, so BuOrd requested the NDRC take over the RCA project. D-2 agreed; they had already been considering electronic "pulse" computing. Thornton Fry had recommended

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simulator at MIT's Servo Lab, and Crawford was instrumental in convincing Jay Forrester to build a digital computer for the project, which eventually became the Whirlwind computer.

[45] Vladimir Zworkin memo describing RCA tasks, June 6, 1941. OSRD7 GP, Project #48, Box 40. "Recommendation for Appropriation," July 1, 1942. OSRD7, General Project Files. J.A. Rajchman, G.A. Morton, and A.W. Vance, "Report on Electronic Predictors for Anti-Aircraft Control," April, 1942 in Brian Randell, ed., The Origins of Digital Computers: Selected Papers, pp. 345-348. Jan Rajchman, who worked on this project with Zworykin, later designed a magnetic core memory, lisenced it to IBM, and challenged Jay Forrester's patent for the technology. See Emerson Pugh, Memories that Shaped an Industry (Cambridge, MIT Press, 1984), 81-87.

Weaver "look into the possibilities of long range development of calculating techniques," specifically the RCA work and Stibitz's relay computers.[47]

In response to the navy's request, Weaver wrote to Zworykin at RCA and informed him of D-2's existing work in electronic (not necessarily digital) computation. He mentioned Caldwell's work at MIT, the BTL analog electronic director (the T-10), as well as Wiener's prediction network. Weaver also warned Zworykin, however, of D-2's commitment to mechanical computing:

> We [D-2] are rather strongly of the opinion that a serious and unnecessary handicap is imposed by ruling out mechanical techniques. We are also of the present opinion that the present state of the art, as regards impulse electronic computing devices, is not sufficiently advanced to warrant the attempt, at this time, to incorporate such devices into an over-all design for a predictor. It is our opinion that sounder and more substantial progress would be made by concentrating all efforts, at the present time, on the problem of simplifying and otherwise perfecting the essential computing elements themselves.[48]

D-2 was not looking to build computing facilities, rather to build fieldable predictors. Weaver pointed Zworykin to the work D-2 found most interesting: improving the "computron" tube and the basic electronics of the problem. The subtext of this note drew an institutional boundary. RCA's existing contract with BuOrd, typical for navy fire control work, was to conclude with the design of a complete fire control system and a proposal to build such a system.[49] The NDRC had a different definition of a research contract. Weaver demarcated what D-2 would support RCA to explore: feel free to look into components, Weaver seemed to say, but systems are our terrain. Other D-2 contractors, especially at BTL, could build systems. Restricting RCA to components meant restricting them to fundamental research, and D-2 was developing a habit of canceling such work.

Conference on "Electronic Fire Control Computers"

Defining organizational roles and defining the place of fundamental research also meant defining the computer. In April of 1942, D-2 held a conference on "Electronic Fire Control Computers" in New York City to clarify its position and to merge existing expertise in computers

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[46] W.H.P. Blandy to Secretary of the Navy, n.d., ca. spring, 1941, OSRD7, Office Files of Warren Weaver, Electronic Computers Folder.

[47] TCF to WW, June 27, 1941. OSRD7, Office Files of Warren Weaver, Thornton Fry Folder.

[48] WW to Zworykin, January 20, 1942. OSRD7, Office Files of Warren Weaver, Electronic Computers Folder.

[49] See Rajchman, ed., "Report on Electronic Predictors for Anti-Aircraft Control." Also "Electronic Fire Control Comptuers," December 16, 1941. OSRD7 GP Box 40 Project #48.

with that in control systems. Attendees represented military fire control experts, computer researchers, and NDRC sponsors. Commander Trichel and Haseltine attended from the army's Frankford Arsenal, in addition to Emerson Murphy, head of BuOrd's antiaircraft fire control section, Zworykin from RCA, Samuel B.Williams and F.H. Hibbard from BTL, Tuttle and Tyler from Eastman Kodak, Caldwell and Overbeck from MIT, and from D-2 Fry, Stewart, Stibitz (and also Caldwell).

The meeting began with an extended discussion on the advantages of electronic computation. Analog methods then in use, both mechanical and electronic, suffered from difficulties of scaling: how "big" could you make the electrical quantities in the machine? Does one mile per hour of target speed correspond to one volt, or to ten volts? These questions echoed Harold Hazen's about the Network Analyzer; again answers depend on accuracy. Given some minimum amount of noise or uncertainty in the signal, scaling depends on how precise the machine needs to be. If it can measure differences of a hundredth of a volt, for example, tracking a target of three hundred miles per hour means thirty volts. In contrast, the participants at the New York meeting noted, numerical techniques had no such scaling difficulties; arbitrarily high accuracies were obtainable merely by adding extra "digits" (or "bits") to the numbers. This, of course, increased the size and complexity of the machine as well, but how much, and with what effects, remained unknown.

Accuracy requirements, however, depended heavily on the application. For mathematical and scientific uses, more accuracy was often desirable. But in control, the system overall was only as accurate as its roughest component, and heavy servos to drive guns were not precision devices. This distinction had critical implications for D-2's interest in electronic computers,

> It is important to remember that it is impractical and indeed useless to carry the accuracy of the computer beyond a certain point. In fact, it is useless to have the computer operate with an accuracy which is unreasonable either from the point of view of the input data with which the computer must operate, or from the point of view of the accuracy with which the output of the computer can be utilized.

Tracking inputs, servo outputs, and the guns themselves had some finite accuracy, so improved computation would not improve the ability to hit a target. D-2 estimated current fire control technology would benefit a factor of two to three increase in computation accuracy, but more could not be justified. Existing computers gave a probable error of about eighty yards for ranges of 7000 yards; other factors, including gun dispersion, the accuracy of the power drives, and tracking errors stacked up total uncertainty to twenty to thirty yards.

Similarly, the advantages of speed turned on perspective. The army complained Sperry M-4 directors had too much delay in their computation. But a number of different speeds together determine speed in fire control solutions. Slowness in mechanical directors, others argued, resulted less from mechanical techniques than from the feedback involved in approximating the calculation (this was in part the issue Claude Shannon had been hired to examine). The speed of raw computation was thus distinct from the amount of time the time of flight circuit takes to converge on the solution (known as "settling time"). Other time delays, namely those due to data smoothing and the time of flight of the shell, inhered physically in the problem and could not be reduced with new computing. As with accuracy, electronic computing improved speed for only part of the system. The group reached "no definite conclusion" on the speed of electronic computation and how it would effect fire control systems. Proponents of electronic computing had a difficult predicament: D-2 did not concede their two critical advantages, speed and accuracy (which seem so obvious today). Without consensus on these points, electronic computing could not establish authority in fire control.

Electronic computation did seem "to be of a character well suited to large scale production," but so little experience on the topic existed that the topic was not discussed in detail. The issue of reliability and maintenance, however, proved more contentious. Because electronic computers would use a yes-or-no signal, "electrical circuits for such signals can be made highly reliable and insensitive to small variations." But many within the services, as well as in research, still distrusted the reliability of electronic equipment in the field. The issue resolved into familiarity. George Stibitz noted that "people with mechanical experience think all electronic devices full of troubles, and correspondingly reverse opinions [were held] by the others." This early meeting on electronic computing defined the issues for members of D-2. Numerical electronic computing might improve fire control systems, but only to the point where other components became limiting factors. The appeal of such machines differed if one was building a large, central computation facility or a mobile, reliable, field-deployed control system. For D-2, primarily interested in the latter, RCA had not made a strong case for numerical electronic computing.

Still, D-2 kept its options open. After the meeting, its representatives again visited RCA, where they saw a prototype "ballistic computer." This device had a "resistive function matrix," which stored firing tables for the guns. Equivalent in function to the Sperry ballistic cam, a derivative of Rajchman's function matrix would eventually be included in ENIAC. Several months had passed since RCA finished its navy contract, and they pressed the D-2 members for a prompt decision on spending money, otherwise the company would commit its resources to other projects.[50] D-2 acted promptly, and issued a "Recommendation for Appropriation" on July 1 for RCA to "carry the work forward" in electronic computing.[51]

But the support was cautious and qualified. Weaver emphasized to Zworykin that, because of other errors in the system, fire control would not benefit from the improved accuracy of electronic computers. Two possibilities could change this situation, Weaver added, and increase the demand for accurate computing: guns with much longer ranges, or radically reduced errors from dispersion, ballistics, and fuzes. These systems-level issues bore directly on D-2's definition of the type of research RCA would conduct:

> Thus the development of electronic antiaircraft predictors should be viewed as of the long-range future, rather than immediate present interest. We therefore do not think that such work should be allowed to compete with first priority developments of immediate interest and need.[52]

The NDRC would support the RCA work for three to four months, Weaver concluded, then decide on next steps. But just as with Wiener's "fundamental" studies of prediction, and nearly at the same time, defining the project as "long-range future" amounted to killing it.

After a few months, in November 1942, the NDRC dropped RCA's numerical computing project. Weaver explained that neither of D-2's two major devices, BTL's T-10 or T-15, could incorporate numerical computing. He did, however, recognize the scientific potential and importance of the work, writing to Zworykin, "We arrived at this decision [to cancel the project] reluctantly, because we all have lively personal and scientific interest in seeing this computron project continued to its successful conclusion."[53] Weaver sincerely tried to find other sponsors within the NDRC who might have taken up the project. He spoke with George Harrison of section

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[50] DJS diary, June 19, 1942. OSRD7, GP Project #48, Box 40.

[51] "Recommendation for Appropriation," July 1, 1942. OSRD7, GP Project #48, Box 40. See also "Automatic Computer for Anti-Aircraft Fire Control," July 6, 1942, OSRD7 GP Box 40 Project #48.

[52] WW to Zworykin, July 20, 1942. OSRD7, GP Project #48, Box 40.

[53] Weaver to Zworykin, November 11, 1942. OSRD7, GP Project #48, Box 40.

D-3 (instruments), who saw great scientific and commercial potential in the RCA project, but no military application. At the end of 1942, Harold Hazen was taking over Division 7. Weaver recommended to him the project be dropped, "with genuine scientific regret...because beautiful as the work is it does not appear at present to have any real prospect of being directly useful in the war."[54] Hazen, for his part, also tried to find other NDRC divisions willing to support the project, but to no avail.[55] The contract terminated March 31, 1943, just weeks after Norbert Wiener's own.

ENIAC Proposal

Defining computers within control systems, and defining electronic computers as fundamental research, shaped Division 7 policy throughout the war. No episode illustrates the ambiguous effects of that policy like the division's response to an idea that became "the first electronic computer." Just as the RCA project was ending, a group from the Moore School at Penn, including John Brainerd and John W. Mauchly, proposed an "electronic diff. analyzer" i to do ballistics calculations (they purposely used the abbreviation "diff." to stand for both "differential," and "difference").[56] Later, the machine was renamed electronic numeric integrator and calculator, or ENIAC. The proposal built on Penn's experience with their differential analyzer, and some project work with the Radiation Laboratory. The term "integrator" in the title reflects the importance of integration in the mathematics of the time, as well as the centrality of integrators in Penn's differential analyzer. Harold Hazen, however, did not think ENIAC could become operational before the end of the war, assumed to be within five years. He discussed the proposal with Sam Caldwell,

> who rightly emphasized the emergence of considerable new differential analyzer capacity in that the new differential analyzer at MIT is now actually doing ballistic solutions in shake-down operations. Only after the demonstration of a real justification by such a group should the undertaking of a new project be given serious consideration.[57]

A month later Caldwell acted forcefully to scuttle the project, writing to Warren Weaver (now at the Applied Mathematics Panel but still an influential member of Division 7),

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[54] WW to HLH, November 20, 1942. OSRD7, GP Project #48, Box 40.

[55] HLH to Klopsteg, February 6, 1943. Beggs to HLH, March 22, 1943. OSRD7, General Project Files.

[56] John G. Brainerd, "Genesis of the ENIAC," Technology and Culture 17 (no. 3, July, 1976) 482-88. The proposal was dated April 2, 1943, just two days after the RCA project terminated. Nancy Stern, From ENIAC to UNIVAC: An Appraisal of the Eckert-Mauchly Computers (Bedford, Mass., DEC Press, 1981), 18-19.

[57] HLH diary, April 14, 1943. OSRD GP, Project #62, Box 46.

> There is a certain amount of agitation, coming primarily from Brainerd at the University of Pennsylvania, for the development of an electronic differential analyzer to do high-speed numerical integration. This is a huge undertaking. I doubt that it can be finished until five years after the war is over.[58]

Division 7 decided not to fund the ENIAC project, which Army Ordnance then supported through Aberdeen's BRL.[59]

Historian Nancy Stern has argued that the conservatism of the NDRC leadership, combined with their "personal commitment to different technologies," blinded them to the value of digital techniques. Hazen indeed was an apostle of the analog art. He had written in his 1931 dissertation that numerical methods "have an artificiality irksome to the physically minded."[60] Penn's Herman Goldstine suggested the NDRC was caught between Caldwell's mechanical analog machines and Stibitz's electromechanical digital approach.[61] Institutional politics surely played a role. Hazen and Caldwell, MIT professors and Bush disciples, had a clear stake in the success of the Rockefeller machine (as did its private sponsor, Warren Weaver). Still, they did not see the Penn project as potential competition; in their eyes it would be time consuming and difficult to build. Caldwell, tired of the costly and drawn-out Rockefeller project (it was years late coming on line), wished to avoid another such headache. Aberdeen, in contrast, did pose a potential threat to Division 7. In 1943, just as he took over Division 7, Hazen visited Aberdeen's new Director Testing Center —a competitor to Division 7's testing infrastructure, and hence its authority in fire control. Hazen reported certain Aberdeen members antagonistic and resentful toward the NDRC, which they believed was siphoning scientific talent from the army. According to Hazen, they threatened not to accept any new NDRC technology into the army.[62]

The NDRC also had technical concerns about the Moore School. Division 7 was already funding a project at Penn to improve the differential analyzer which was moving slowly and had technical problems, probably including stability problems with its servos. When Weaver and Caldwell visited Penn in October of 1943 to evaluate it, they reported "a depressing day. Initiative

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[58] SCH to WW, May 15, 1943. OSRD GP, Ballistics, General Correspondence Folder, Box 80.

[59] For participant accounts of ENIAC development, see H.H. Goldstine, The Computer: From Pascal to von Neumann (Princeton: Princeton University Press, 1972) and Brainerd, "Genesis of the ENIAC."

[60] Stern incorrectly implies the MIT engineers were jealous of Penn's "more powerful" differential analyzer — in fact they felt Penn. had not adequately acknowledged their intellectual debt to MIT. Stern, From ENIAC to UNIVAC, 21.

[61] Goldstine, The Computer, 150.

[62] HLH diaries, March 5, 1943 and April 14, 1943. OSRD7 GP Box 72, Folder HLH Diaires.

and candor were entirely lacking."[63] Division 7 also found the electronics of Penn's proposal to be behind NCR and RCA. Jan Rajchman visited the Moore School group in 1943 and thought their technical ideas "extraordinarily naïve."[64] Indeed the ENIAC architecture, while numerical and electronic, was in an important sense less advanced than the Rockefeller Differential Analyzer. That machine used a digital crossbar switch and a punched paper tape for reconfiguration and programming. ENIAC resembled the Network Analyzer, programmed with a plugboard. To set up new problems, the machine needed to be rewired with cables — a process as difficult and time-consuming with a digital computer as with an analog one. The ENIAC designers wrote, "no attempt has been made to make provision for setting up a problem automatically." Their attitude toward programming reflected their experience with the mechanical differential analyzer, "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 is placed on the machine."[65]

We must see Division 7's interest (or lack thereof) in electronic digital computing in the context of its overall work on fire control. Weaver and Hazen did not fund generic technology research; they rather explored all avenues that would get them closer to a pressing, immediate, and short-term goal. When they received the ENIAC proposal, they had just shut down at least two projects as too fundamental and far off (Wiener, RCA). Indeed, ENIAC did not become operational until the war was over. Weaver and Hazen certainly saw the scientific and intellectual value of digital computing research; they expressed sincere regret at not funding the RCA project, and their efforts to find it another sponsor are well documented (although the record shows no

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[63] Division 7 Meeting Minutes, October 6, 1943 and November 3, 1943. OSRD7 GP Box 72, Folder Division 7 Meetings. Also see HLH diary, April 14, 1943. See HLH to Edward Moreland, May 10, 1943 for a clear, concise statement of Division 7's position on the ENIAC proposal, including Caldwell's opinion and references to the RCA project. OSRD7 GP Box 46 Project #62. Weygandt, in his discussion of G.E.'s Kuehne and Peterson, "A New Differential Analyser," wrote "Those of us who have spent a great deal of time and effort in trying to design a servo system for this purpose [a differential analyzer] realize the difficulty of the job....[it] is a difficult one because of of the wide speed range which must be covered and also because in the setup of a problem a number of the servo mechanisms may be cascaded.. A servo system which is stable in itself may not remain stable when interconnected with other similar systems."

[64] Rajman quoted in Stern, 44.

[65] "ENIAC Progress Report," December 31, 1943. Quoted in Stern, 75. For ENIAC programming, see Mitchell Marcus and Atsushi Akera, "Exploring the Architecture of an Early Machine: The Historical Relevance of the ENIAC Machine Architecture," *IEEE Annals of the History of Computing* 18 (no.1, 1996) 17-24.

similar effort for the Penn proposal). Weaver, in fact, began funding an electronic digital computer at MIT through the Rockefeller Foundation immediately after the war.[66]

By 1943 fire control was moving out of a period of radical innovation and into a period of refinement, incremental improvement, and system integration. Other elements in the system simply could not benefit from more accurate computing. More than speed and accuracy, military control systems required reliability, ruggedness, and compactness — characteristics decades away in digital computing. Furthermore, Penn did not propose such a field-deployable system. They wanted to built a university calculating center, akin to the differential analyzer. Division 7 might support fundamental work in fire control computers, but not a machine to produce firing tables, an application already peripheral to its charter. For the differential analyzers, it sponsored only improvements to existing machines.

Such a view does not diminish but rather underscores the radical nature of the early proponents' faith in digital techniques, despite great difficulties of reliability, size, and complexity. These problems, however, made electronic digital computing unsuitable for Division 7 support. The NDRC's failure to pursue such work, despite their recognition of its scientific importance, outlines the limitations of the wartime research paradigm, focused on short-term results and practical devices rather than fundamental research.[67] Wartime research in control systems achieved success, but within, and perhaps because of, the narrow goals it defined for itself.

"Topological, not Metric:" Relay Computers

D-2 and Division 7 did build computers, but those which met the key qualifications of institutional position, rapid construction, and immediate application. George Stibitz of Bell Labs had been instrumental in shaping these criteria, and he satisfied them all. Stibitz's computers, neither fire control systems nor ballistics machines, stood between applications and mathematics, between machinery and information, between control and communication. They were testers.

In late 1941, Barber Colman's Dynamic Tester began redefining the performance of fire control systems in the laboratory. It quickly became standard; contractors and fire control vendors, including Sperry and Ford Instrument, wanted their own machines to prepare for the Army's now-

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[66] Wildes A Century of Electrical Engineering, manuscript version, 5-127. This project, and the Center for Analysis itself, were eventually terminated due to the increasing prominence of the Whirlwind project run by the Servomechanisms Laboratory.

rigorous acceptance tests.[68] But the Dynamic Tester was difficult to reproduce. Different flight profiles, simulating differing paths of attacking airplanes, were "programmed" by specially machined two-dimensional cams. Changing the cams would change the flight profile, from dive bombing to evasive action, for example, but in a difficult and time consuming process.[69] These cams required a great deal of attention to create, including precision machining which itself introduced errors. As with Sperry's ballistic cams, a machinist would drill a number specific points around the cam and then grind it smooth. Mechanical cams could not produce the uniform, reproducible testing D-2 needed to disseminate its authority in fire control beyond the walls of its own laboratory.

Tape Dynamic Tester

During 1942, George Stibitz, technical aide to Division 7, began thinking about building repeatable and uniform testers, "it is now desirable to have a simplified form of dynamic tester which can be duplicated readily." In October he proposed the "Tape Dynamic Tester," which replaced mechanical cams with punched paper tape to program flight profiles. Comparatively inexpensive, the machine would consist largely of common parts already in production for the telephone network. Its "cams," i.e. tapes, could be easily, cheaply, and exactly duplicated for multiple installations, "typists replace machinists."[70] Such a machine would continue Stibitz's earlier work; he had experimented with relay computers since 1937. And Stibitz had credibility with the NDRC. In addition to being an increasingly active technical aide to D-2, he and associate Samuel B. Williams (one of BTL's premiere switching designers) demonstrated the "Complex Number Computer," or Model I, at the Dartmouth meeting when D-2 first met. In late 1942 D-2 let a contract, Project #60, to Western Electric and Bell Labs for a "Punched Tape Dynamic Tester." The original appropriation was $2,500; by the end of the war, Division 7 spent almost $400,000 on the project and more than half a million on Stibitz's three major computers.[71]

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[67]It would take Vannevar Bush's 1945 report to the President, Science: the Endless Frontier to add the crucial ingredient to the postwar research paradigm: government support of basic research.

[68] OSRD7 GP, Office Files of Harold Hazen, Folder Director Trials, Box 4.

[69] See OSRD7 GP, Project #25, April-November, 1942 for problems with dynamic testers.

[70] George Stibitz, "Proposed Dynamic Tester," October 19, 1942. OSRD7 GP, Project #60 Box 44. Also see Stibitz, The Zeroth Generation, 167-8.

[71] "Project Recommended for Appropriation, No. 60, Section D-2 — Fire Control, Simplified Dynamic Tester." OSRD7 GP, Project #60, Box 45.

Feedback lay at the heart of Stibitz's device, which amounted to a "tape controlled servo." [*Figure 9-3: Tape Servo] [*Figure 9-4: Tape Dynamic Tester] A rotating shaft connected to a series of 32 contacts and sensed the *actual* shaft position. These switches fed a set of relays which read the *desired* position off holes on the punched paper tape. Five holes, coded (in binary) which of the 32 positions the shaft ought to assume, the *desired* position (2^5=32). A relay network compared desired position with actual position and determined which way the shaft should rotate. The network drove the shaft one way or the other to make the error, the *desired* minus the *actual* position, equal to zero. To synthesize the relay network to perform this comparison, Stibitz employed the "relay algebra" developed by Claude Shannon in his master's thesis.[72] Stibitz termed the tape servo a "sampled" data system because it operated in discrete rather than continuous time intervals. Using a Nyquist diagram, Stibitz analyzed the servo as a feedback amplifier, using the "sampling period" to establish equivalence with a continuous servo.[73]

The Tape Dynamic Tester was less a computer than a data-driven servo (it did not acquire a model number in the Stibitz series). Digital tapes driving mechanical movements resembled the numerically controlled machine tools which appeared after the war. The application was not lost on the NDRC. In 1941, Stibitz and Duncan Stewart contemplated a tape-controlled device to mill the mechanical cams for the Barber Coleman Dynamic Tester (possibly the source of Stibitz's idea for the tape tester). In 1943 a Division 7 contractor wrote to Stibitz proposing "a means of constructing the metal cams by the use of a duplicating device on a milling machine, controlled by one of the Bell Laboratories tape controlled units." Indeed the company adapted the Tape Dynamic Tester to mill cams for an antiaircraft gunsights tester at the University of Texas (the "Texas Tester").[74] For fire control, the means for loading raw ballistic data into the machine proved a significant, sometimes limiting, component of design. Sperry solved it with ballistic cams,

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[72] George Stibitz, "Relay Servo Circuit," October 28, 1942. OSRD7 GP, Project #60, Box 45. Stibitz sent copies of this memo to Poitras, Stewart, Weaver, Mooney (Division 7's liaison with the Antiaircraft Artillery Board) and W.A. MacNair of BTL.

[73] George Stibitz, "Nyquist Loop for Tape Servo," February 19, 1943, and "Equivalent Feedback Amplifier for Tape Servo," February 14, 1943. OSRD GP, Project #60 Box 45. In 1944, Claude Shannon performed a similar analysis for a different application, with equivalent results those Stibitz produced, "Feedback Systems with Periodic Loop Closure, Memorandum for File," March 16, 1944, OSRD GP, Project #60, Box 45.

[74] Daniel Silverman to GRS, April 29, 1943. OSRD7 GP Box 45. Also see David Noble, Forces of Production: A Social History of Automation (New York: Alfred A. Knopf, 1986), 88. Noble incorrectly states that BTL never used the Tape Dynamic Tester for its original purpose.

BTL with specially-shaped potentiometers, the testers with paper tapes, RCA and ENIAC with the function matrix.

Stibitz's computers embodied the conjunction of communication and control. Input and output devices were all borrowed from telephone systems: tape readers, keyboards, teletypes. A teletype printout in the Division 7 archives dramatically demonstrates the equivalence of messages, signals, and data. A brief typed conversation, clearly between observers and operators of the machine, precedes a long series of nonsensical letters. This nonsense, however, codes data for the machine, "1000 cos N/2" a perturbation signal for the tester.[75] [*Figure 9-5: Equivalence of Signal and Text] Stibitz noted his design could also serve as a data transmission system, effecting action at a distance, "with no essential modification, the impulses could be transmitted over a single telegraph channel from the tape transmitter...and be reproduced in the form of motor rotation at the other end of the line."[76] The Tape Dynamic Tester, like the Complex Number Computer, could operate remotely over phone lines with little modification. Where Norbert Wiener theorized the fundamental notion of "the message" in computing systems, Stibitz implemented it in practice, even turning teletype messages into custom metallic parts.

Relay Interpolator

The Tape Dynamic Tester could absorb and process so much data, however, that it drove further automation. To prepare target courses for input to the machine, human computers calculated a series of points which described the track of the simulated attacker. The Tape Dynamic Tester needed about twenty of these points per second, all of which operators had to manually punch onto paper tape, a laborious and error-prone job. A typical run required six functions (three input and three output variables) of about 150 seconds in length, requiring about 20,000 points. The sixty or so courses required to thoroughly test a new director amounted to over a million points, or "about three years of a skilled [human] computer's time." Division 7 investigated a number of options for automating this process, including specialized machinery from NCR and IBM punched cards. Each printed its output data on paper, which still needed transcription to tape.[77] In June of 1942, Stibitz proposed another relay computer to "to generate

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[75] OSRD7 GP Box 45, Project #60.

[76] GRS diary, February 10, 1943. OSRD7 GP, Project #60, Box 45.

[77] Caldwell to Stewart, June 28, 1945. OSRD7 GP, Project #70, Box 50.

dynamic tester tapes punched at 1/20 second intervals from data given at one second intervals."[78] Thus only one point in every twenty would need to be calculated, and the machine would fill in, or interpolate, the rest. Stibitz called the device the "Relay Interpolator," (RI) (it became "Model II") [*Figure 9-6: Relay Interpolator]. On July 1, 1943, Stibitz and Bell Labs began Division 7 Project #70, to design and build the relay interpolator in three months. Ten weeks later, the machine, comprised of about 500 relays, began running and producing paper tapes for the dynamic tester.

In his proposal for the Relay Interpolator, Stibitz noted "the Applied Mathematics Panel is very much interested in this device and considers that its construction would be justified on general grounds, even if it were not to be used for this particular application."[79] Indeed, as soon as it became operational, Stibitz and the Applied Mathematics Panel began offering it to NDRC and military researchers as a general mathematical machine. They distributed a pamphlet describing the device and its programming, announcing, "NDRC now has a calculator of rather low native intelligence but of indefatigable energy." While limited to reading, writing, storing, and adding numbers, it could be programmed to repeat those operations with mind-numbing repetition.[80] The pamphlet goes on to describe the structure of the machine, which had nine "registers," or locations for storing numbers, two of which could add numbers and store them in a third register. To manipulate these numbers, "A system of designation of the orders to RI has been worked out, and each order is identified by two letters." The command CA, for example, would copy a number from register C to register A — similar to modern computer "assembly language." Other commands could add two registers, input data from the tape, or output to a typewriter.

Hence the Relay Interpolator could do more than prepare trajectories; it could perform on any sequence of numbers. For example, in addition to interpolating points for the dynamic tester, the RI could calculate the "one second" points which were themselves the source of the interpolation. Stibitz's extensive writing on the RI traces his conception of the machine as it evolved from a special purpose trajectory calculator to a general purpose signal processor. He refined his thinking in a series of memos distributed to Division 7 in 1943, including, "Harmonic Analysis on the RI," "Harmonic Analysis as a Smoothing Operation on the RI," "Finding Complex

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[78] GRS to SHC (cc to WW and DJS), June 9, 1943. George Stibitz, "An Application of Relay Interpolator," June 10, 1943. OSRD7 GP, Project #70, Box 50.

[79] Contract Proposal, "Relay Interpolator," August 10, 1943. OSRD7 GP, Project #70, Box 50.

[80] Applied Mathematics Panel, NDRC, "A Statement Concerning the Future Availability of a New Computing Device." AMP Note No. 7, November, 1943. OSRD7 GP, Project #70, Box 50.

Roots of Polynomials on RI," and "The Relay Interpolator as a Differential Analyzer." Properly set up, he argued, the RI could solve not only ordinary differential equations, but also partial differential equations, which the MIT machines could not do. While the RI could not explicitly solve ballistics problems, it could interpolate and improve them.[81] To clarify the difference between the differential analyzer and his own machines, Stibitz coined a new term: "digital," to contrast with the older "analog" techniques.

In the most ambitious of these memoranda, "Unified Theory of the Relay Interpolator," Stibitz connected the RI to the general processing of signals. Interpolation, he recognized, was really a smoothing operation, akin to that in fire control directors. As he later recalled, "interpolation looked just like the 'filtering' that communication engineers applied to noisy telephone signals." The "Unified Theory" paper showed how the RI could simulate electrical filters, electronic oscillators, differential equations, and Fourier analysis. These simulations, in fact, would even face the stability problems of continuous, linear systems: "The data generated by the Computer Model 2 and punched or printed on the output list tape could be made "stable" or "unstable" by choice of the coefficients used in the program given to the computer. Unstable programs would cause the output to oscillate with increasing amplitude, and conversely with stable programs."[82] The Relay Interpolator continued the redefinition of flight profiles as general signals, the computer as the general signal processor.

The Ballistic Computer

Soon after this project had begun, Stibitz proposed yet another machine to automate testing for the Anti-Aircraft Artillery Board (AAAB) at Camp Davis, North Carolina. [*Figure 9-7: AAB data flow] Planes flew simulated bombing runs, and as the directors tracked them, their outputs were logged, as was a visual record of the plane. These data were then transcribed to tapes and fed into the "AAAB Computer" (Model III). Unlike the Dynamic Tester, which compared actual and ideal data, the AAAB Computer actually calculated the ideal response, using firing tables stored on tape. Another "master control tape" programmed the machine with the requisite formulas, then "the operator pushes the start key and leaves the machine to do the rest.

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[81] For all these memoranda, see OSRD7 GP, Project #70, Box 50 Folder Relay Interpolator BTL. For a technical description of the Relay Interpolator see O. Cesareo, "The Relay Interpolator," <u>Bell Laboratories Record</u> 24 (1947) 5-9.

If the machine, which consisted of about 1,300 relays, experienced a failure or a jam, it "stops further computation and sounds an alarm," requesting human intervention into the calculating process. Analyzing a typical set of data took a human computer using a calculating machine forty minutes. The relay machine could do it in about two and a half.[83] Division 7 let a contract to Bell Labs in late 1943 (Project #74); the machine began its first calculation the following May. BTL eventually built several similar machines, one for NACA, one for the Aberdeen Proving Ground, and a derivative, "Model IV," for the Naval Research Lab.[84] While the Relay Interpolator and the "Ballistic Computer," were intended for specific fire control applications, Stibitz and BTL came to offer them as general-purpose computers. Stibitz's Relay Interpolator and Ballistic Computers (Models II, III, and IV) computers remained in service for military applications until 1961.

Stibitz's Digital Philosophy

How did Stibitz obtain and sustain Division 7 support for his extensive computing program while Penn and RCA could not? He certainly had institutional advantages: he himself was a member, the NDRC tended to favor BTL and MIT, and BTL was the site of the leading fire control project in the country, the T-10. Still, politics alone might explain a single, isolated project, but not this expensive and unique series of machines. The source of Stibitz's success lay in his very conception of digital computing, how that conception mapped on the distinction between fundamental and applied research, and how his notion of information maintained a delicate balance of abstraction and physical form.

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[82] Stibitz, "Unified Theory of the Relay Interpolator," OSRD7 GP, Project #70, Box 50 Folder Relay Interpolator BTL. Stibitz, The Zeroth Generation, 181.

[83] George Stibitz, "Outline of Relay Ballistic Computer," July 7, 1943. OSRD7, GP, Project #74, Box 54. Joseph Juley, "The Ballistic Computer," Bell Laboratories Record 24, (1947) 5-9. The "Ballistic Computer" or model II could calculate the errors in one of two ways. First, the machine could replicate the calculations in the director and compute the correct gun orders based on the three inputs, then subtract them from the director's gun orders to find the errors. This method has the advantage of isolating errors in each of the three variables so they can be analyzed separately, Stibitz called this "Class 1" error. Class 2 error, in contrast, takes as input the gun orders produced by the director, and essentially performs the ballistics calculation to determine the point and time at which the shell would explode. The machine then interpolates the position of the target plane at that particular time, and produces a distance by which the shell missed (or hit) the target when it exploded. This technique lumps the errors in all three output variables together, but has the advantage of producing a "miss distance" which quantitatively compares the performance of different directors. Stibitz's AAB computer, or Model III as he called it, could calculate by either of these methods.

[84] George Stibitz, "Progress on AAB Computer," May 13, 1944. OSRD7 GP, Project #74, Box 54. F. L. Alt, "A Bell Telephone Laboratories Computing Machine," M.T.A.C. 3, (1948) 1-13, 69-84, reprinted in Randell, ed., The Origins of Digital Computers, 263-292. See also Stibitz, The Zeroth Generation, Chapter 9, "Planning and Building the 'Ballistic' Computer, Model 3, 1943-44."

For Stibitz digital computing was as much a structural as a mathematical strategy. Physical proximity did not constrain a digital machine like the complex interconnection of gears, differentials, and shafts constrained mechanical analogs. "The electrical computer," he wrote, "was topological, not metric."[85] This "topological" nature of Stibitz's machines separated function from physical form. As in the telephone network, design inhered not in the components (which, after all, were standard telephone relays) but in the wiring between them — Stibitz's digital machines were defined by connection and communications. Shannon's relay algebra allowed him to manipulate and combine digital circuits as network diagrams and mathematical notation.

Both Penn and RCA could design with similar abstraction (no evidence suggests they employed Shannon's relay algebra, but the didn't work with relays). One key difference set BTL apart: the translation from design to structure was simple, unproblematic, and proven. Western Electric's thick Standard Operating Procedures (SOP) manuals specified "how to do almost anything that could be done legitimately in the Bell System." Using telephone relays, Stibitz stuck to the SOPs when laying out his systems. Thus Western Electric could build the machines quickly and reliably, relying on the technical culture of telephone engineering:

> All parts required are in production and are available at short notice in any quantity likely to be required. Construction does not demand highly trained or scarce personnel. Design of the mechanism, once the fundamentals are sketched out for them, *is a familiar and routine matter for telephone machine switching engineers*. This group has not been drained as completely as have most skilled groups.[86]

Any of thousands of Western Electric wiremen could build the machines as they built any telephone switching system,

> The fact that the computers were completely novel devices, and of a kind they had never before constructed, was no deterrent; the wiremen worked at the speed and precision with which they would have done had they been constructing dozens of relay computers in their careers.[87]

Digital processing made data interchangeable, just as Bell's SOPs standardized wiring skill. Rapid, reliable translation of ideas into things not only made for predictable project schedules but also reliable, maintainable hardware. It also fostered architectural innovation: Stibitz built three successive generations of computers in eighteen months (late 1942 to mid-1944) each responding

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[85] Stibitz, <u>The Zeroth Generation</u>, 106.

[86] Stibitz, "Relay Interpolator as a Differential Analyzer," emphasis added.

[87] Stibitz, <u>The Zeroth Generation</u>, 109.

to new problems and building on prior experience. ENIAC, in contrast, took more than two years to build, based on a differential analyzer model from early in the war.

Thus Stibitz owed his success not only to digital computing but to computers based on telephone relays and not vacuum tubes. For him and for Division 7, the difference mapped onto that between system design and component development — the former could contribute to the war effort in short order, the latter represented fundamental research and might not pay off before the war ended. Stibitz summarized his philosophy in 1943: "electronic methods may well be the computing means of the future, but their application at present would present a *research* as contrasted with a *design* problem."[88] Stibitz, Bell Labs, and relay computers thus reinforced the distinctions Weaver had imposed on RCA, and which informed Division 7's rejection of ENIAC.

Digital relay computers, while neatly separating design from construction, did not fully separate machinery from information. Bell Labs machines moved data around ever more interchangeably, stretching the tie to mechanics but never quite breaking it. Computing remained mechanical, information remained a thing — a switch position, a paper tape, a list of numbers. "Speed" of computation translated the physicality, the heaviness of information. Later, electronics would radically separate data from mechanical (but not physical) limitations, as the repeater amplifier had done in the phone system thirty years before. For D-2 and Division 7, however, information remained classical and Newtonian: tied to things, to movement, and to machinery.

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[88] Stibitz, "Relay Interpolator as a Differential Analyzer," emphasis added

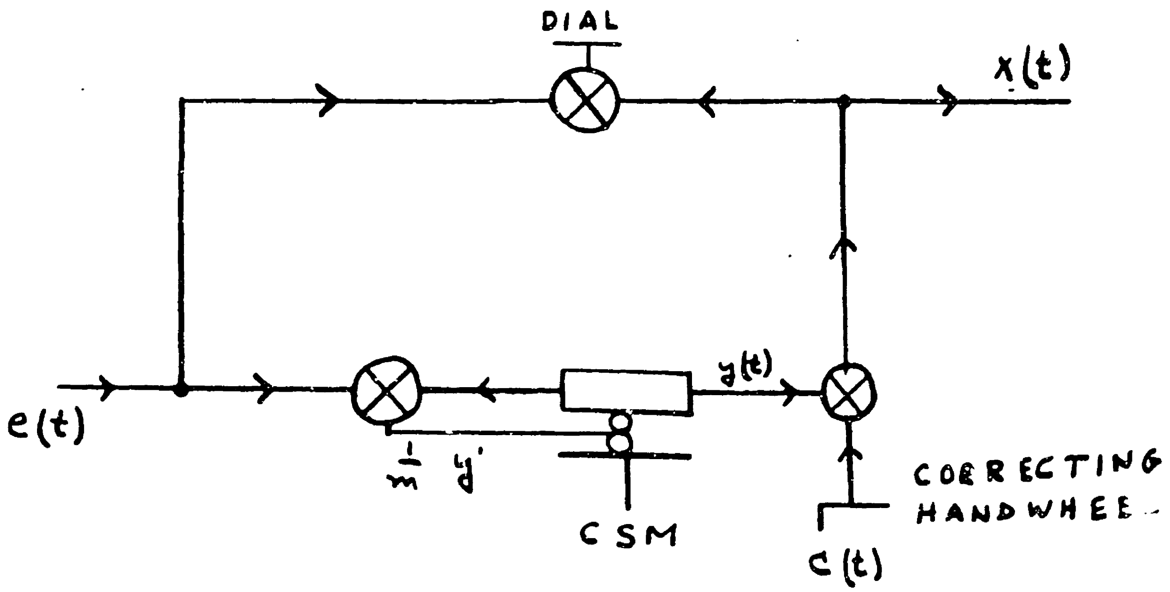


FIG. 1

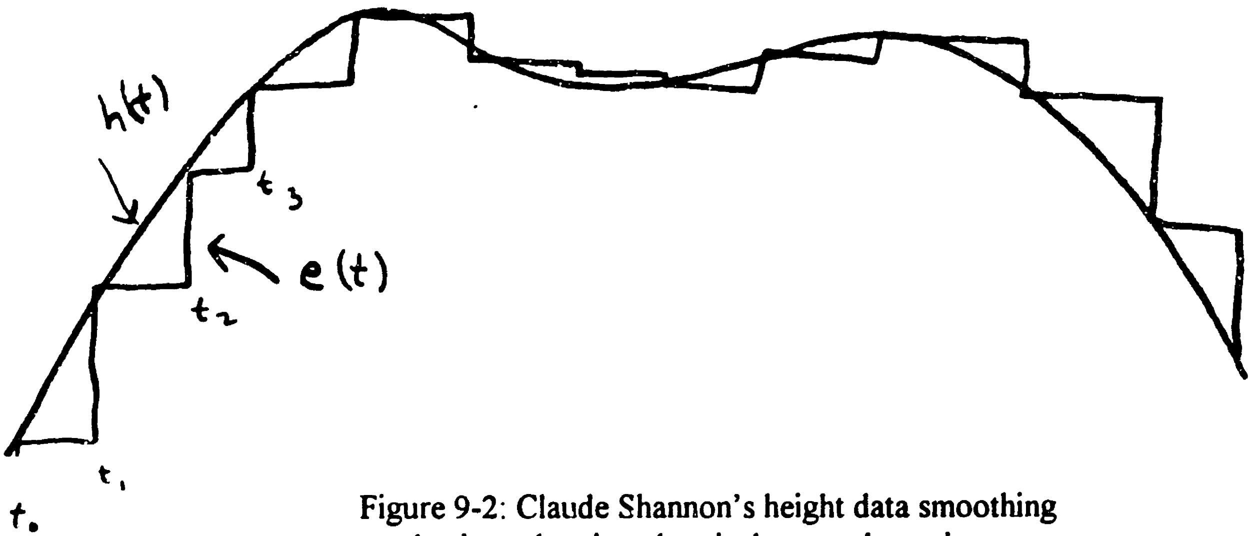
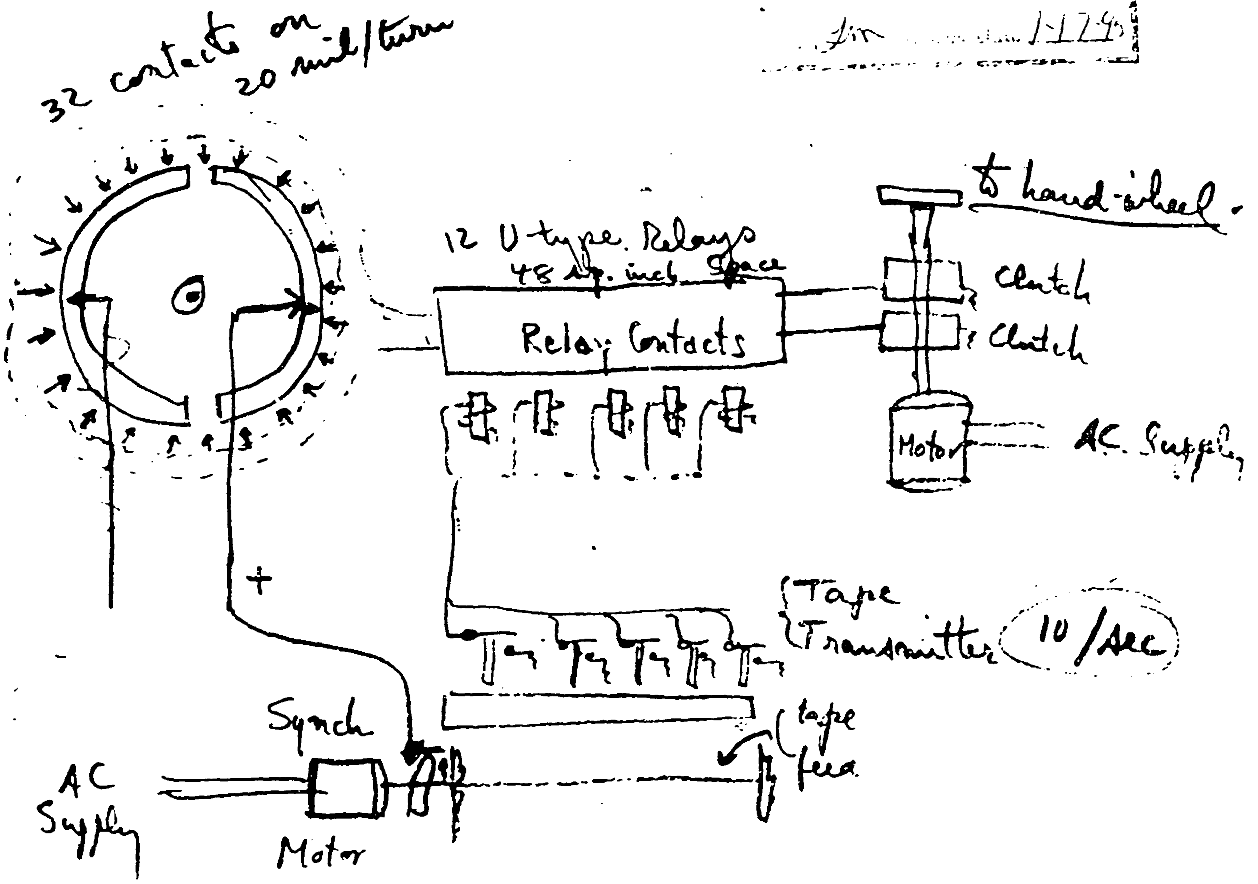


Figure 9-2: Claude Shannon's height data smoothing mechanism, showing electrical-type schematic symbols, feedback loop, and smoothing of jerky tracking data. (From C. E. Shannon, "A Height Data Smoothing Mechanism," May 25, 1941, Shannon papers, MIT Archives).



Dynamic tester.

Tape "Cam."

GRStibitz
Oct 18 1942

Figure 9-3: George Stibitz's Sketch for a "Tape Controlled Servo," October 18, 1942. (From G.R. Stibitz, "Proposed Dynamic Tester," October 19, 1942, OSRD7 GP Box 45, Project #60).

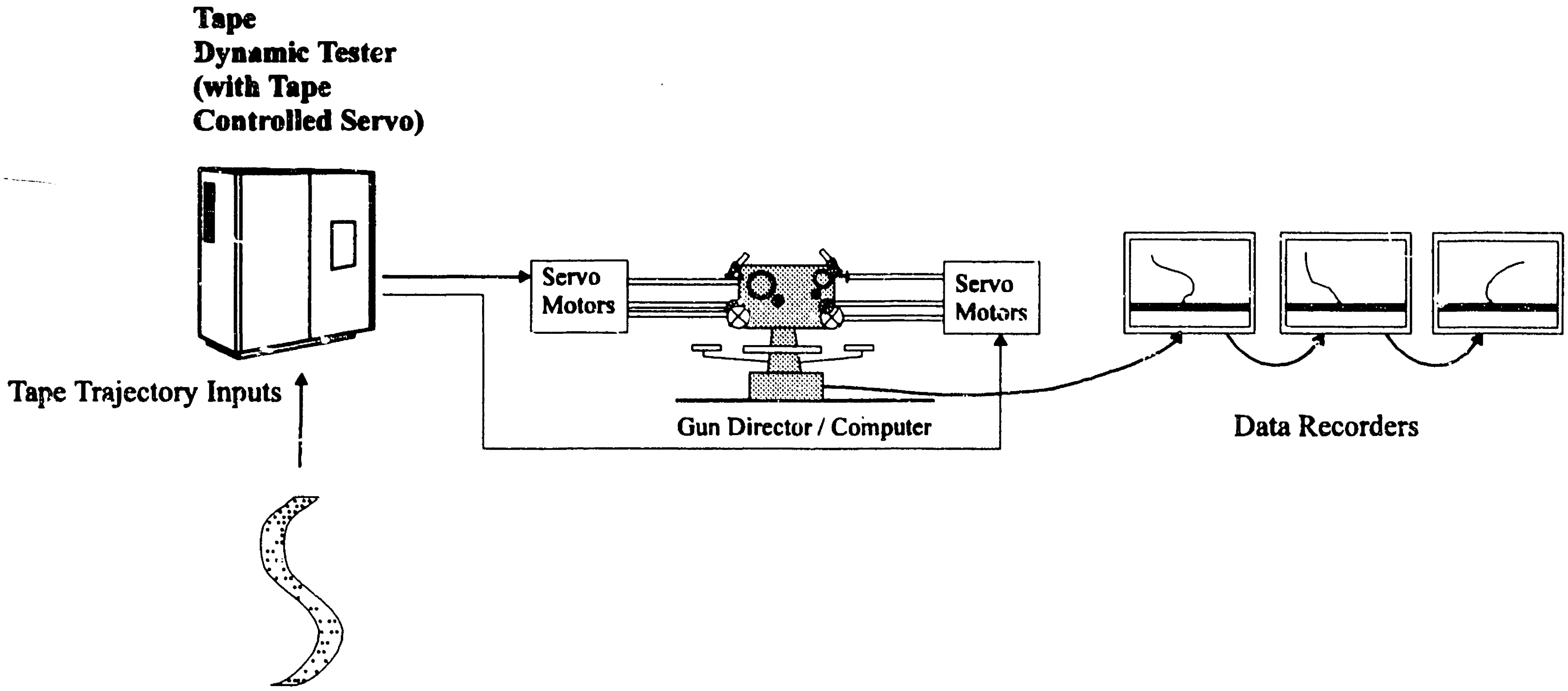


Figure 9-4: Data flow of Tape Dynamic Tester showing digital data driving servos to actuate handwheels on gun director under test.

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CONNECT YOUR
U MEAN  THAT U TGP XX TYPE DOWN THERE AND IT COMES ON OUR
TAPE UP HERE   WE HZXX HAVE NO REPERFORAGXX REPERFORATOR
HOW DO YOU PUT MESSAGES ON TAPE
WE PUT THEM ON THE TAPE RIGHT HERE BUT THAT DOES NOT WORK
FROM THE RECEIVING END




PLEASE PUT FOLLOWING MESSAGE ON TAPE


OK




NNNNGGGRRPIBBDYWAO CR SP LZEARIDSA CR SP LF TGIDW CR SP Z NPDAH  LF
TPDA SP ZGBWHZGBA SP EPY CR ZGDO LF GDO LF GY CR ZPS SP NBOZPW SP GY
HTB CR EIOZIOZIOZIOEB CR TD SP GSLPAEB CR NSLPOED SP RWZBHGWZB SP RA
ED SP POTS LF IHGAEYLI CR GWEDLI CR GWEYLIHRATS LF B SP PONWZDLI CR
GAEY LF BHROTS LF E SP ROTS LE BHRATYY LF I CR GWZD SP RAEDLPOTYLPOED
SP RWZB CR NSLRAZB CR TD SP GSLRW LF PW LF PW LF PW LF RSLGYHTDOZPWLND
CR ZPSHTIW SP TIW SP TPSHZRDALTPYOLTPDA SP ZGIYA SP ZNPDSO SP LF TGIDSA
CR SP LF EARPBDSWAOA SP LL LF ZEETTTNNNN

THIS TAPE GIVES 1000 COS N/2 WHERE N IS IN DEGREES AT THE RATE OF 5
DEGREES PER SECOND.  AXX MZMXXXX MAXIMUM SPEED IS ABOUT 9 POINTS PER
SECOND OR 170 RPM.
```

Figure 9-5: Teletype printout from Bell Labs demonstrating equivalence of messages and data in Relay Computers (OSRD7 GP Box 45, Project #60).

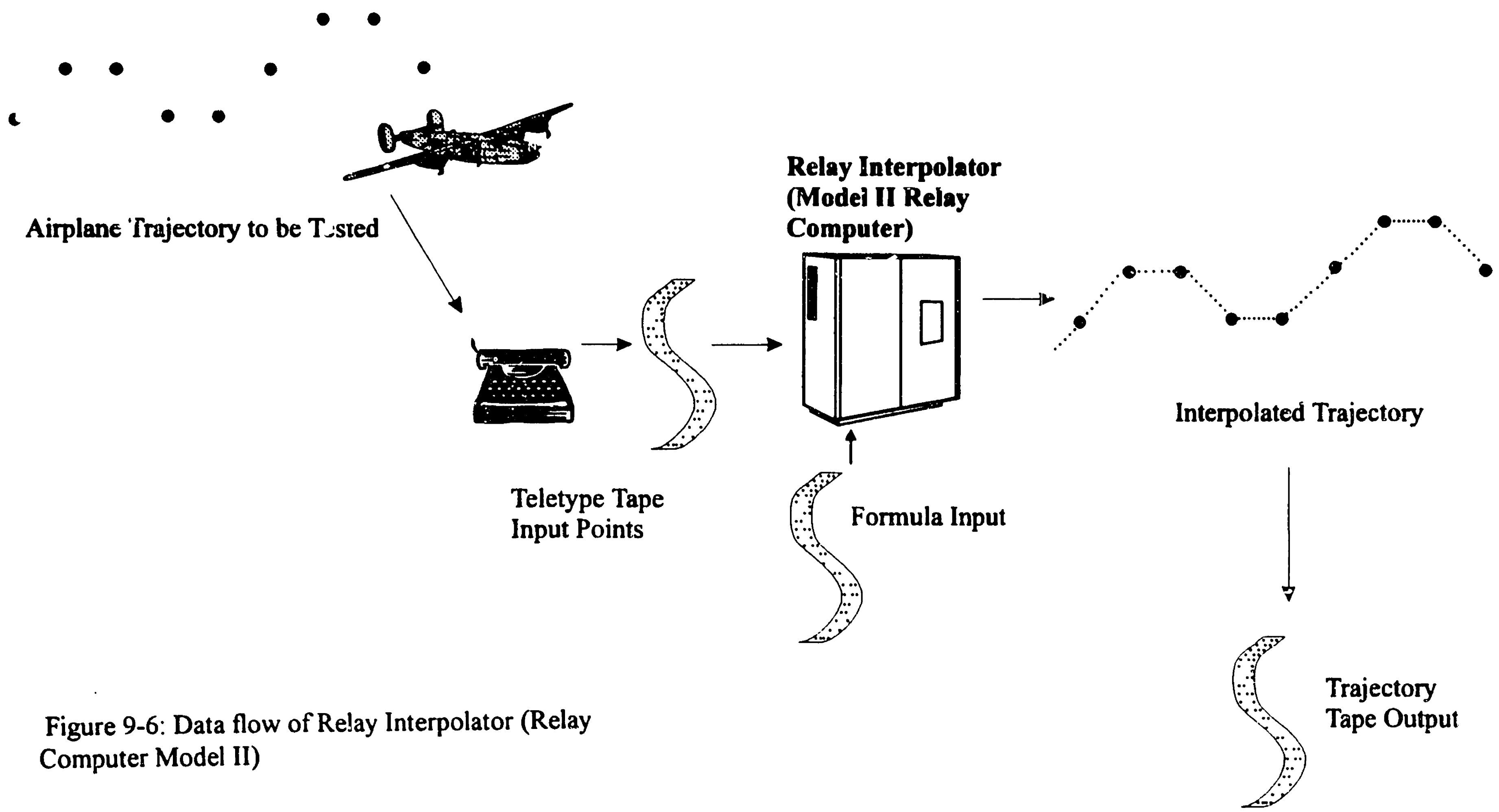
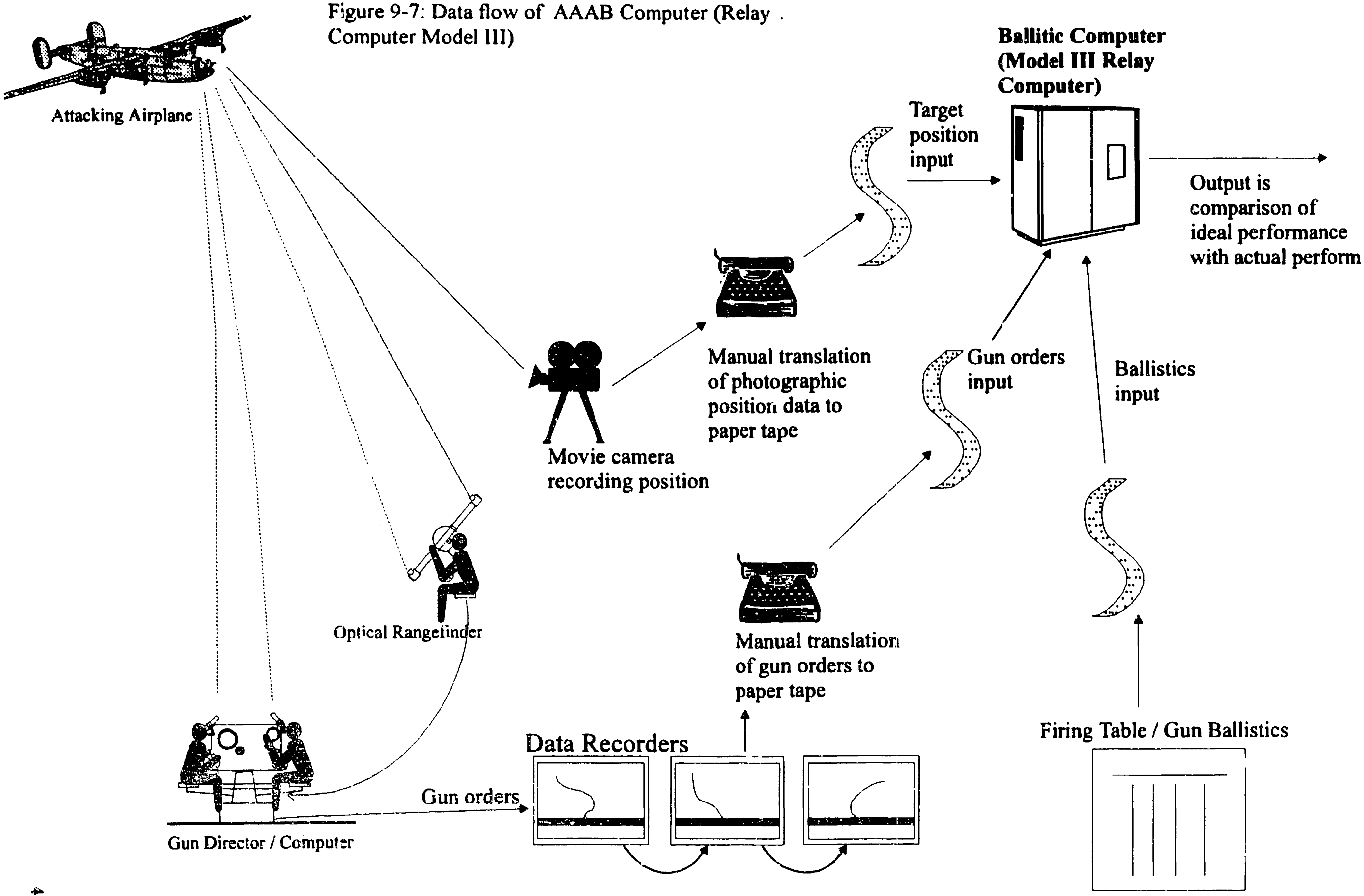


Figure 9-6: Data flow of Relay Interpolator (Relay Computer Model II)

Figure 9-7: Data flow of AAAB Computer (Relay Computer Model III)



Chapter 10

Conclusion

"Datum for its own annihilation:"

Feedback and Information in 1945

They thought in terms of airplanes in space, not totally in terms of symbolic logic.

Robert Wieser on programmers of the Whirlwind Computer[1]

D-2 and Division 7's work on control systems extended the signal, a general, reconfigurable carrier, from communications into control. In electrical engineering these extensions took the form of spectra and Fourier transforms, frequency response servo techniques, statistical approaches to prediction and filtering, and a model of human operators as dynamic signal processors. They congealed in three new quantities: information, the computer, and government-supported basic research. In 1945, however, none had settled on a common definition. Soon after the war each would stabilize, break from its foundations, and mix in new networks.

Division 7 ended on the cusp of those breaks. Its three-volume Summary Technical Report, published in 1946, provides a unique window into the state of control at the close of the war. The first volume, Gunfire Control, edited by Harold Hazen, surveys the NDRC's broad range of projects. Volume Two, Range Finders and Tracking, summarizes extensive work in optics and applied psychology undertaken to support that fire control research. The final volume, Airborne Fire Control, contains three separate essays on aiming controls, aerial torpedo directors, and aerial gunnery written by Division 7 members George Philbrick, Al Ruiz, and John Russell, respectively. The volumes depict what Latour would call the "previous sciences," of feedback, stability, computation, and the human operator, and hence the social context of their immanent transformations into post-war technology.

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[1] C. Robert Wieser, "A Perspective on Sage: Discussion," *Annals of the History of Computing* 5 (no.4, October, 1983), 387.

Synthesis and Control

George Philbrick, for example, worked in Sam Caldwell's Section 7.2 on fire control for airplanes and articulated his own "cybernetic vision," distinct from, but parallel to, Wiener's own. Philbrick introduced the volume with a section, "Feedback in General," describing the basic feedback loop which 'may be entirely automatic in nature, or may contain one or more human elements as an essential connecting link."[2] The idea of tracking, he wrote, extended to human behavior, "people 'track' during every conscious moment...alignment processes, in which the alignment error serves as datum for its own annihilation, are forever being carried out in the familiar operations of living." D-2 and Division 7, with their distinctly behaviorist approach, defined the dimensions of human activity acceptable for machine operators: speed of response and accuracy in tracking. Other factors, including fatigue and emotional stability, were external variables to be excluded. To clarify these boundaries, Philbrick drew an extended analogy between human operators and automatic regulators, including the human nervous system in his model of tracking. His conception of the operator derived from an engineering practice of simulation: building models of systems both for analysis and for training. Philbrick elaborated at length on "simulation as an aid in development," and "the philosophy of models." Engineers used mechanical and electronic representations (like the Network Analyzer) to replicate the behavior of existing systems, but soon began to simulate systems before building them. As an example, Philbrick wrote, "the differential analyzer may be thought of as a synthesizer, or flexible model, as well as an *analyzer*...the equipment under discussion is really a bridge between analysis and synthesis."[3]

As Hazen had done with the Network Analyzer, Philbrick conceptualized a general machine,

> For the increasingly diversified uses to which the simulative methods were being put, requiring new construction or at least major physical rearrangement of components each time, it would be preferable to build an extremely general and flexible assembly, covering every conceivable type of system which could be adapted to any particular problem simply by the manipulation of conveniently provided organizational controls.

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[2] George Philbrick, Summary Technical Report of Division 7, NDRC Volume II: Airborne Fire Control (Washington: Office of Scientific Research and Development, National Defense Research Committee, 1946), 5.

[3] Philbrick, Airborne Fire Control, 24, 48.

Philbrick called such a machine a "*supersimulator,*" but today we would call it a computer, the "manipulation of conveniently provided organizational controls" a kind of programming.[4] Philbrick, firmly ensconced in analog electronics, did not conceptualize the machine as digital.[5] In 1947, he started a company, George Philbrick Researches, to build analog computers and components for industrial process control; the "Philbrick Amplifier," was a popular building block for analog computers after the war.[6] Still, the impetus behind Philbrick's "supersimulator" stood between that driving the Network Analyzer and the general-purpose computer: the unified machine to simulate the entire world.

Philbrick exemplified the multiple "cybernetic visions" nascent in engineering practice. Military control requires a close coupling of people and machinery — a coupling which presses ever closer to instability as it stretches human and mechanical limits. As war became more technological, new weapons became increasingly difficult to test under realistic conditions, and increasingly difficult for operators to handle. Conceptual tools (signals, feedback loops, and frequency spectra) helped engineers understand how to stabilize and optimize these systems. Concrete tools (models, instruments, and simulators) separated a machine's dynamics from its physical embodiment. Thus engineers can experiment with the system during development and operators can experiment with it during training. Simulations tied to a single referent are useful for one purpose only, but a flexible simulator can replicate all systems. "There is, in reality," Philbrick concluded of his virtual machines, "no limit at all."

Philbrick was not alone in his vision for a "supersimulator." The navy's Bureau of Aeronautics had an office devoted to "synthetic training," the Special Devices Division. This group built simulations of combat situations such as bombing or submarine chasing.[7] Its director,

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[4] Philbrick, Airborne Fire Control, 65, emphasis original. As he notes on page 51 of this volume, Philbrick was elaborating ideas he had before the war; he built an industrial process simulator for the Foxboro Company. See Per A. Holst, "George A. Philbrick and Polyphemus: The First Electronic Training Simulator," *Annals of the History of Computing* 4 (no.2, April 1982), esp. 144-45.

[5] George A. Philbrick, "Designing Industrial Controllers by Analog," *Electronics* 21 (no.6, June, 1948), 108-11.

[6] *Electronic Design*, December 16, 1995, p. 8, reprints the original 1955 announcement of the Philbrick K2-X Operational Amplifier. The magazine's retrospective called the device "an industry classic," and notes that a number of today's leading analog electronics designers got their start at Philbrick Researches.

[7] Perry Crawford, interview with author, January 10, 1995 (notes in author's possession). Kent C. Redmond and Thomas M. Smith, Project Whirlwind: The History of A Pioneer Computer (Bedford, Mass.: DEC Press, 1980). In his report, Philbrick mentioned the BuAer/Special Devices Division project, "It is now possible...to experience 'electronic' flight in the laboratory, the whole illusion being accomplished by simulative components..." Airborne

MIT graduate Captain Luis de Florez, had been the navy liaison for Sperry's aircraft instrument projects in the twenties. Special Devices' flight trainers proved among its most useful products, but they needed to be recreated for each airplane. De Florez wanted a generalized flight simulator which he could reconfigure for different types of airplanes, even for airplanes still in design. He wanted, in effect, a "supersimulator," similar to Philbrick's.

To build this machine, in 1944, de Florez and Special Devices went to Bell Laboratories, the leader in electronics and analog computing. The group already had a number of similar projects underway and declined to take on the work. Special Devices then went to MIT's Servo Lab. The project ended up in the hands of Gordon Brown's student Jay Forrester as the "Airplane Stability and Control Analyzer" (ASCA). In 1945, after about a year of work, Forrester grew frustrated with implementing ASCA in analog electronics. Just then Perry Crawford joined Special Devices as the project's supervisor. Crawford, who had proposed digital fire control in his MIT master's thesis, suggested a digital computer. Forrester, inspired by Crawford, by the ENIAC group at Penn, and Howard Aiken's work at Harvard, decided to go digital. Special Devices, increasingly interested in simulating human interfaces in Combat Information Centers, supported the switch. The project developed into Whirlwind, MIT's first electronic digital computer, the first such machine for real-time control. During the 1950s, it spawned the continental air defense system SAGE, a host of institutions and companies (MITRE, Lincoln Labs, Digital Equipment Corporation), and new computing technologies (numerically-controlled machine tools, magnetic core memory, digital modems, graphics displays).[8] [*Figure 10-1]

Other traditions, including scientific computing (Howard Aiken, Jon von Neumann), cryptography (Alan Turing, Colossus), and business processing (IBM, Remington Rand) also

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Fire Control, 63-4. For the typical products of the Special Devices Division, see *CIC Magazine*, 1944-45, World War II Command File, CNO, Naval Operational Archives

[8] See "Special Issue: SAGE," *Annals of the History of Computing* 5 (no. 4, October, 1983) for a number of personal accounts and oral histories of SAGE and related projects. George E. Valley, "How the Sage Development Began," *Annals of the History of Computing* 7 (no. 3, July, 1985), 196-226. Also see Paul Edwards, Closed World: Computers and the Politics of Discourse in Cold War America (Cambridge: MIT Press, 1996), Chapter 3 for a discussion of SAGE as a Cold War icon of technology and its connection to other large-scale command and control systems. Descendants of these systems found their way into modern computer technologies including graphics, networking, and user interfaces. See Arthur Norberg and Judy O'Neill, Promoting Technological Innovation: The IPTO of the Defense Advanced Research Projects Agency (Charles Babbage Institute research manuscript, 1992). For numerical control at the Servo Lab, see David Noble, Forces of Production: A Social History of Industrial Automation (New York: Knopf, 1984) and Francis J. Reintjes, Numerical Control: Making a New Technology (New York: Oxford University Press, 1991).

shaped information technology. ASCA, Whirlwind and SAGE, however, exemplify the particular approach molded by wartime research in control systems and fire control. Engineering practice based on models and simulation drove the search for fast, flexible machines. General, reconfigurable analog simulation machines became immediate predecessors of digital computers. Whirlwind came from a tradition of simulation and control rather than one of mathematics. Forrester and his group, as engineers and not mathematicians, emphasized reliability, human/machine interaction, and connection as much as "speed and accuracy." As one Servo Lab engineer recalled, Whirlwind programmers "thought in terms of airplanes in space, not totally in terms of symbolic logic."[9] SAGE extended the basic antiaircraft fire control problem: tracking targets, smoothing signals, predicting future positions, directing weapons. Through perception, integration, and articulation, Whirlwind and SAGE brought antiaircraft fire control together into the world of digital electronics, information processing, and national systems.

SAGE became the prototype computerized "command and control" system of the Cold War. It spawned a number of derivative systems, including the famous NORAD command center buried deep inside Cheyenne Mountain. These systems contributed a popular image of the control of technology: men sitting at glowing terminals in darkened, air-conditioned rooms, examining representations of the world (computerized maps and radar images), and speaking orders into telephones. This configuration derived from radar, fire control, combat information centers, and the conception of the human operator as a system component. It stood for the series of abstractions which would direct (and perhaps lead to) nuclear Armageddon; it stood for technically-mediated action at a distance; it stood for the margins of technology where political control coupled to large technical systems.

Control rooms provided the stage on which dramas of technology and society were acted out. The mythical "button," ever under the anxious finger of the president, crystallizes the representation in a single, binary figure. The "button" has two functions: to launch the missiles when commanded, not to launch when not commanded. Such singular decisiveness, however, masks the complex and distinctly non-binary nature of command and control systems, which in reality depend on extensive concatenations of radars, computers, telephone lines, and human

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[9] C. Robert Wieser, "A Perspective on Sage: Discussion," *Annals of the History of Computing* 5 (no.4, October, 1983), 387.

operators.[10] The more quickly the system can respond, the more it risks accidental firing. This so-called "hair trigger" phenomenon replicates the stability problem at the intersection of technologies of control and the control of technology.

Philbrick's industrial simulators and Whirlwind/SAGE command and control were just two of several distinct trajectories of control which emerged from World War II. Norbert Wiener's Cybernetics, despite its author's anti-militarism, defined the issues of human/machine interaction for a generation of technologists, military and civilian alike. Wiener's assistant, Julian Bigelow, became chief engineer under John von Neumann at Princeton, building the so-called "IAS Machine," among the first stored-program computers. Ivan Getting, spent the fifties as Vice President of Engineering and Research at Raytheon and became the first president of the non-profit Aerospace Corporation, which grew out of TRW in 1960 to do system engineering for the Air Force. Nathaniel Nichols, Rad Lab alumnus and servomechanisms author, followed Getting to Raytheon and then to Aerospace as head of the company's guidance and control.[11] Bell Labs' fire control group, which included Hendrik Bode and Walter MacNair, built the Nike series of antiaircraft missiles based in part on their wartime work. Bode himself played a prominent role in Cold War scientific advisory committees and finished his career as Gordon MacKay Professor of Systems Engineering at Harvard.[12] Gordon Brown, as Dean of Engineering at MIT, ushered in the "university polarized around science," that defined the institute during the sixties. Perry Crawford left the navy in 1952 for IBM, where he spearheaded SABRE, an adaptation of military command and control systems to automate American Airlines' national ticketing operations. In the navy, the "gun club" became the nuke club: BuOrd chief William Blandy (with aid from Horacio Rivero) directed Operation Crossroads in 1946, when the navy tested its first atomic bombs. In 1947, Edwin Hooper became military advisor to the Atomic Energy Commission.[13] All of the MIT "four horsemen," (Rivero, Hooper, Mustin and Ward) became admirals.

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[10] See, for example, Daniel Ford, The Button: The Pentagon's Command and Control System — Does it Work? (New York: Simon and Schuster, 1985). One of Ford's main points is the dependence (at least until the 1980s) of the Pentagon's command and control systems on standard long-distance telephone lines leased from AT&T.

[11] Ivan Getting, All in a Lifetime: Science in the Defense of Democracy (New York: Vantage Press, 1989). Aerospace Corporation, Aerospace Corporation, its Work: 1960-1980 (El Segundo, Calif.: Aerospace Corporation, 1980).

[12] M.E. Van Valkenburg, "In Memoriam: Hedrik W. Bode, 1905-1982," *IEEE Trans. Automatic Control* AC-29 (no. 3, March, 1984). Also see Bode Papers, Harvard University.

[13] Hooper, Rivero Admiral's Biographies, Naval Operational Archives. Blandy Papers, Library of Congress.

Several textbooks published soon after the war, reflected the NDRC's work and defined control from diverse angles, including the telephone company (MaColl), the Radiation Laboratory (Nichols, James, and Phillips), the process industries (Ahrendt and Taplin), and the Servo Lab (Brown and Campbell). Professional activities followed a similar trend: in 1946 the AIEE established a subcommittee on Servomechanisms and the IRE founded a Feedback Control Systems committee in 1952. Control engineers became increasingly concerned with standardizing language and terminology. "almost every early postwar paper made some reference to a 'new language,' to 'problems with terminology,' to the need to 'translate' the jargon of one or other group."[14] In the fifties, due in part to the proselytizing of former NDRC members such as Brown, Campbell, Warren Weaver, and Louis Ridenour, "automation" became a popular icon for the technological future.[15] Rather than unifying, control systems engineering took different, even diverging paths: cybernetics, systems engineering, automation, process control, inertial guidance, command and control. Even this brief survey of post-war careers, publications, and professionalization in control gives the flavor of its multiple, overlapping legacies. Each elaborated the classical governor's elements of perception, articulation, and integration.

Of these multiple paths, few attained the currency, both in engineering methodology and in popular discourse, of Claude Shannon's theory of information, proposed in his 1948 paper, "A Mathematical Theory of Communication."[16] Shannon defined the act of communication as transferring a given message, or a series of symbols, from one place to another (one person to another or one machine to another), with some additional noise in the channel. Echoing Hartley, Shannon used information to measure freedom of choice in selecting a message: if only one

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[14] Chris C. Bissel, "Spreading the word: aspects of the evolution of the language of the measurement of control," *Measurement and Control* 27 (June, 1994), 154, Hubert M. James, Nathaniel B. Nichols, and Ralph S. Phillips Theory of Servomechanisms (New York: McGraw Hill, 1947), Gordon S. Brown and Donald P. Campbell, Principles of Servomechanisms (New York: Wiley, 1948), Leroy MacColl, Fundamental Theory of Servomechanisms (New York: Van Nostrand, 1945), William R. Ahrendt and John Taplin, Automatic Feedback Control (New York: McGraw Hill, 1951). See Chris C. Bissel, "Textbooks and Subtexts: A sideways look at the post-war control engineering textbooks, which appeared half a century ago," *IEEE Control Systems* 16 (no. 2, April, 1996), 71-8. Stuart Bennett, "The Emergence of a Discipline: Automatic Control, 1940-1960," Automatica 12 (1976), 113-121.
[15] See, for example, the Scientific American book Automatic Control (New York: Simon and Schuster, 1948) with articles by Brown, Campbell, William Pease, Warren Weaver, and Louis Ridenour in addition to Arnold Tustin and Wassily Leontief.
[16] Claude Shannon, "A Mathematical Theory of Communication," *BSTJ* 27 (July-October, 1048), 379-423, 623-656, reprinted in N.J.A. Sloane and Aaron D. Wyner, ed., Claude Elwood Shannon: Collected Papers (New York:

message is possible, no information is transmitted; if ten messages are possible, more information is transmitted, still more if a hundred are possible, even though the same message might be sent. Information is measured as the logarithm of the number of choices, related to the thermodynamic measure of entropy.

Shannon built on his own experience in fire control, computing, and cryptography as well as on Nyquist and Hartley's ideas, from twenty years before at Bell Labs. Shannon provided provide a measure of channel capacity, in bits per second, which describes the maximum amount of information possible to send down a given channel. He added a serious consideration of noise and a statistical approach to the problem. "Communication theory is heavily indebted to Wiener for much of its basic philosophy and theory," Shannon wrote, citing Wiener's NDRC report.[17] Shannon's measure leads to a theory of efficient coding, how to optimally translate a series of "primary symbols," such as English text, into "secondary code" to be transmitted, such as Morse code or ASCII, "It is possible to send information at the rate C through the channel *with as small a frequency of errors or equivocation as desired* by proper encoding."[18] Maximum use of an available channel capacity depends on optimal coding, a translation which reduces redundancy in the message (normal English already has about twenty percent redundancy, Morse code about fifteen percent). Redundancy, however, can help compensate for the presence of noise in the channel, which disrupts the message and effectively reduces the channel's capacity (English is still readable with about twenty percent erroneous characters). As if to solidify the connection between Shannon's theory and fire control, Louis Ridenour (who had directed Ivan Getting at the Rad Lab) asked Warren Weaver to write a popular introduction and explication of information theory, published with Shannon's paper in a small book.[19]

IEEE Press, 1993), 5-83. Claude Shannon and Warren Weaver, The Mathematical Theory of Communication (Urbana and Chicago: Univeristy of Illinois Press, 1949).

[17] Shannon, "A Mathematical Theory of Communication," 53n. The relationship between Shannon and Wiener's work is more complex than alluded to here. In a later interview, Shannon related "I don't think Wiener had much to do with information theory. He wasn't a big influence on my ideas there [at MIT], though I once took a course from him." Shannon, Collected Papers, xix. Semantic confusion sometimes exists over the "Weaver-Shannon" or the "Wiener-Shannon," theory of communication. The former derives from the book listed in the previous note, and is inaccurate because Weaver served only to translate Shannon's work to make it more accessible (Weaver claimed no more).

[18] Shannon, "A Mathematical Theory of Communication," 36.

[19] Shannon and Weaver, The Mathematical Theory of Communication.

Today, "information," implies a discrete category, something independent, infinitely mobile, and prior to the networks it inhabits. But whence this independence and this mobility? The very idea of an "equivalent quantity" which we today call information results from a historical process. That process intimately involved the conditions of creating, coding, and sending signals through the network. Indeed the idea of information developed in parallel with the networks through which it travels.

Information exemplifies what Bruno Latour calls an "immutable mobile," that is, a representation of the world which retains its internal integrity, accuracy, and authority through a series of manipulations and translations in a variety of different networks. Given these two qualities, immutability and mobility, objects can be presented, read, recombined, shifted, inverted in a dizzying variety of ways without changing their basic structure. A historical view of information, then, lays bare the work of producing this abstraction; it exposes the labor behind universal coding. It is a story of representation, of making machines to manipulate analogs, symbols, and simulations (the Ford Rangekeeper, the Network and Differential Analyzers, the PPI radar display). It is a tale of struggle, often lost, against the tenacious stickiness of representations to the physical world. The "freedom" of information in our networks today followed a strenuous historical fracture of the bond between signs and referents.

In machinery, that fracture owes to digital processing. A determinist view of technology naturalizes the transition from analog to digital in the 1940s as an instant transformation due to the obvious superiority of the speed and accuracy of digital techniques over their analog predecessors. Speed and accuracy, however, the two primary arguments for digital computing, map directly onto mobility and immutability; they result not from a natural dichotomy but from a conscious abstraction. Speed stands for the lightness of information in a machine — the "heavier" it is, the more energy it takes to move it, and the slower it moves. Electronic computers have speed because the vacuum tube divorced data from its mechanical weight. Accuracy stands for the freedom from decay as signals are manipulated and transmitted — digital signals retain their integrity as they travel through networks. Digital computers maintain accuracy because they manipulate and transmit data without decay, much as Harold Hazen's original servomechanisms renewed signals between stages in the differential analyzer, or as Sperry's "human servomechanisms," filtered data and fed it back to the machine. Proponents of electronic digital computing repeated the dual mantra, "speed and accuracy." Later, the two would combine to describe "power" in computers.

Communications engineers, in their efforts to translate between the analog world and its coded representations, long dealt with the dichotomy of analog and digital signals. Claude Shannon himself defined the boundaries of this translation with his early work on digital switching. His theory of information similarly charted the boundary between analog and digital, between continuous and discrete: it determined how well, and under what conditions, a digital message can survive in the noisy, chaotic, analog world.

Understanding control, computers, and information as historically defined categories, then, and narrating their definition as labors of abstraction, counters an "instant and obvious" view of the transition from analog to digital, and hence of the emergence of the modern computer. Rather, it shows a subtle and lengthy evolution from machinery to information, a progressive stretching of the tie between signifier and signified. Telephones transmitted a continuous simulacra of the speaker's voice. Analog computers directly simulated the world in the laboratory. Differential Analyzers affected "continuous" integration. Digital computing before electronics represented numbers with things. Feedback loops always remained within 180° of their own referents. Only a believable immutable mobile could break these signifying links — *information* could only set be adrift from its mooring in mechanics when it carried a credible portrait of the shore.

1948 was a critical year for congealing the intellectual products of the war. Textbooks, information theory, and cybernetics (not to mention Orwell's 1984) helped break the moorings tying technical signifiers to the mechanical world. In 1945, however, when the war ended and the NDRC closed down, the connection remained intact, if stretched. A special essay in Volume I of Division 7's Summary Technical Report exemplified these limits of the modern. "Data Smoothing and Prediction in Fire-Control Systems," by Richard B. Blackman, Hendrik Bode, and Claude Shannon, formally integrated communications and control and pointed toward generality in signal processing. The authors treated fire control as "a special case of the transmission, manipulation, and utilization of intelligence." They assessed control as a problem in electrical communications, developing analogs to the prediction problem, "couched entirely in electrical language." The authors, like Wiener, recognized the broad applicability of their study, "The input data...are thought of as constituting a series in time similar to weather records, stock market prices, production statistics, and the like."[20] Acknowledging the importance of Wiener's work, Blackman, Bode, and Shannon devoted significant effort to summarizing his statistical approach. Ultimately they rejected it, however, due to problems applying the RMS error criterion to fire control, as well as its assumptions about statistical behavior of human pilots. Instead, the paper formulated the problem as one of communications engineering, drawing heavily on Bode's work in feedback control: "there is an obvious analogy between the problem of smoothing the data to eliminate or reduce the effect of tracking errors and the problem of separating a signal from interfering noise in communications systems." Hence tracking is a filtering problem,

> The spectrum of the "signal," or true [flight] path is concentrated principally in a low-frequency band, in most instances, while the energy of tracking errors or "noise," appears principally at higher frequencies. Thus the two can be separated by a low-pass filter.

While noting "this analogy...must of course not be carried too far," the paper considered inputs and disturbances in fire control systems as signals in the frequency domain. After a detailed comparison of various smoothing methods, the paper closed with several examples; smoothing circuits for the M-9 and T-15 gun directors, which employed electric motors to attenuate perturbations in tracking.

The Blackman, Bode, and Shannon article illustrates how control engineering in World War II began to combine feedback with notions of generalized machines and digital processing. Still, by the end of World War II, speed and accuracy had acquired only partial authority. Data stretched its tie to mechanics, but the tie had not yet broken. Norbert Wiener's statistical treatment of signals proposed general methods of processing number series, but wartime research could not realize the fundamental research in practical hardware. Philbrick's analog supersimulator was speedy but not accurate. Stibitz's digital relay computers were accurate but not speedy. Jay Forrester rejected a general, analog flight simulator, neither speedy nor accurate. Penn's vision of a speedy and accurate machine did not yet have institutional authority. Blackman, Bode, and Shannon, while recognizing the generality of smoothing and prediction, tied it to familiar technologies of feedback amplifiers and servos, not to the new computers.

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[20] R.B. Blackman, H.W. Bode, and C.E. Shannon, "Data Smoothing and Prediction in Fire-Control Systems," in Harold Hazen, Summary Technical Report of Division 7, NDRC Volume I: Gunfire Control (Washington: Office of Scientific Research and Development, National Defense Research Committee, 1946). Also see H.W. Bode and C.E. Shannon, "A Simplified Derivation of Linear Least Square Smoothing and Prediction Theory," *Proc. I.R.E.* 38 (April, 1950) 425, which addresses Wiener's prediction in more detail. Also see R.B. Blackman, Linear Data-

Technology was turning from machinery to information, but the turn was incomplete when the war ended. Still, a generation of engineers had learned to think of electricity as signals and to think of machines as systems. They also learned the delicate place of the "fundamental" in federally supported research. They taught themselves to see action in the world, by humans, machines, or systems, as a recursive series of perception (telescopes, radars, sensors, gyroscopes), articulation (servos, plotters, telephones, keyboards), and integration (switches, integrators, computers, human operators).

Research in control systems developed the tools which directed the technologies of nuclear confrontation. Had the Cold War turned out differently, the history of control during World War II would be a precursor to holocaust, akin to Nazi eugenics. Close as the world came, however, it escaped the ultimate instability of a technical system: a nuclear transient (such as an accidental launch or a false warning) initiating mutual destruction. Do we owe this success to control systems, reining in the unstable war machine? Or does the promise of control fuel the obsessive drive for technological power? As we enter a period defined more by distributed information than by military command and control, these historical questions frame an anxious paradox. Sitting at a personal computer, we experience our most powerful and intimate relationship with a machine: the thrill of control, extending our powers. In that very moment, however, we sense an abstract and impersonal force: the specter of technology, threatening instability.

Smoothing and Prediction in Theory and Practice (Reading, Mass., Addison-Wesley, 1965), an extension of the 1948 work.

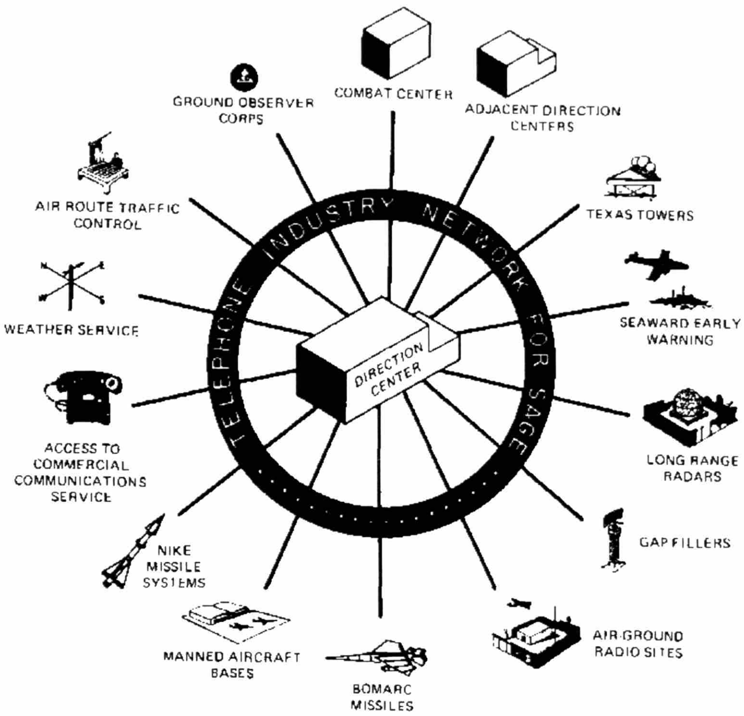

Figure 10-1: SAGE computerized air defense system. Note the role of the telephone network in the diagram above. Below, operators use "light guns" to designate targets on a computer-driven radar screen (Claude Baum, The System Builders: The Story of SDC (Santa Monica, California: System Development Corporation, 1981)).