

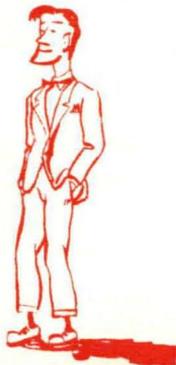
THE SAGE AIR DEFENSE SYSTEM

• A Personal History

Jacobs

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SAGE
AIR DEFENSE SYSTEM

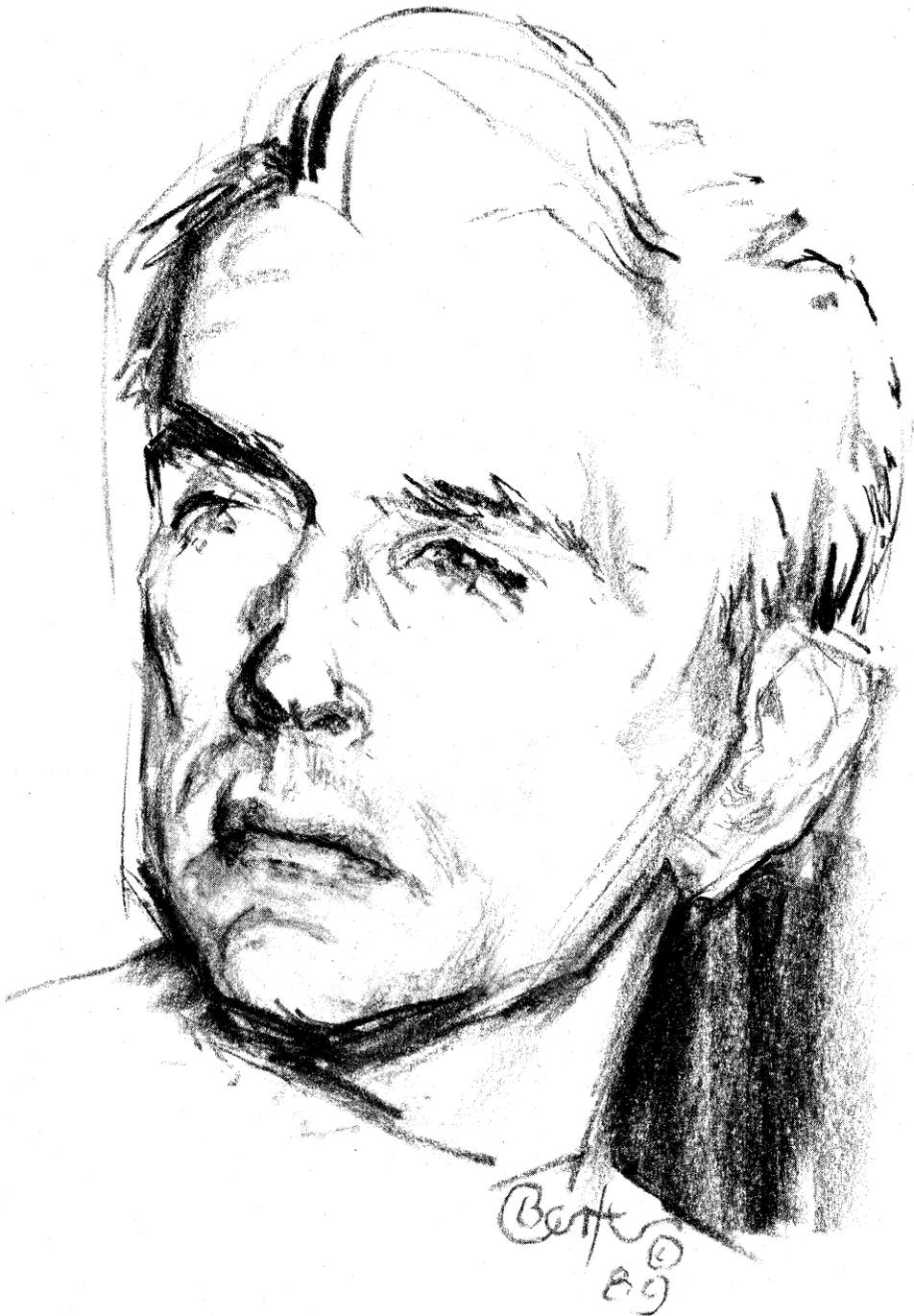
A
Personal
History



MITRE

John F. Jacobs

THE SAGE AIR DEFENSE SYSTEM
A PERSONAL HISTORY



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A PERSONAL HISTORY

JOHN F. JACOBS

The MITRE Corporation
Bedford, Massachusetts

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Portrait of John F. Jacobs by Robert Berks

To Mary

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FOREWORD

SAGE was a remarkable development that had profound effects on the development of computers, information systems, and military capability. Many capable people were involved in its creation, in the Air Force, at MIT, and in industry, and a number of articles have been written about the project and a few of its leaders. This memoir is the first inside story of the project and how it looked and felt to those who were involved. Jack Jacobs is one of the great participants in the SAGE program; his contributions, largely unrecognized outside those who worked with him, were fundamental to success. I have known Jack for 35 years now. He is a modest man but a first-rate engineer and manager, and a major contributor to the art of large information systems. His memoir tells us much about SAGE, about how it was to work within a large pioneering development, and about Jack himself.

I was at the MIT Lincoln Laboratory in the early 1950s as an associate division head, working on the design of the SAGE computer. I had transferred to Lincoln with the Digital Computer Laboratory. The division was growing rapidly; we had moved to another larger building, and I found that the closely knit old group, where everyone knew everyone else, was becoming a memory.

One day, a tall, good-looking young fellow approached me with a question. He introduced himself as John F. Jacobs, our newly acquired graduate student assigned to the logical design of the adder in the computer's arithmetic element. He knew I had done the logical design of the Whirlwind computer and wanted to know why Whirlwind's adder design wasn't satisfactory. As I recall the occasion, I gave him a brief and somewhat lofty lecture on adders, told him the Whirlwind adder was really quite satisfactory, but suggested he make a new review of the possibilities. He looked at me with a mixture of curiosity and courtesy and disappeared. I thought I had handled it pretty well, and that he would review the alternatives and agree that mine was best. Imagine my surprise when, some time later, I found out he had come up with an improved design. This first contact with Jake, as we all called him, turned out to be typical. He was curious and courteous, did his homework, and came up with an

improved design. Neither of us realized at the time that this first meeting was the beginning of a long and effective partnership.

As Jake describes at greater length in this memoir, we came to know each other well while working with IBM in Poughkeepsie, New York. In this short time Jake had become one of the senior MIT design engineers. IBM had the job of engineering design and manufacture of the SAGE computer under Lincoln's overall technical direction. "Direction" meant traveling to Poughkeepsie every week and arguing out the design with the IBM engineers. Public transportation was inconvenient and we usually drove, a 4½-hour trip. This meant leaving Boston at 5:00 in the morning and starting work at IBM at about 9:30. A day or two later we would leave Poughkeepsie at about 6:00 P.M., arriving home around 11:00. Over the course of a few months, we all had a chance to ride with all members of the group. After exhausting the technical discussions, we naturally turned to each other's personal lives and backgrounds.

Jake was especially adept at telling stories of his early life in North Dakota. The snow, the grain elevators, and the poached deer all came alive to us in the dark car in the Connecticut hills. My clearest recollection, however, is of his tales of his education at "Meat Boners" college. I almost came to believe there was such a place and that the people of his hometown really did go down to the station to see him off, the local boy setting forth to make good in the big world.

We took turns driving, and Jake's turn always raised my anxiety the most. We would be moving fast, and ahead would be a stoplight with a line of cars. It was absolutely inevitable that we had to stop, but Jake would keep speeding on, talking all the while, and at the last possible moment, he would cram on the brakes and come to a shuddering stop. He maintained that he had learned to drive this way in North Dakota where there were few cars and no stoplights, and where coming to a stop was considered an emergency procedure. His part of North Dakota was laid out in sections and he claimed that he had trouble with turns because he hadn't learned to make them until he left home. That may be true; but as far as I know, he never had an accident.

Not long after this, we began to cope with the problems of being a relatively small group at Lincoln without legal authority, trying to maintain technical control of the very large SAGE program involving many government agencies and many contractors. The usual approach to such a problem is authoritarian: decide what to do, tell everybody to do it,

and make sure that they do. It wasn't at all clear that we wanted to run things that way, and it wasn't at all clear that Lincoln could make its orders stick if we tried. There were too many others and too few of us. Jake came up with the idea of the Systems Office, whose job it was to find and foresee problems and to get the large masses of people involved to understand and agree on appropriate action. Jake's view was to use leadership. He would analyze the problems, propose solutions, and get everybody together to agree. "Thousand-man meetings," they were called. They worked because everyone wanted the problems solved and no one (besides us) wanted to take responsibility for the whole system. Success was dependent on sound and thorough homework, however, and this is where Jake excelled. His willingness and his ability to do whatever was necessary to make a program come out right were his most distinguishing characteristics and among the chief reasons he played such a major role in the design of SAGE and in the creation of The MITRE Corporation.

Once the SAGE hardware was well under way, Jake assumed direction of the software part of the job. The specification and preparation of the necessary computer programs turned out to be an enormous undertaking. The Lincoln group of less than one hundred, which had been preparing operational, test, and support programs for the prototype, was augmented by a new organization, System Development Corporation, with thousands of new and untrained people. Overnight, the Lincoln people became supervisors and Jake's skills were needed to make the new situation work. He dove in with his usual combination of friendliness, firmness and hard work. I used to go to his organizational meetings from time to time and noticed that the charts on his blackboard were filled with misspelled names. He maintained that he had never learned to spell, but I felt no one could be that bad and accused him of deliberately misspelling in order to cover up his real mistakes. I now think I was right but for the wrong reason. He had many new people to deal with and was bound to misspell some names. But, he deliberately misspelled many, not to cover up for his mistakes but so that no one would feel too unimportant to be spelled correctly. He was concerned about them, not about himself.

Throughout our careers at both Lincoln and MITRE, we were called upon to work with many Air Force officers at all levels. In such situations there is always tension between the need for independence, which is necessary to maintain quality and effectiveness in the long run, and the need for responsiveness, which is necessary to maintain

effectiveness in the short run. Jake spent a great deal of time making sure that there was the complete understanding required between the two groups to make the system work and work smoothly. He genuinely liked and respected the Air Force officers, and they in turn liked and respected him. He made many lasting friendships while assuring that the job went forward with a minimum of conflict.

The first SAGE center went operational on July 1, 1958. Later that year, The MITRE Corporation was formed to carry on the system engineering for SAGE and, as it turned out, for many other systems as well. By 1958, I was head of the SAGE design division at Lincoln and Jake was my associate. Together we organized MITRE, talked our associates into transferring from Lincoln to MITRE, and began the long task of building MITRE into an effective, first-rate professional organization. MITRE is another story, but Jake and I worked together as a team until Parkinson's Disease forced his early retirement some ten years ago. Throughout our long association, his courage, his intelligence, his concern for me, for the job, for the organization, for everyone, have never failed. Our friendship is one of the great pleasures of my life.

Jake is still telling stories, from Meat Boners to SAGE; from fancy to real accomplishment. I think you will enjoy this memoir of a great experience.

Robert R. Everett

PREFACE

This is my story of the development of the SAGE (Semi-Automatic Ground Environment) air defense program. At the time of its operational deployment beginning in 1958, the SAGE system was the first military program to utilize a large-scale, real-time digital control computer supporting a major military mission. The development of the system was initiated at a time when the perception among Department of Defense (DOD) officials was that Soviet bombers carrying nuclear bombs were a primary threat to the United States. The generally held belief in the validity of this threat gave the SAGE program the highest DOD priority, and the DOD was willing to cover whatever costs were necessary to counter the threat. The SAGE design, including its architecture, components and computer programs, drew on R&D efforts throughout the United States, but it drew mostly on work being done at MIT on Project Whirlwind, at the Air Force Cambridge Research Laboratory, at IBM, AT&T, the Burroughs Corporation, and the RAND Corporation.

These memoirs were designed to describe how it was to be a part of this large and complicated program. They cover the period from the late 1940s to the early 1960s, which includes the time taken up by the conception, design, development, manufacture and installation of SAGE. They were written to complement a documented history of the SAGE program, being prepared by historians Kent C. Redmond and Thomas M. Smith.

By the time I became involved in SAGE, much of the conceptual and political framework for the project had already been settled. In the following pages, I will include enough of the background and chronology of the project to place my role in context. I was a middle-level manager caught up in events which were beyond my control, and yet I appeared to have as much control as anyone else. My contribution was mainly in the area of effecting a rational management control of the design of that system. As were most of those who participated, I was closer to some projects and people than to others, and, as a consequence, those are the people and projects I have emphasized here. We decided to add pictures of some of the people who played major roles in the SAGE job and of those who are mentioned in this book. The choice of photographs was based mainly on their availability, and by no means attempts to represent all or

even most of the important people, but rather, illustrates the diversity of the participants' backgrounds, organizational positions, and experience. I don't claim that everything reported in this book was "reality." To some extent, my own memories are colored by the memories of others who have discussed these events with me, and the repetitions have created a modified picture of my reality. This, then, is my memory of the events at the time they occurred, and of the people as I saw them then.

Giving credit to all those who assisted in the preparation of this volume would require the better part of these pages, but I'll try to mention those who assisted me throughout the course of its preparation. Most prominent among these people was Louise Meyer, an editor for MITRE. Louise managed the integration of the various parts of the piece as it was developed over the course of several years. Her insight and good judgment have materially improved on what I had originally prepared. In addition, Charlotte and Gerry Klein volunteered their services as readers and assisted in the critical review of the chapters as they were developed. In the historical research required to verify the content and chronology of events, Louise Sullivan, Edward Galvin and David Baldwin of MITRE Archives spent a large fraction of their time in support of the entire project. And from the beginning, I was supported by the Word Processing Center's Fran Jonuskis, with help from Bobbie Statkus. Fran made it possible for me to have several reviews of the text as it progressed. Many people read the piece for content and offered criticism, ideas, anecdotes and support. Their time spent considering the manuscript and their comments and suggestions were appreciated. But this project could not have progressed at all if not for Bob Everett and Charlie Zrakat, who provided me with encouragement and with access to MITRE services.

A SAGE CHRONOLOGY*

1949

- Aug. Russians detonate atomic device.
- Nov. George E. Valley, MIT, proposes to Theodor von Karman, chairman, Air Force Scientific Advisory Board, that a study of air defense requirements be undertaken.
- Dec. Air Defense Systems Engineering Committee (ADSEC) is established, with Valley as chairman.

1950

- Sep. First MIT experiments transmitting digitized data from Microwave Early Warning (MEW) radar at Hanscom Field (Bedford, Mass.) to Whirlwind computer in Cambridge, Mass., over commercial telephone lines.
- Oct. ADSEC's final report is issued, defining the air defense system that will become known as SAGE.
- Dec. Gen. Hoyt S. Vandenberg, Air Force Chief of Staff, asks MIT to establish and administer an air defense laboratory, and to perform an intensive investigation of the air defense problem.

1951

- Jan. Air Force contracts with Bell Telephone Laboratories to improve existing ground-radar-based air defense system.
- Jan. Air Force contracts with University of Michigan to expand ballistic missile program into a system for air defense.
- Feb. "Project Charles" established at MIT for short-term investigation of air defense problem.

*Previously published by AFIPS Press in an article by the author entitled "SAGE Overview," appearing in *Annals of the History of Computing*, Vol. 5, No. 4, October 1983, pp. 328-329.

- Apr. First live demonstration of automatic aircraft interception using Whirlwind computer and MEW radar.
- Jul. "Project Lincoln" established at MIT as laboratory for air defense — original charter for MIT Lincoln Laboratory.
- Aug. Air Force Air Research and Development Command (ARDC) assumes responsibility for administration of Project Lincoln.
- Oct. MIT's Whirlwind staff at the Digital Computer Laboratory joins Project Lincoln as Division 6.

1952

- Feb. Secretary of the Air Force T.K. Finletter assigns top priority to air defense matters; promises MIT whatever funding required.
- Apr. Name "Project Lincoln" changed to "Lincoln Laboratory."
- May. Memory Test Computer (MTC) under design.
- Jun. Plans for "Cape Cod System" published — scaled-down simulation of nationwide SAGE system.
- Jul. Lincoln considering several manufacturers for production of air defense computer.
- Oct. IBM awarded subcontract by Lincoln to study computer project; Division 6-IBM engineering collaboration under way.

1953

- Jan. Lincoln publishes Technical Memorandum No. 20 — a proposed air defense system called "Lincoln Transition System."
- Jan. First Division 6-IBM technical meeting, Hartford, Conn.
- Mar. Lincoln publishes report, "Cape Cod System and Demonstration."
- May. ARDC decides to pursue Lincoln Transition System and phase out University of Michigan system.
- Jun.-Jul. Division 6-IBM "Project Grind" meetings.
- Summer. Division 6 staff moves from MIT in Cambridge to Lincoln Laboratory in Lexington.
- Aug. First bank of core storage wired into Whirlwind after MTC tests succeed.

Sep. IBM receives contract to produce two single-computer prototypes: the XD-1 and XD-2.

Sep. Cape Cod System fully operational.

1954

Nov. Decision made to have duplex computer system.

Dec. Cape Cod System tracks 48 aircraft.

Feb. First production contract for SAGE computer — called the AN/FSQ-7 — awarded to IBM.

May Air Materiel Command establishes Air Defense Engineering Services (ADES) at Wright-Patterson Air Force Base for acquisition of the Lincoln Transition System. Western Electric becomes involved in ADES management.

Jul. Lincoln Transition System is renamed SAGE — Semi-Automatic Ground Environment.

Sep. ADES moves to New York City and acquires representatives from ARDC, ADC, and AMC.

1955

Mar. “Red Book” operational plan is published — complete definition of SAGE.

Apr. ADES becomes part of newly formed Electronic Defense Systems Division.

Jun.–Jul. Simplex version of AN/FSQ-7 (XD-1) installed at Lincoln by IBM.

Dec. System Development Division emerges from RAND Corporation.

1956

Feb. Development of TX-0 announced — experimental transistorized computer.

Apr. Lincoln urges Air Force to find agency to manage integration of weapons with SAGE system.

Jun. IBM’s first production FSQ-7 system accepted in manufacturing test cell.

- Sep. Air Force asks Lincoln to manage weapons integration task; Lincoln declines.
- Nov. ARDC holds conference on weapons integration problem.
- Dec. Experimental SAGE Sector (ESS) begins shakedown tests.
- Dec. System Development Division of RAND begins independent operation as System Development Corporation.

1957

- Dec. ARDC recommends establishment of Air Defense Systems Management Office (ADSMO) to oversee integration.
- May SAGE Weapons Integration Group (SWIG) assembles at Hanscom Field.
- Jun. Lincoln urges that Division 6 take over weapons integration responsibility.

1958

- Mar. Secretary of the Air Force proposes to MIT that a new organization be formed to provide systems engineering support to ADSMO.
- Mar. To strengthen ADSMO, Air Defense Systems Integration Division (ADSID) is established.
- Jul. First of 24 SAGE direction centers operational at McGuire Air Force Base, New Jersey.
- Jul. Division 6 becomes basis of new systems engineering organization, incorporated as The MITRE Corporation.

1959

- Jan. Transfer of technical personnel from Lincoln to MITRE.
- Nov. Air Force Command and Control Development Division (C²D²) activated at Hanscom Field, takes over ADSID mission.

1960

- Feb. Gordon Thayer of AT&T named director of Winter Study.

1961

- Mar. ARDC redesignated Air Force Systems Command (AFSC).
Apr. Electronic Systems Division (ESD) of AFSC activated at Hanscom Field under Maj. Gen. Bergquist; includes former AFC²D².

1962

- Dec. ESD and MITRE sign memorandum of agreement establishing a basis of cooperation.

1963

The SAGE system is fully deployed in 23 air defense sectors: 22 in the United States and one in Canada.

INTRODUCTION

EVOLUTION OF AIR DEFENSE OF THE UNITED STATES: A BRIEF REVIEW

During the First World War, the airplane was used for surveillance and, late in the war, for some bombing. It did not play a very significant role in that war, but the potential was evident. The surveillance (reconnaissance) role of aircraft during this war, carried on by tethered balloons and aircraft, led to the need of maintaining air superiority. Later, as larger airplanes were developed which could carry bombs, aircraft began to play a role in defense. In the twenties, the future importance of air power was dramatically demonstrated by Billy Mitchell's post-war efforts in sinking the captured German battleship "Ostfriesland" and the American battleships "Texas" and "Indiana." He illustrated the possibilities for the utilization of bombers as a strategic force.

The air defense of the United States before 1935 had a low priority. Canada and Mexico were both friendly, and the Atlantic and Pacific served as effective barriers against land-based aircraft of the time. This complacency began to erode by the time of the Spanish Civil War when German and Italian aircraft were employed with considerable effect on the side of the rebels.

About the same time Great Britain, anticipating possible confrontation with Germany, began the development of radars. By adapting a high-frequency radio device (a sounder) for measuring the height of the ionosphere, Sir Robert Watson-Watt created the first British radar around 1935. The angular accuracy of this radar was poor, but the range accuracy was good. Its first demonstrations were at ranges of less than ten miles. With the invention less than four years later of the cavity magnetron, an efficient generator of microwave power, ranges of hundreds of miles with good angular accuracy were achieved.

The British developed a chain of radars along the coast of Great Britain called the "chain home" radar system. With aircraft being guided to invading bombers by radar rather than by sight, the British were able to use their air fleet much more efficiently. The British also developed

airborne radar which aided interception at night. During the Battle of Britain, when Germans deployed both day and night bomber missions in large numbers over the British Isles, the British chain home radar system made it possible for British fighter/interceptors to stay on the ground until it was absolutely necessary. They were then vectored by the radar systems to the German bombers. The British system became the model for United States air defense systems. The British had other, highly classified methods of locating German aircraft. Early in World War II, they had broken the German cryptographic code, and thus had access to the orders being given to the German aircraft. The precise locations and times of German attacks could thereby be used directly by British fighter commands.

The need for air defense was driven home in the United States in 1941 by the Japanese with their attack on Pearl Harbor. Pearl Harbor demonstrated the need for surveillance and warning and real-time control.

Sobered by these events, the United States became serious about air defense within its continental limits and, near the end of World War II, there were more than 70 radar stations known as Ground Control Intercept (GCI) sites. This network of GCI sites became known as the "Manual System."

Each of these GCI sites consisted of one or two search radars, a height-finder radar, ground-to-air and air-to-ground communications. The operators sat in front of plan position indicator (PPI) scopes, which presented the air situation on a scope that employed long-persistence phosphors. Aircraft appeared as "blips" of light on the face of the tube, and information on targets from adjacent sites was cross-told by voice telephone. The control centers were usually built around a large, edge-lit plexiglass board which showed the local geographic features. Aircraft of interest and status information were marked on the board with grease pencils by operators who worked standing on scaffolding behind the board.

The GCI sites were spread along the East and West Coasts, with some in Mexico and Canada. There was also a Ground Observer Corps of more than one million volunteer observers. But, as V-E Day approached and it became clear that it was only a matter of time until Japan surrendered, the priority of United States air defense was again lowered, and support of the existing sites began to erode. After V-J Day, when the Allies had won and the United Nations was instituted, and when the most powerful air forces were in the hands of the Allies, including Russia, there

seemed no justification for the expense of maintaining the radar sites established during the war.

This attitude began to change, however, as it became clear that the Russians were bent on creating a different political order and clamped down the Iron Curtain. In June, 1948, Berlin was cut off. The Berlin airlift began shortly thereafter and was a further demonstration of what could be done with air power. The United States became determined to be second to none in the areas of strategic war.

In 1947, the Air Force was organized as a service separate from the Army, reporting to a newly established Defense Department. The Air Force was given the air defense mission and proceeded to plan the revival of the Manual System. The importance of this mission was increased with the subsequent Russian achievement in 1949 of producing atomic bombs, and was further strengthened by later events in Korea. While these events were evolving, the Air Force Chief of Staff, General Hoyt S. Vandenberg, became more and more concerned about United States vulnerability to airborne attack. The Air Force Scientific Advisory Board, under Dr. Theodor von Karman, was exposed to the problem, and in 1949, the Board set up an Air Defense Systems Engineering Committee (ADSEC) under George E. Valley, a physics professor at the Massachusetts Institute of Technology.

The Valley Committee began by looking at the newly reactivated air defense system. This system had been authorized by Congress through the Air Force, and consisted of many of the 70 or so GCI sites which constituted the Manual System set up during World War II with improved radars and height finders. The Valley Committee quickly concluded that the air defense system, as reshaped by the Air Force, had a very low capability, and characterized it in their report as "lame, purblind and idiot-like."

The Committee recommended that a competent technical organization look into what could be done to improve the system in the short run. The Committee also suggested that a longer range look be taken at the problem. It recommended the extensive use of automation, particularly computers, to handle the bookkeeping, surveillance and control problems in the implementation of next generation air defense systems. This conclusion was partially driven by the fast-developing Whirlwind computer at MIT. The Whirlwind promised to provide real-time control over a large number of aircraft. It was also noted that the ability to pass digital

information over phone lines had been demonstrated at the Cambridge Research Laboratory and at Bell Telephone Laboratories. To deal with one of the major problems, low altitude surveillance, the Committee recommended the establishment of a large number of short-range, low-maintenance radars, more than in the current system, which would be placed closely together to fill gaps in coverage.

The Valley Committee report triggered General Vandenberg to ask MIT to study the entire problem of continental air defense. Accordingly, MIT set up a study called Project Charles under Professor F. Wheeler Loomis, on leave from the University of Illinois, and brought in a number of distinguished scientists, including George Valley, from a broad spectrum of the United States scientific community. Valley, supported by most of the participants, was given the task of defining, insofar as it could be done, the longer range air defense system. The Charles Study recommended first that the existing system be upgraded (this was the task of the Western Electric Company and Bell Laboratories, and was known as the Continental Air Defense Survey, or CADS, project). Second, it recommended that a laboratory be created to deal with the research problems associated with the development of a more capable "transition system." This laboratory was established within MIT, and in 1952 was endorsed as the MIT Lincoln Laboratory. Work was also to be carried on there toward a future ultimate system. The transition system would become known as SAGE, for Semi-Automatic Ground Environment.

For the next ten years, there would be a strong and sustained effort, led by Lincoln Laboratory, to realize a solution to the air defense problem.

C H A P T E R 1

HOW I CAME TO THE DIGITAL COMPUTER LABORATORY

I stumbled into the SAGE milieu by chance. In 1950, I was an MIT graduate student, married, with two children. I was unhappy with one research assistant job and looking for a different one to help pay the way through graduate school. I did not know when I happened upon a job at MIT's Digital Computer Laboratory the scope of the undertaking with which I would become involved. This undertaking was the design and development of the air defense systems of the United States.

I had been working at the MIT Research Laboratory of Electronics (RLE) since September 1950. RLE was formed, in part, from the residue of the famous MIT Radiation Laboratory that had been established during World War II to develop microwave radar. RLE was headed at that time by Professor Albert G. Hill of the Physics Department. Hill had been a division head in the Radiation Laboratory. His division, devoted to transmitter components, was one of the larger divisions. Later, Hill would join Lincoln Laboratory as its second director, bringing with him many of the people who had worked for him at RLE. MIT's research assistant program paid me \$135 per month plus tuition. These monies, along with the \$95 per month I received under the GI Bill as a Navy veteran, made it possible for me to work toward a master's degree in Electrical Engineering while working at RLE.

My assignment at RLE was to Professor William H. Radford, later associate director and then director, Lincoln Laboratory, who was in charge of a telemetry section. Indirectly, this section worked for Professor Robert C. Seamans, Jr., of the Aeronautical Engineering Department (later he became associate director of NASA and then secretary of the Air Force). Seamans had the responsibility for a ground-to-air missile project called Project Meteor, and Radford had been subcontracted to provide the telemetry for it.

Although I was technically assigned to Radford, I seldom saw him. He had several offices, and he always seemed to be in transit from one to another. So it was actually Benjamin J. Dasher, a doctoral student

and professor of electrical engineering on leave from Georgia Tech., who was in charge of the group. There were eight or nine people in that group — primarily graduate students — working on various aspects of the Meteor telemetry. Among them was Walter E. Morrow, Jr., who would later become director of Lincoln Laboratory.

The organization of the telemetry section was very loose, and each member of the group was more or less free to choose his own project. It was expected that our projects would result in something of use to the Meteor program and at the same time pave the way toward a thesis topic. I tried to do some work on a miss distance indicator, a sensing device that would measure how close Meteor got to its intended target, but with the demands of the course work, the nebulousness of my assignment, and the general feeling that no one cared, I didn't do very well that year at RLE.

I happened to have a professor, Dr. William K. Linvill, who had been using the Whirlwind computer, which was then in its final stages of development at MIT, to do some studies that contributed to his field of network analysis. I became friendly with Bill Linvill, and he piqued my curiosity about the Digital Computer Laboratory, where Whirlwind was being developed. I think it was he who told me that the laboratory was looking for more people. With that information, I went to the laboratory and spoke with John C. Proctor, who was then acting in an administrative capacity handling personnel functions.

Proctor confirmed that there were job openings at the Digital Computer Laboratory that would be appropriate for me. He showed me around the Barta Building which was the Computer Laboratory's home. In the basement there was a tube shop where the laboratory was producing its own high-speed memory tubes. On the upper floors was the Whirlwind machine. My first impression of the computer was of rows and rows of racks with bundles of wire everywhere and thousands of vacuum tubes. In the week or two following my discussion with Proctor, I discovered that a number of people who lived where I did, in Westgate West (a community of renovated Navy barracks converted to apartments for married students at MIT), also worked at the Digital Computer Laboratory. Everyone I spoke with confirmed that the Computer Laboratory was a good place to work.

I also met David A. Huffman who was on a doctoral program and whose thesis advisor was Dr. Samuel Caldwell, who had collaborated with Dr. Vannevar Bush on the MIT differential analyzer. During the time I was

looking into the computer field, Dave told me about a course taught by Caldwell on logical design and Boolean algebra. All this interested me, so in the summer of 1951, I made arrangements to transfer from the Research Laboratory of Electronics to the Digital Computer Laboratory, and I signed up for Caldwell's courses. I went around to see RLE's director, Al Hill, to tell him about my transfer. He said he didn't care, and so I left.

Thus, I didn't get to know, or see for that matter, Al Hill until I was leaving RLE. Hill had the reputation for being an up-and-coming senior member of the MIT research organization. An incorrigible punster, he was a round-bodied, round-faced, cherubic person, who reminded me of Burl Ives. He was also reputed to like an occasional drink. Hill was to indirectly affect my career throughout my involvement with the air defense systems.

I found the atmosphere at the Digital Computer Laboratory quite different from that at RLE. Everyone seemed to feel he was doing something important and interesting. Although there didn't appear to be any more formal organization than there was at RLE, there was a kind of invisible control structure in operation. People seemed to know what they were trying to do. It became clear after a while that the organization was very carefully managed by Jay W. Forrester and his associate director, Robert R. Everett. However, I didn't meet either for some time after I joined the laboratory.

C H A P T E R 2

THE DEVELOPMENT OF WHIRLWIND

The Whirlwind project evolved out of a Navy program established during the war in the MIT Servomechanisms Laboratory under Professor Gordon S. Brown, the laboratory's director, and Jay W. Forrester. It began as a program to create an airplane stability control analyzer (ASCA) for the Navy's Special Devices Center. It was intended that this analyzer include a simulated airplane system, wherein a hydraulically operated, simulated cockpit would react to wind tunnel data and other environmental conditions, as well as to the aerodynamics of the proposed aircraft. It was expected that one could "fly" the airplane in a simulation mode before it was built. The ASCA could also be used for training.

The key component of this airplane stability control analyzer was a computer. It was originally intended to be an analog computer of the sort developed during the war at MIT by Bush and Caldwell, but it became clear to Jay Forrester from discussions with Perry Crawford of the Navy Special Devices Center that the emerging digital computer technology promised more flexibility and capacity than could be generated by the then-existing analog computers. Sometime during 1946, Forrester decided to attempt to build a digital computer. Although there were a number of digital computers working or under development — primarily at the University of Pennsylvania, Princeton, Bell Laboratories, Harvard, and in industrial organizations such as IBM — these computers were dedicated to the solution of mathematical problems, and not to the problem of real-time control. Real-time problems required speed and reliability well beyond that which could be expected from the mathematically oriented developments.

What had begun as an airplane stability control analyzer soon became a project to develop a general purpose digital computer to be used for a variety of real-time control problems. In 1948, the responsibility for Whirlwind was shifted from the Navy Special Devices Center to the mathematics branch of its parent organization, the newly established Office of Naval Research (ONR). Judgments as to Whirlwind's direction and funding were made on the same basis as were the mathematically

oriented machines administered by the branch. Since the Whirlwind project consumed a large fraction of the mathematics branch budget, funding became a serious problem after the transfer. In 1949, however, the United States Air Force contracted with the Servomechanisms Laboratory to do a study of the uses of digital computers in air traffic control, and within a year, the prospects for obtaining Air Force funding for the air defense mission became a real possibility. Jay Forrester, at George Valley's invitation, had participated in the Valley Committee (ADSEC) study and had laid the groundwork for Whirlwind support to air defense development. Sometime during this period, the Servomechanisms Laboratory set up the Digital Computer Laboratory (DCL) specifically for the Whirlwind project.

The aircraft stability control analyzer, as well as the other real-time control applications which were thought to be possible with the use of the projected Whirlwind machine, dictated two basic design principles: (1) a systems architecture and components which provided the highest processing speed, and, (2) components and logical design which maximized reliability of operation.

Speed of operation is necessary to assure that all the processes are completed when the answer is needed (thus, real time). Speed translates into capacity to deal with a complex process such as the solution of many simultaneous differential equations, as in the ASCA system, or when a large number of similar processes are required in a given time, as in tracking in the air defense system. To obtain this capacity, the Whirlwind group chose to create a parallel machine, rather than a serial machine, which had been their original intent. Thus they could operate on whole numbers rather than a bit at a time. The group wanted to create a short memory cycle, very short transfer time, and very short calculation time, so the basic circuit development emphasized speed of operation. Fortunately, the World War II radar developments provided high-speed pulse circuits.

The group chose a simple architecture for a parallel binary digital computer with single address instruction. It included a high-speed memory, an arithmetic element, a control element and an input-output system — all operating on a common parallel bus. This architecture was refined and detailed in a set of block diagrams. The block diagrams group under Bob Everett became the watchdog over a number of groups devoted to the design and implementation of the necessary functional electronics needed to cause the logical blocks to work as predicted. A 16-bit parallel bus was

used to deal with the word length chosen by the block diagrams group. Sixteen bits was enough to supply the necessary dynamic range and accuracy required in most control applications, and it provided an instruction word of appropriate length to accommodate the number of instructions and the number of memory addresses expected to be necessary. Sixteen bits was too short a word for mathematical or scientific calculations which generally require more precision, but for the real-time applications it was quite adequate.

With reliability as the other priority of the machine, the Whirlwind group adopted a policy of "try before buy." It was important to test the basic circuits in a realistic environment to be able to predict, as the machine built up, its ultimate performance. It was this philosophy that led to the design and construction, under the guidance of Norman H. Taylor, of a five-digit multiplier, made up of the kind of circuitry which was expected to be used in the Whirlwind machine. This multiplier, using the vacuum tubes which were chosen as the system's basic components, was operated continuously, solving the same problem and checking the answer, for weeks without failure. The purpose of the multiplier was to gather reliability data on the vacuum tubes and other critical components. The multiplier used about 400 vacuum tubes which were programmed on a read-only toggle switch memory.

David R. Brown and two of his engineers, Edwin S. Rich and John A. ("Gus") O'Brien, performed studies in depth on the characteristics and failure modes of the circuits and tubes that went into Whirlwind. They worked with the tube manufacturer, Sylvania, to correct what could be corrected. Since the Whirlwind component specifications were very tight and produced a tube that was over-specified for non-computer usage, direction of tube design and quality control was effectively taken over from Sylvania. One result of this careful attention was that these tubes cost from \$5 to \$10 apiece, which was high for the time.

Three kinds of difficulties became evident with the vacuum tube. The first was mechanical failure due to faulty construction. Second were the problems with the cathode, which reacted with the oxide coating forming a barrier and causing it to act as a resistor rather than as a conductor. The third difficulty with the vacuum tubes was their gradual deterioration of performance over time.

This last problem became an important aspect of the development of the Whirlwind computer. Noting that this deterioration could be

measured by various means, an important method of evaluating the margin of deterioration was conceived by Forrester. All of the circuits in the Whirlwind computer were subjected to this "marginal checking." For the majority of the circuits that used one of the standard tubes (for example, the 7AK7), the screen voltage was lowered to simulate the deterioration. (In actual vacuum tube deterioration, the margin would become lower and lower until it was advisable to pull the tube before the failure occurred.) Thus, through a combination of working with the tube manufacturer to deal with the mechanical failures and the cathode barrier problem, and with the application of marginal checking, tube loss during computer operation was reduced to a tenth of a percent per thousand hours, which was a dramatic improvement over previous statistics. With the improved vacuum tubes and with marginal checking, the Whirlwind people stimulated the development of tubes of very high reliability.

High-speed memory was also a problem. The speed of operation of the system depended on it. Many organizations were developing and using cathode ray tube storage devices, the most successful of which was the Williams tube. When the Whirlwind machine was conceived, this kind of memory was essentially the only choice available. The Digital Computer Laboratory set about the task of designing and fabricating its own electrostatic storage tube. Like all such tubes, it depended on using the electron beam as the device for selecting a spot on an insulator on which "1" or "0" could be stored, and on the beam's ability to change the charge on an insulating plate. Mica was the first insulator to be used; others were to follow.

In order to fabricate its own electrostatic storage tubes, the DCL set up a tube shop where tubes could be built and tested. The responsibility for managing the electrostatic storage tube project was Stephen H. Dodd's. Steve had worked with Jay Forrester in the early part of World War II, when both men were in their early twenties. One of the lessons that was learned early in the game by the people who worked in the MIT laboratories was that a well-rounded engineer is one who participates in all of the stages of the development practice, from the conceptual to the operational phase, and that feedback from every phase modifies or tends to modify earlier phases. This approach creates the best product and involvement, but sometimes includes hazardous duty. Steve Dodd was an MIT graduate student at that time, working in the Servomechanisms Laboratory. He had done his thesis on the hydraulic transmission portion of a ship-board

antenna stabilizing device, designed to keep the antenna boresight on the horizon as the ship that carried the antenna on her mast was tossed about. The stabilizing system was Jay's responsibility, and Steve was his principal supporter. The stabilizing device was mounted aboard a lightship off the coast of New England, and Jay and Steve monitored its performance by observing it in operation. Steve relates a story of his experience during the test operation: fearing that the hydraulic fluid was too dirty and believing that a filter should be changed, Jay asked Steve to climb the mast to read a gauge that had been installed for the purpose of indicating the pressure of the fluid. Steve, who was not fond of heights, climbed the mast, taking all his courage. When he got to the top, he found that the gauge was color-coded. Steve was color-blind. He came down the mast and told Jay. Forrester grumbled something about research assistants not taking physicals to qualify for their work, and so he climbed the mast himself, but found that he couldn't read the gauge either. They decided to change the filter, which would have made the climb unnecessary in the first place.

When Whirlwind was conceived, the responsibility for the electrostatic storage tube memory was assumed by Steve, and Jay looked to him for its development. When Jay decided that the DCL would build its own tubes, Steve became a part of that effort. Steve was a conscientious, personable, thorough engineer, whose pleasant countenance was dominated by a wide smile. His eager and alert personality couldn't help but make me think of Bugs Bunny's energetic and wise ways.

The tube shop, located in the basement of the Barta Building, reported to Patrick Youtz, who was a research associate (as distinguished from a research assistant). I think the difference was mainly that he was not a graduate student, but a full-time researcher with an academic appointment. Youtz was a real character: stories attributed to Pat would have you believe that he had been a coal miner, a professional football player (Chicago Bears) and a lawyer. He was a large, powerfully built man and reminded me of Daddy Warbucks with a jet-black toupee. I was led to believe that he had learned about vacuum tube manufacturing while presumably working at American Television, Inc., in Chicago. Coincidentally, I had also worked there before coming to MIT. I had not known Youtz then; for though he was supposed to have been there when I was, we may have worked in different divisions.

American Television was headed by a man named U. A. Sanabria, a Cuban who had come to the U.S. as a young boy. He claimed to be

one of the inventors of television and that he had set up a TV station in the Chicago area in 1925. He also claimed that he held a patent on interlace scanning on the flying spot disc scanner that was used in the first TV experiments. He had become associated with Lee de Forest, inventor of the vacuum tube. When I worked at American Television, they were manufacturing cathode ray tubes for television sets, and in addition were running a television engineering school, where I taught TV maintenance and theory as well as differential and integral calculus. When I was there, Lee de Forest was in his late seventies or early eighties. Each day he came to work in the laboratory on the top floor of the building, where he maintained a covey of German glass blowers and where he was alleged to have been working on experiments in vacuum tubes. He was a spry man. He and U. A. Sanabria seemed to be the antithesis of each other: Sanabria was a promoter; de Forest seemed to be a researcher. Sanabria was also a friend of "Mad Man" Muntz, the legendary Chicago used car dealer. Together, they manufactured a line of televisions under the name of Muntz TV. Sanabria and de Forest did have one thing in common, and that was their attraction to buxom blonde girls, who were hired for many of the secretarial, clerical and administrative positions.

Whether Youtz had obtained his experience at American Television or not, by 1951 his DCL tube shop had managed to produce enough vacuum tubes so that a 16×16 (256 words) memory could be used with Whirlwind. The mean-time-to-failure of these tubes was very short; much too short for a practical system. In fact, in order to get anything near satisfactory performance, the engineers who were putting the memory together had to know the idiosyncrasies of each of the tubes that was used. One of those engineers, Alan J. Roberts, remembers one of the times he was asked to fix a problem with this memory system. One of the 32 tubes in the system had a mechanical fault in the signal plate assembly that was used for writing ones and zeros. Apparently, there was some play in the structure that held the signal plate screen in place. Several times when he was called in, Al went directly to that particular tube, tapped it in a certain way, and cleared up the trouble. The necessity of an engineer's knowing the weaknesses of the individual tubes, practically calling the tubes by name, promised to be the rule, and not the exception, in CRT memory.

By 1953, a second bank of these tubes had been produced and installed. It is a credit to Steve Dodd, Pat Youtz and others that it worked as well as it did. Possibly, if they had concentrated further on the design, they

might have made a practical system. But in the meantime, largely due to the personal efforts of Jay Forrester, the first random-access magnetic core memory had been devised. Although the first operations of the Whirlwind machine used cathode ray tube memory, the core memory showed promise for the reliability and capacity required, but as Norm Taylor remarked tongue-in-cheek, the available cores were as big as doughnuts and the driving vacuum tubes were as big as coffee pots. After the magnetic core was substituted for the electrostatic memory tubes, the tube shop concentrated on cathode ray tube displays, helping in the development of the "Charactron" and "Typotron" display tubes. The storage tube, however, seemed to peak out at a performance level well below that desired by the group.

The core memory technology had to be developed practically from scratch. The assignment for guiding the development of cores for use in the projected random access core memory was given to Dave Brown. Dave hired Frank E. Vinal and John B. Goodenough, and enlisted the help of a Professor von Hippel from the MIT metallurgy department. They set up a core manufacturing facility where they made the cores that went into the first memories.

To begin with, they selected a core size that was the smallest it could possibly be (while still practical) to assemble into the patterns of rows and columns that made up the various types of arrays. Once the small ceramic cores could be reliably produced, the problem of core array assembly and manufacturability was addressed. A jig was invented that held a large quantity of cores in a hopper, and automatically dropped the cores onto a grooved tray, and shook the tray until the cores settled into position in the grooves, where they were then sucked in by partial vacuum and aligned, so that they were then ready to be wired. The X and Y wires could be automatically installed with little difficulty through the appropriate rows and columns, and the excess cores swept off. However, the sensing wire had to be threaded through every single core, and this job had to be done by hand.

In the course of core development, visitors came to MIT to see how things were done. John Goodenough was giving a contingent from Germany an explanation of the wiring processes. John had had some German in his education, and chose to use some German words with this group. In describing the process, he was explaining that young women had been hired to do the sensing wiring. He used the word "liebfrau" to

describe the women. He meant to connote young woman, but the word also implies virgin. After the tour, the Germans spent much of their time trying to determine how the *liebfrauen* were selected.

In parallel with the development of practical cores, their fabrication into a random-access, high-speed core memory was being overseen by Forrester's right-hand man, William N. Papian. In order to raise confidence in the core memory, a computer called the Memory Test Computer was built around the cores to test them. The Memory Test Computer was, in fact, the first computer to have a high-speed core memory, and it was also Kenneth H. Olsen's (founder of Digital Equipment Corporation) first computer. The Memory Test Computer was used for several years after its testing of the core memory for data reduction and for testing SAGE peripherals.

The core memory was an immediate success when installed in August 1953 (over one weekend) in the Whirlwind system. It doubled the operating speed and quadrupled the input data rate. Maintenance time was reduced from four hours a day to two hours a week, and the mean-time-to-failure was increased from two hours to two weeks. It also freed the people in the tube shop from working on the cathode ray tube memory for work on the needed operational displays.

C H A P T E R 3

THE DIGITAL COMPUTER LABORATORY JOINS LINCOLN LABORATORY

As work on Whirlwind was progressing in the late forties and into 1950, the Air Force was acting on the recommendations of the Valley Committee to establish a laboratory for research and development on air defense matters, and to carry out an interim study of the air defense problem. The short-term study, Project Charles, produced a report in 1951. Meanwhile, MIT was planning the permanent laboratory, which was to be funded by the federal government but staffed and run by MIT. The laboratory would be built on federal property in Lexington, Massachusetts, next to Hanscom Field. Initially called Project Lincoln, the laboratory began operating in temporary quarters in Cambridge in July, 1951. When the first buildings in Lexington were completed in 1952, it was officially renamed Lincoln Laboratory. The laboratory was headed by Wheeler Loomis for a time and then was turned over to Al Hill, who had been the head of the Research Laboratory of Electronics.

In the course of our work on Whirlwind, the team at the Digital Computer Laboratory had become very closely involved with Lincoln. In the summer of 1951, those of us in the Digital Computer Laboratory who were involved in applying Whirlwind to the air defense problem joined Lincoln's newly established Division 6, Digital Computer, headed by Forrester. We remained in Cambridge for a couple of years but eventually moved out to Lexington.

With its DCL identity, Division 6 was unlike the rest of the divisions at Lincoln. The DCL still continued its Cambridge existence under Forrester with its responsibilities for supporting the math group and other groups at MIT, and for operating Whirlwind. DCL/Division 6 had its own service organizations: its own publications, mailroom, and administrative support. The justification for these organizational deviances was that Division 6 had some obligation to maintain the level of services to which it had become accustomed as the independent DCL, and these services could not be expected if it were totally integrated into the Lincoln Laboratory framework.

The other division of Lincoln Laboratory most directly connected with the air defense job was Division 2. Division 2, headed by George Valley, concentrated on radar and communications. Valley, a radar expert, had been a senior member of the Radiation Laboratory staff during the war and was one of the principal editors of the famed "Rad Lab" series of technical books. He had also been the chief man on ADSEC (Air Defense Systems Engineering Committee), where his work was primarily responsible for initiating the air defense upgrading. Valley had participated in Project Charles, and it was largely due to his and Forrester's efforts that the concept of the air defense system we were working on was chosen by the Air Force. Valley was also Dr. Hill's assistant director at Lincoln. Their names, Hill and Valley, were prophetic: as I would discover, they were miles apart in their approaches to management.

Valley was dedicated to doing something for air defense in the relatively near future. Hill, on the other hand, was interested in the longer view and felt that the work Valley was doing was not as revolutionary in approach as the problem demanded. In 1952, in order to eliminate this difference of opinion, a second group was formed under Jerrold R. Zacharias of MIT. The findings of this group, called the Summer Study, illuminated what Zacharias considered to be a better approach than that backed by Valley and other interested scientists. Valley found that this version worked against the approach he was trying to take. This led to differences of opinion about important management decisions, and for a long time Valley found himself faced with internal Lincoln opposition.

C H A P T E R 4

CONTRIBUTIONS OF AIR FORCE CAMBRIDGE RESEARCH CENTER

When ADSEC was given the task of reviewing the U.S. air defense situation, it was also given administrative and technical support from the Air Force Cambridge Research Center (AFCRC). Most of the technical contributions were related to the work being done by AFCRC's Relay Systems Laboratory on relay techniques for remoting radar PPI pictures. This pioneering work, headed by John V. (Jack) Harrington, would eventually lead to the development of automatic radar data networking. A good deal of this work took place in the late 1940s, before I was a part of MIT. My first encounter with it would come when I participated in meetings with Harrington's group some years later, when we were pinning down design alternatives for cross-telling and surveillance reporting.

The Relay Systems Laboratory was led by Jack Harrington, a soft-spoken, strong-minded Irishman, and his associates, Ernest W. Bivans and Paul Rosen. Bivans was a prickly, argumentative person who was very competitive and who had Harrington's confidence. Harrington himself was a researcher who liked to prove the feasibility of his theoretical designs, but he was not interested in the business of designing and packaging for field use, and had Bivans and Rosen participate in carrying out those functions. I found Rosen to be a likeable, entertaining raconteur, full of stories about the workings of bureaucracies, and especially about his experiences in the Navy during World War II. Through a series of bureaucratic blunders to which he fell victim, Rosen started as an officer candidate, but somewhere during the course of his service, he was classified "illiterate." In fact, he had been attending Tufts University and was in the Navy's college training and midshipmens school programs, as well as electronic technicians school. Nonetheless, they made him a stevedore. He was never able to straighten out the error, and spent much of his time in the Navy on the loading docks in the islands of the Pacific.

The efforts of Harrington's lab in the late 1940s were centered on the completion of a microwave relay system that had been started during the war by the MIT Radiation Laboratory. The Relay Systems Laboratory

had taken on the problem of remoting radar PPI pictures — that is, automatically relaying the pictures and data over long distances to some central location remote from the radar which generated them. Two approaches were taken to the problem.

The first approach was to use microwave as the relay method. The microwave relay system transmitted radar video signals that could be reconstructed and displayed at the receiving end. Experiments were set up using the Microwave Early Warning (MEW) radar at Hanscom Field to send signals to AFCRC in Cambridge some 20 miles away. The system worked very well, although it was costly and required several megahertz of bandwidth to transmit the unprocessed video.

The second approach was the result of attempts to create a more efficient method of accomplishing the same objective, and was known as Digital Radar Relay, or DRR. DRR took advantage of the fact that the information content of the desired video signals was quite low: out of the entire radar picture, only a few blips were of interest, since only a few would represent aircraft while the rest would represent ground clutter and other noise. To automatically identify the aircraft returns from the entire field of returns, a detector was employed to integrate signal returns from pulses within the radar's beamwidth. When enough energy had been returned at a given range to pass a certain threshold, the signal could be identified as an aircraft.

Once the targets had been detected, their locations (range and azimuth coordinates, R , θ) could be transmitted in binary digital form. This reduction in bandwidth allowed the microwave circuit to be replaced by telephone lines as the means of transmission. A device was invented to convert the blips into R , θ coordinates in binary form, and a modem was constructed to impress these digital signals onto the phone line. The information was transmitted to a remote location at 1300 bits per second, and demodulated.

The DRR system was demonstrated successfully over a phone line from the MEW radar at Hanscom Field to CRC in Cambridge sometime in 1949 — and automatic detection of radar targets had been achieved. A year later, ADSEC was to use DRR in the first radar tracking experiments, in which the MEW radar, equipped with DRR, would transmit radar target data in real time to Whirlwind in Cambridge, where digital track-while-scan functions were performed. The work at the Relay Systems Laboratory had been the basis of ADSEC's confidence in the

feasibility of automating the detection and cross-telling requirements in their system concept, and also led to the transfer of most of the people in the Relay Laboratory to Lincoln. Jack Harrington's group left the government laboratory and joined us in the air defense effort as Group 24, Data Transmission, in George Valley's Division 2.

At Lincoln, the DRR experiments were continued, and in addition work on a so-called slowed-down video (SDV) system was initiated. SDV, a variation of DRR, divided the coverage area of short-range radars into a large number of wedge-shaped boxes, the number bounded by the range resolution required and the angular resolution that one could achieve with the radar antenna. If there were signal returns of a certain magnitude in a box, it would be called a target, and sent as a one (1) on a telephone signal stream synchronized with the radar pulses and angular position of the radar. SDV was inexpensive and effective, and was produced as the FST-1 processor by the Lewyt Corporation. Its major disadvantage, however, was that its azimuthal accuracy was inherently poor. While it was appropriate for gap-filler radars, it was too inaccurate for the long-range radars.

These inadequacies led to the next level of enhancement of DRR — the Fine Grain Data (FGD) system, produced later by the Burroughs Corporation as the FST-2. Its major improvement over its predecessors was better resolution, attained by a beam-splitting technique. This technique depended on the fact that as a radar beam rotates the pulse rate is high enough so that it receives a number of returns, on the order of 10, from a single aircraft. The beam splitter was able to determine the center of the beam after it had swept across the target. Utilizing this device, it was possible to greatly increase the angular accuracy of the target location. The Fine Grain Data system also included automatic control and transmission of height finding signals, and control of beacon data from friendly aircraft.

Further FGD improvements included an enhanced data transmission system for telephone lines. Jack Harrington and Paul Rosen held the patent to this improvement, which allowed for an increase in data rate from 600 bits per second to 1300 and later 1750. As contrasted to schemes that used single sideband transmission, where the carrier must be added for demodulation and synchronization, they transmitted part of the carrier frequency, thereby sending the signal along with its own timing. The system was the foundation of what became Bell Telephone's A-1 Data Service.

In those days, I did not know Jack Harrington too well but would get to know him better in the process of the SAGE computer design and specification of the communications cross-tell system. I remember him as being the type of person who protected his people. He was basically good-natured, had a wry sense of humor, and was enjoyable to talk with. Somehow, he did not seem to open his mouth when he talked. With the loquacious Bivans acting as his front man, Jack reminded me of a ventriloquist who moved his lips only when necessary to emphasize the authority of his pronouncements.

With Whirlwind having the potential to process data in real time, and with Group 24's data transmission schemes successfully tested, an experimental air defense system could be set up.

C H A P T E R 5

THE CAPE COD SYSTEM

By early 1952, Lincoln management decided that the Whirlwind computer was operating well enough so that it could be used as part of Lincoln Laboratory's experimental air defense system. Called the Cape Cod System, the experimental network consisted of a direction center at the Barta Building in Cambridge, an experimental long-range radar at the tip of Cape Cod in South Truro, and a number of short-range radars called "gap fillers" at various locations around New England. The direction center was equipped with UHF ground-to-air communications. For purposes of creating a realistic test of the system, aircraft were supplied by the Air Research and Development Command and by the Air Defense Command. The design of the Cape Cod System was headed by C. Robert Wieser of Division 6.

It was determined early that the operational computer program for the Cape Cod System would require on the order of 20,000 instructions. Whirlwind's electrostatic storage tube memory could only accommodate 2,048 words at a time. Thus, it was necessary to arrange for the storage of that operating program, and its efficient movement into and out of the high-speed memory at the appropriate time. To facilitate this movement, it was decided to add two magnetic drum memories to Whirlwind: one to act as an intermediate storage primarily for the core computer programs, and another magnetic drum to act as a buffer to record input from phone lines. As the central computer needed the data, it could select it from the appropriate head on the drum.

Although the need for high-speed memory presented the greatest challenge and the magnetic core memory would represent the greatest innovation in meeting that challenge, there were several other memories in Whirlwind. There was a test memory consisting of 32 registers of toggle switch read-only memory which stored the start-up routines. Five additional registers were electronic vacuum tube (flip-flop) registers which could be interchanged with any of the 32 toggle switch registers. And in addition to the magnetic drum memories, there was also a tape memory for backup which stored the whole program including simulation, evaluation,

and diagnostic routines. Additionally, the tape was used to record operational data, so that analysts could diagnose the entire operation or use the data for whatever they needed. With Raytheon, the Whirlwind team developed a memory capacity of 125,000 words per tape.

In 1950, as soon as the Whirlwind computer was able to perform, the experimental Microwave Early Warning (MEW) radar located at Hanscom Field in Bedford was connected using Jack Harrington's equipment and commercial phone lines to Whirlwind in Cambridge, and the first tracking programs were developed. By 1952, the Cape Cod team had demonstrated the ability of the computer to track and control aircraft in small numbers.

The man in charge of computer programming for these feasibility experiments was David R. Israel. Dave had joined the Digital Computer Laboratory in 1949, and he was one of the first people in the Cape Cod design group. In 1949, the DCL was looking at alternative uses of Whirlwind for real-time applications, and was awarded a contract by the Air Force to study its application to military air traffic control. Dave joined DCL in 1949 and did his thesis at DCL on civilian air traffic control, and he investigated the uses of Whirlwind in ship and harbor control. When the DCL became involved in air defense, he began programming Whirlwind for the feasibility experiments — a remote display experiment, a track-while-scan experiment, and the first interception using the MEW and DRR. At that time, there were no formal computer programming courses, but attempts were made at it in which Jay Forrester and Gordon Welchman participated. Both Jay and Gordon had to learn the process from scratch.

The first single-thread system (that is, the first system that contained both tracking and weapons control), was programmed using a 256-register, high-speed cathode ray storage tube. It was originally intended that the cathode ray storage tube have 1,028 registers, but because of the migration of the charges on the surface of the insulator used, it was felt that the tubes would be much more reliable operating at the lower register count, and so the track-while-scan and weapons direction program was first implemented in 256 registers. All other programming support functions, such as the assembler and checker, required too much storage to fit into 256 registers. Since there were no utility aids, the programming had to be done in machine code. The translation of programs into machine code had to be a direct process in which all of the manipulations and translations took place in the programmer's head.

Dave was a tough negotiator, and I would get to know him years later when we were settling on the SAGE computer's order code. He played a key role in the development of the early programs for operational surveillance, initiation, identification, weapons direction, and weapons management. He also spent a good deal of time on the operational design of the display console used in the Cape Cod System.

For the Cape Cod "53" design (so called because it was expected to be operational in 1953), the programming responsibility was divided up between Israel and Robert Walquist. Bob was a hulking 6'4", 200 lb. man with a jutting jaw and a head that sat atop a bull neck — physically, he was as imposing as an Easter Island monolith. Because of his size and general bulldozing characteristics, he had earned the nickname "Moose." Moose added a great deal of color to the Cape Cod design group. He was bright, energetic, and extremely argumentative. He was given the track-while-scan functions, and Dave all the others. Walquist would speak his mind clearly and argued with whomever he thought needed straightening out. On one occasion, when George Valley visited the laboratory to witness one of the demonstrations, Walquist got into an argument with Valley in front of Forrester and the other group leaders. Valley remarked that Jay should promote Walquist so that he would be at a level appropriate to Valley's rank. After the 53 program was completed, Walquist decided to join TRW, where he later became a vice president.

The Cape Cod System was intended to demonstrate the operations that were to be executed for field use; in particular, the surveillance function and weapons control function. Both of these functions required information on the position of hostile and friendly aircraft. Some scheme wherein all of the operators in the Barta Building direction center operated from the same positional data base became a requirement. The scheme that was adopted depended upon the idea that all data of interest would originate at the radar or ground observer sites. This data was transmitted to the direction center in angular coordinates, where it was translated into Cartesian coordinates, and where the position of the radar that picked up the data was added. With this scheme, each piece of radar data had an X-Y position in a common coordinate system, and data from overlapping radars could be combined. All of the operating stations were equipped with consoles that had cathode ray tube PPI-type displays. During the course of the operating cycle, this data was presented to an X-Y register which deflected the beam to the proper coordinates on all of the operating positions. It was

then necessary to associate these positions with other information, such as track number, identification, altitude, speed, armament, and whatever other data seemed to be necessary for the execution of the functions at that position. Making this association was one of the first applications of the light gun, for which Bob Everett holds the patent. As the data with the coordinates of interest was presented on the cathode ray tube to the operator, he would place the light gun, which contained a photoelectric cell, over the spot where he expected the data to appear. He would then press a trigger, and when the screen was illuminated at that position, a signal was sent to the computer which told the computer that this was the data of interest which should be associated with this position.

In order not to clog the information channels from the radars, a device called a video mapper (which was really a filter) was added between each radar input channel and the computer. This mapper was a plan-position display with a photocell over the whole display. Returns such as those one would receive from fixed objects in their radar picture were covered with opaque material so that when this data appeared, it did not activate the photocell and was rejected before it got into the main operating system.

By the time the Cape Cod System was finished, it had about 30 operational positions with appropriate displays. Not all the data could be displayed on the face of this cathode ray tube, so the Whirlwind group created an auxiliary tabular display, and data associated with a particular track could be shown alphanumerically on the display. This eliminated the need for a large number of data symbols on the PPI itself. This need for symbols on the face of the display tube eventually led to the selection of the Charactron tube which would be used in SAGE.

The Cape Cod System was in essence a model of the large-scale air defense system. It was used in exercises which included Strategic Air Command (SAC) bombers playing the role of hostiles, and the Air Defense Command and Air Research and Development Command interceptors playing a friendly role. Before the Experimental SAGE Sector (ESS) which grew out of the Cape Cod System was finished, 5,000 or so sorties had been flown against the system to test the whole system as well as its component parts.

The Lincoln air defense system was not the only system being considered by the Air Force. In fact, the Air Defense Command, through its representative, Colonel Oscar T. "Tom" Halley, was placing its efforts

in support of a system concept proposed in the early fifties by the Willow Run Laboratory of the University of Michigan. This concept favored manual tracking aided by an analog computer at the radar sites and coordination at the direction centers. It was an offshoot of Michigan's BOMARC project, a ground-to-air missile under development for the Air Force. The Willow Run proponents stressed the superior discriminating power of the human eye in distinguishing between noise and radar returns from aircraft. In their system, the track was followed by a tracking ball-operated cursor. When the cursor was placed over the data on the screen, a calculation was made which predicted the next expected appearance of the radar return. The Willow Run system did not have the Lincoln advantage of overlap coverage, which increased the amount of data and reduced the cross-telling, handover, and backup problems by centralizing the surveillance function. It also suffered from the fact that there was no experimental gear that could provide proof-of-concept and confidence in its eventual operation. Thus, Michigan's paper design faced the competition of a live system in MIT's early Whirlwind MEW demonstrations. The ultimate decision favoring the Lincoln approach was based partly on this fact and partly on the tactics used by Dr. James R. Killian, who was president of MIT at the time. Killian let it be known to the Air Force that he knew the Willow Run system was being favored by the Air Defense Command. He offered to withdraw from the competition if it were the Defense Department's intention to go with the Michigan system, saying in effect he didn't want to waste his time playing second fiddle to Michigan if the Air Force had indeed made that decision, and to please let him know. This put the prestige of MIT in competition with the University of Michigan.

Killian, in effect, had finessed the situation, but would not have succeeded without the senior military's confidence in George Valley, a seasoned and formidable infighter who argued the technical merits competently and who had the military R&D community in his camp. In the early days of the program, George was very helpful to the Digital Computer Lab in the selling of Whirlwind to the Air Force. He believed, as did Jay Forrester, that real, live demonstrations of the parts of the system were essential in judging the quality of the devices that were chosen to be part of it. He also believed that one must choose some systems on the basis of their demonstrability, sacrificing idealized projected performance. It was this belief that corresponded to Jay's belief in the value of the practical demonstration. Having something concrete to demonstrate was the cornerstone of

their marketing strategy, and they cooperated in demonstrations when Valley needed backup for the things he was trying to promote. Throughout the history of the SAGE system, people in Washington would be given practical demonstrations of the workings of subsystems and systems.

You could almost say that Whirlwind liked George Valley. Whenever George would ask us to get Whirlwind ready for a demonstration to people he was trying to "sell," the machine would invariably be down. But even on short notice, the Whirlwind people would patch things together, and it would perform flawlessly for the duration of the demonstration, only to crash as soon as George and his party left.

One incident stands out during the period when the Air Force was trying to make up its mind about Lincoln Laboratory, and when MIT's ability to carry out the project came into question. Bell Laboratories was asked to evaluate the Lincoln plan, and a review group, headed by Mervin J. Kelly, president of Bell Labs, was formed. They were critical of the approach being taken by Lincoln, and considerable tension was generated between the two organizations, with Bell Labs criticizing and Lincoln defending the Lincoln approach. MIT had recommended that Bell Labs do the systems engineering for the air defense system. Bell refused to take responsibility for the design, but agreed to help with the administration of its execution. The tension was great enough so that the Bell Laboratories and MIT management thought it necessary to hold a peacekeeping or fence mending get-together, which took the form of a banquet for the MIT and Bell Labs people associated with the evaluation. Conciliatory speeches were made, and pledges of mutual respect and friendship formed the basis of these talks. Each speaker de-emphasized the differences and emphasized the common ground. It was an organizational love-fest. When it came time for Al Hill to speak, he got up and said, "Our computer can beat your computer," and sat down. This put a perfect cap on the proceedings.

C H A P T E R 6

WHIRLWIND II

After Whirlwind was operating successfully and the feasibility experiments were done, the design of the operational air defense system had to be initiated, and this was paced by the design and development of its computer. It had become clear to those who had participated in the Valley Committee and in the Charles Study that Whirlwind was more of a brassboard than a prototype of the computer that would be used in the air defense system. To turn the ideas and inventions developed on Whirlwind into a reproducible, maintainable operating machine would require the participation of an industrial contractor. This follow-on computer became known in the early fifties as Whirlwind II.

Within Lincoln, there were many alternative concepts of how this computer would be employed in a system. The most popular concept consisted of three computers in different locations, any two of which could carry the whole air defense load while the other one maintained itself, corrected errors or expanded the capacity of the machines carrying the operations. This and many other different combinations of computer arrangements were eventually discarded in favor of a scheme of duplex computers, where two identical computers at the same location shared the same operational data base — one operating the air defense business of a sector, with the other checking itself and checking the operating machine. When a fault occurred, the standby computer could quickly take up the operational load, while the failed operating computer could turn toward diagnosing its own difficulty. Replacement components could then be substituted to correct the problem.

The most important goal established for Whirlwind II was that there should be only a few hours a year of unavailability of the operational system. The Whirlwind II team at Division 6 thought this was possible, extrapolating from the experience on the Cape Cod System. The speed and capacity of the projected machine were increased by a decision to employ a dual arithmetic element, one to work on the X portion of the position calculation, and one to work on the Y portion, since so much of the processing time involved data in Cartesian coordinates. The capacity of

the machine was also increased by providing a display drum which relieved the central computer from the job of refreshing the displays.

Most of the design choices faced by the Whirlwind II group involved the tradeoff among the number of tracks that could be processed, the number of interceptors that could be employed simultaneously, and the availability criteria. The Whirlwind II group had been set up as Group 62 in 1951 under Norm Taylor to deal with all hardware design questions, including whether transistors were ready for large-scale employment (they were not) and whether the magnetic core memory was ready for manufacture (it was).

The SAGE system was to be the first system to use computer-to-computer communications. There would be a forward-telling link to carry data from the direction centers to the combat center as well as from each of the radars to two or more of the direction centers. In addition, there were cross-tell links between direction centers. This networking part of the design was dominated by Ronald Enticknap, a former R&D exchange officer and RAF squadron leader from Great Britain, who had been a junior member of the RAF delegation to Project Charles.

In order to accommodate the volume of traffic among sensors and radars, several multiplexing devices were required. In addition, the phone lines had to be engineered to reduce phase distortion caused by the inductive characteristics of the lines themselves, so each line had to be engineered to square up the pulses used in the communication. Each line was duplicated for backup reasons. The modulator/demodulator units were an extension of a design by Harrington's group. Paul Rosen had the responsibility for bringing these units to a point where they could be used in the Cape Cod System. Once he had the experimental modems working in the field, Ronnie Enticknap took over the completion of the job and the negotiations with AT&T.

Ronnie was partially responsible for the "quick fix" system, aimed at providing immediate upgrading of the Manual System by substituting an automatic display surveillance picture for the handwritten, grease-pencil updates. The quick fix system was to employ a Polaroid fast-developing camera/projection system that took information from the various PPI scopes and projected it in overlay fashion onto a horizontal, table-top like plexiglass board. Operators sitting around the periphery of the board were to see a picture which was more comprehensive than that which could be seen on individual PPIs. I never knew why it was felt that

the quick fix system was superior to the existing one. Since it was never deployed, I suppose that people eventually found it actually had minimum capability and no growth potential.

I first met Ronnie on a tour of the Truro, Cape Cod, site. Part of his standard tour included the witnessing of a simulated air strike. The thing that stands out in my mind about the simulation demonstration was the orientation, given by Robert Davis and Anne Smalley. Anne was squarely built, had a severe short haircut and usually wore mannish suits. She gave the introduction in a baritone voice. Davis, her teammate, was skinny and nervous, and spoke in a high-pitched voice. The North Truro operational site was a typical GCI site in the system, with the big plexiglass board in front, and bleacher-like working areas with remote PPIs to the various functional officers, the two main functions being surveillance and weapons direction. When we arrived in this room for the demonstration, it was all business. There was a steady background noise of electric motors and the crackling of ground-air radio. A faint blue light dominated the darkened room. When we entered, there were a few blips on the big screen identified as “friendlies.” Davis was visibly nervous about whether they could demonstrate the system without any hostile blips. Enticknap wasn’t sure that they could give the demonstration, so he started to buy time by explaining who did what. We had almost given up seeing the simulated intercept and were starting to leave when a new aircraft was grease-penciled onto the screen. Almost simultaneously Davis’s high voice shattered the businesslike atmosphere of the room as he shrilled, “Oh, Ronnie — hostiles!” The room suddenly lost its authoritative feeling.

C H A P T E R 7

ASSIGNMENT TO GROUP 62

When I joined DCL in mid 1951, I was assigned to work with Norm Taylor in Group 62. Taylor had come to the laboratory in the late 1940s from Western Electric. As well as having responsibility for Whirlwind II, he was DCL's chief hardware systems engineer, and was one of the few at the laboratory who had had industrial experience. To Taylor's credit was the design and construction of the five-digit multiplier.

Norm was a good engineering manager, with an instinct for analyzing any given situation. He had an intuitive feeling for structuring problems so that they could be studied. He had a way of synthesizing opinions and projections into an overall direction for the group. He never pulled rank, and he always brought people in on problems when he could. You could get the idea that he did virtually everything, and as a matter of fact, he did contribute directly to most of the design judgments that were made. Norm was older than most of us, and we had many conversations with him about what our next career steps should be. On the job he preferred to work one-on-one, and he and I worked well together. Norm was a computer system gadfly. He had an enormous capacity for optimism and much of his communication to the staff was in the form of pep-talks, which he would give whenever things seemed to be coming apart. This optimism and his way of dealing with it came to be known as "Taylor's sunshine pump," which could be turned on whenever one of the staff needed it.

The first task Norm assigned me to was to determine whether transistors, which had just recently been invented, should be a candidate for a major Whirlwind II component. The only transistors that existed at that time were point contact germanium transistors which were scrounged during visits to Bell Laboratories. At that time, there was no commercial manufacturer or supplier of transistors but there were experimental models being developed by various companies — Bell Labs, RCA, GE, Philco, and others. Since transistors were quite different from the vacuum tube circuitry that I was used to — solid state, low voltages, holes instead of electron flow and the like — we first had to learn a whole new vocabulary

and way of thinking about circuits. We had to learn to design and build flip-flops and various kinds of gate circuits. Then we tested the transistors for speed of operations and reliability, and we tried to determine how they might be applied to an arithmetic element. Through my research and with the assistance of other people at MIT, I came to the conclusion that the transistor speed characteristics would allow us to achieve very high rise times and very high speeds, but the reliability and availability of the point contact transistors would be a matter of concern. Eventually this research led to my master's thesis topic, "A High-Speed Counter Employing Transistors," which involved actually building and testing a transistor counter. We did not have enough transistors to build anything more complicated.

I built the transistor counter using the Digital Computer Laboratory's famous tool kit which was issued to all new staff. The tool kit was a sort of symbol of the hands-on character of the Computer Laboratory's work. It contained such basics as a soldering iron, diagonal cutters, needlenose pliers, and a variety of wrenches. My first year there, I used it often, but after that, it found a home under my desk where it served both as a footrest and as a constant reminder of its lack of use. As time went on I found various tools missing from the kit. Often, I felt guilty about not turning it in; but at the same time, I didn't want to because I was unable to explain what had happened to the missing tools. This unreturned and semi-depleted tool kit lay on my conscience for four or five years until I finally decided to turn it in and take the consequences. It wasn't until a while after I'd turned it in that I learned that tool kits had been declared as surplus and designated as expendables by the laboratory — and that the people in the tool room had laid claim to the returned tools in my kit.

Some months after I joined the Digital Computer Laboratory, I finally saw Bob Everett. I remember looking into a room in which several people had gathered and, sitting in a lotus position on the table was a slightly chubby, pleasant looking man in animated conversation with the others. I listened for a while and it became clear that this soft-spoken man was held in high regard by the people with whom he was talking. When I told Taylor about it, he said it must have been Bob Everett. What clinched the identification was my mentioning that in the midst of conversation, the man would put his head back and whistle while he thought. It was at about that time I found out that Everett was the real inside man at the laboratory, that he had been in charge of the block diagrams for the Whirlwind I machine, and that he had the complete confidence of Forrester.

Professor Sam Caldwell was my thesis advisor. Caldwell was a helpful advisor and my personal relationship with him was good, but to outside appearances he was not a happy man. He talked freely about his belief that he had not received the credit he deserved for the development of the differential analyzer. He also seemed very negative about what was going on at Whirlwind. He seemed to feel that some of the people at the Digital Computer Laboratory were against him. I never did understand his negativeness, but presumed it could have been due to a serious physical ailment he suffered at the time.

I also encountered criticism of the Digital Computer Laboratory from another unexpected source. In the course I took from Caldwell on logical design and Boolean algebra, we had used a book that had been produced by the Harvard Computer Laboratory, under Dr. Howard Aiken. During my final semester (spring 1952), I thought it would be profitable to take a course on digital computers from Aiken, who had developed the Harvard Mark I selective sequence calculator. I went to visit Aiken to discuss my taking his course. We met in a conference room that he chose to use for the interview. I did not see his office, which was alleged to have his desk set up on a platform, so that his visitors were forced to look up to him. He spent most of our time criticizing what was going on at the Digital Computer Laboratory and at MIT, and it became apparent that he was not enthusiastic about my signing up. The gist of Aiken's criticism of Whirlwind seemed to center on his estimate of the reliability of vacuum tubes and the large number of vacuum tubes in the system. He used these numbers to show that the tube reliability was such that the mean-time-to-failure precluded any useful time for the machine; that is, the failure rate would be so high that the machine would not run long enough to do any useful work, and besides, even if it did work, there weren't any problems requiring the projected capacity of the machine. I understood from discussions with Taylor, Forrester, and Everett that he had used the same arguments with them.

The work that was done by the people at MIT, including the work that I did, convinced Norm Taylor and his superiors that transistors were too early in their development to be applied to a production machine in the short term. As my thesis work came to an end, I had become more and more aware of the other project activities within Group 62. Taylor's Whirlwind II Group 62 was set up in the Whittemore Building on Albany Street in Cambridge, a stone's throw from the Barta Building where the

Whirlwind I was being finished. The Whittemore Building had been a shoe polish factory and in the early days there were still signs of shoe polish on the walls and on the floors, as well as a steam-powered engine with a ten-foot flywheel in the center of the building.

I graduated in June 1952, just a few days after our third child was born. Some time earlier I had been looking for a full-time job. Staying on where I was seemed to be a good idea because by this time I had established a reasonably good reputation with Taylor, Everett, and Forrester. I remember Everett offering me some small monthly amount to join the laboratory. I was surprised that the offer was so low, and I pointed out to Everett that I had been making almost as much before I decided to return to college and to study engineering (I had studied architecture before the war). I learned later that Everett was surprised by my attitude since his own salary wasn't much greater than what he offered me. But I liked the people, I was establishing a reasonable reputation in the laboratory, and it was clear that the air defense program was going to be a significant one. I was learning a lot about digital computers and I was contributing, so I took the job.

My first assignment on a full-time basis was with Dave Brown, who had been responsible for the control subsystem in Whirlwind I. He had also been responsible for some tube reliability studies. I had just finished the course on logical design and Boolean algebra with Caldwell, and Dave didn't know what to make of me. I tried to apply Boolean algebra to the problems that cropped up in the logical design of Whirlwind II, and I covered sheets of paper with equations trying to make the algebra work. Although I could describe mathematically what logical design existed, it didn't help much in the design process. Dave Brown was shy, but it was clear that he didn't think much of the product that came from all of my algebraic manipulations. I finally dropped the project.

Dave was a continuous cigar smoker. I asked him once why he smoked so many cigars, and he replied that when he went away to college, and he was lonely, he found that smoking a cigar reminded him of home and his father, and this tended to calm him down in times of stress. After that conversation, I would always pick up a cigar at any banquet where they were passed out and bring it back for Dave.

I didn't work very long with Dave. Shortly after I had been hired, he was assigned the task of overseeing the production of cores for the random access core memory, and I continued my work under Norm

Taylor. My experience with Group 62 gave me a broad base of pertinent computer component know-how which, in turn, opened doors to knowledge of other components and subsystems that were also part of the overall computer system. It was an excellent preparation for subsequent jobs assigned to me, among which was the design of the arithmetic element.

C H A P T E R 8

DEFINING THE WHIRLWIND II ARITHMETIC ELEMENT

After the transistor work was completed in early 1952, I was named section head with the responsibility for defining the arithmetic elements for Whirlwind II. We began by looking at a cross-section of the computers that had been produced to date — the Whirlwind I, the SEAC, the Remington-Rand machines, the IBM 701 and others. We knew it was essential to the air defense problem to increase the number of radar tracks which could be processed within a certain time frame. In order to maximize the tracks covered, it was essential to minimize the processing time, to the point at which the only limiting factor would be the read/write cycle of the high-speed core memory. We had some statistics on user programs for the air defense problem and realized that in the programming for the Whirlwind I computer, each of the operations in the instruction code could be executed within the high-speed memory cycle of the computer — except for multiply, which took up about 10% of the air defense program code, and divide, which was used much less frequently. Our task, then, was to minimize the time required for the multiply instruction, which would also benefit the divide instruction. Since multiplication involves a series of additions, we were led to the design of the fast adder. My section took charge of its design. It depended on a circuit which, when pulsed, propagated from number column to column as fast as it could be transmitted, automatically shifting the carry over one column. It was a new idea. We, in the arithmetic element group, along with our IBM counterparts who would join us later in this work, would be named as the inventors in 1954. Like most patents, the title didn't seem to describe the work we were doing. The title of our patent was, "Digital Computer with Inherent Shift." For one brief, shining moment, the Whirlwind II arithmetic element was the fastest in the free world.

I found I was helped greatly in the design task by the experience I had had prior to coming to MIT — teaching in television trade school and, before that, as a chief electronic technician's mate in the Navy with responsibility for a part of the electronic technician's mates curriculum,

including teaching a course in special circuits, particularly pulse circuits. I was also familiar with radar and with direction centers as they were designed for the Navy (combat information centers), so, shortly thereafter, when the Digital Computer Laboratory was integrated into Lincoln Laboratory, and the air defense system became the central concern, I had the necessary background. A significant number of the staff of the Digital Computer Laboratory had backgrounds similar to mine: that is, they had learned about radar, communications, and radar direction finders in World War II. Most of them had also been in the Navy in the electronic technician's program, where the ideas of flip-flops, memory, pulse circuits and delay lines were familiar ground. I found out later that hiring people with a background in military electronics was a conscious policy that Everett and Forrester had adopted.

While the recruiting philosophy of the DCL stressed hands-on problem solving at the expense of some of the theoretical niceties, most of the staff members had master's degrees in engineering, including Jay and Bob. But there was always a vague, subconscious feeling that a greater number of Ph.D.'s would be helpful in counteracting some of the criticism from our government sponsors, and that it might eliminate some of their continual investigations. And so, we were all pleased when Jay was made an honorary Ph.D. by his alma mater, and there was a period during which the "Dr." in his title was stressed. However, most of the people who were hired to fill the need for Ph.D.'s did not survive, because their more abstract work could not be applied effectively to what was essentially a large engineering project. Sometimes, people would assume that those of us who did not have Ph.D.'s did in fact have doctorates. This would happen occasionally at an event where we were mixing with known Ph.D.'s. If you were addressed as "Dr." and were silent on the subject, those in the group who knew better would brand you as pretentious, but if you corrected the speaker, you were forced to tell him that you were not as well-educated as you appeared. The people who had honorary doctorates had a double problem of explaining that yes, they had a Ph.D., but that it was not a "real" Ph.D. However, if you had enough of them, it didn't matter.

Another source of trained electronics people was the Harvard/MIT radar program, which was open to military people who had had degrees in engineering when they were inducted, especially degrees in electrical engineering. Typical of this was the case of Gus O'Brien, a

circuits engineer who became a great contributor to Whirlwind I. Gus went into the Army shortly after he graduated from the University of Maine. He went to officer's training and then into the Harvard/MIT radar program. By the end of the war, he was a captain in the Army Signal Corps at Clark Field in the Philippines. He had written his professor at the University of Maine, telling him that he wanted to go to graduate school when he returned. His professor urged him to apply to MIT instead of Maine because of the opportunities open there. Gus was in the process of setting up a radio and radar maintenance school for Air Force communicators when he wrote to MIT. His resume was sent to Forrester, who expressed interest in having him join the research assistant's program and coming to the lab.

Gus had always been called John O'Brien, rather than Gus, until he interviewed with Forrester when he returned to the States. According to Gus, near the end of his interview, Forrester asked him what his middle name was. Gus told him it was Augustus. Forrester complained that he already had one John O'Brien in the laboratory, and that from now on he would be known as "Gus." The name stuck even after the other John O'Brien left the laboratory. Gus O'Brien worked for Dave Brown on the development of the control system and the selected flip-flop.

C H A P T E R 9

JAY FORRESTER AND COMPANY

I don't remember exactly when I met Jay Forrester. I believe he was off working on Project Charles when I joined the Digital Computer Laboratory, but I was keenly aware of his presence because of the policies he had fostered in the DCL, one of which was the biweekly report. It was clear that Forrester believed in full communication among the people in the laboratory. Once every two weeks, everyone in the laboratory had to make a written report on what he had done and accomplished during the previous two weeks. All these reports were bound together and circulated. Through the biweekly report we could find out what everyone else was doing, and it also helped us to discipline ourselves by giving us an incentive to do something during that time period that was worth reporting.

My office at the Digital Computer Laboratory was next to that of Forrester's principal man on the core memory, Bill Papian. Papian was also in Group 62, and it was through this coincidence that I became even more aware of Forrester's presence. Papian was working on the feasibility model of the random access magnetic core memory which Forrester had invented in response to the need for a reliable high-speed memory. Forrester's invention replaced the unreliable electrostatic storage tubes with magnetic cores whose magnetic field could be switched and held by the hysteresis characteristics of the cores.

What Papian was trying to prove was that a way could be found to string up magnetic cores so a single core could be reliably selected from the array and in the switching process would not interfere with the magnetic orientation of other cores in the plane. Forrester, of course, was interested in Papian's work, and they had many discussions which Papian would later relate to me. It seemed that Papian and Forrester were opposites in temperament. Papian was a careful, thoughtful, sensitive engineer who saw his problems from every angle, and continually had arguments with himself about the best way to go. Forrester, in contrast, never seemed to have any doubts about anything. This, from what Papian told me, made for a very interesting working relationship. Not only did knowing Papian give me insights into Forrester, but I was also able to see from Papian's

work what the read/write time would be in magnetic core memory, and I gained the confidence that the fast adder and multiplier my section was working on for Whirlwind II was desirable.

I also met another contributor to core memory: Ken Olsen. Olsen was later to go on to found the Digital Equipment Corporation, which evolved from his work on the memory test computer he had developed to test out the Whirlwind core memory. Olsen didn't fit my image of the successful entrepreneur. He came across as a happy, ebullient, practical man, almost evangelical about his work. He was a stout man, large head and shoulders, with a firm jaw, and very large teeth.

Olsen always considered the whole design. He realized that the box the electronics was housed in, the switches that were used, and power supplies were as great a source of trouble as were the working elements, vacuum tubes, transistors, or whatever. When he talked about computers, he made them sound almost metaphysical. But, he was an engineer's engineer — very pragmatic.

Although Olsen had been assigned to design modular test equipment for use in the laboratory, he saw, in the task of testing the core memory, the potential for using these modules in building a general-purpose computer. The computer could test the core memory as well as be a usable machine itself, so he designed what would now be called a minicomputer. The memory test computer found ample use in its time in testing and demonstrating SAGE peripheral devices. It would also be used to do some of the data reduction on data generated in the testing of the prototype SAGE computer, the XD-1.

The modular test equipment (out of which the memory test computer was partially built) was another example of how the Digital Computer Laboratory had committed itself to building standardized test equipment. The first commitment was made in 1948, when Forrester had assigned Harry Kenosian responsibility for coordinating the design and procurement of standardized test equipment that could be used to create logical networks which simulated the environment the tested element would face.

Forrester had instituted other ways of communicating within the laboratory. On Friday afternoons, he invited a large number of his staff to his "teas." At these affairs, his favorite cookies, which he purchased from a local bakery, would be brought in, and people would sit around eating cookies, drinking tea, and talking about anything that came to their minds.

Forrester oversaw this ritual, assuming a kind of fatherly stance, but even then he appeared to be cool, correct, and erect, and to those who didn't know him well, unapproachable. In point of fact, he was quite approachable and very interesting to talk with in person. Forrester had the reputation for being extremely shy in the early days, but he disciplined himself by deliberately placing himself in situations where he would have to overcome shyness.

Forrester was an idea-directed man. He believed in practical solutions to problems and tended to generalize from specifics that were arrived at through empirical means. He gave short shrift to fuzzy philosophical or unfounded theories or approaches which required esoteric mathematics as the basis for their proof. He was also a confident man — confident of himself and of his organization. When Whirlwind I was being criticized for not having enough influence from the mathematical community, his response was to the effect that he really didn't need that kind of participation, but if the Navy wanted it, he would arrange for his own mathematicians and let them see if they could be of any help. Forrester never seemed to take criticism personally. He had the unusual ability to project the consequences of decisions on the overall program, and he was willing to fight for anything he believed in.

Although I had not met Forrester when I joined the laboratory, I was aware that he had gone to the University of Nebraska, and that he was the son of a teacher-rancher who taught in a one-room schoolhouse. I felt a kinship with Jay, having shared the prairie growing up in the Depression — the dustbowl with its jackrabbit boom and its grasshopper scourge, and its loneliness. My first professional encounter with Jay was through the work my section did on the arithmetic element. I had derived a way of placing all existing arithmetic techniques in computers under development in relationship to each other in terms of system reliability and speed. I treated system reliability as a function of the complexity of the circuits (number of components and individual component reliability), and speed was the speed at which they could do a standard multiply problem. Each of the arithmetic elements was given a weight based on these factors. (We designed an arithmetic element using each of the techniques we had observed.) The result of this analysis was a factor in the choice of standard circuits for Whirlwind II.

The few conversations I had with Forrester before I started work on the arithmetic element were usually one-sided, with Jay expounding

on the various schemes that intrigued him, such as marginal checking. When we discussed the arithmetic element and the study my section had done comparing all the currently available techniques for making a multiplier, he was impressed and immediately became interested. I think it was that as much as anything that brought me into contact with the broader concerns of the SAGE program.

C H A P T E R 1 0

SELECTION OF A COMPUTER CONTRACTOR

The idea of engaging a manufacturer to help with the design and engineering and to do the manufacturing of Whirlwind II was implicit in the nature of the R&D mission of Lincoln Laboratory. To achieve this end, a group consisting of Jay Forrester, head of Lincoln Division 6 and of the Digital Computer Laboratory; Bob Everett, associate director of Division 6 and of the Computer Laboratory; Bob Wieser, leader of the Cape Cod System project; and Norm Taylor, head of Group 62 and chief engineer of Whirlwind II, was charged with the responsibility for finding the most appropriate computer manufacturer and designer to translate the progress made so far in the Cape Cod System into a design for the next generation transitional air defense system. This system was to become known as the Lincoln Transition System, and in 1954 was renamed SAGE, for Semi-Automatic Ground Environment.

Early in 1952, this team made a survey of the possible candidates for that task, and chose four manufacturers which seemed to be the best qualified for this role. They were IBM, Remington-Rand (two different divisions) and Raytheon. The team visited all three managements and reviewed their facilities. They established a set of criteria and applied appropriate weighting. They graded these companies on the basis of top management's enthusiasm for the job, technical understanding, ability to get decisions made quickly, and esprit de corps. They looked at the staff, its availability, technical ability and enthusiasm for the project as well as the ability and availability of the support staff.

They also looked at the technical contributions of the companies and their abilities to bring the Whirlwind II from development to production. The team evaluated the production organization, the quality of the field service organization and closeness between field and headquarters for resolution of difficulties in development and production. Finally, they looked into the proximity to MIT and the train travel time to the various headquarters.

Each of the four men on the team made his own assessment, using the weights decided upon before the trip. Using the weighting

scheme they had agreed upon, IBM came out on top and was chosen. The scores were IBM, 1816; Remington-Rand, 1374; and Raytheon, 1067.

During the course of these investigations, the Lincoln team interacted with the highest levels of corporate managements. For example, at Remington-Rand, they dealt with Leslie Groves during the day, and one night they were entertained on the corporate yacht by President James H. Rand. General Douglas MacArthur was there also. He had just joined Remington-Rand as chairman of the board, having been dismissed from his command by President Truman for insubordination. MacArthur, having had no experience with computers, did not talk about the purpose of the visit, but held forth instead on the interesting business of current events. At the end of the evening, MacArthur took the team back to their hotel in his limousine.

CHAPTER 11

IBM BACKGROUND

Out of the four corporations evaluated, IBM was unique in its holistic approach to business. IBM was founded by Thomas J. Watson, Sr., who had a feel not only for the sales and administrative activities of the company, but also for the fact that the employees' work environment was very important to the success of the business. Watson seemed to believe that when he hired someone for IBM, he should be able to count on that person's fitting into the IBM whole life picture: not only was the IBM product expected to be superior, but the individual engineers, production workers, and so on should be a credit to themselves, their families, and their communities. IBM believed it important that not only the workplace be orderly and pleasant, but that the home life of the IBM man also be exemplary. When we were dealing with IBM, it became clear that the IBM man was expected to be more than just a worker — he was part of the IBM family; a family that was dominated by what I called the "Tom Watson Code." There were stories about the IBM dress code — an IBM man who was in contact with the public was expected to dress conservatively — dark tie and dark suit, appropriate shoes, and so on. This was evident at Lincoln, where a large contingent of IBM personnel was assigned to work on the Experimental SAGE Subsector. The IBM people, when initially assigned jobs in the area, would show up at Lincoln in conservative dress, but after a time went native. I suppose they relaxed their dress code so as not to draw too much attention to themselves. But the IBM man lurked just below the surface. The proof was aptly demonstrated on Tom Watson's visits to the laboratory. As soon as his visit was announced, the blue serge suits were retrieved from the backs of closets and appeared, newly brushed, in the Lincoln hallways.

The recreational behavior of the IBM man was also guided. Alcohol was bad for business; consequently, an IBM man was expected to turn down offers for cocktails. IBM provided its people with country clubs, which in turn supplied the IBM family with a vehicle where conservative dress and sobriety were considered the norm, rather than odd behavior, for its members.

“THINK” signs were expected to be seen on every worker’s desk. Watson believed in symbolism of this kind. He also believed in group activities that would have a unifying effect on his family of workers. At one time, the company issued song books containing songs with inspirational themes to its workers. The most important benefit the IBM people had was job security. IBM chose the jobs they took on partly to maintain the level of their work force. Often, they would not bid on jobs where they couldn’t foresee long-term employment for their people. Therefore, an IBM man was not too inclined to jeopardize his job by fighting the code. I was impressed with what I saw.

When the Lincoln committee that chose the computer contractor visited IBM, they were impressed by the esprit de corps of the company, and their decision was positively influenced by it. At the time we became connected with IBM, the family control was loosening up, partly because Tom Watson, Sr. was gone, and Tom Watson, Jr., who was more liberal, had taken over.

IBM seemed particularly well adapted to produce the machine for the SAGE system because of its extensive background in computer development and electronics, applications design, operator training, maintenance, and customer service as well as its large production capacity. IBM had, in a sense, been in the data processing business since World War I when it developed the card data processing system. The card data processing system included key punches, gang punches, vertical sorters, nonprinting tabulators and punched cards. These were leased to companies or were provided to IBM customers for use in their customer service offices. The punched card represented such a major breakthrough in data storage and classification that a short time after its introduction, it found extensive use in business accounting and file and retrieval systems.

After a thorough review of the market, IBM introduced new processors, such as the printing tabulator in 1920 and the horizontal sorter in 1925. Regional IBM offices were set up to promote the service and to maintain the necessary equipment. Much of IBM’s investment at this time was in the education of operators and maintainers, and processor design decisions were based primarily on what IBM could profitably charge its customers to use the machines.

The introduction of the 80-column card and the first accounting machines opened the market for IBM equipment for services in many different business areas. The 600 series machines provided the ability to

add, subtract, and multiply. In 1941, IBM participated in the development of the automatic sequence calculator, whose architecture was developed by Howard Aiken at Harvard. This calculator, called the Mark I, was composed entirely of electromagnetic relays.

IBM's first step into the use of many vacuum tubes was in the selective sequence calculator, which had over 12,000 vacuum tubes and almost twice as many relays. The selective sequence calculator was followed by the card program computer, which was all electronic. By 1950, IBM had a number of data processing machines to its credit, but there was still a demand for more capacity and speed. These demands came mainly from the needs of government projects for calculating weapons effects, and from various scientific problems which were too big for existing industry machinery to handle. So, in 1950, IBM decided to build another electronic calculator, which was called an electronic data processing machine. Thus were born the 700 series machines.

The 701 was IBM's first high-speed electronic data processing machine. It consisted of the 701 itself, which was the electronic analytic control unit, an electrostatic storage unit, a punched card reader, a printer, a punched card recorder, as well as magnetic tape and drum readers and recorders. The 701 was designed to be the first in a series of broad application machines with interchangeable peripheral components that allowed easy alteration and upgrading of the system.

Although the 701 was a typical von Neumann type of machine — a binary, 36 bit-word machine with parallel arithmetic registers and 32 single address instructions — it had several unusual features, such as being able to address half-words and the ability to use copy instructions to execute READ and WRITE, thereby allowing computing between the reading and writing of the columns of a card. The 701 also incorporated the Havens delay unit, a substitution for the flip-flop circuit.

The 701 was an extremely reliable machine for its time. In addition to the high quality of the initial design and the rigid specifications for the vacuum tubes used, the 701 incorporated a cathode-ray tube memory, mounted in eight-tube pluggable units which could be stored on site and which allowed for immediate replacement if necessary. Also, subsystem components were constructed and housed in individual boxes; this permitted extensive individual and parallel testing prior to integrated testing. User programs were interchangeable among 700 series machines; thus IBM could test customers' programs before delivery. The effects of

the SAGE computer's development were first felt in the 700 series machine in the 704, which included a random access core memory of the Whirlwind II kind. Other IBM machines would incorporate some of the other components developed for the SAGE computer.

C H A P T E R 1 2

LINCOLN MEETS IBM

After IBM was selected as the preferred manufacturer for the Whirlwind II computer, the Air Force and Lincoln were faced with the problem of getting IBM on contract. After some deliberation it was decided that Lincoln would provide IBM with a subcontract and, about December 1952, such a subcontract was signed. The purpose of this subcontract was basically to bring IBM up to speed and, in the process, to help both IBM and Lincoln establish their respective roles and working relationship in such a way that both organizations could contribute to their maximum ability. On the strength of this subcontract, and with the expectation that it would be brought on as the prime contractor by the Air Force to produce the Whirlwind II machine, IBM quickly procured an old necktie factory on High Street in Poughkeepsie, New York, and assigned a number of engineers to the task of formulating an assessment of what the job would entail. Consequently, the IBM project was called Project High. The first IBM people on the job were John M. Coombs, Morton M. Astrahan, and Walker H. Thomas.

Within Division 6, Group 62 guided IBM in the design of Whirlwind II, and Group 61 and the Cape Cod System provided guidance in system concepts, data capacity requirements and the computer instruction repertoire. There were a number of common points of reference. Beyond agreeing on the twin goals of speed and reliability, the Whirlwind II machine had to have a processing capacity commensurate with the SAGE system design objective of 300 tracks; it had to generate a certain number of displays and provide guidance and instruction to interceptors. Another common point of reference was the knowledge of the statistics of operational programs, such as the rate of usage of each of the instructions in the order code. These common points of reference gave us a framework which disciplined the feedback to the computer's design.

The Lincoln Whirlwind II team organized itself along several major subsystem lines. There was an arithmetic element section, a memory section, drum design section, and so forth. IBM fell into a similar organizational pattern, and these counterpart groups then began the work

of trying to design the system on a joint basis. The Lincoln group, fresh from its experience with Whirlwind I and the Cape Cod System design, tended to view the IBM task as that of packaging Whirlwind devices so the system could be reproduced easily and quickly. On the other hand, most of the IBM people who were either currently working in the laboratory or had had experience with the IBM 701 computer development believed that the best way to proceed was to use the methodology which was then standard in the company. The first several months were a period of considerable stress on the parts of both teams trying to find complementary roles. In order to meet the tight schedule, which called for delivery of a machine ready for field use in just three years (by 1956), it was essential that the main elements of the design be completed within a year or less.

In the forming of counterpart groups to carry out the design of the Whirlwind II, IBM set up a group under Harold D. Ross as counterpart to the Lincoln arithmetic element section which I headed. Ross' group concentrated on circuitry design and circuitry choices while my section concentrated on overall CPU design. Although my section sometimes interacted with Ross formally, the greater interaction took place among the people around Morton Astrahan, who were doing most of the CPU logical design. His group included B. L. Sarahan, Walker Thomas, and Bennett Houseman. At MIT, my section included, among others, Richard C. Jeffrey, Rollin P. Mayer, Samuel L. Thompson, and Robert J. Callahan.

Astrahan and Thomas had come to Project High from the 701 program, IBM's defense computer project. Astrahan acted as the leader of the logical design group at IBM. He was a bright, aggressive and assured man. Although he was not in a position of great responsibility at that time in the IBM organization, his influence on the computer design was greater than most of his superiors. Astrahan was clearly the most innovative of the people working on CPU design, and he vigorously supported his proposed designs. This aggressiveness at first created resentment in my section, but the quality of his thinking merited the success he was to achieve.

Our first discussions about the design of Whirlwind II reflected our respective organizational backgrounds. We were anxious to develop the techniques we had learned through experience on Whirlwind I. The Lincoln people preferred a variation of the Eccles-Jordan flip-flop as a register element, whereas the IBM people were inclined toward the Havens delay unit. After many discussions, the flip-flop was chosen. IBM

also had developed an electrostatic storage tube that seemed to work quite well, and IBM was advocating its use.

Lincoln people had begun to meet with Astrahan's group beginning in late 1952 — even before the contract was signed. The deliberations of this joint group led it to the conclusion that the single address order code ought to be used and that a 16-bit word length would be sufficient to give the precision and dynamic range required for the air defense job. However, everyone felt that to allow for expanding the high-speed memory capacity from 2048 to 16,384 words, we would need between 11 and 14 bits for address. The operational code would take six bits for the selection of 64 basic instruction code words and the inclusion of automatic indexing in the machine would also require about five bits. This all added up to a single address instruction which varied from 25 to 28 bits, and which was larger than the data word length, 16. There was some talk of word lengths bigger than 16 bits to cover the needs of single address instruction word lengths of 28.

During these conversations, the observation was made that most of the data used by the machine in SAGE was positional data and would be in Cartesian coordinates, having an X component and a Y component (each requiring 16 bits), and that both numbers had to be operated on in the process of tracking, thus taking up a great part of the machine capacity. The suggestion arose out of these conversations that in order to avoid having a 32-bit word length, part of the instruction code could call out a single data register which contained both the X and Y elements of the position data. Examining the consequences of looking at it this way, it appeared that if both parts of the position data were operated upon simultaneously instead of in sequence, you could reduce by perhaps half or more the number of instructions that would be required. This led to the idea of building a dual arithmetic element.

We also studied the payoff of using index registers for stepping through the series of addresses either to bring out the instruction code in proper sequence or to step through the data. The index register basically held address data, which was automatically indexed as the program continued to process blocks of tracking data. We also determined that it would be beneficial if the input/output equipment were asynchronous to the CPU; thus, some buffering would be required between the CPU and the input/output elements. This called for the use of drums, and to save computing

time required an interrupt mode which would search the drums for the next piece of data required when the central computer was free.

SAGE would be the first digital computer system to provide facilities for time-sharing of a common data base and displaying different functions such as tracking, air surveillance, and weapons direction. Actually, the first display consoles were designed to control the operation of the Cape Cod System from the Barta Building direction center. As the SAGE computer prototype, known by now as XD-1, became operational, the location of the Cape Cod System direction center shifted from Cambridge to Building F at Lincoln, which housed the XD-1. These consoles required a major portion of the power consumed by the system.

In the Cape Cod System, the display console image was continually refreshed through the CPU; that is, by a sequence of programs devoted to the task of refreshing the picture before the characters had decayed due to the lack of persistence of the phosphor. This method of retaining the picture was very costly in its use of the CPU's arithmetic element, so a set of magnetic drums was added to be used in the task of renewing the display at three-second intervals. A problem concerning the display console was its layout. No one had ever built one before. Determining the best angle for the face plate covering the display tube itself was the subject of concern of many human factors engineers. Bob Wieser remembers the experiment that Dave Israel designed that led to the answer. A display tube shipping carton was suspended from the ceiling by four strings. The outline of the tube face was drawn on one end of the carton. People were seated in front of the mock-up and were asked to adjust its position by pulling the strings until they arrived at the best height and tilt angle. The heights at the front and rear of the carton were measured and recorded. After many trials, the measurements were averaged; these averages were used in the final design.

Poor readability of the display screen, along with resultant operator fatigue, were major problems to be solved. An elaborate scheme was designed and employed to solve them. The most serious problem was the reflection of the room lighting off the surface of the glass covering the display. Looking at the display was like driving at night with the dome light on, always seeing its reflection in the windshield. Further, the display itself was difficult to read because of the lack of brightness: the phosphor chosen created a blue-green initial color, followed by a yellow afterglow that persisted. To enhance readability, the room light was filtered. This

filtering was accomplished by enclosing the fluorescent room lights in plastic sleeves that fit over the tubes, eliminating portions of the visual spectrum that competed with the flash and the afterglow. The majority of the light from the fluorescent tubes was thus blocked. Even the remaining light that was allowed to filter through was collimated by a honeycombed ceiling. This set-up did not correspond to the specifications used by the Air Force in establishing lighting plans for buildings, and it contributed to Lincoln's reputation for "gold plating" — but it worked.

The work on the displays brought out practitioners of the relatively new field of "human engineering" — a field concerned with the match between machine and operator. Each of the companies involved in the design and fabrication of the various parts of the SAGE system had acquired their own "human factors engineers." These people, who had usually majored in psychology, commented on all aspects of the design, giving particular thought to the interactions between the machine and the man. Lincoln followed the trend by setting up a psychology group under a Ph.D. who had the improbable name of Fred Frick. Frick (who was the nephew of the baseball commissioner of that time) assigned a Dr. James W. Degan to help with the SAGE design. Because there was no standard approach in human engineering and because many of the rules its practitioners followed were subjective, each psychologist tended to follow his own different drummer. This inevitably led them into interminable arguments about how the human fit into the design. It finally got so complicated that the role the human engineers played in the SAGE design was brushed off to an advisory capacity so their arguments didn't disturb anybody. Both the Lincoln and IBM engineers then relied on their own judgment, prodded here and there by advice from our human factors group.

Degan was a short, balding man who had graduated from the University of Chicago with a degree in psychology. He spent much of his time analyzing the procedures that the display console operators were expected to perform. He had had no significant management experience or training. He was largely a passive person who concentrated on acquiring talented people to do the work. He was successful at this, but his passivity tended to turn off his colleagues and his input would often get lost in the internecine squabbles. Degan was competent in his field, although he had trouble with communication within his group and division. You could count on him for thoughtful analysis and good judgment, but not for

recommendations. He had a reputation for failing to show up on time at the meetings he was invited to, and when he was traveling, he would more often than not dig too deeply into the nightlife of the cities he visited.

The Cape Cod display console constructed at Lincoln was a prototype for the SAGE production models. For the SAGE system, IBM subcontracted to Bendix for the hardware, and used the Charactron and Typotron cathode ray tubes provided by Convair and Hughes, respectively. Lincoln people working on the display consoles included Steve Dodd, Charles L. "Chuck" Corderman, and Pat Youtz. The SAGE displays became a basis for their future work on remote terminals. At IBM, the displays were managed by Ralph G. Mork, and contracted through the Bendix Corporation. Mork was a hard-driving project leader who was stationed at IBM's Endicott facility in central New York State, and thus he was somewhat isolated from the rest of the computer manufacturing. Mork was his own man, and, although he made an effort to please his own management as well as the Lincoln people, he ran his own show. People remembered him for the eccentric way he smoked his cigarette. He would clamp the filter between his front teeth and then have to close his mouth over the filter in order to puff at it. It was very disconcerting, and one got the impression that he did it to distract his visitors. He accomplished that.

Although we worked well with IBM on an informal basis and were able to resolve many important design questions, none of the Lincoln-IBM work could be implemented until a formal mechanism was established for making joint decisions.

C H A P T E R 1 3

THE HARTFORD MEETINGS

As a first step toward joint consensus on the design of Whirlwind II, the IBM and Lincoln managements decided that several conferences would be held at a point about halfway between Poughkeepsie, New York and Lexington or Cambridge, Massachusetts. The managements, having sensed the competitive instincts and healthy rivalry of the two teams, decided on a halfway point as a symbol of the evenness of the two groups. Hartford, Connecticut was chosen. These meetings were designed to ensure that there would be an exchange of knowledge and technology between Lincoln and IBM as well as to provide an opportunity for IBM and Lincoln to review each other's work. They were also an important device for illuminating potential problem areas and for introducing people on both sides to each other. About 20 people were designated as a design management committee for the Hartford Meetings, with about ten from MIT and ten from IBM. I was included because I was working on the arithmetic element and the general logic design of the system, and was able to attend all of the meetings.

The first Hartford meeting was held on January 20, 1953. Jay Forrester was the first Lincoln speaker, and he described the background of the program. I thought his speech was a good summary, and it apparently was compatible with IBM's position on the relationship. Lincoln, according to Forrester, was devoted to research and development, and not to production. To get the system into production, it would be necessary to collaborate with industrial companies, such as IBM, which had the necessary administration and facilities to handle the design, construction, deployment, and maintenance that the production of hardware would require. Forrester recognized that this kind of collaboration was unusual; it was a relationship born of necessity. The program was urgent, he said, and a prototype system was required by 1954. He noted that there was no computer in existence — including the Whirlwind I and the IBM 701 — which would be suitable for the transition system as it had been documented in Technical Memo (TM) 20. The existing machines did not have the reliability required for the job nor did they have the specialized

peripherals that would be needed. Forrester noted that the Cape Cod System, which was being put together as an example of the proposed system, could be quickly modified for production in a TM 20-like system architecture; therefore, he suggested that IBM place a representative at the Cape Cod facilities. Forrester concluded his talk with a description of the status of Whirlwind II thinking at MIT and included implicitly and explicitly what Lincoln thought IBM's role should be in the program. Forrester's talk was followed at the conference by discussions of several important topics, such as the current status of the thinking about the arithmetic element, basic circuits, input-output equipment, principles of designs, proposal for the logical design of the Whirlwind II computer, magnetic memory, and core production status. Also discussed were plans for the memory test computer, other magnetic core applications, buffer storage and display, component reliability, and, finally, the schedule.

Near the end of the program, Lincoln's Norm Taylor discussed the schedule. Taylor told the group that Lincoln's objective was to have the prototype computer with its associated equipment installed and operating by January 1, 1955. If seven months were required for final installation, testing and integration of the equipment in the air defense system, and nine months were required for procurement of materials and construction of the model, there would be about nine months left for all the engineering work necessary to prepare specifications, block diagrams, development of basic circuit units, special equipment design, and to do all the other things necessary to permit actual construction to begin. The schedule for all this work was very tight, and Taylor estimated that about 235 professionals were needed on the program right away (at that time, about 20 percent of that number were assigned).

The meeting was concluded with a talk by T. A. Burke of IBM who indicated IBM's current status on the subcontract, which would end in three months. IBM had spent the time so far learning about the project and getting its own house ready to undertake the assignment. According to Burke, 25 people had been assigned to the project, \$130,000 worth of test equipment had been purchased, and the building on High Street which had been procured was currently undergoing certain necessary modifications. The most serious problem that Burke brought out for discussion was the uncertain status of the Air Force prime contract, which he saw as a follow-on to the Lincoln subcontract. The first meeting had been constructive, and set the tone for follow-up.

A second joint meeting was held three months later in Hartford on April 21. The first meeting had resulted in the formation of a number of committees made up of IBM and Lincoln engineers who were to draw conclusions concerning the character of the subsystems. This second meeting was basically a report of the findings of the committees. A great deal of progress had been made on the design of the arithmetic element, the control subsystems, and the magnetic core memory. Good progress was reported on the input-output committee work. It was agreed that better coordination should be achieved in the basic circuit activities, and it was proposed that a power supply committee be formed to make judgments about the design of the power supply. There was also some progress reported on standards. The main results of this meeting were a better understanding on the part of the joint management group of the current status of their deliberations and decisions, a feeling for the status of the decisions that were being made, and the discovery that these were not occurring at a rate commensurate with the needs of the schedule which had been established for the design of the system.

While waiting for the committees to act, Forrester, Taylor and Everett tried to get a better fix on what had to be done to assure the schedule. In their zeal to obtain this knowledge, they analyzed all of the activities that went into production, often to the annoyance of the people responsible for these activities. In talking to the IBM man responsible for supervising the factory, Forrester apparently pushed too hard, for he was told, "You run your —ing university and I'll run my —ing factory!"

On May 21, 1953, the third Hartford meeting was held, this time to deal with packaging of Whirlwind II. Much of the meeting was spent considering the standardization of pluggable units. It was agreed that the mechanical design group should proceed with the design of a six-tube pluggable unit, but at the same time continue with the design of the four- and nine-tube units in case they might be needed. One of the problems in pluggable unit design was to maintain an even, cool temperature around the vacuum tubes. An important innovation, the brainchild of Norm Taylor, took advantage of the fact that the pluggable unit racks could be stacked up to form a plenum for cool air under pressure. The air was forced in around the phenolic component boards inside the racks, and then escaped out into the room through the annular rings at the base of the tubes, thus cooling the tubes uniformly. Another meeting on packaging was held on June 1, 1953. More time was spent on standardization of pluggable

units, and sketches of the pluggable unit design were shown. Also, the design of the central machine was described.

These Hartford meetings provided opportunity for an exchange of information, acted as a catalyst for initiating action, provided the means of identifying overlooked aspects of the machine, and served as a forum in which people could interact on a personal level. By the time of the fifth and last Hartford meeting on June 15, a *modus operandi* had been established between the IBM and the Lincoln staffs.

John Coombs was the senior IBM person assigned to the project. He had earlier been hired from Engineering Research Associates in Minneapolis and now held the position of project leader of Project High. Coombs was a gentle man — not a very dynamic person. In fact, some at Lincoln thought him a little too passive. I remember talking to him once after there had been a clash of personalities on the arithmetic element design team. We had been pushing for the inclusion of the fast adder into the arithmetic element, and one of the IBM people, a man named Richard Richards (who had changed his name recently from Richard Steinberg), insisted that we did not have time to think about including something new. (This struck me as particularly ironic at the time, because in the room in which we were working at IBM, all of the desks had THINK signs facing out at us.) Richards was acutely aware of the short time schedule, and he was advocating replication of the best known design. I lost my temper because I felt he was obstructing the progress of the meeting, and I went to Coombs to complain about his behavior. Coombs, in his philosophical way, compared the situation to what goes on on the stage: We are all actors, he said, and each of the roles we play is a role forced on us by the circumstances we find ourselves in. I thought the exchange told a lot about Coombs' passive nature.

IBM seemed to have the knack of balancing its management teams. Coombs reported to a man named J. E. Zollinger who was at IBM World Headquarters in New York. Zollinger was a real go-getter: an aggressive, rough-cut sort of man who was noted for his salty talk — not at all a typical blue suit, white shirt, black tie IBM man. His contracting philosophy could be summed up in a statement he himself once made: that he always gave his customers a little of what they wanted so he could sell them what they needed. Zollinger usually attended the high-level meetings between Lincoln and IBM when SAGE was being discussed, but, when he was unavailable, his assistant, Glen Solomon, attended in his place.

Solomon's personality was in direct contrast with the brash personality of Zollinger. He was the real statesman of the contracting business: smooth, bright, diplomatic, and adept at smoothing the feathers Zollinger regularly ruffled.

Much had been accomplished during the Hartford meetings, but in the judgment of the Lincoln management, decisions had to be made faster if the schedule was to be met. There was not enough time to really study and contemplate the alternatives available, so choices had to be made primarily on the basis of the experience the individuals had already had with the subject area under consideration.

C H A P T E R 1 4

PROJECT GRIND

In order to actually produce an experimental prototype by 1955, Lincoln and IBM managements established an effort called Project Grind. Project Grind took the form of a series of meetings held back-to-back, essentially to grind out the necessary design decisions. This exposed many of the differences in approach of the two groups, but it also established a forum in which alternative choices could be examined in some detail. There were seven Project Grind meetings between June 24 and July 15, 1953, resulting in an embryonic system design with issues identified and people assigned to address those issues. I took part in all the meetings, and remember them as productive. But as the meetings went on, the need for a formal mechanism for design approval became obvious. It also became clear that not only IBM and Lincoln Division 6 were involved, but the rest of Lincoln, other associated contractors, and various parts of the Air Force were involved as well. Not only did the computer have to be manufactured, but changes had to be made in the communications system, the computer program, the radar sites, and the connections with Nike ground-to-air missiles and interceptor bases. Additionally, certain changes were needed in the relationship with the Army concerning anti-aircraft systems, with the Navy concerning picket ship inputs, and with the Civil Aeronautics Authority (the predecessor of the FAA) concerning air traffic control matters.

By this time, IBM had a prime contract from the Air Force. In order to identify the machine under design with IBM as well as with MIT, it was decided that the Whirlwind II name would be dropped in favor of Air Force nomenclature, and the system was given an Air Force number, AN/FSQ-7. This designation would be informally shortened to "FSQ-7" or simply "Q-7." An AN/FSQ-7 planning group was identified, with members drawn from both IBM and Lincoln.

The procedure that was followed in the Project Grind meetings consisted of taking subsystems one at a time and forging whatever decisions could be made based on existing background and knowledge. Minutes of the meetings were taken in order to put on record some of the

decisions as they were made and some of the reasons for those decisions. Any problem could be brought into the open and discussed so that decisions could be made as soon as possible. Toward this end it was also agreed that everyone should feel free to present even tentative plans for various parts of the system, with the general understanding that they were tentative. Any errors or omissions would be called to the attention of the authors of the minutes.

The first meeting was devoted to the radar inputs. Slowed-down video inputs, video mappers, and slowed-down video input registers were discussed, and a general design description of the input registers was agreed upon. The subjects of the second meeting, held June 25, were marginal checking, power supplies and magnetic memory. At the third meeting on June 26 on magnetic drums, it was tentatively agreed that there would be six fields per physical drum, and five physical drums in the computer. The fourth meeting, held June 30th, was concerned with output display systems. A display rate once every two seconds was tentatively accepted. It was agreed that there would be 16 words per track, allowing for display of the history of all tracks. It was also agreed that the system was to operate with a minimum of degradation even when only one computer was functioning. The fifth meeting, July 1, was concerned with cross-telling, output drums, output links for digital information on a display maintenance console, and mechanical design. At the sixth meeting, July 2, the concern was standard circuits and the action of the Standards Committee. All tube types to be used were definitely approved, and it was decided that one-tenth microsecond pulses would be used wherever possible in the system. It was generally agreed that a project meeting should be held at least once every other week. The seventh and last meeting discussed mapper subcontracts, cross-telling, review of the drums, paper tape machines, input counters, manual inputs and power supplies. It was agreed that paper tape would not be used in the FSQ-7 unless there was good reason to do so.

Project Grind resulted in fewer decisions than the Lincoln group had considered necessary to meet the schedule, but it had a remarkably positive effect on the working relations of the people involved. It also demonstrated the real need for some means or technique to help people in the process of coming to a consensus on various aspects of the system design in an orderly way, and this at a level of detail which was great enough to ensure that the major outline of the design was visible, but not so

detailed that the judgment of it could only be made after all the drawings and design details had been worked out. It was the need for such means that eventually prompted Lincoln to set up a Systems Office, which I was to head. We did maintain an industrial liaison office under Art Kromer, an experienced administrator hired from Western Electric, and one of the few people on the project with grey in his hair (he had grey sideburns). Art added a touch of maturity to the picture we presented, but the scope of his office was limited in part by his own training — we used to say that you couldn't become a supervisor in the Bell System until you were ready to retire. We called him "Greylocks." He was a pleasant, conscientious man who was not given to initiating action. He might have been the man for the Systems Office had he not felt uncomfortable working in the loose structure he faced in Division 6.

The Systems Office would be aimed primarily at consensus formation, and it was one way that the technology was transferred from Lincoln to IBM. In addition, senior Lincoln people like Gus O'Brien and Ken Olsen were moved to Poughkeepsie to monitor progress and to be available first-hand to apply the lessons learned from Whirlwind I to the design of the FSQ-7. As it turned out, this was an effective means for managing the transfer of technology.

CHAPTER 15

GENESIS OF THE SYSTEMS OFFICE

One of the key results of the Hartford Meetings and Project Grind was the creation of Lincoln and IBM subcommittees to establish the FSQ-7 subsystem designs. The subcommittees forced the integration of Lincoln and IBM engineers, and although each had their own approaches to design, the groups were surprisingly harmonious and most of the time were in agreement on the design of particular FSQ-7 components and subsystems. However, a problem that had not been solved by the creation of subcommittees was the seemingly simple matter of the routing and signing off of the designs through those groups that would be affected. Even though subcommittee members felt they had achieved a reasonable design, their work still required some official signature or announcement to give it status so that the necessary contracts and work statements could then be properly written.

Although several organizations were directly involved, the responsibilities for design management did not fall plainly into the lap of any one of them. IBM, Burroughs, and Western Electric were prime contractors. The Air Force was just starting up its System Program Office in New York City, while the Air Defense Command maintained a planning group at its headquarters in Colorado Springs, and an ADC contingent was housed at Bedford. Additionally, Lincoln's management interpreted Lincoln's charter to mean that Lincoln was to be research-oriented only, and that it was not to be an architectural engineering firm that might have taken on the design management task.

In the case of the FSQ-7's basic circuits, whose designs were reaching completion at both Lincoln and IBM, I took the initiative myself, after consulting with Taylor and Everett, to gather together all of the information and design justifications which then existed on the circuits. I solicited inputs from the Air Force and those contractors who would be affected by the design, and spent a great deal of time getting these people to try to arrive at a consensus. Since effecting this consensus required so much time and effort and great patience, in this area we also relied heavily upon several other people, including Richard L. Best and Ron Mayer.

Dick Best was the principal Lincoln Division 6 circuits man; he had contributed significantly to the selection and design of the Whirlwind I basic circuitry. For the FSQ-7, he took part in both the design and the evaluation of the basic circuits. An uncomplicated and dedicated person, he was rather tall, very slightly built, and he spoke in a very soft voice that nonetheless carried considerable authority based on past experience and performance. He had been a great help to me when I was working on transistors, and even more of a help while we were trying to settle on the basic circuits. People liked to have him on their projects because of his guilelessness and lack of game-playing. He had achieved a certain notoriety around the lab for a book that he and his wife had published on folk songs of America. What could have shaped up to be a partisan fight between IBM and Lincoln became a competition in performance of the candidate circuitry. It was so clear that Best was objective about his choice of circuits that both IBM and Lincoln trusted his judgment.

Ron Mayer was also very important to the Systems Office process. I had heard about Ron Mayer before I ever met him. It was said around the lab that Ron could draw the block diagrams of Whirlwind on the head of a pin. Since Ron had worked on the Whirlwind I block diagrams for Bob Everett, he was assigned to me because I was then responsible for the FSQ-7 arithmetic element and for overseeing the logical design of the central computer element. Ron worked out a set of symbols for all of the logic elements used in a computer: registers, gates, and so on. He had devised a way of outlining the whole computer in terms of these symbols and block diagrams, so that the entire logical design could be reproduced on an $8\frac{1}{2} \times 11$ sheet of paper. Ron lived and breathed computers. He became famous for writing the first program to play music on the Whirlwind, and he also programmed some of the digital graphics used in the early computer program demonstrations. During one of the design reviews of one of the FSQ-7 elements, I shared a hotel room with him. He loved to talk about where the computer business was going, and had a vision in which computers would take over the majority of functions performed by humans. This take-over would become so pervasive that people would lose their ability to perform any functions that computers could perform. In the final analysis, he said, it would come down to a moral question of whether or not the plug should be pulled on the computer; because, if this were to be done, man would then be without all those functions around which he had built his life.

In the absence of a designated authority within Lincoln or the Air Force, the first principle of behavior for the Systems Office was to shun the appearance of representing oneself as authority. So, with this in mind, I began with the easy decisions, those that were well within the scope of Group 62. We gathered together all of the backup documentation we could, including the design proposed by the appropriate subcommittee. We then did an analysis of the design, and proceeded to draw conclusions which were primarily consensus-oriented — that is, conclusions that tended to be the least unacceptable to any of the affected parties: Division 6, Division 2, the Lincoln Project Office, the Air Force and participating contractors. I then wrote a brief, summarized the conclusion, formulated the action to be taken by all of the parties and solicited the signatures of those who were in a position to block the action.

Our own organization of a group to handle the design control problem prompted IBM to set up a counterpart group, which was called the Engineering Design Office (EDO) and was manned by three senior staff people, and reported to Robert P. Crago who would shortly take over the whole project. Although John Coombs was the IBM project leader to begin with, the slippage in the rate at which decisions were made at IBM prompted him to take on Bob Crago, who then had charge of the IBM drum group, and to put him in second place under himself. Crago was an ambitious, quick-witted engineer who was willing to do what was necessary to ensure that IBM met its schedule. He took on the job of trying to gain control of the IBM design process and matching it better with the capabilities of their production departments. Crago, in order to provide himself some running room, would not be tied down by a nit-picking review process. He insisted that the less important decisions should be the prerogative of the IBM people in charge of the subsystems; indeed, much of his time was spent loosening the grip of Lincoln on IBM operations. In general, Lincoln agreed to go along with him as long as IBM provided reasonable feedback. Crago rose in the IBM hierarchy until he was given practically full charge of the operation and then, finally, replaced Coombs. Largely due to promotion by Forrester and Everett, he would be named the outstanding young engineer of the year in 1957 by Eta Kappa Nu.

It was relatively easy to prepare briefs for the basic circuits, the instruction set and the arithmetic element, because these were computer problems and the computer expertise was clearly in Lincoln's Division 6. I knew who was involved and who had the authority to make design

choices. But a difficult “political” situation existed between Division 2 and Division 6. Although my procedure brought together all groups affected by a design, it could not become formalized immediately as part of the standard Lincoln procedures because of the tension between Forrester and Valley and their two divisions.

Nonetheless, several more briefs were successfully produced. Those of us in the Systems Office involved in their production made conscious attempts to gain the confidence of all the participants. We strove to be objective and not to take sides in the design. We did not give ourselves titles and we worked around the line structure. Rather than operating formally as a whole division ourselves, we worked within other divisions and groups — we sort of floated in a sea of ideas, special interests and proposals, and our unobtrusiveness was effective. It wasn’t until 1957 that the Systems Office attained enough history of successes to be recognized as a Lincoln group.

All this is not to say that a near-optimal result was always achieved. The example of the control panel is one of the exceptions. The Lincoln and IBM designers of the FSQ-7 were influenced by their own respective backgrounds, having built machines that had different objectives. The IBM people had developed a series of machines (directed at business activities) using large numbers of vacuum tubes, which did not require the reliability and the speed needed by a real-time control application. In addition, they tended to be satisfied with the reliability they could achieve using more or less standard radio and radar circuitry. Lincoln’s concern for the reliability and speed needed in real-time applications led them to employ test aids, such as machine-directed marginal checking.

To make it easier to isolate programming and machine errors, every register in Whirlwind was displayed on the control console. Two neon bulbs indicated the presence of a one or zero in each of the individual digit registers. The IBM people, on the other hand, tended to depend on printouts of register contents. The trouble with that was that machine errors and programming errors both contributed to the printout, so it was difficult to sort out the two. In the design of the FSQ-7 control console, there were many arguments about the cost benefit of the very large number of neon lights and their controlling circuitry. Lincoln prevailed, however, with the concession that one light per digit register would be satisfactory. With these lights the machine operator and the programmer could step through the program and see where the error or failure had occurred. After

the design was fixed, everyone was amazed by the size of the control console. It was about 15 feet long and three or four feet high and was covered with lines of lights which could be used both by the programmer and the maintenance people. The final judgment was that such additional costs, though justified in the experimental XD-1 machine, would not really be necessary in the production machines. Nevertheless, all FSQ-7s were built with this control panel.

Once our procedure on preparation of briefs was firmly established, it was institutionalized and extended to the subsystems which were outside the responsibility of Division 6. The procedure resulted in a document which was later to become known as a TIR (Technical Information Release), the mechanism for reporting to the Air Force what the design was and what should be done with it so that it could work in concert with other contractors' designs. The TIR was a package which contained the design specifications, pertinent background to the design problem, analysis papers, supporting material for the recommendations, and of course, the recommendations themselves. The TIR was delivered to the Program Office, where it was accepted as the formal input upon which the Air Force could then act. At the Systems Office, the TIR moved up through a list of selected signatories, those who had to sign off before the Air Force would take action.

Part of the success of the operation of the Systems Office was due to careful attention to the words that describe what the Systems Office did. What it did came close to what is now called configuration management (we called it design control). We called its output a "Technical Information Release," thus avoiding the question of whether it was directive or not, or of who was in charge. It appeared on the surface that this was advice to the military and had no force except that which the Air Force chose to give it. Some people felt that there should be a design group or person responsible for directing the design. However, we could not put out a directive because that would force the organization to determine who was in charge. There were people at various levels in the organization who felt they needed this authority in order to take responsibility for the result. There were many others who would not subordinate themselves. Although the Air Force had all the authority it needed to designate someone in its own organization to be responsible for the design, this responsibility was precluded by the fact that the Air Force did not have the technical capability to effectively monitor the design as it progressed, much less initiate it.

So for every design decision which was made, a list of opinion leaders was drawn up. This was a list of people who were expected to concur on the contents of the technical information release. We dug up the word "concurrency." The words "direct" and "control" were too strong. "Oversee" and "coordinate" were too weak. But concurrency had the right feel — it gave everyone who was named as necessary to support the technical information release the implied power to veto what was being included in the TIR. The word "concurrency" provided a dignified way for the principals who managed the system to effect changes without answering the question of who was in charge.

What had begun in late 1953 as a diplomatic method of reaching design consensus thus ended up as a full-fledged Lincoln group devoted to Design Control. I feel that some of my own best work went into the preparation of those early briefs; work which is not as evident in the subsequent documentation as was work of far less influence and importance. The success of this deliberate deemphasis of authority was actually a kind of testimony to the effectiveness of Taylor's, Forrester's and Everett's private and pragmatic solution to an otherwise difficult and touchy issue. The Systems Office taught its participants, as well as many in the organization, a form of diplomatic maneuvering which, in time, would be accepted as a normal way of doing business.

While these approval procedures were evolving, an arrangement had been made in which Jay Forrester's signature was required for final approval before a design could be presented to the Air Force. Although the IBM team respected Jay, he often tried their patience. He, Bob Everett and Norm Taylor had a way of spotting design flaws — especially those resulting from the fact that the FSQ-7's various subsystems had been designed by different groups.

Several times during the design of the FSQ-7, criticisms by these three men led to major design revisions. Jay, especially, felt free to comment on anything, and often he angered those whose work was under criticism. Most of the time, his suggestions were well-founded and IBM complied, but occasionally his suggestions seemed to IBM to border on the whimsical, especially given the realities of production and schedules. In one case, he called for extensive "aging tests" to be performed on the circuits to be used in the SAGE computer, arguing that when the computer was running, the little-understood phenomenon of "silver migration" might occur, wherein the silver which lined the holes of the phenolic

boards would spread or migrate across the surface of the boards. Since this was possible, he argued, it was also possible that migration could short out the circuits and crash the whole system. IBM's position was that running the tests to see if this phenomenon might take place and threaten the circuit board design integrity would be disastrous to the schedule of the project. Further, they said, the system would have to go through a major redesign, and that an automatic circuit board assembler which had been contracted to General Mills would have to be modified as well. Forrester still insisted the tests be run. At that point, IBM had its own engineers look into the technical risk, and they satisfied themselves that no problem would occur. Forrester accepted their analysis with good grace, and the tests were never run.* As can be imagined, Forrester drew mixed reviews from the IBM staff; their reactions to his varied inputs ran the gamut from "pronouncements from Mount Sinai" to "pain in the neck."

*As it turned out, the migration problem never presented itself.

CHAPTER 16

FROM BOSTON TO POUGHKEEPSIE

One of the big problems in getting together with IBM was the time it took to get from Boston to Poughkeepsie. It took the better part of a day to drive that distance over poor roads, and it took even longer to take the train to New York and then on up to Poughkeepsie from New York City. Lincoln owned a fleet of cars, mostly 1950 Plymouth station wagons, which were by then almost worn out. Those of us who traveled to Poughkeepsie every week would take these cars, although they were uncomfortable and unreliable. I remember one incident when Everett and Taylor and I took one of the Plymouths to Poughkeepsie. I was driving and I had a very bad time of it. The next time, instead of taking a Plymouth, Everett decided to rent a car. This caused quite a stir with Harris Fahnestock, who was then the head of the Administrative Division of Lincoln Laboratory, but it did give Everett the opportunity to demonstrate the need for better transportation.

My friendship with Bob Everett grew naturally out of respect for his intelligence and his unassuming manner. In his work he had a way of seeing problems clearly and going right to the heart of the matter, of examining the extremes and identifying the range of alternatives in such a way that decisions could be made. Our professional friendship was overtaken by a personal friendship during the course of the trips to Poughkeepsie, when we would engage in a kind of banter based on the differences in our backgrounds. Everett's father was a successful civil engineer who owned his own New York firm which had hydraulic projects in many parts of the world, from South America to Poughkeepsie where he had designed the water works. Bob had lived in New York and in Florida while he was growing up; he had tutors and went to private schools. In contrast, I had grown up in North Dakota. My father was a grain buyer and we lived in a small town with three or four other families. I walked two miles to school to a one-room schoolhouse, and carried my .22 rifle with me and kept it in the cloakroom. Going to and from school, I used the rifle to shoot jackrabbits which I later sold for ten cents a hide.

During our travels to Poughkeepsie, I used to make up stories,

some partially founded in fact. I told Bob that I left home to go to a meat boner's school, and that when I left, all the townspeople gathered at the railroad station, and the band played as I went off to make my fortune. I said I came from Yellow Jump, North Dakota — a fictitious place, but with characteristics similar to those of the place I had actually come from. And I described the reaction of the people in the town to my career: when I graduated from MIT with a master's degree in engineering, the local paper in our town had a small article with the headline, "Local Man Completes Electrical Course in the East."

When Everett discovered that I was a terrible driver, I pointed out that in North Dakota I had never learned curves. In fact I didn't have to learn anything there, because the roads were plain dirt and once you got into the track, all you had to do was work the accelerator to get where you wanted to go. When he found that my spelling was bad (it seemed to him that nobody could be that bad, and that I must have been putting him on), he tended to treat it as a lovable eccentricity, rather than as a sign of chronic illiteracy. Anyway, the trips cemented our personal friendship. We had fun together.

Later, we began using chartered aircraft. It was not much more expensive than driving and it got us to Poughkeepsie in an hour as opposed to five or six hours on the road. We got our first taste of air transportation when we were stuck in Poughkeepsie without a car, and with no car rentals available. A local pilot volunteered to fly us home to Massachusetts in his Grumman Goose. We were pleased to see that it had two engines, but this complacency soon disappeared when we were told that the plane could not fly on just one engine. The fact that there were two engines increased the probability that you would go down. However, it was a successful flight, and spoiled the Plymouth wagons for us.

After that, we started flying with a man named Leo Kerivan who had a Beechcraft Bonanza, one with a V-shaped combination rudder and elevator. Kerivan had just retired from the Navy and was trying to make a business out of executive transportation. Our first round trip with Kerivan was quite an adventure. Richard Fallows (another engineer from Group 62) and I had taken the plane to Poughkeepsie. It was a very cold below-zero day, and after a short while it became clear that the internal heater wasn't working very well. Kerivan was upset about this as we were his first company sponsor, but aside from being quite cold, the trip was good by comparison to the automobile alternative.

The return trip, however, was a different story. Everett had joined Fallows and me in order to make his own evaluation of the service. As we passed over Worcester, a grinding noise was heard coming from the direction of the engine. Kerivan, already concerned about the sale of his services because of the lack of heat, told us the noise was a frozen tachometer cable. He explained that the tachometer cable had frozen up before, but that he thought he had fixed it. He told Everett to reach down under the instrument panel and disconnect the tachometer so it wouldn't annoy us any longer. Everett unfastened his seat belt so he could reach under the panel. As he moved forward to do this, his shoulder caught the door handle and the door flew open, immediately exhausting the little warmth there was in the cabin. The door on the Bonanza is situated in the slipstream so that, once opened, the door is held open about six inches. To close the door, it is necessary to pull the door back against the slipstream. Kerivan tried to appear nonchalant as he reached over Everett and caught the leather strap that acted as a door pull, but when he pulled on the strap and it broke, so did Kerivan's composure. He tried once or twice to pull the door shut by other things he could grasp on the door but to no avail. Meanwhile, the below-zero temperature was chilling us to the bone. Finally, Kerivan put down the flaps, slipped the airplane in the direction of the open door, and at last was able to get it closed.

We landed at Hanscom Field, but not without further incident. After landing, and as we were about to turn off the main runway, the engine stopped and Kerivan wasn't able to get it started again for several tries. He finally did, and he brought us safely up to the terminal. But by this time Kerivan was one dejected would-be executive flight service person; he must have felt certain he wouldn't get the job. In spite of that first experience, we used his services for a long while afterwards. Later, we were able to supplement Kerivan's services with IBM's Aero Commander, but only when our travel schedules meshed with IBM's. Some time after that, the Steering Committee at Lincoln decreed that MIT personnel could only fly in two-engined aircraft, so at that point we had to stop using Kerivan's Beechcraft Bonanza. One evening leaving Poughkeepsie we had two engines, but an inexperienced pilot. As we flew over the hills of Connecticut, the visibility declined to the point where we could not see the ground. You could look down and see the surface within a very small radius, but you couldn't see anything forward or aft. The pilot could not seem to get a fix, so he went looking for familiar terrain. I

remember trying to help him figure out where he was while we flew over Route 128. Finally the plane was within a short distance of Boston's Logan Airport control tower. He could hear the tower, but could not get in touch with it. Unlike most pilots, who are trained to be cool, this one was May Day prone. After yelling "May Day" into the mike, he finally got their attention and was patched into the Boston tower. We ended up circling the control tower at Logan for 15 or 20 minutes until traffic lightened up and we could be brought in.

Our need for air transportation continued, however, and finally, Lincoln, in cooperation with Wellesley Travel Service, bought a two-engine, ten-passenger DeHaviland Dove, which we used for several years. Eventually, though, the pilot of the Dove began to complain about skimpy maintenance, and on one flight — in fact the last flight — he blew an engine on take-off, and that was the end of the Lincoln-sponsored executive aircraft service.

CHAPTER 17

FEATURES OF THE FSQ-7

It took IBM only two years from the time it was awarded its first production contract in February, 1954 to the time the first production system was accepted in its manufacturing test cell. From then, it was only two more years until the first production system was declared operational at McGuire Air Force Base, New Jersey, in July, 1958.

As the first real-time control digital computer, the FSQ-7 incorporated many innovations which gave it capabilities unparalleled in its time, and which broke important ground in computer technology. It used 49,000 vacuum tubes, weighed 250 tons, and required three megawatts of power which was provided by five very large diesel motor generators. Heat generated by the computer had to be neutralized by a gigantic air conditioning system. The system down time, not counting power supply or air conditioning, was approximately four hours per year.

Far and away the most dramatic innovation for the SAGE computer was the core memory. From the initial 32×32 planar array of cores incorporated in the memory test computer, the technology grew until the final core memory incorporated into the FSQ-7 was a 256×256 unit (65,536 words). By this time, Lincoln had developed new methods, such as automatic core testing and a core plane stringing process, which allowed reliable and inexpensive core memories to be manufactured and tested in production quantities. The FSQ-7 also incorporated a dual arithmetic element which permitted simultaneous processing of both the X and Y coordinates of the data, thereby giving the computer greatly increased speed.

The FSQ-7 was also among the first computers to use time-sharing for real-time control problems. It was among the first to extensively utilize cathode ray display consoles and light guns to direct pertinent information from the screens to the computer. The SAGE computer used the Convair 19-inch Charactron tube for PPI display and the five-inch Hughes Typotron for textual display. Digital transmission over phone lines, originally developed at CRL, was pursued for the FSQ-7, and it was Division 2 that designed the first modems to convert digital data to

and from analog data for phone line transmission. Introduced for the FSQ-7 was the input/output (I/O) break, or memory cycle stealing, that allowed computation to continue during input/output operations. The FSQ-7 also incorporated a form of associative memory access: a buffer drum in which data from different sources was tagged, thereby allowing the computer to select, at a later time, all the information from a particular source. The branch and index instruction, developed for the FSQ-7, allowed instructions to decrement an index register, test for the end of a loop, and branch back to the beginning of the loop.

From the start, the planners of the SAGE system emphasized reliability of components, and it was only through their insistence on reliability and their refusal to accept components that did not meet their standards that manufacturers began to develop higher quality components. Since reliability was such an important feature of the SAGE system, the FSQ-7 was the first production computer to incorporate marginal checking, the procedure for continually monitoring the deterioration of vacuum tubes, so that deteriorating tubes could be detected before actual failure occurred. For the FSQ-7, IBM worked with a contractor to develop a machine that would automatically assemble and solder printed circuit boards, thus greatly increasing their reliability. There was also a central circuit design group for the FSQ-7 which made sure that all CPU circuits met design standards based on component tolerances and compatibility with marginal checking.

A major feature to promote reliability was the duplex computer scheme, which had one computer operational and the other on standby status in case a breakdown or failure occurred. This scheme was substituted for the arrangement described in TM 20, (the first SAGE system proposal) known as the Transition System. The Transition System scheme had called for three computers at three separate locations within an air sector. Each computer would be capable of carrying the full load. The expected operating protocol was that two machines would carry the load while the third would be available for routine maintenance and for substitution in the operation. This plan was scrapped because it was too costly.

In the duplex scheme, two computers were housed in the same building. One machine was active, while the standby machine was used for diagnosing its own troubles while maintaining readiness to take over. This duplex computer plan to attain reliability was rapidly assembled by a team of IBM and Lincoln people. Steve Dodd, working with the

appropriate IBM personnel, completed a design very quickly, causing hardly any delay. The design included a separate drum that was available to both computers. The computer carrying the operational load continuously sent critical operational data to the drum. In this way, if the operational computer crashed, the standby duplex could pick up the essential data and carry out the operation. The most complicated part of the design was the switch. In concept it was very simple, but in addition to switching the CPUs, it had to switch all of the peripherals (mostly the 90 or so display consoles) from one computer to the other.

In 1953, the SAGE project realized the need for support facilities and a development environment for the SAGE production system. It was also recognized that a pre-production model of the FSQ-7 should be built to verify the overall design, including the integration of all peripherals and remote inputs. Accordingly, IBM received a contract to build two prototype FSQ-7 computers, known as XD-1 and XD-2. A new command and control center was constructed at Lincoln Lab in Lexington to house the XD-1 and pre-production models of the SAGE consoles. This became the nucleus of the Experimental SAGE Sector (ESS), and an expanded number of radars, airfields and ground-to-air radios were tied in such that the ESS covered most of the New England states and the offshore waters to the south and east. The XD-1 also became the computer on which the SAGE direction center master program was developed. The XD-2 remained at IBM's plant in Kingston, New York, where it served as the proving ground for the computer design itself as well as the facility for developing diagnostic hardware and software. IBM also created a training center around the XD-2 for Lincoln Lab, RAND/SDC, Western Electric and IBM programmers and the IBM hardware maintenance personnel.

During the development of the various SAGE subsystems, there was a common misconception that the overwhelming majority of the vacuum tubes in the SAGE system resided in the FSQ-7. In fact, almost as many vacuum tubes were contained in the radar sites — in the radar itself, and in the FST-1 and FST-2 modems that interfaced with the radar. The FST-2, for example, had 3,300 vacuum tubes. Each direction center had as many as ten radars feeding it, and the aggregate of radar-related vacuum tubes was almost as many, and in some cases more, than in the direction center computer.

CHAPTER 18

ELECTRONIC WARFARE

The jamming threat to SAGE was a persistent problem to everybody, including the Systems Office. It was liberally discussed throughout the course of the system design. Many of the schemes that were generated were analyzed by the Systems Office. This continuing problem brought me into contact with the Countermeasures Group in Division 4.

The Manual System was extremely vulnerable to all forms of jamming. To counter this threat, the SAGE design incorporated anti-jamming measures at all levels. The basic problem was that it was easy to jam the SAGE radars. It was relatively simple for a jammer to radiate strong signals at the radar's frequencies so that at all ranges, there was enough energy to swamp any normal radar return. It was, therefore, impossible to detect targets in the usual way. Normally, a target appeared on the PPI scope as a single blip for each pulse of the radar. The jamming signal appeared as a strobe reaching from the center of the PPI picture out to the edge of the screen. This solid strobe made it impossible to tell the actual location of the jammer on the line. However, when the radars were jammed, the strobes could be used to triangulate and fix the position of the jammer. The jammer location data could then be used to vector interceptors to take out the source of the jamming.

Another way of countering the jamming threat was to use the height finder radars, which operated at different frequencies than the search radars, and to take advantage of their additional power. The height finder was slewed in azimuth and sent out a vertically scanning pencil-shaped beam, which measured the angle from the earth's surface to the aircraft elevation. The height was calculated from this data.

The design of the radar receivers themselves employed anti-jamming circuits which, while not able to eliminate the jamming, nevertheless constrained the jammer-induced false alarm rate. Generally, these circuits modulated the outgoing signal and looked for the same modulated pattern in return.

Besides the jamming of radar frequencies by means of strong signals generated within the frequency range of the radar, the SAGE

system would also face the possible use of chaff — a tinsel-like material that could be dumped into the atmosphere by incoming enemy aircraft. Radar returns from chaff caused the system to report false targets at the range and azimuth of the drop. As a countermeasure, Moving Target Indicator (MTI) circuitry was incorporated into many of the SAGE radars. It operated by discriminating between the relatively high speed of the penetrating aircraft, and the lower speed of the windborne chaff.

Another approach to the jamming problem was the siting of the radars themselves to provide double and triple coverage of a target. While a determined jammer could normally prevent radar detection at forward aspect angles, it was less likely that all radars within range of the target would be denied coverage all the time. Radar “burn-through” was more likely to occur for those radars having a broadside view of the aircraft and a closer range to the aircraft. The beneficial effects of triple radar coverage were substantially enhanced by having the radars operate at significantly different frequencies (which would require the hostile aircraft to carry multiple jammers, ultimately displacing some of the available weapon payload). The spreading of radar frequencies across distinct bands, as an anti-jamming measure, was to be achieved through the Frequency Diversity (FD) radar program. Although never completed, the program involved installation of a family of radars whose frequency bands were chosen to cover the entire spectrum used by the ground environments.

We also took advantage of the fact that jammers were less effective in denying radar coverage of our interceptors, because the interceptors were equipped with a beacon that was interrogated from the ground and that radiated a coded response which was much stronger (by several orders of magnitude) than the skin return from these interceptors. So, although the search radar signal might be jammed, a jammer would require considerably more power to swamp out the beacon signal return as it came back to the ground antenna. This beacon was a variation on one which was used in World War II for Identification, Friend or Foe (IFF). It employed a coded pulse pattern that could be used to communicate certain information, such as an identity code, to the ground control station along with the regenerated return pulse. Thus, although the unidentified and hostile data might be missing, friendly aircraft tracks could be followed.

Work at Lincoln on triangulation of jammers led to investigations of a related problem. Triangulating on jammers becomes rapidly more difficult as the number of jammers increases, since spurious strobe

intersections are formed in locations where there are no jammers. (These are commonly referred to as "ghosts," as distinct from intersections associated with actual jammers.) Several approaches to identifying and eliminating the spurious intersections, a process known as "deghosting," were tried.

One deghosting approach used computer processing of the strobe intersection data. For example, an intersection could be tracked (provisionally) for a time, to see if the presumed target exhibited reasonable flight dynamics. Intersections that moved and accelerated in uncharacteristic ways could be considered as ghosts. Several other deghosting schemes were proposed. One, by Ramo-Wooldridge, used noise correlation processing that depended on signal correlation and time-of-arrival techniques. An experimental system was tested (the AN/TLQ-8), but was never deployed as part of SAGE.

Effort on another implementation of the noise correlation approach was initiated at Lincoln by Andrew Bark, head of the Countermeasures Group, and would be carried forward in the early days of MITRE. Andy had a hard time relating his program to the needs of SAGE. When I spoke to him about solutions to the jamming problem, he would invariably turn the question around from what was needed for SAGE to how far you could get with schemes that his group was pursuing. His own personal inclination was to spend his effort on the design and fabrication of parts for the schemes that interested him. Andy's noise correlation approach was known as the Strobe Intersection Deghoster (SIND). It was based on a bistatic correlation system that employed antennas at a primary site and a remote site, and a computer-based triangulation process (JAMTRAC). This deghosting technique was very powerful, but in the final analysis cost considerations precluded its implementation in SAGE.

We tried to test the effectiveness of our anti-jam measures via a cooperative program with SAC. SAC provided an experimental control group of B-47s which played the role of the enemy. The Countermeasures Group, under Andy and his associate, Harry Schechter, helped to design broadband jammers of a general kind to be mounted on the B-47s and to be included in simulated attacks on the Cape Cod System and later, on the Experimental SAGE Subsector. The first attempt at this was to connect a Lincoln-designed jammer to an antenna available on the B-47. The effect of the antenna pattern was such that it radiated most of the energy downward instead of toward the horizon, so that the effects of the jamming in

the experimental trials did not appear to be serious. It took some time to come to this conclusion. To make use of the B-47 in the early tests, it was necessary to fly the airplane in a slip mode (one wing lower than the other) with its belly exposed in the direction of the radars that were being jammed. This procedure was proposed to the SAC people, but they were reluctant to do it for a variety of reasons. They had preferred to fly (and finally did) with the belly toward the jammed antenna by circling in long ovals, where there is a natural tendency for the plane to fly with one wing lower than the other. In the pursuit of a longer time in this attitude, one of the Lincoln people asked the SAC man responsible for providing the service why he couldn't just fly a straight line in a slip mode. The tongue-in-cheek response was that with this kind of tilt, the airplane's battery would spill its water. In any case, a new antenna was developed by Andy's group, based on an earlier Radio Research Laboratory design. Test models of the antenna were fabricated in Andy's subterranean laboratory at Lincoln's field station, located on the hill above Lincoln Laboratory. This antenna had the proper radiation characteristics to act as an enemy jammer and to produce enough jamming so that strong strobes were generated by the radars.

Andy, a very careful experimental researcher who tended to be introverted, took no real pleasure in being a supervisor. People who worked for him respected his approach, but did not count on him for seeing the big picture. He avoided planning and speculation, and proposed concrete experiments. He was basically a researcher. Harry Schecter tended to front for the organization. He came from AFCRL and shared many of Andy's personality characteristics. The group as a whole tended to be introverted and it was difficult for me to assess their work. The people in this group, however, seemed generally happy with what they were doing, and did not expand their field of view.

The identification of Lincoln people with the organizations they were part of was very strong. It was difficult for people to move from group to group. But when this was necessary, they tended to be "loaned" for the duration of the project they were moved to. A typical example was Gerry Klein, an engineer who was loaned from Andy's group to Dave Israel for Dave's project in support of the design of the NATO Air Defense Ground Environment (NADGE) system. Gerry spent several years on loan in Paris before he returned to his "home" group with Andy.

Another area of concern to Division 4 was the vulnerability to

jamming of communication links between SAGE and its interceptors. One of the objectives of the SAGE design was to provide an automatic communication link between the direction centers and the interceptors. The first ground-air system employed in SAGE was that used in the Manual System, UHF voice radio, which had omnidirectional antennas both on the ground and on the aircraft. Instructions to the interceptors were transmitted by voice. This link was quite vulnerable to jamming. The initial approach to providing an automatic link was the frequency division data link, in which each interceptor aircraft was assigned a unique frequency channel at the start of its mission. The pilot tuned his radio to the assigned channel, and during the course of the mission the direction center routed guidance instructions to individual aircraft by commanding a ground-to-air transmitter to broadcast on the appropriate frequency.

Later, under the direction of Herb Sherman and Ronnie Enticknap, Lincoln developed the time division data link scheme that permitted all interceptors to remain tuned to a common frequency channel. A guidance message to an interceptor then included a digitally encoded address indicating for which aircraft the message was intended.

The electronic countermeasures work was important in that it helped ensure the adequacy of the sensory environment that provided the input data to the SAGE computers. Continuing an active involvement in the surveillance/countermeasures/counter-countermeasures arena would help MITRE maintain first-hand knowledge of threat and counter-threat dynamics as these fields evolved. It is interesting to note that as the sensory part of the SAGE system was maturing, planning for the systematic thinning out of the ground environment (reflecting the increasing emphasis on the emerging missile threat) was under way.

C H A P T E R 1 9

DEFINING THE SAGE SYSTEM

While IBM and Lincoln were learning to structure the process of making decisions about the design of the FSQ-7, the Air Force was struggling with similar problems in managing the various elements of the SAGE system. To facilitate the design of the air defense system, Air Defense Command Headquarters set up a special staff group under Colonel Oscar T. Halley, reporting to Major General Frederic H. Smith, Jr., Vice Commander of ADC. The function of this group was to work out the overall system design and provide a general description of the operational employment of the air defense system and its parts — including a definition of the Air Defense Command divisions, cross-telling and backup philosophy, as well as a description of the location of direction and combat centers. This ADC special task group negotiated with Lincoln on the design of the Transition System. Halley himself had been a backer of the system proposed by the University of Michigan's Willow Run Laboratories, which advocated an air defense system based on their BOMARC interceptor. He continued to push that system until the choice of the Lincoln system was made at the highest Air Force levels. Once the choice was made, Halley's team saluted smartly and went to work providing guidance to the Lincoln system.

In 1954, Lincoln developed the initial architecture for the Transition System, described in the TM 20 document. Halley and his ADC team then helped Lincoln create the SAGE Operational Plan, informally known as the "Red Book," which outlined the design and deployment of the entire SAGE system and established a framework within which each participant could find his place.

In spite of the fact that Halley had favored the Michigan system, he brought to the task of preparing the Operational Plan the drive and enthusiasm that were sorely needed. He was a good staff officer, and was responsible for helping to initiate the SAGE system design. He was a worrier. He tended to be a bit vague when it came to definitions of the operational design, but this was probably necessary in order to give the subsystem designers room as they went about the detailed design. General

Smith, to whom Halley reported, was a backer of the SAGE automation concept. He took a broader, more philosophical attitude toward the system, and expressed his belief that SAGE was a good thing because it would accelerate development of this new technology, which, in his view, would have significant consequences in applications to civilian as well as military problems. Many people likened it to the Air Force KC-135 tanker, which was designed to refuel bombers in flight, and its effect on civil aviation up through the development of the 707. The 707 evolved out of military experience with the KC-135.

The design of the SAGE system had a profound effect on the Air Defense Command, impacting its geographical divisions, its personnel requirements, its organization, and its operating procedures. In addition, the Air Defense Command, with the encouragement of Lincoln and the Air Research and Development Command, decided to establish a wing at Hanscom Field. The purpose of this wing, organized about the time the Red Book came out, was to provide Lincoln with local advice and approval from ADC on the work they were doing on operational specifications. This wing, called the 4620th Air Defense Wing, was commanded by Colonel Joseph D. Lee. Lee was a feisty, dapper, experienced fighter pilot. He was opinionated and expressed his opinions clearly. Lee had spent a number of his earlier years on the air defense problem. He had worked with Major General Gordon P. Saville on the establishment of the first air defense command early in the war, and he had accompanied Sir Watson-Watt on his tour of the U.S. air defense system during the war. After the war, in his assignment to ADC, he interacted with the ADSEC group, and partly because of his background, he was assigned at Lincoln to provide input to the operational specifications for the computer program.

The 4620th Wing was structured along the standard Air Force organizational lines, having a deputy commander, intelligence and operations sections, and other common Air Force organizational entities. The operational specifications were discussed in open sessions; modifications and changes were made in conjunction with various people in the Wing. The Lincoln people who were involved in negotiating the operational specifications were led by Charles A. Zraket, who would continue as a leader of SAGE operational planning throughout the 1950s.

When Lee first arrived on the scene, I thought he was going to be trouble, so I took the opportunity on a trip to Colorado Springs to invite him to my hotel room to discuss how I'd like us to work together. Lee

confided to me years later that because of that invitation, he had thought I was gay, and he came to my room with all his defenses on alert. (When he told me this, I chided him by asking why he had come to the room!) I remember the careful way that he joined in the conversation. He said he had been relieved to find that all I had wanted was an understanding on how we would work together. I told him that from my point of view, we were both in this job together and that we would see that he was thoroughly informed about what we were doing. We would give him veto power, although not directive power, over what we did. He appreciated that and he and I became good friends. Shortly after the SAGE system became operational, he resigned from the service, went to work for IBM for six years, and then came to MITRE.

Lee was also famous for the fact that he called the first and only air defense alert on the East Coast during World War II. According to Lee, an intelligence report from the Navy had indicated that German bombers had the range capabilities to hit the East Coast. At that time, there were two radars in the system and a large ground observer corps. One of these radars was at Montauk Point on the tip of Long Island, and it detected what looked like a multi-bomber formation headed for New York City. Based on the intelligence report, the Command Center decided an attack was under way. When the radar picked up the airplanes heading in the direction the intelligence report cited as potential bomb routes, Lee decided that the circumstantial evidence was great enough that it was necessary to immediately send out available tactical aircraft — even though many were not equipped for ground control intercept nor had they the appropriate armament. This caused havoc all up and down the East Coast. After that incident, the Army issued the directive that all sightings of airplanes that looked threatening would automatically be called friendlies, since the havoc of a real bombing would probably not be as great as that caused by that alert. Three or four months later, it was determined that the planes picked up by the radar were Navy PB4 amphibians, returning from a secret mission in Nova Scotia.

In 1954, the Air Force established an Air Defense Engineering Services (ADES) Project Office in New York City, headed by Colonel Richard M. Osgood of Air Materiel Command, to carry the main management load. Basically, the ADES Project Office had been charged by the Air Force to see that the design, construction, and deployment of the SAGE system was accomplished in accordance with approved schedules,

costs, and specifications. The ADES office had contingents from Air Defense Command, Air Materiel Command, and Air Research and Development Command. The ADES office was unique in the sense that it was the first full-fledged electronics systems project office; prior to this time, most of the items slated for SAGE or parts of the air defense system were treated as parts of the weapons systems they supported. It was this New York office that broke that pattern.

There were monthly meetings at the ADES Project Office at 220 Church Street, New York City, where the status of the SAGE system was reviewed. These reviews were usually chaired jointly by S.P. "Monk" Schwartz of Western Electric and Colonel O.M. Scott, who was Colonel Osgood's deputy. Scott was a thorough, conscientious, big-brother type of man, who took a lot of flak from the Lincoln people with great patience, even though most of the Lincoln people were younger than he and his ADES staff.

Of all the military people connected with the SAGE project, Colonel Albert R. Shiely, Jr. remained with the project longer than any of the other colonels with whom we had direct contact. Shiely had the ARDC responsibility for the ADES project office when it was first formed. He later went to the Pentagon and became the chief spokesman for the SAGE upgrading program. For several years, he was the SAGE program officer. He then went to Communications Command for a couple of years and later, as a major general, became commander of the Electronic Systems Division.

CHAPTER 20

GEORGE VALLEY

The New York ADES Project Office naturally looked to Bell Laboratories and Western Electric to provide the working support to handle the planning and contracting required to bring this large-scale electronics system into being. Bell and Western were prepared for their role since they had been working on the Continental Air Defense Survey (CADS) project which had as its aim the upgrading of the manual air defense system in the U.S. MIT recommended to the Air Force that Bell and Western play the prime role in the system engineering and design of SAGE. Bell, in turn, recommended that they supply help as they did in the CADS project, but they thought Lincoln ought to have the system engineering role. Lincoln, which had been set up as a research and development laboratory, did not see its role as architect engineer or large-scale system designer. Furthermore, one of the original principles advocated by the Air Force as well as by MIT in the establishment of Lincoln Laboratory was that, based on the charge to develop a revolutionary air defense system, Lincoln ought to have great discretionary powers in defining its own program. In practice, however, the notion that Lincoln had discretionary power was not consistent with the desire of the Air Force for a responsible, accountable technical organization to be in charge; from the beginning, the Air Force, including the ADES Project Office, tried to monitor what Lincoln was doing, recommending changes as they saw fit.

Within Lincoln itself, there was disagreement among three principal managers — Director Al Hill, Associate Director George Valley and Division 6 Head Jay Forrester — as to what the SAGE system should be, how to organize the development of the design, and who would be in charge. The problems generated by the disagreements among Hill, Valley, and Forrester only exacerbated the Air Force's growing frustration with what they saw as Lincoln's unwillingness to organize itself and support the Air Force people who were responsible for getting the job done. Further, the interactions between Hill and Valley and the Air Force led to frustration on the part of ARDC Headquarters, and involved the ARDC commander, Lieutenant General Thomas S. Power. Power wanted the

connection with Lincoln, as it would help solve the Air Force's need for guidance, but he and his deputies saw chaos when looking at what was happening. It was impossible for them to identify who was in charge. Power's reaction was to pressure MIT to put somebody in charge who could marshal Lincoln's talent.

These problems and the divergence of opinion on the matter of Lincoln's independence grew and festered until they contributed to the resignation of Al Hill, who left to join the new Institute for Defense Analyses (IDA), and later to the resignation of George Valley. In the reorganization that followed Hill's departure to IDA, his MIT management, led by President Killian and retired Navy Admiral Edward L. Cochrane (then Vice President for Industrial and Governmental Relations at MIT), chose not to name George Valley as director but appointed instead Dr. Marshall G. Holloway, who had been working in the atomic energy field. The Air Force hoped that this change of directorship would make it possible for them to get what they wanted out of Lincoln. In my opinion, Holloway was never quite accepted by the Lincoln staff, nor did he make SAGE a priority program, so he was really unable to deliver to the expectations of the Air Force. The tension continued to increase until the disagreement between General Power and Holloway was becoming obvious. The rumors were that it had gotten so bad that Power would not speak to Holloway.

General Power kept in touch with the various elements of ARDC through a series of "Commander's Conferences," which were held at the different centers under his command. These conferences took up the problems of the particular center during the day, and were topped off with a party during the evening. The entertainment at these parties often included impromptu skits. Many of the Air Force people I knew reported on one of these skits, which satirized the situation between the Air Force and Lincoln. The ARDC Headquarters staff, taking the parts of Lincoln managers, ridiculed the situation. In effect, they played prima donna roles, not taking orders from anybody but demanding buckets of money.

Holloway, under the circumstances, could not accommodate the Air Force, and this, among other things, eventually led to his departure. As a consequence of all this, Valley, as associate director, became the chief negotiator. By that time, as a way of dealing with the problem, the Air Force had assigned the responsibility for the design of the air defense system to the Air Force Cambridge Research Laboratory, and they

assigned Lieutenant Colonel Ralph S. LaMontagne to head a Lincoln Project Office which was set up to oversee Lincoln Laboratory. Needless to say, LaMontagne had a hard row to hoe. He was caught between a very strong head of ARDC, General Power, and an equally strong and obstinate Lincoln management. LaMontagne continuously complained about his ulcer — which seemed to be developing at great speed in his negotiations with Valley. Valley would not swerve from the original intent in the charter of Lincoln Laboratory and insisted upon the lead role in designing the system.

In spite of the fact that I had been with Lincoln from the beginning, I didn't get to know Valley very well. From my perspective, he had performed brilliantly on the ADSEC group and on the Charles Study, but he seemed to be an independent worker and he did not communicate freely with me. He once told us, apparently in jest, that his idea of a good leader is a man who finds out which way people are going and then runs up ahead of them. One of his characterizations was of a scientist as the kind of man who would run around with an unwrapped sandwich in his pocket. With his tweed jacket with leather patches on the elbows and his generally rumpled and preoccupied look, Valley seemed to fit that characterization. He was a chronic gum-chewer, and relied heavily on cigars in social situations. He was of above-average height, a little more than average weight, well built, with dark hair and bushy eyebrows. His eyes were his most outstanding physical characteristic — they were unusually pale, like those of a timberwolf, and gave me an impression of cunning and unpredictability.

The military people and others with whom Valley had a lot of interaction seemed to either love him or hate him. In any case, people were not neutral in their relationships with him. Even those closest to him never seemed privy to his complete confidence. He consciously or subconsciously projected an image of himself as aloof, distant, and superior. One was never quite sure whether he was candid in the things he said. In spite of these things, he was trusted by those who worked directly for him and by most of the more senior military officers, whom he needed for backing. I was impressed by Valley's practical approach to the big system problem of SAGE and always regretted that I didn't know him better and that most of my encounters with him elicited a defensive mood, causing me to back off because of my own self-doubt.

As Lincoln Laboratory had evolved and as Al Hill assumed the

directorship, it had appeared that Hill and Valley were working at cross purposes. I suppose that Valley felt he should have been made director because of his contributions to the ADSEC study and to the laying of the groundwork for SAGE. It seemed to me as though he often felt that there was some sort of conspiracy he had to deal with, and his method of dealing struck me as indirect. I think he found Forrester, who was inner-directed, a difficult subordinate to lead; in fact, there seemed to be a competition going on between them as to who was the intellectual "father" of the SAGE system. In retrospect, it is clear that Valley was a driving force who could claim a large part of the credit for generating and negotiating the creation of the Lincoln Laboratory and the SAGE system. It seemed to me that he ought to have been given more direct responsibility earlier on; if he had, perhaps some of the conflicts that became evident well into the program would have been avoided, or resolved sooner. Among Division 6 personnel Valley was held in high regard, and there was always a residual feeling of appreciation for his having drawn us into Lincoln Laboratory to continue our air defense work.

Although CRL was now officially in the chain, there was no way it could accomplish its mission without the acquiescence of the Lincoln management. The conflict between LaMontagne and Valley continued. It reached the point at which the New York Project Office, in the person of Colonel Shiely, had to intervene. He made sure that the Lincoln-generated technical information reports went directly to the New York office, where they became directive in technical matters to LaMontagne's CRL Lincoln Project Office.

C H A P T E R 2 1

MA BELL

The Bell System held a unique position among the SAGE contractors — they had the greatest experience with integrated ground electronics. With their renowned Bell Laboratories, they had the most thorough laboratory support, with proven performance.

In most instances, the contracting was handled through Western Electric. Western Electric, in turn, contracted for specialized help with Bell Labs and AT&T. The Bell/Western people were used to managing large projects, and their support to ADES numbered in the hundreds of people. They had been introduced to upgrading the air defense system in the CADS program. For SAGE, they increased their responsibilities by acting as a general contractor on a site-by-site basis. They carried the baseline components list and that part of the system design which fell into the support category, such as the building that housed the direction center, the prime power system, the air conditioning, administrative support, office furniture, internal telephone, etc. They also maintained the master schedule on which they tracked the progress of all the participants. In addition, they were responsible for the acceptance test methods and the evaluation of the system. Most of the Bell people took pride in the organization and were aware of its uniqueness.

Since Bell did not compete openly for work, and because demand for their unique capabilities was always greater than the supply, Bell/Western could sit back and allow themselves to be pursued by people who wanted to get things done on a large scale in electronics. For the projects they wanted to do, they needed only to let themselves be wooed by those people having the jobs. And, when they accepted a job, it was almost as if they were doing their customer a favor.

This attitude permeated the company. It had an effect similar to the MIT effect, wherein a lot of authority was assumed based on past performance and reputation of the company (and not necessarily on any single individual's past performance and reputation). Part of the mystique of the organization was that it contained a large number of Nobel laureates and other prize-winning performers. It was almost as if these people,

somehow, were a part of the package that you got when you contracted with them. The actual people who worked on the SAGE job were very good. At Western, S.P. "Monk" Schwartz stood out.

Monk was clearly the outstanding contributor. He was level-headed, pragmatic, and applied common-sense solutions to problems as they arose. He was a pleasant looking, well-built, energetic man. I remember him as wearing tan clothes consistently. Monk clearly had the respect of all the people who worked with him and around him. It came as no surprise later when he was picked to become president of Sandia Corporation, Western's contribution to the atomic weapon business.

At one point in the program, Western needed complete plans for an alternate NORAD command operation center, to be located at Richards-Gebaur Air Force Base in Kansas City, Missouri. One of the additions to Lincoln's responsibilities was the design responsibility for this center. I assigned Lawrence R. Jeffery the task of preparing the essential specifications. Larry and his people turned out the necessary specification in less than a week. This caught Monk's attention. He was dumbfounded by the speed of our response, and was very complimentary. He remarked that the Lincoln staff was so young, and after hearing that the document had been produced by an overnight push, he said that the Bell System was a mature organization that wouldn't give its younger staff such administrative responsibility, and that it required the stamina of youth to do what Larry had done. He also remarked that the Lincoln people were too young to know that they couldn't do what they did, and then he contrasted the people in the Bell System to Lincoln and pointed out (in jest) that serious responsibility at Bell was given only to people over 40. However, there was more than "youth" involved here. Larry was unusual, and, by coincidence, had worked at American Television. He had also learned electronics in the Navy in the electronic technician's program. He hadn't gone to college, but was teaching advanced mathematics courses at American Television. In the early 1950s, he entered the University of Chicago, and in two years qualified for a master's degree. Larry also had the ability to talk backwards — he could take a paragraph, read it aloud, and then say the paragraph in reverse order and the words in reversed sound.

Monk was backed up by a large number of Western Electric and Bell Labs people. Their duties included keeping a master schedule and managing phasing group meetings, at which all the participants (contractors) gave progress reports on their parts of the job. These phasing group

meetings were designed to lay out the situation clearly, so that the Air Force could make informed choices. The products of the meetings were modifications to the contracts held by the associate contractors. The participants also carried on with activities showing the financial status of the whole program. One of the key Western men was Robert Bright, who was a kind of expeditor, motivated by the discrepancies found at the phasing group meetings. When some incongruity was uncovered in the phasing group meetings, Bright would tear into it like a bulldog and carry on mini-phasing group meetings with the parties most responsible for the trouble. Bob was a short man with a large head; he was self-confident and energetic. His activities were fueled by anything that had an effect on the SAGE schedule. It was a pleasure to solve the problems or do anything to get Bob off your back.

When we began thinking about integrating SAGE with the weapons systems it was to control, Bob Everett and I met with Bob Bright periodically. At these meetings, Bright generally brought Clair W. "Hap" Halligan with him. Halligan was director of military engineering at Bell Labs and would later become the first president of The MITRE Corporation. Halligan had a good reputation among the SAGE participants. He was personable and flexible, and generally supportive of those of us who were charged with the responsibility of the SAGE design. His judgments were pragmatic — he did not like confrontation and would compromise when he saw no chance for holding to a principle. He brought with him to these meetings his ability to engage Bell/Western expertise. He was used to working within the power structure of the Bell System, and seemed to feel unsure of himself outside of that context. This was to become a factor in the development of the early MITRE operations.

In addition to Bell's contractual responsibilities, MIT leaned on the Bell System for their judgments about the overall program and about the quality of Lincoln's performance; it was important to MIT management to have the backing of the Bell System's opinion leaders. This had its dark side as well, because if the judgment of Lincoln's work was negative, it was difficult for us to overcome it. The whole program reacted to these negative opinions, which tended to affect the design decisions in awkward ways. For example, in one such instance, a paper was written by Bell stating that the data rate from the radars in the system was too low and suggesting ways of getting higher data rates. None of the suggestions helped materially, but the focus changed from solving the problem to

pleasing the individuals who had come to that conclusion. But in spite of this aspect, the Bell-Lincoln connection worked quite well.

The network of phone lines between the radars and the direction centers, the cross-telling lines and the lines to headquarters (NORAD) constituted the first major digital network in the country. On the Lincoln end, this part of the design and the negotiations concerning it were headed by Ronnie Enticknap. Ronnie, a good-natured, articulate, well organized man, carried on the negotiations with AT&T in an exemplary manner. I often felt that his British accent increased the authenticity and importance of his pronouncements, even on trivial issues.

C H A P T E R 2 2

GROUP 61

I stayed with the Systems Office until 1955, at which time I was transferred to become Bob Wieser's associate group leader in Group 61. This group had charge of the Cape Cod System, and was to be assigned the SAGE computer program. Heading the Systems Office had been unusually educational. By the time of my transfer, the Systems Office had processed most of the designs of the original SAGE system, and I knew the system as well as anyone.

When I left the Systems Office, it continued on under Benham E. Morriss and was called the Design Control Office. Ben had gone to military school and had always been a clean-featured, friendly, capable member of the staff. Eventually he became head of the group that installed the Lincoln version of the master computer program at the first three SAGE sites. Ben was well-organized and had good management skills. He had a master's degree from MIT's Sloan School of Management. He did a very fine job with the Systems Office while he had it, and an excellent job in managing the installation of the program at the sites. In 1958, he would choose to go with SDC rather than remain with Lincoln or spin off to MITRE. He gave as one of his reasons his feeling that I was too dominant, and he didn't think he could get along with me. Before this, I had felt that we had a good rapport, and I was surprised and saddened by his remarks.

At the time I joined Group 61, the design of the FSQ-7 was pretty well set, and attention was turning to the computer programming that had to be done. I joined the group with a certain amount of apprehension. I had not been in the programming business nor did I know a great deal about it; furthermore, the group of people working for Wieser who did know about it had already arranged, to a large extent, the organization of the program. The key people were David R. Israel, Charles A. Zraket, Herbert D. Benington, Robert Walquist, Walter S. Attridge, and Jack A. Arnow. Israel and Zraket concentrated on operational specifications, while Arnow, Attridge, Benington and Walquist concentrated on the "programming" of the system. I was apparently selected to help Wieser because of my ability to draw projects and people together and because I knew the

FSQ-7 machine better than most. As soon as I had joined Wieser's group and had reviewed the status of the computer program, I became alarmed by the lack of planning on the program. As a consequence, most of my efforts were devoted to resolving the schedule problems.

I found Wieser to be a delightful person to know. He was extremely witty and quick and was a master of memorable one-liners. He tended toward the ironic and bordered on the cynical, but I became his friend and enjoyed his company very much. Wieser wore a hearing aid that gave him several advantages: he could turn off conversations that were boring; he could turn off the noise at night. But by far the most unusual ability his hearing aid brought to him was recalled by some of the members of his group when they were to visit an operational radar site for a live demonstration. Wieser said there was no need to hurry to the demonstration, because he knew the radar wasn't working. His group couldn't figure out how he could have known it, until he revealed that his hearing aid could pick up radar sync pulses.

C H A P T E R 2 3

THE RAND CORPORATION AND SDC

The assignment of the programming tasks to the various organizations involved in the development of the SAGE program was fairly straightforward — except for the assignment of the master operational program. It was clear that the machine diagnostic program ought to be done by IBM, and Bell and Western Electric ought to do the test and evaluation programs. Lincoln assumed a responsibility to create the first operational master program. But the inevitable evolutionary changes of that program, as well as the program maintenance at the site, required a commitment MIT was unwilling to make. Lincoln felt that Bell and Western ought to take this responsibility, but it was Colonel Tom Halley and the ADC people who suggested that the RAND Corporation take on this job. Part of the reason for this was that RAND was already under contract to ADC for operational training in the Manual System. The training had grown out of a series of human factors experiments proposed by Dr. John L. Kennedy at RAND.

Also involved in the project were Dr. William C. Biel and Dr. Robert L. Chapman. They had set up a test facility at which they simulated the job of the operators working at PPI scopes in the Manual System. The purpose of the simulated test facility was to determine the value of simulated training in control situations where the inputs were known and the operators could be debriefed about their errors immediately after they were made. It began as a crude simulation — the scopes were simulated by wooden consoles with round holes the size of PPI scopes, and a continuous sheet of computer printout paper presented operators with a picture similar to what would be represented at a manual site — but the results were surprising. These experiments with volunteer operators proved them to be much more proficient than might have been expected. ADC became interested in this experiment and sent some operators to RAND to participate in these tests, and it was demonstrated that those operators who were doing poorly in the field did well at the simulated facility. ADC attributed this to the training setup, and as a consequence, ADC contracted for the System Training Program (STP). With Air Force support, STP became a

highly sophisticated training program, using electronic devices to simulate radar signals. Through STP, simulated radar data from RAND could be entered at any site and the whole manual direction center could be exercised, corrected and trained.

One of the features of SAGE was the extensive use of simulation programs and operational testing aids; the STP program created simulated airplanes by signals entered into the system at the radar sites. These and other simulated inputs were integrated so as to create a simulated scenario against which the operators could direct intercepts and be scored on their performance. Dave Israel pushed for a battle simulation program within the direction center and internal to the FSQ-7. In this system, an internal program simulated airplane signals that could be mixed with live signals generated by real aircraft. The simulation was an extremely useful feature and was coordinated with the STP design.

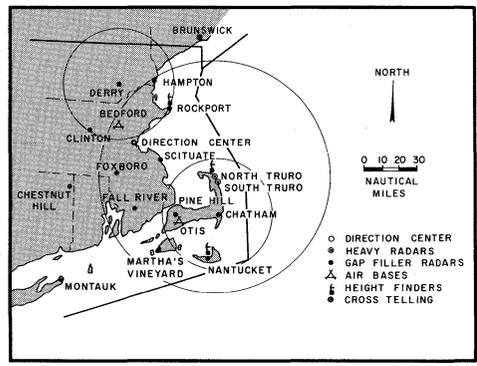
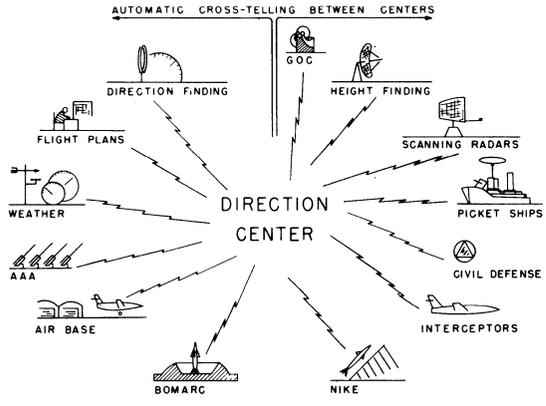
In the process of generating these training materials, RAND relied heavily on their computer division for the printouts that acted as the training materials. In order to do this simulation, it was necessary for the computer division to understand the specifics of the air defense process. When an organization was being sought to take on the task of operational programming, Colonel Halley and others brought to the attention of the ADES Project Office RAND's familiarity with the air defense problem. By this time RAND had set up a systems training division under William Biel and Melvin O. Kappler, and was approached by the Air Force to determine whether or not they could take on the operational programming job for SAGE.

Wieser and I met with those people at RAND who would be likely to assume responsibility for the SAGE master programming job — M. O. Kappler, Wesley S. Melahn, Toby Oxtoby, and John D. Madden. Kappler led the group; he was an outspoken, dynamic, and aggressive member of RAND's training division and former member of the engineering division. Wes Melahn and Don Madden were from the mathematics division; Oxtoby was from the system training division. It was clear to Wieser and me that Kappler was an entrepreneur who wanted very much to do this job; and by the middle of 1955, there was already talk of splitting off that section of RAND to provide systems training material and computer programs for ADC. This spin-off came to be known as the System Development Corporation (SDC).

In these discussions, we came to the conclusion that RAND

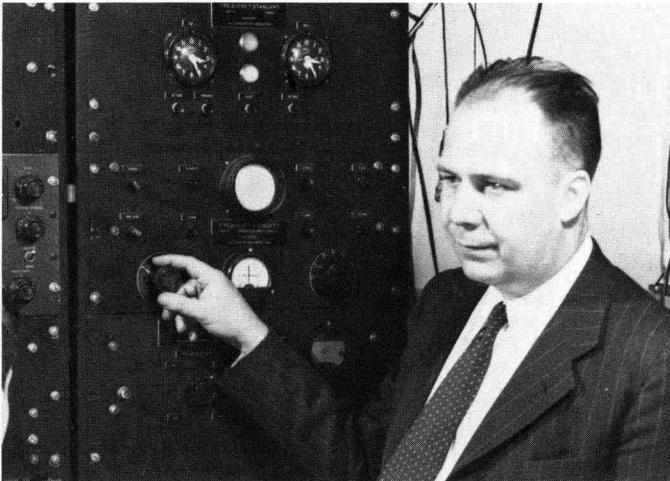
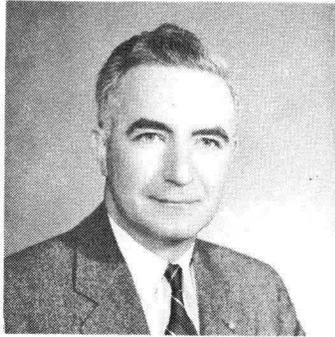
would aid Lincoln in producing the master program and in installing it at the first three SAGE sites (McGuire, Stewart, and Hancock), as well as in preparing the first program for the combat center at Headquarters, 26th Air Division at Hancock Air Force Base in Syracuse. It was agreed that Group 61 would use RAND programmers, who were destined to be part of a follow-on RAND/SDC job to prepare programs for the fourth and subsequent sites.

Wes Melahn, who was later to become president of SDC, was chosen as the principal RAND technical person at Lincoln. I shared the responsibility with him for transferring the operational program from Lincoln to SDC. Wes was a quiet, unassuming mathematician who had worked in Howard Aiken's laboratory before joining RAND. He was a good analyst, and brought to the project his considerable practical knowledge about the programming business and what it took to produce programs. Since the FSQ-7 XD-1 machine was located at Lincoln Laboratory, RAND personnel were provided housing by the Air Force in the specially built Butler buildings adjacent to the laboratory.



(Top) The elements of SAGE.

(Bottom) A map of the Cape Cod System, showing principal facilities and locations.



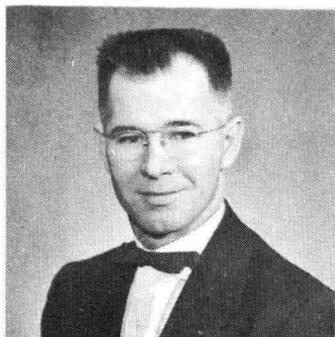
Courtesy MIT Museum

(Top) George Valley, “father” of SAGE.

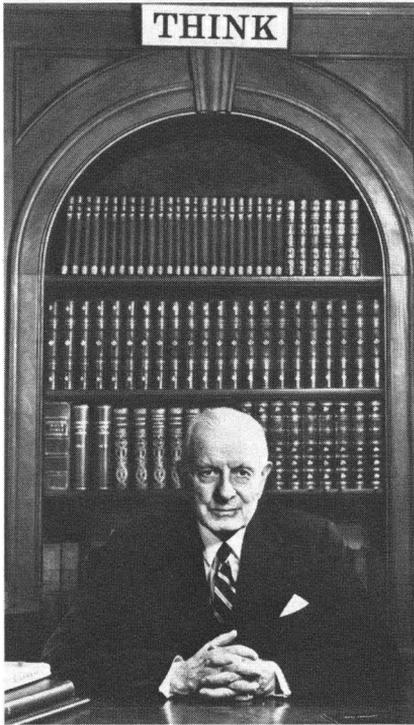
(Bottom) Al Hill as director of MIT Research Laboratory of Electronics; he later became second director of Lincoln Laboratory.



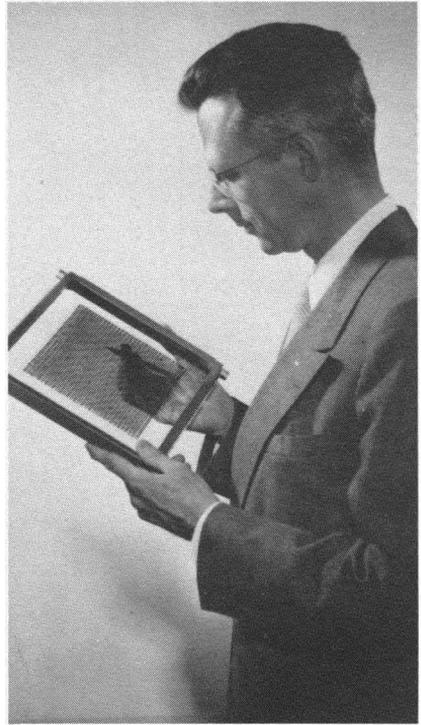
**(Left to right) Pat Youtz, Steve Dodd and Jay Forrester
in the Digital Computer Laboratory tube shop.**



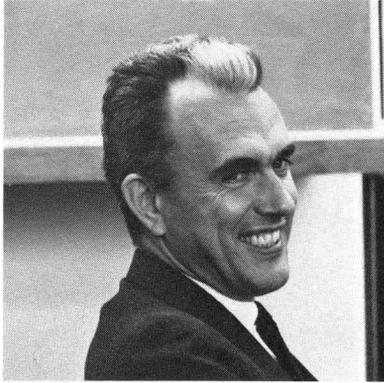
Jack Harrington (top), Paul Rosen (lower left) and Ernie Bivans of Division 2, whose data transmission work made the SAGE network possible.



Courtesy IBM Archives

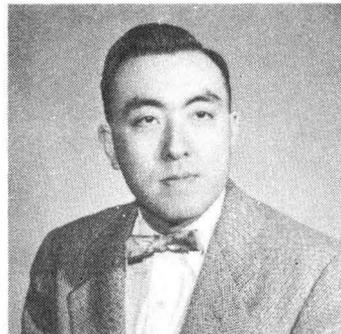
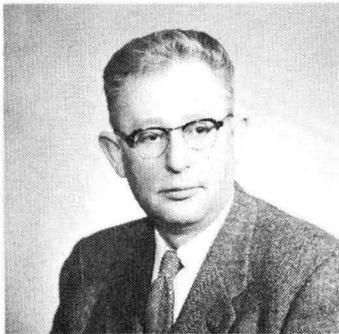


**(Left) Thomas J. Watson, Sr., the thinker's thinker.
(Right) Jay Forrester and his invention, magnetic core
random access memory.**



(Top) John F. Jacobs.

**(Bottom) Bob Everett, "architect" of the
SAGE system.**



(Top) Carl Overhage, fourth director of Lincoln Laboratory.

**(Bottom) Jerry Freedman (left) and Bob Naka of
Lincoln's Division 4, Radar Systems.**



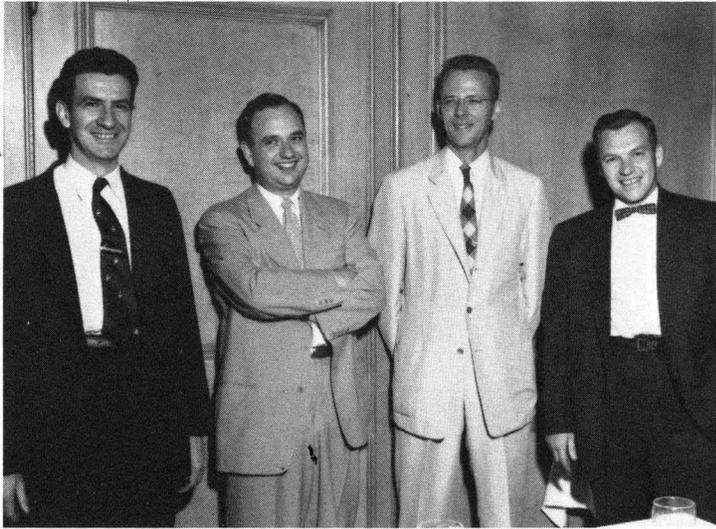
**(Top) Major General Albert R. Shiely, Jr.,
sixth commander, Electronic Systems Division;
primary Air Force technical man on SAGE.**

**(Bottom) Lieutenant Colonel Ralph S. LaMontagne,
Air Force project officer for Lincoln Laboratory.**

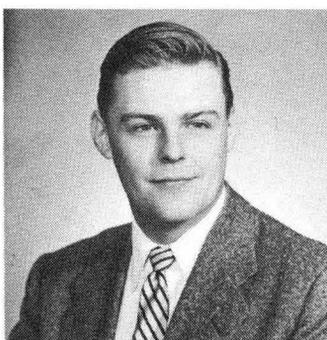
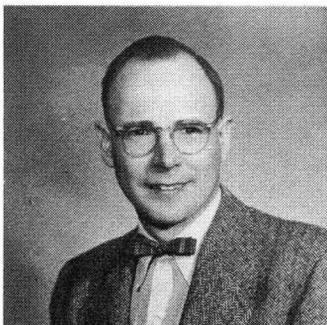


**(Top) Colonel J.D. Lee, commander, 4620th
Air Defense Wing; coordinated SAGE operational specs.**

**(Bottom) Colonel Oscar T. "Tom" Halley, ADC,
Operational Plan manager.**



**Forrester's farewell party (l. to r.): Bob Wieser, Bob Everett,
Jay Forrester, Norm Taylor.**



**Some Division 6 group leaders (clockwise from upper left):
Gus O'Brien, Dave Brown, Ben Morriss, Jack Arnow.**



(Top) Charlie Zraket, leader in SAGE system planning and operational specs.

(Bottom) Dave Israel, a SAGE system designer and the air traffic control expert.



Courtesy Sandia Corp.



(Top) Monk Schwartz, Western Electric's top man on SAGE.

**(Bottom) M.O. Kappler,
president, System Development Corporation.**



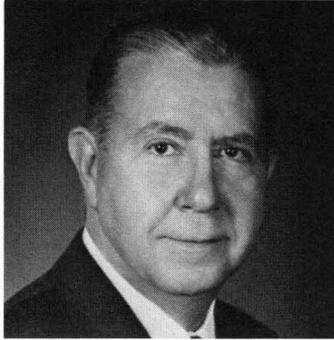
**(Top) Brigadier General Arthur C. "Sailor" Agan,
commander of the first SAGE air division.**

**(Bottom) Ed Rich (left) and Charlie Grandy, Experimental
SAGE Subsector shakedown test managers.**



(Top) Jim Croke, director of BOMARC test and evaluation.

(Bottom) Ken McVicar, master program installer.



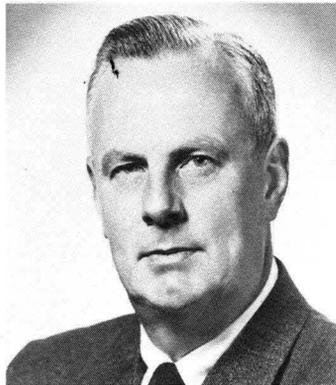
**(Top) Jim Killian, MIT president at beginning
of SAGE project.**

**(Lower left) Jim McCormack, MIT vice president;
negotiated establishment of MITRE.**

(Lower right) Hap Halligan, first president of MITRE.



**(Left to right): Hap Halligan; Major General
Kenneth Bergquist, commander, ADSID;
Lieutenant General Bernard Schriever,
commander, ARDC.**



Courtesy AT&T

(Top) General Thomas Power, commander, Air Research and Development Command; later, commander, Strategic Air Command.

(Bottom) Gordon Thayer of AT&T; director, Winter Study.

C H A P T E R 2 4

GENESIS OF COMPUTER PROGRAMMING IN SAGE

In the development of any new technology, the distinction between state of the art and state of the craft must be made. In the computer field, at this time, the state of the art was advancing rapidly, with technological innovations and new products coming at a fast pace. The state of the craft, on the other hand, was progressing much more slowly. Although computer developers often exchanged general ideas about the architecture of computers, there was no recognized need and hence little agreement upon standardization for circuits, order codes, speed of operation, input/output protocols, and other technological components or features. There had yet to be developed an infrastructure that would make possible the transfer of new ideas and new developments from one organization to another.

With Whirlwind I, however, some parts of an infrastructure were already under development. For Whirlwind, it had been necessary to build, from scratch, all test equipment — gates, pulse circuits, flip-flops, and other logical elements. Harry Kenosian, a former Digital Computer Laboratory staff member who transferred to Burroughs, thought he saw a business in these test modules and began to market them. These test modules were one of the first examples of ready-made computer test components.

The infrastructure became more established by the time of the FSQ-7. Ken Olsen and others had designed a line of modular testware that could test computer parts as they were developed. This modular testware was used in the memory test computer, and it was a variation of this testware that would become the basis for the original product line of the Digital Equipment Corporation which Olsen, his brother Stanley, and Harlan Anderson began in August, 1957.

What was true for the hardware was also true for the software: everything, initially, had to be done from scratch. Each of the users of the Whirlwind computer (and of other computers of the time) was faced with a bare-bones machine — a machine that responded only to numerals in binary code. This meant that users had to be familiar with binary code, and

a great deal of time was spent translating from alphanumeric code into binary code and vice versa. Most of the Lincoln programmers had their first programming experience on the 32-register toggle switch memory. There was also an MIT course on digital computer programming taught by W. Gordon Welchman, but basically, the early programmers had to learn programming techniques through experience. This was the state-of-the-programming-art through 1950 at the DCL.

Initially there were two major users of Whirlwind — a mathematics group under Charles W. Adams, and the air defense group under Bob Wieser. As the time for creating the software approached, almost everyone thought there ought to be programs that would translate automatically from alphanumeric code into binary code, making it easier for others to use and to program the machine. Thus were born the first assembler programs at the DCL.

The assembler was only one of many utility programs designed to simplify the preparation and operation of the program. The Comprehensive System of Service Routines (CSSR) was devised in 1952, and made possible floating point calculation in an interpretive language permitting the much larger word lengths required by scientific project work. CSSR included subroutine libraries of interpretive programs to simplify access to peripherals and to permit direct use of decimal notation. These utility programs were shaped, initially, by the needs of Charlie Adams' mathematics group to simplify the programming task, by the needs of the air defense group for an integrated system program, and, because of small high-speed memory capacity, by the need to do coding efficiently. As SAGE came into being, the new Lincoln Utility System was planned to meet both the need to develop a very large computer program which would be worked on in sections by inexperienced people, and by the need for an operational program which could evolve as more was learned about the whole system.

Development of the Lincoln Utility System was initially the largest programming task and it was also the most controversial. The Utility System consisted of a set of programs that facilitated the task of programming, and there was a great deal of debate as to whether or not to invest time and effort in developing these tools at the expense of immediate progress in the development of the operational programs. We took the risk and forged ahead with the Utility System.

The "COMPOOL" (Communication Pool) was a pivotal design

feature that greatly simplified the task of programmers. This COMPOOL centralized data base descriptors shared by all programmers and allowed them to symbolically specify the data required without having to know its exact value or location. The initial assembler concept from Whirlwind was discarded in favor of a much more sophisticated compiler by the time of SAGE. The compiler for SAGE took on many of the features of CSSR, containing symbolic, floating and relative addresses which allowed memory locations to be assigned either by the programmer or automatically, based on information in the COMPOOL.

By the latter part of 1955, the Lincoln Utility System for SAGE had grown to about 40,000 instructions supporting an operating program of nearly 100,000 instructions. Programs were developed to load information into the machine as well as to print out the contents of the memory and to format the printout on a page or a scope. A “checker” program was developed which made it possible for a person to test his program within the machine, thus allowing inexperienced programmers to take part in the process.

When I became associate group leader of Group 61, it took me some time to gain the confidence of those people who had been programming for the Cape Cod System, especially those responsible for the program, since they knew I hadn't worked as a programmer myself. But I had studied the process and I had done some practice programs. I had used the statistics they had gathered to work on the FSQ-7 order code and to contribute to the justification of the fast adder design. Herb Benington and Jack Arnow had the responsibility for formulating the program specifications and for the actual programming.

Herb Benington became the architect of the Lincoln Utility System. It was largely due to his work that the COMPOOL feature of the Utility System was added to the package. He was tall, high-strung, egocentric and wore a butch haircut. Herb was very intelligent and had taken time off from MIT to be a Rhodes Scholar, spending a year or so in England before joining us in the big push for the SAGE program. He tended to be an independent worker. In my opinion, his complex and contradictory personality made it hard to get new people to work with him, except for those who met his high standards and those who wanted to learn a great deal about the programming process. When I gave him an assignment, he would more often than not find some reason, sometimes justified, for not doing it my way, and would end up changing the problem I was

trying to solve. He alienated many people, including me, by his constant use of put-downs. These put-downs often took the form of anecdotes he would weave into the conversation in which he associated himself with the smartest and most powerful people. I was never sure whether he did this deliberately or not. I didn't contribute as much to the art as he did, and I don't think I ever convinced him of the fact that I knew how to run the programming business. I was not able to change his opinion before he joined SDC. He was later to become a supergrade member of the Department of Defense, and still later, a vice president of MITRE.

Jack Arnow had a big hand in programming Whirlwind for the Cape Cod System. He was a short, friendly, bright man whom I met at parties that he and his bachelor friends threw from time to time, and to which they would invite a variety of Lincoln people. He was always friendly and entertaining, but we always seemed to be on different wavelengths. I tried to analyze why it was difficult to communicate with Jack, and it seems to me that he assumed knowledge that his listeners did not have and he did not provide necessary background to ensure the transfer of information. He was a nice fellow to work with and was willing to invest the time in helping you understand, if he could. He chose to stay with Lincoln when Division 6 split off to become MITRE, and later became a very successful entrepreneur. He founded two companies: Interactive Data Corporation, and Advantage Systems, Inc.

Although the operational program was large and complex and drew most of the attention of the programming group, it was not the largest SAGE programming task. The problem of analysis of system performance was facilitated by a comprehensive series of recording and data reduction programs that distilled the essence of the activities during the operations. Most of these programs were created under the direction of Edward L. Lafferty, who had been hired during the big push for programmers.

The recording programs recorded the operational data mainly from processed track information as it was manipulated by the operational program. This data was recorded as the operational program performed its calculations on the real track data. As the operational calculations, such as tracking equations, proceeded, the recording programs were instructed to pick the numbers that were important in determining whether the operational system did what was expected of it. It was impossible to process the raw data manually because of the sheer volume recorded during the system operation. The data reduction programs made this possible. Ed Lafferty

estimated that the data reduction programs, altogether, required well over a million words of instruction code. Ed and the data reduction group added their own innovation to the process by incorporating the COMPOOL concept into the newly available higher-order language, FORTRAN. Called FAST (for FORTRAN Automatic Symbol Transfer), their new language enabled the one million lines of code to be quickly processed. Ed Lafferty was one of the programmers we hired in a special category of people who were destined to join the new support group for ADSID. He spent the better part of his career on data reduction problems in support of SAGE and later in support of a large number of computer-based systems. Lafferty was a reliable craftsman of the programming business. He was light-hearted and personable.

The data reduction area was a good training ground for a large number of programmers who were instrumental in the successful completion of the SAGE system program. Utility and operations programs were developed, and there was a drive to standardize languages, in the hope that this would simplify the production process. A number of ideas were put forward about standardizing languages used in military operations, which caught the attention of one of the RAND programmers, Jules Schwartz. Jules was one of the programmers who had been hired from RAND to carry on the programming tasks after Lincoln bowed out. He had been working on the utility program, and dedicated himself to creating a standard programming language, and, in fact, he did eventually develop a language that became the standard for military computers — JOVIAL, for Jules' Own Version of the International Algorithmic Language.

As the SAGE design progressed, the demand for computer support to programs in Divisions 6 and 2 increased. Early in the fifties, Division 6 became hooked on the use of computers for support of many kinds. At first, there was only the Whirlwind computer to provide that support. Whirlwind was used in the development of the operating program for the Cape Cod System, and also for the associated utility and data reduction programs. Later, the memory test computer absorbed some of the load, and the use of the XD-1, one of the two prototypes of the AN/FSQ-7, also contributed.

Even after MITRE was formed some years later, the progression of upgrading the support computers was still a necessity, and the latter part of this progression would go well beyond the point at which MITRE had taken over and SAGE was completed. Because of the demands for

computer time being made on the available facilities, first Lincoln and later MITRE concluded that they should buy or lease commercial machines to provide centralized computer support to the projects. Lincoln decided to lease an IBM 704, which was IBM's first commercial machine to feature a random-access core memory. Although fraught with friction and frustration, this centralization of computer support proved to be a boon to the projects. The 704 immediately found use throughout Division 6, and it replaced some of the need for Whirlwind to serve as a machine for evaluation and design support.

James H. Burrows, a programmer in my division who had graduated from West Point but who had chosen not to stay in the Army, evolved as the central figure in the management of the centralized computer support facilities both at Lincoln and then MITRE. We tried to keep up with the computers that were available on the commercial market, replacing one IBM model with the next, and we upgraded the central computer until our policy (by this time, MITRE's) led us to buy the last of the IBM 700 series machines, the 7030, aptly called the STRETCH. This "ultimate" machine was the first IBM computer offered for sale instead of lease.

Jim Burrows was prematurely grey and of stocky build. I remember him as always being on some diet or another. It was his responsibility to maintain an efficient configuration of centralized computers. Although we had been in the computer software business for many years, we suffered from an Air Force perception that we were mediocre in technology. Much of Burrows' time was spent in trying to overcome that reputation. This reputation for mediocrity in programming was undeserved, and was due mostly to the fact that we did not contribute as much to the literature as did other organizations devoted more heavily to research. The questions we always got from the Air Force were, "How many nationally known programmers do you employ?" and, "How many Ph.D.'s do you have in the programming business?"

Burrows was very competent, but was overloaded by the charge that was given him. The undeserved reputation for mediocrity dogged him, and he was not able to find a satisfactory balance that would alleviate the pressure. Burrows eventually went to work for the Air Force, as the assistant to Major General J.T. Robbins at Air Force Headquarters. Robbins had the Air Force responsibility for managing software and computer purchases, and Burrows was finally on the other side of the questions.

C H A P T E R 2 5

MEETING THE NEED FOR PROGRAMMERS

One of the biggest problems I faced when I came to work with Wieser was finding trained programmers. The people at Lincoln who knew how to do the programming had been working almost full-time on the Cape Cod System through 1954, and although they knew they were going to pick up the responsibility for the SAGE programming, they did not know exactly what the programming demands would be. First, the SAGE program had to be developed as a master program for many sites; second, it involved the FSQ-7 computer, which was quite different from the Whirlwind computer; and third, it involved the training of new programmers for the FSQ-7 so Lincoln could eventually phase out of the programming aspect of the system. We projected that we needed over 200 new programmers and presented this problem to the ADES Project Office.

By the time I joined Wieser in Group 61, the programming requirements were emerging from the organizations involved in the SAGE job. We concluded that in order to have enough trained programmers to carry on the job, it would be necessary for us to set up a training course, and IBM was asked to do this.

Division 6 refined the estimates that had been made earlier on the number of programmers that would be required. Lincoln would need programmers to do the first version of the master program and to install it at the first three sites as well as to develop the first version of the combat center program. RAND was committed to preparing programs for subsequent sites, including adapting, maintaining and revising the master program during the evolution of the SAGE system and the integration of new weapons and sensors. In addition to that, RAND needed programmers to supplement its STP operation, and had committed itself to building up a staff of 200 programmers for the STP by 1957. Bell and Western Electric needed programmers for both the acceptance test and the evaluation. IBM needed programmers to do internal diagnostic programming and to instruct novices in the training course they were offering. The initial requirements for programmers were based on estimates for producing the operational program. Since the utility programs were to be drawn from those programs

used in the preparation of the master program, additional people were not expected to be needed for this task.

Over the course of a year, IBM trained over 200 people at their XD-2 facility in Kingston, giving each person two months of training. The estimate that was used at the beginning of the course called for 55 Lincoln people, 58 RAND people, 16 ADC people, 24 Bell people, 29 Western Electric people, and 31 IBM people to be trained. This course was remarkably successful. Before the formal course was finished, over 500 programmers had been trained.

Although we naively thought that only engineers could become expert programmers, we found them in short supply, and were forced to look for other professionals who could perform the task. Mathematicians and others from disciplines in science, as well as some from very unlikely (to us) disciplines, such as music and psychology, were forced, like square pegs into round holes, to fit. To our surprise, it was soon evident that this broader approach created almost the same number of competent programmers as we might have reached using only engineers.

In order to understand the programmer situation, one should know that an "experienced" programmer was someone who at one time had finished the act of writing a program. More often than not, the career path was from some other discipline to programmer to programming supervisor. Many programmers only wrote one program, and then they were given the job of overseeing the next new bunch of recruits. Lincoln sent recruiters to college campuses all over the country to dig up candidate programmers.

One University of Chicago student, Richard C. Jeffrey, was attracted by Lincoln's ad in the *Chicago Tribune* that called for logical designers. Dick decided to investigate the opportunity, and tells of standing in line for an interview. Dick was a philosophy major whose specialty was logic. When it finally came time for the interview, he confronted the recruiter with what he thought was his obvious fit to Lincoln's needs. In spite of the fact that he and the recruiter could not come to terms on a definition for "logical designer," the recruiter's last words were, "I don't know what to do, but we haven't got any logicians and maybe we ought to hire some." Jeffrey ended up being hired and was assigned to a systems analysis group, which contained a variety of people whose functions were never clear, or who never found a niche in the SAGE project. It was another coincidence that Jeffrey had come from American Television and had

taught the same courses I had taught there. I remembered him vividly, because I had picked up one of his classes in differential calculus, a class that was happy with Jeffrey as an instructor and that saw me as a poor substitute. Jeffrey had a habit of walking with his head down and not being aware of the people around him. I claimed that he recognized people by the types of shoes they wore. Years later, he would earn his doctorate in philosophy, and became a professor at Princeton University.

Others in the systems analysis group included H.R.J. Grosch, who had had some experience at IBM, and Irving S. Reed, a theoretical mathematician who came from CRL where he had worked on the beam splitter for the AN/FST-2. Grosch was also a logical designer who couldn't apply his Boolean algebra to the SAGE problem. He proposed a computer that used ternary arithmetic, which theoretically would have resulted in a reduction of the number of components in the machine, but he neglected the engineering problem of adding another stable state to each register. Reed was unable to find a place for himself in the straightforward system engineering job. Eventually, most of the members of this group were placed in other jobs around Lincoln, although Grosch left and Reed took a job with a consulting firm on the West Coast. Jeffrey was assigned to the Systems Office.

C H A P T E R 2 6

SCHEDULING AND OTHER PROBLEMS

We had all grossly underestimated the overall programming job for SAGE. During the early part of 1954, it had become clear that the programming job would be much larger and much more expensive than we had predicted. The operational program would be over 100,000 instructions and the utility and instrumentation programs would be almost as large. In 1955, for example, the Lincoln Utility System contained about 40,000 instructions, and the cost of programming at that time was estimated by Herb Benington to be about \$55 an instruction word.

In 1956 enough was known about the computer programming job requirements so that we could state with certainty that we were too ambitious with respect to the capacity requirements for tracking and interception that we had originally set, and that it would take considerably more time to train the programmers and create the program than we had anticipated. This was a very delicate matter because the whole SAGE schedule was keyed to the availability not only of the FSQ-7 machines, but also of the operational programming. Wes Melahn and I and the programming team made our best estimate of when the first operational computer program could be delivered; we concluded, reluctantly, that it would take approximately another year beyond the schedule to complete the program.

It was my duty to report this news to the ADES Program Office. The news was viewed with both alarm and relief by everyone there — alarm because of the slippage, and relief because other slips which had not been reported in other parts of the system could be masked by the major slip in the program. It was partly this slippage that caused tension to escalate between Division 2 and Division 6. This tension over the management of the program was to increase as time went on.

An example of a subsystem that was also behind schedule was Division 2's FST-2 radar data processor. The processor that was used in the experimental transmitting of video signals from the radar to the central computer existed in "breadboard" form, and had to be turned into a production model and installed in the field on a crash basis in order to catch up to the rest of the system. The Burroughs Corporation did a magnificent

job in accomplishing this in a short time (18 months) under the guidance of Jack Harrington's group.

Periodically, the New York Project Office would report to General Earle E. Partridge, ADC commander and later to become the first NORAD commander, on the progress of the SAGE system. These presentations were usually given by Colonel Scott, who was a thorough and competent reporter. General Partridge had previously been head of ARDC and had come to know the Lincoln organization and George Valley in particular. Partridge and Valley seemed to have a rapport, and Partridge had confidence in what George was supporting. I remember attending one of the progress report meetings in the NORAD command post in Colorado Springs, with Partridge at the head table, his staff surrounding him. Lincoln and other groups occupied a bleacher-like seating arrangement. Colonel Scott went through his report which, at this time, included the bad news about the schedule slip. Afterwards, I talked with one of the colonels who was responsible for coordinating the NORAD command's participation. He told me that Valley had already made a separate report to Partridge and that Partridge accepted Valley's judgment, which corresponded to that expressed by Scott. This private meeting was typical of Valley's inner-circle maneuvering.

On the way home from Colorado Springs, Bob Everett, Norm Taylor, George Valley, myself, and others were grounded in Chicago by a snowstorm. We were put up at a hotel in Chicago for the evening. Since I had spent the better part of 10 years in Chicago, I was the man the group looked to for planning a recreational evening. I was the junior member of the group and took them to the Rathskeller of the Old Heidelberg, where you can drink beer and sing German songs. It became very clear that I had mistaken the sense of the group. In retrospect, I would have done better to have taken them to Calumet City — Chicago's Times Square. It turned out that both they and I were hung up by protocol: the junior member of the group should not make too much of liking "show bars," and imply that his superiors would want that, and his superiors were unwilling to come out and say what they wanted, as it might be a bad example for the kids.

In order to expedite the schedule, Group 61 devised a way of controlling the program through the use of a hierarchy of specifications — from the Lincoln/ADC Red Book (Operational Plan) to the actual coding. At the highest level was the Operational Plan, prepared by Lincoln senior staff and Colonel Tom Halley's contingent from ADC, which guided the

development of the operational specifications. Dave Israel and Charlie Zraket, in conjunction with Colonel J.D. Lee's 4620th Air Defense Wing, had responsibility for the development of the operational specifications which described the transfer functions that would be performed in terms of inputs and outputs.

Israel and Zraket were both outstanding men. Dave Israel had been an undergraduate at MIT as well as a graduate student. Israel never got over his liking for programming in machine code, and he swore by it throughout the period of the Cape Cod System and well into the work he did on the SAGE system. I had dealt with Israel on the instruction set for Whirlwind II/FSQ-7 as well as on the design for the central processing unit as a whole. He was a very bright, self-initiating, competitive engineer, who was famous for his aptitude test for programmers. He devised this test himself, and gave it to all new employees being considered for work as programmers. Since there weren't many programmers in the country and because there was no standardization of order codes or similarities in the usage of the computer, each development program for digital computers emphasized a slightly different twist. As we mentioned before, Whirlwind emphasized processing speed and reliability.

My first encounter with Dave in a social setting was in a restaurant in Poughkeepsie. We had both gone to Poughkeepsie to participate in the selection of the order code. I looked at the menu and decided I wanted a drink before dinner. I selected a drink that I had never had before called applejack, which is alcohol made out of apples. This gave Dave the impression that I was extremely sophisticated, well beyond the fact of the matter. He always remembered that occasion.

Charlie Zraket had graduated from Northeastern, and joined the Digital Computer Laboratory the same year I did. He did his master's thesis at MIT on the buffer drum memory. He worked on the time-division data link, created one of the first weapons control programs, and participated in the Cape Cod experiments before he was given the responsibility for a major part of the SAGE operational specifications. Charlie was an extremely well-organized man, orderly in his thought and in his personal life. He was characterized by Howard Kirshner, a Division 6 engineer, as being so neat that he drove his car at 50 mph so that the two halves of the speedometer would be of equal size. Zraket was smart and productive and had a low frustration flash point. He had a habit of blowing his top, and his arguments would become hyperbolic. Once the storm was over, Charlie

would forget it and continue to operate serenely until the next time the pressure developed. In spite of these characteristics, he was very reliable and I liked to have him on my projects because I knew they would be done right — and a little destruction of the furniture was a small price to pay.

The group under Charlie Zraket prepared the operational specifications and these were cleared with the 4620th and given to Benington and Arnow who had the responsibility for the programming specs. The programming specs outlined, in flow chart form, some major subfunctions that would have to be performed in order to satisfy the operational specifications.

Shortly after we announced the slip in the program, the specifications were showing up the capacity that each program required for its execution. The basic cycle for the master program was approximately 17 seconds, based on the time that it took for one rotation of a long-range radar. An analysis of the time it would take for running each of the procedures on each piece of data, as well as for performing all of the more general functions, showed that we would not be able to meet the intended track capacity of the system. This called for a general paring back of the program, and a rather drastic cut was made in some of the functions. Other remedies included rewriting certain sections of the program, as well as dropping some functions entirely. At any rate, the program was not only late but required a significant overhaul.

About that time, George Valley set up a series of seminars in the Lincoln cafeteria to exchange information on various aspects of research that went into air defense. I was asked to prepare one of these seminar sections on the SAGE programming business. I was hesitant to do it since there were programmers around who had considerably more experience than I did, so I spent the better part of a week reviewing what all the people who worked for me were doing and drafted a speech which was partly tongue-in-cheek, partly tutorial, and partly to introduce the Division 6 people to the rest of Lincoln. I called it “The Romance of Programming,” and it seemed to have been the best thing I’ve ever done. More people remember me for that speech than for more significant contributions I made to the program.

Valley had responsibility for Division 2 as well as his responsibilities as associate director of the laboratory. Forrester remained in charge of Division 6, but tension was growing between the two divisions. When the slippage occurred, Valley took the opportunity to make management

changes — ostensibly to improve the efficiency and balance of the organization. Wieser, one of the key people in Forrester's division, was transferred along with the responsibility for the Cape Cod System and promoted to become associate head of Division 2. Valley resigned his position as head of Division 2, concentrating on his responsibilities as associate director, and promoted Carl F. J. Overhage to head Division 2, with Jack Harrington as second (to Wieser) associate head. With this reorganization, Valley attempted to circumvent the competition between the divisions, but actually only escalated it, and, in 1956, Forrester resigned to accept a position as full professor at the MIT's Alfred P. Sloane School of Management and to work on the patent for his invention of the random access core memory. Bob Everett succeeded Forrester as head of Division 6. I remained in Division 6 as assistant division head, retaining the responsibility for the SAGE operational computer program.

The Valley reorganization, which had led to all of the personnel changes, did not, as had been intended, close the rift between Divisions 2 and 6. Part of the purpose of the reorganization had been to ensure that the technical results turned up by the Cape Cod System would influence the design of the various parts of the SAGE system. Wieser, and a number of other management people, felt that the Cape Cod System results were being deliberately ignored, or at least subordinated to the problems of hardware and software production and scheduling. Just prior to Valley's leaving, Overhage called a meeting of Division 2 and Division 6 supervisors to work out a modus operandi and to clarify the issues that were supposedly preventing feedback from the Cape Cod System to the SAGE system. The meeting was surprisingly tense, and the attitudes of the participants bordered on hostility. Overhage, who had called the meeting, apparently believed that simply talking over the problem would lead to an operational solution. The meeting began with a summary by Bob Wieser of the presumed lessons that were learned from the Cape Cod System. This speech, along with others by Division 2 people, simply reinforced the rift. The Division 6 people who were caught up in the SAGE design and production believed that they had all they needed from the Cape Cod System to go ahead and freeze the specifications of the hardware elements and most of the software. Everett, in his characteristic way, took the position that the design was essentially set and it would require significant argument and proof to change it, and he stated that he was willing to change anything for which Division 2 could make a substantial case. This,

of course, put the burden on Division 2 to demonstrate the consequences of small changes on performance of the system. It called for more effort than Division 2 had manpower, and presumed that the SAGE design had already taken into account the lessons learned from Cape Cod. The judgment of whether or not it did was not delegated to any group or person, so Division 6 won by default.

All of this non-productive activity served to reinforce Overhage's opinion of Division 6 as being obstinate and rigid. I could feel the change in Overhage's mood during the course of the meeting as it turned from a benevolent hopefulness to a resigned discouragement. I think that it contributed to his willingness to encourage Division 6 to split from Lincoln, and, at the same time, made him unwilling to rely as much as he should have later on our inputs to the process of formulating the structure of The MITRE Corporation. This would be the genesis of the sentiment that the original MITRE technical groups were part of a "Good Old Boy" network. The original MITRE team was referred to as the "Lincoln clique." This sentiment would continue to be used as an argument for getting new blood into the organization, until Bob Everett eventually became president of MITRE, and the situation was put into better perspective.

Shortly before Forrester transferred back to the Institute and took up duties as a professor, I accompanied him to an air power demonstration at Eglin Air Force Base in Florida. At that time the Air Force annually put on a show of the capabilities of aircraft in the Air Force. It included bombing, strafing, reconnaissance flying, photo taking, and the whole ensemble of aircraft functions. On the way back from the demonstration, we stopped in New Orleans. As I had never been there before, I was glad that we managed to see many of the cultural sites such as the above-ground burial cemeteries, and the French architecture. In the evening, most of the people disappeared down Bourbon Street. I wanted to go also, but Jay wasn't having any of it and I felt obligated to keep him company, so I missed one of the most renowned features of one of our most important cities!

C H A P T E R 2 7

A PROMOTION AND THE STEERING COMMITTEE

After Forrester left Lincoln Laboratory to go back to MIT, Bob Everett was named head of Division 6. He chose Norm Taylor to be his associate head. Almost immediately, however, Taylor was invited to participate in a study on defense under H. Rowan Gaither, then head of the Ford Foundation, and he went to Washington. Shortly after his return from Washington, Taylor left Lincoln to become president of Scientific Engineering, Inc. He was later to go on to become one of the vice presidents of the Itek Corporation. When he left Lincoln Laboratory, I was made associate division head under Everett, and a member of the Lincoln Steering Committee.

In the five or so years that the laboratory had been in existence, the directorship had turned over three times. Marshall Holloway's attempts to smooth relations with General Power and the Air Force had not gone well; for this and other reasons, Holloway left the Laboratory. George Valley, too, left Lincoln. I remember going to a cocktail party at Admiral Cochrane's house given in Valley's honor, to which the division heads and associate heads were invited. Everyone there seemed to be unaware of the reason for the party, and George could just as well have been one of the guests. I remember thinking that it was just as well that it was handled that way, without speeches or sentimentality. Valley returned to MIT as a full professor (perhaps as part of the settlement) and took the job of chief scientist with the Air Force shortly after that.

Carl Overhage was promoted from his position as head of Division 2 to become Lincoln Laboratory's fourth director. He was in charge when I became associate division head. Carl was a statesmanlike, genteel diplomat, who more or less expected that a gentle suggestion would be enough to persuade his subordinates to do the right thing. But under his old-world gentility was a hint of an iron will — he even had a slight foreign accent. He was a good example of successful typecasting, complete with a magnificent pair of arched, bushy eyebrows.

By the time I joined the Steering Committee, many of its original members were gone. Among those who had not left was Henry

Fitzpatrick, who was Lincoln's chief financial officer. To be a member of the Steering Committee, one had to be either a division head or an associate division head. These division heads all had their own fiefdoms and guarded their turf with extreme ingenuity. Although the Steering Committee was set up to give advice to Overhage on matters of mutual interest, and I think it did serve that purpose, it was nevertheless an unruly bunch, to say the least. One area in which there was a certain amount of lively discussion was in budget and finance, which was headed by Henry Fitzpatrick. Bob Everett, one of the original Steering Committee members (he had joined as associate head of Division 6), had an uncanny ability to upset Fitzpatrick. Everett was able to add up columns of large numbers in his head, something he always claimed that he never practiced doing, as it was just something that occurred without his even trying. His ability to do this added to his reputation for quickness and technical capability. As a financial man, Fitzpatrick often came in with long columns of numbers with totals in which there was often some mistake. Everett would invariably catch the mistake and choose the most opportune moment, one which would cause the greatest effect, to point out the mistake. This would usually put Fitzpatrick off his stride. It seemed to contribute to his doing the same thing over again at the next meeting.

There was very little discussion of the SAGE problem at these meetings by the time I became a member; the Committee was much more concerned with research and development. Ben Lax, associate head of Division 3, was interested in creating a magnet laboratory where magnetism could be studied. His proposal envisioned a magnet that would take half the water in the Charles River to cool. I found it an interesting group, and became close friends with Joseph A. Vitale, who headed Division 7 which ran the shops and other services for the laboratory. Joe Vitale and I spent a lot of time in what we thought was humorous banter, but, as time went on, it became clear to us that Overhage did not appreciate this, and our behavior contributed to some of the tension surrounding my support of Overhage's planning with the Air Force as it tried to take over the management of the SAGE system. In retrospect, I think if I had been more sensitive to Overhage's feelings, the difficulty Everett and I would experience during the time that Overhage and General James McCormack, Jr. were setting the stage for The MITRE Corporation might have been moderated. I believe that Bob and I could have had more influence on the principles established.

As Bob Everett's associate, I quickly developed a way of working with him which utilized both of our strengths. Bob tended to act as a filter on all the internal actions of Division 6, as well as on the work program and products. He was a decision procrastinator, leaving his options open until the last possible minute. On the other hand, I devoted myself to putting pressure on those who were assigned different tasks and seeing that there were products that could be filtered by Bob. I was an initiator who proposed actions and gave out the orders. I saw to it that the decisions that were required were made. This often meant that I formulated instructions for Bob to approve, and he in turn released his potential action items to me for action or distribution. Beyond this division of labor, we acted as each other's alter egos on the entire Division 6 program. I generally took the role of counterpart to the Air Force program officers, and Bob tended to take the role of scientist, through his association with the Scientific Advisory Board and other similar committees. It seemed that it was a good match because we became quite proficient at this way of working.

C H A P T E R 2 8

SAGE BECOMES OPERATIONAL

The master program was created at Lincoln and tested on the XD-1 under the guidance of Jack Arnow and Herb Benington. As soon as it cycled — stepped through the air defense process without hanging up — we began to implement its installation at the first three SAGE direction center sites. Ben Morriss and his associate, Kenneth E. McVicar, were given the responsibility for installing these programs at the sites.

I had known Ken McVicar since I was an MIT research assistant and lived in Westgate West. At that time, Ken was working at the Digital Computer Laboratory, but had decided that he wanted to go to law school. He spent some time at Harvard Law School, decided he didn't want to pursue law, and came back to the Digital Computer Laboratory. As Ben Morriss' associate group leader when the master computer program was installed at the first three SAGE sites, he bought a new Chrysler to circulate among McGuire, Stewart, Syracuse, Poughkeepsie and Kingston, seeing that everything was being handled properly. Since the facilities at XD-1 were limited to a simplex computer, all duplex features had to be tested out on a duplex machine. Thus, we requested of IBM the use of the FSQ-7 duplex machines which were in the test cells at Kingston during construction of the field machines.

Ken was a collector of clocks and other things. He never lost the natural curiosity of his childhood. He tells a story of being out with his son who said to him, "Daddy, I'm glad you didn't grow up, like other daddies." In addition to clocks, he collected ship models. He built radio-controlled airplanes, and sailed. He was also an amateur magician. I remember a party we had at our house when Ken was sitting with the Commander of the Electronic Systems Division and one of our other friends. To show a magic trick, he asked for the use of a handkerchief. It was offered by my other friend. The trick involved stuffing the handkerchief through his fist, lighting the protruding center of the handkerchief with a match, pulling it back through his fist, and voila! The hole that was burned disappeared! Only this time, it didn't. Ken was faced with having to give back this burned silk handkerchief to its owner.

As a member of the SAGE team, Ken was aggressive but never pushy. At raise time, he would always object to the amount of his raise on the grounds that he didn't deserve it. Similarly, on promotions, he was quick to point out that others deserved a promotion as much as he. His characteristically gracious and fair-minded ways, as well as his proven reliability, stood him in good stead and he continued to rise within the organization at Lincoln, and later within MITRE.

The differences in physical characteristics of each site required that there be a mechanism for adapting the program at each site to the system. The adaptation process allowed particular features of a sector to be added to the program and tested for errors. The Air Force was responsible for providing Lincoln with the adaptation parameters — which included information such as location of radars, airfields, ground-to-air radio, and geographical features of importance. A team of Lincoln people supplemented by RAND staff took these modified master programs to the sites, where their first job was to make the program cycle through all its processes without hanging up. The first program installed was at McGuire Air Force Base in New Jersey, and it was stepped through all its advertised capabilities by Al Roberts, one of Ken McVicar's men who headed an installation team. Al was an intense, emotional, and capable engineer. When excited, he developed a tremor which added to the effect. Al received his master's degree from MIT in 1953. He had been a research assistant, worked on the storage tube memory for Whirlwind I, and then worked on the Cape Cod System.

Errors were caught and corrected by an on-site team. We had not predicted the volume of these errors, and in the process of the installation, we got a better fix on the need for maintenance programmers to remain at the site to maintain the master program. McVicar, working for Ben Morriss, was given the responsibility for seeing that these master programs, adapted to the particular site, worked as expected.

The computer and the computer program first became operational at McGuire in the summer of 1958. A party was set up to celebrate. It was a disorganized affair, attended by the press and by people who had played a role in the development of the SAGE system. My memory of it is not very clear, but I do remember a story told by Carl Overhage, who was the master of ceremonies. It was about Texas and Texans, and how everything in Texas is big — big men, big hats, and so on. An Easterner went into a bar, and the bar was shoulder-high to an ordinary-sized person,

but just right for a Texan. The bar stretched for hundreds of yards and the beer was served in gallon-sized jugs. After drinking a beer, the Easterner staggered to the end of the bar and towards the rest room, but he opened the wrong door and fell into a swimming pool. While he was struggling to stay afloat, two Texans came through the door. Horrified, the Easterner yelled, "No! Hold on a minute! Don't flush it!"

Lincoln was also responsible for building the combat center programs. The combat center consisted primarily of a large-scale display that enabled an air division commander to manage and monitor air defense operations being controlled at the sector direction centers. It received its surveillance and weapons control information from the direction centers within the air division. The combat center, via data link, also kept an inventory of the status of forces available to its air division, and was responsible for allocating interceptor aircraft supporting the air division through the direction centers.

A team under Walter S. Attridge, one of the Lincoln men, was assigned to Hancock Field, Syracuse, New York, to create the combat center master program and install it at the 26th Air Division's new SAGE facility. Attridge had been one of the original programmers for the early Whirlwind experiments and the Cape Cod System. His team was composed primarily of RAND and SDC employees along with a few Lincoln employees. The combat center program was much less complicated than were the direction center programs at the sites, and it turned out that this team was able to produce the program faster than the schedule called for and at less cost than was estimated. It was probably a first (and last?) in that regard. The first combat center was declared operational in the fall of 1958.

The first SAGE sector was assigned to Brigadier General Arthur C. "Sailor" Agan when it went operational. Shortly after that, a major design flaw was discovered: the tracking program was creating more than one track per aircraft. It was concluded that it was a "registration" problem. One of the features of the system was the overlapping radar coverage, and the registration problem might have been caused by having an error in the location of one of the radars. Since at least three radars could see the space at the same time, an airplane flying through the space would be reported by three different radars. These returns would not be synchronous, but were supposed to lie on a track corresponding to the flight path. Agan made a fuss about it, and we dispatched a team to analyze and correct

the problem. That team was headed by John H. Monahan, who was assisted by two people from our South Truro site. Jack Monahan had joined Lincoln after graduating from Boston College, where he had earned a master's degree in mathematics. Initially, he spent most of his working time on the Experimental SAGE Sector (ESS). Jack played a crucial role in the design of the SAGE surveillance system. He applied stereographic projection to compensate for the earth's curvature and solved many of the other problems associated with creating a common grid for locating all points in the sector's air space. He contributed to the plan that would lead to the BUIC (Back-up Interceptor Control) system, and became its project leader. Jack was a level-headed, laid-back, no-nonsense manager who could be counted upon to solve most of the software and hardware problems that came his way. He had a way of treating his subordinates fairly, which added to his general competence and created a desirable environment for the staff who reported to him.

In trying to track down the registration problem, Jack and I chartered an airplane from Provincetown/Boston Airlines, which flew a plane manufactured by Cessna that was then known as the "Bamboo Bomber," because of its wood and fabric construction. The Air Force would not let us land at McGuire, and we were waved off. We were forced to land on a dirt strip in Red Bank, New Jersey, and take a cab to McGuire.

When we arrived at the McGuire direction center, we were shown the symptoms of the problem; namely, each airplane generated two or more tracks. It was discovered that the National Geodetic Survey data, used for radar position information, had been incorrect. The Air Force had to get the correct position data in order to modify the locations held by the computer. This was done, but the problem of multiple tracks still remained.

Each site had both beacon and radar antennas, and attention was then turned to the alignment of the beacon antenna with the radar antenna. The beacon was used for Identification, Friend or Foe (IFF) and to enhance the radar returns from the interceptors, which were smaller than the bombers. The beacon antenna was attached to the top of the radar antenna and every beacon radar was slightly off-center with respect to the radar antenna on which it sat. There was a separation between the angles reported by the two antennas. The result of this was that the beacon-reported position would be off from the radar-reported position enough so that the tracking program presented two tracks to the surveillance system.

The designers of the program had anticipated this problem, and solved it by adding a correction to the angle reported. In fact, the beacon and radar antennas were lined up mathematically in the computer. The correction parameters were right but carried the wrong sign: plus instead of minus! Instead of subtracting the difference, the program doubled the correction and increased the problem. After the error was found and rectified, the “registration” problem went away.

Although we had anticipated the majority of the problems we would face in the field, there were nonetheless unpredicted problems that required a knowledge of the details of subsystem designs. Those people who had had a hand in the designs were pressed into service whenever the field system showed the need.

CHAPTER 29

SAGE SYSTEMS TESTING

As the SAGE design progressed from the component to the system level, Lincoln's testing philosophy evolved along with it. This philosophy was to be characterized by the use of simulations to determine the operating characteristics and reliability of a specific subsystem or component within the larger system. As a component reached completion, a breadboard or prototype was built which would then be subjected to as close an approximation of the external environment as could be achieved with available testware. If crucial testware was not commercially available, it was built in the laboratory. In this way, Lincoln was assured of thoroughly tested, predictable parts that had been tested as they would work in the whole.

In the early stages of development of Whirlwind I, for example, the individual electronic circuits were built and tested. Then, larger aggregates of components, such as the five-digit multiplier and the memory test computer, were built and tested to determine if these principal components of the system worked as the designers had expected them to work. This procedure progressed up the line as larger and larger aggregates were built: the high-speed memory was added, then the drums, then the input/output equipment, and so on. At each step, the appropriate tests were made of the added parts working as part of the whole system.

The same philosophy was followed when the project reached the Cape Cod System stage, when the peripherals were added to the computer. Several radars were added along with phone line connections and radar connections, so that the system could be tested in terms of overlap coverage. Similarly, the air surveillance elements were then tested together to provide an air surveillance picture. With both the tracking and surveillance portions of the system now operating, the weapons direction system could be assembled.

An important principle that engineers tried to apply to this kind of additive testing was to look directly at the function being tested, and do no more than would assure that first, the system would work, and second, that it would come somewhere close to specification. One of the lessons

learned in this process was that the amount of overhead required for test and design verification was almost as extensive as the amount required for the system itself, at least up to the direction center level. This overhead (mostly testware) would contribute to a later criticism concerning the expense of the SAGE project. But Lincoln held to its conviction that step-by-step, systemic testing would avoid costly, unforeseen problems later on.

At the systems level, the process of testing was called “shakedown” testing, and its purpose was to demonstrate how the whole system worked when doing the job it was built to do. Here, the idea was not simply to get numbers or to verify that the system could function, but to provide a more comprehensive, evaluative function to the entire project. Testing at this level was seen as part of a process which fed back to the designers, who then would be expected to take advantage of shakedown insights to improve system performance, or to correct problems that had become evident under test. Shakedown testing attempted to avoid the isolation commonly experienced when testing is carried out by a third party who is tasked with acting as an honest broker.

Overall responsibility for SAGE testing was held by Lincoln. Steve Dodd and Ed Rich took the lead in exploiting Lincoln’s Experimental SAGE Subsector, the prototype of SAGE used to test system elements and to measure operational criteria. Charlie Grandy was their associate. Ed Rich was an excruciatingly thorough, passive engineer who had a very good reputation from his work on Whirlwind, where he was in charge of a sort of “autopsy” group that analyzed every failure of vacuum tube or other component that went into Whirlwind. This activity was carried out by Ed with the support of a number of Whirlwind people, so that the choice of components could be based on actual experience. It was largely due to this activity that special vacuum tubes were designed by the manufacturers. And where Ed Rich developed the reputation for being slow, he was very careful and thorough in his diagnoses of component failure, and his work was responsible for informed judgments about the components. Charlie Grandy, on the other hand, was articulate, stubborn, confident and quick. Rich and Grandy carried out the shakedown process associated with functional capabilities, surveillance, identification, tracking and weapons control.

In addition to the shakedown tests, which were performed by Lincoln and IBM, acceptance tests were run at each of the SAGE direction centers as part of the acceptance process of the operational sites. Lincoln

was contractually obligated to specify the acceptance test, but in actuality, Bell Laboratories provided the manpower for this task. In many cases, Bell Labs' experienced staff proposed the test specifications, which were then signed off by Lincoln. Bell Labs also fulfilled their contractual responsibility of translating the acceptance test specifications into workable test methods, to be carried out in the field by Western Electric at the various SAGE sites.

The Bell-Western team was represented at Lincoln by Mike Burger, who had an office just outside the offices that Bob Everett and I worked in. He was a tall, affable, pear-shaped man who was given to long speeches about how things were done at Bell Labs. His most visible preoccupation was with his relative rank among the contractors and Air Force organizations that were participating in the SAGE job. I remember going to see him to talk about the test specs. His first reaction was to decide whether I was at an appropriate level in the Lincoln organization to be speaking to him. He had the organizational hierarchies of all the participating contractors drawn out on a paper and his level, he felt, corresponded to either Jay Forrester or to Marshall Holloway, the director of Lincoln Laboratory. In spite of this, we were able to work with him.

Within the Air Force, the testing area was the responsibility of the New York Project Office, but at Lincoln, the testing was monitored by Colonel William C. "Scottie" McLaughlin from ADSID. Scottie was a nervous, excitable gadfly, and he was continually proposing new tests that ought to be run. People liked him at first when he was operating as part of ADSID, but in time it seemed that his ambitions to manage the processes overcame his assignment to monitor what was going on. He set about trying to build a functional organization that would make testing a separate activity, rather than one of several steps in the development process that provided feedback to the design. His status in the Air Force was never quite clear to us; in fact, our most serious problem with Scottie was his lack of status. This lack became evident when he was not invited to a Lincoln Christmas party because he didn't rank high enough in the organization to be invited — the list of Air Force attendees had been made out by his superiors in the Air Force. We heard through various branches of the grapevine about the "status problem" and we felt badly for Scottie and had him invited. But his presence caused such an atmosphere of tension among the higher-ranking Air Force people at the party that the Air Force decided not to become involved in future Lincoln Christmas parties. Besides, the

Air Force was starting to think that Air Force/Lincoln parties might represent a conflict of interest.

Acceptance testing presented designers with a thorny problem, as it was difficult to project from a subsystem's test results its performance within the system. But in general, the range of performance expected of a particular machine or subsystem being accepted was determined by comparing it to the performance on the Experimental SAGE Subsector, whenever that was practical.

The Bell-Western team held the responsibility for evaluation, the last formal step in SAGE testing. Here again, it was hard to be definitive about what to expect in evaluation testing, as no acceptable definition of the system's value could be determined from contrived scenarios. The best that could be done was to demonstrate how well the system would perform each of its basic functions, in comparison to what the Experimental SAGE Subsector had demonstrated was possible during the shakedown and other testing.

CHAPTER 30

DIVISION 4

Within Lincoln Laboratory, Divisions 2 and 6 were dedicated to the SAGE job. The people in those divisions had come primarily from MIT's Digital Computer Laboratory and the Air Force Cambridge Research Center. Two other Lincoln divisions were peripherally involved in SAGE: Division 3, Communications and Components, and Division 4, Radar Systems and Components. Many Division 3 staff had worked under Al Hill at the Research Laboratory of Electronics, and had come with him when he joined Lincoln. This division concentrated on long-distance communications, and was working on troposcatter techniques. Division 3 fed information obtained from the Distant Early Warning (DEW) Line System into the SAGE communications network. Division 4 was dedicated to radar research, and covered ground radar as well as airborne surveillance radars. Although technically the radars were not a part of SAGE, the interfaces between the radars and SAGE presented serious integration problems. This was the area where Division 4 operated with us.

Like Division 3, Division 4 drew heavily on people from RLE. Division 4 was headed by Louis Smullin, a professor on loan from the Electrical Engineering department at MIT. Smullin was concentrating on a project called "Porcupine," a competitor for what was to become the Raytheon Hawk ground-to-air missile system. Porcupine depended on a large number of short-range radars. After the Army decided to concentrate on Hawk, Smullen returned to the MIT campus, turning over the division head job to Jerome Freedman. Jerry was a graduate of City College of New York in electrical engineering, and the Polytechnic Institute of Brooklyn. During World War II, he was in the Signal Corps and was stationed in the Pacific, where he was responsible for installing long-range, low-frequency search radars built by Westinghouse. Toward the end of the war, the new CPS-1, CPS-6 and MEW microwave radars were sent out, but they had a problem with backscatter clutter off the water.

In 1952 while at Lincoln, Freedman was involved in the "Summer Study Group — 1952," the project headed by Jerrold Zacharias of

MIT, investigating early warning for air defense of North America. A group was formed, which became Lincoln Group 45, to do work on airborne early warning (AEW) radars. The first AEW radars operated in the S-band and had clutter problems. Drawing from his experience in the Pacific, Freedman suggested the use of UHF instead. Once the change was made, the clutter problems were eased considerably. This airborne radar, the APS-95, was improved not only by going to a lower frequency, but by the addition of moving target indicators (MTIs). Freedman headed this group, and when he was promoted to Lou Smullin's job, Mel Herlin took over Group 45. Jerry was a gentle, pleasant man, and a capable and dedicated engineer. Because of his friendliness, and also his appearance, he reminded me of the home-spun comedian Sam Levinson. Jerry was to grow a magnificent walrus mustache that further emphasized his benevolent personality and enhanced his image considerably. He was invited to join MITRE when it was formed, but chose instead to stay with Lincoln, where he eventually became assistant director.

The APS-95 radar was mounted on Lockheed Constellation aircraft. One of these aircraft, belonging to the Navy, was used for an extensive series of tests run by Lincoln. In the design of the BOMARC system, it became clear that it was necessary to track low-flying airplanes over water and to get position data on these planes quickly. In one of the few meetings between Division 4 and Division 6, it was agreed that a direct data link ought to be included aboard the aircraft, and the data produced be fed directly into the SAGE network. After it was agreed that the idea had merit, we sold the idea to the Air Force, which, in turn, set up a project called the Airborne Long-Range Input (ALRI) program. William Canty was named project leader, and spent many years on the ALRI and the systems that evolved from it, including AWACS.

One of the groups that reported to Freedman was the Ground Radar group, headed by Fumeo Robert Naka. Naka was one of the Japanese-Americans who had been interned by presidential decree at the beginning of World War II. Due to the efforts of the American Friends Service Committee (Quakers), he was able to continue his education at the University of Missouri and the University of Minnesota for his bachelor's and master's degrees in electrical engineering, and later, at Harvard for a doctorate. He was taller than the average Japanese, was a deacon of his church, and was generally well-liked. Although thoroughly Americanized, he retained his ancestral tradition of politeness. He would gain a

national reputation for his work in intelligence collection. He was to serve as MITRE's chief scientist, became assistant undersecretary of the Air Force, and later, chief scientist of the Air Force. Eventually, he was made vice president at GTE Laboratories. I met Bob when we were assigned to the problems of integrating frequency diversity (FD) radars with SAGE. Bob and I became good friends, and we often engaged in a kind of repartee based on my trying to penetrate his oriental inscrutability, which made the negotiations for the radar integration activity fun as well as productive.

Under the frequency diversity program, many radars, operating at various frequencies between 300 to 3000 megahertz, would be geographically interspersed, making it difficult for the enemy to jam, as they would have to carry equipment to jam the entire FD radar spectrum. This program provided support for Lincoln's development of a large experimental radar to cover the low end of the radar spectrum. Under Jerry Freedman's direction, the radar was built and installed at Bath, Maine. Its antenna was placed on a circular tower 100 feet high, and the antenna reflector was 120 feet by 30 feet, requiring a very sturdy rotary bearing. The Bath radar went into operation in the summer of 1955, and was tied into the Experimental SAGE Subsector, for which it turned out to be an excellent data source. The success of the Bath radar prompted its redesign for use in the FD program. A prototype was built and installed on Boston Hill in North Andover, Massachusetts. Had the enthusiasm of the military for the FD program persisted, it was expected that this radar would have been produced for use throughout the country.

The long-range "workhorse" radar of SAGE was the FPS-3, a production ground-control intercept radar built by Bendix for the Air Force. Bob Richardson from Bob Naka's group was sent by Freedman to South Truro on Cape Cod to deal with a sea clutter problem apparent in the FPS-3 radar there. The "clutter" at first appeared to be random, and was considered to be noise, but Naka's people found that the returns seemed to move out from the land by day and return to the land in the evening. After a short-term (one week) study of the data, the sources of the returns were identified as birds. The use of radar in tracking birds caught the interest of the Audubon Society, and Lincoln was able to contribute to the Society's knowledge of the migration patterns of birds. Among other things, the study answered a question the Audubon Society was trying to answer: do migrating birds hug the coastlines or fly directly over water to reach their destinations? (The latter turned out to be the case.) The "clutter"

discovery was written up by Bob Richardson, and became the classic paper on the problem of radar clutter caused by birds. For many years afterward, Lincoln was considered to be the bird clutter expert.

CHAPTER 3 1

THE TASK OF INTEGRATION

As the machine and the computer program were assembled, the enormity of the computer system integration task stood out. The original SAGE design focused on the integration of existing radars and one class of interceptor. To be integrated now were at least three new interceptors, four or five different kinds of radars, "Texas Towers," picket ships, AEW&C aircraft, the B-version of BOMARC, the Nike Hercules ground-to-air missiles, and the Talos ground-to-air missile — all to be controlled by the SAGE direction centers. In addition, there was cross-tell and forward-tell integration among the centers themselves. Originally, one of the main arguments for having a general-purpose computer in the air defense system was that computer programs could be changed much more easily than hardware. One of the major misconceptions of many of the SAGE designers was that the computer program was so flexible that integration decisions could be postponed, to be handled later by adapting the computer program. As it turned out, not only did the integration process require changes in the computer program, but also often involved modifications in the hardware in the form of new peripheral devices or modifications in the input/output system. Furthermore, the cost of programming, of maintaining the program, and of documenting the code was almost as much as that of building the specialized hardware.

The integration of weapons into the SAGE system effected many changes in SAGE hardware and software. It was Lincoln's responsibility to coordinate these changes, and that function was handled through the Systems Office. The original Red Book Operational Plan was modified by Lincoln and ADC's Directorate of Planning into a document called the "Operational Employment Plan" (OEP). The OEP became the basis for the new specifications, which delineated the necessary changes to the hardware and software. These specifications were drawn up by Zraket's group. The Systems Office coordinated the various changes with other groups, and the changes themselves were produced by ad-hoc teams made up of our most knowledgeable people on each added subsystem, working in concert with the ADC Directorate of Planning.

The largest of the integration tasks was the integration of SAGE with the Air Force's BOMARC system. BOMARC was an unmanned interceptor that had a solid rocket booster which brought it to altitude, and ram-jet engines for cruising to the target. It also had look-down radar, which, when the target came within its range, took over and carried out the final phase of the intercept. In order to demonstrate that BOMARC could work with SAGE, a two-step process was instituted. The first step was to demonstrate that SAGE control was adequate for BOMARC. To do this, BOMARC was placed at Cape Canaveral, connected by long distance phone lines to XD-2 in Poughkeepsie. The radar in the Canaveral area was also connected to XD-2. This single-thread demonstration went very well, and proved that the surveillance and control equations were adequate.

In order to demonstrate the system aspects of the integration, a team was set up at the direction center in Montgomery, Alabama, under James J. Croke, to do more complex testing involving several intercepts and using all SAGE components. The test took place in Florida at the Eglin airbase which had a test range over water. BOMARC missiles were installed at Santa Rosa Island right outside of the airbase and were directed, based on instructions from the Montgomery direction center, to intercept drones which acted as targets. The tests were successful and proved that SAGE could automatically direct the aircraft in a multiple intercept situation. However, the effort and organization required to integrate BOMARC with SAGE led to a growing awareness that the job of integrating new weapons, radars and other capabilities into the system required central design control of all of the elements that affected the integration. The Lincoln team found itself immersed in the integration problem, under the joint direction of Colonel James Walker, ARDC, and Colonel Charles Minihan, ADC.

To be truly effective in maintaining system design control, Jim Croke had as members of his team (in addition to Lincoln people) small groups of experts from IBM, Bell, Western, SDC, Boeing, Burroughs, and other participating contractors. Jim was a consciously unconventional engineering manager. He wore his hair long when flat-tops and brush cuts were the rage. When the hippie movement materialized in the sixties, Jim cut his hair short and started to dress conservatively. He was a very bright engineer and emotional by nature. He attributed his emotionality to his parentage: Jewish, German, and Irish. Jim had an extraordinary sense of the morale of the people who worked for him, and was very successful at

placing them in positions of mutual advantage. After Jim was transferred back to Massachusetts, the Montgomery site was taken over by Lawrence Holmes. The only significant change in the operation that occurred there was that the secretary-receptionist was discovered to be running a call service from the office — moonlighting, more or less. She was quickly terminated by Larry.

The integration of Nike, the Army's ground-to-air missile designed and engineered by Bell Laboratories, illustrated another aspect of the integration problem: interservice disagreement on mission and doctrine. The Nike system had two versions: a short-range Ajax and a medium-range Hercules. These were essentially point defense weapons — groups of about ten missiles had their own sets of radars — one set for surveillance, one for target tracking and one set for missile tracking and guidance. Conceptually, the Nike could be placed on target by signals directly out of SAGE, but the Army, reluctant to relinquish control of its weapons, developed a coordination center called the "Missile Master" which would designate targets to Nike. The Air Force was opposed to the Missile Master, arguing that direct integration with a coordinated system like SAGE was essential in a situation in which several weapons might be brought to bear on a target and which would result in fratricide. The Army's reluctance to integrate with SAGE finally manifested itself in an argument about the bit rate that could be reliably transmitted over phone lines connecting the Nike system to SAGE. The Army claimed that 750 bits per second was the proper transmission rate. The SAGE system had adopted a bit rate of 1300 bits per second, which could easily be converted into 750 bits per second for directing the Nike batteries. Elaborate arguments were made on both sides about whether or not the translation was feasible. After some years of haggling, it was decided that it was possible, and SAGE control could extend to the Army missile battery level.

At one point during the disagreements between the Army and Air Force, I found myself as the Lincoln representative to an Operational Employment Plan meeting for Nike. A group of five or six of us from Lincoln and from the Army assembled at Colorado Springs for two weeks. An Air Force colonel was in charge. The colonel would start every meeting by providing minute descriptions of the symptoms of his ulcer, which seemed to correlate with the amount of haggling done the previous day. After the two weeks, he had added colitis and enteritis to his list of maladies. He felt sure that his career would come to an end with the

publication of the proposed plan, which would show that the Air Force had had to compromise, but this did not deter him from taking advantage of happy hour at the Officers Club every night. By the end of the two weeks, however, we had a workable plan.

The air defense system, as it was laid out prior to the advent of SAGE, had concentrated on a ring of heavy radars around the northern, eastern and western perimeters of the country. Nevertheless, the approaches from the sea were especially vulnerable. A bomber, flying low over water, could be almost on target by the time it was detected. BOMARC's effectiveness would be vastly improved if there were some way to detect targets at long range. The Airborne Early Warning and Control planes (forerunners of AWACS) attempted long-range detection, but, because they relied on voice or teletype warnings, were not very effective.

It was not until the AEW&C planes were connected with SAGE through the Airborne Long-Range Input program, which was established at Lincoln initiative, that it was possible to automatically report targets from the aircraft to the ground network. Another solution to the low flyers was the creation of the Texas Towers — heavy radars on platforms about one hundred miles off the coast. Three such towers were built on the continental shelf off the East Coast. These sites had heavy radars, height-finders, ground-to-air radio, and other detection equipment found at the manual sites, and were connected into the ground network. Eventually, the Texas Towers were abandoned after one of them was destroyed in a storm.

Not only were there problems integrating weapons like BOMARC or Nike, there was a new family of manned interceptors to be tied in as well. The F-106, an advanced supersonic air defense aircraft, was integrated into SAGE in the late fifties. The F-106 had a computer on board which allowed it to fly the mid-course under its own control rather than having to rely on vectoring instructions from the ground. The F-106 still had to be told the target location, but with that information, it could fly itself near the target, lock on its radar, and complete the final phase of the interception.

Each direction center was tied by phone to its combat center and there were phone line connections for cross-telling information from one direction center to another. Each radar was connected to at least two direction centers. This made it possible to pass hostile target data from one sector to another without losing the target. It also provided backup in case

one direction center was knocked out. In that case, another sector's control center could easily pick up the responsibility by utilizing radar data from the adjacent sector's radar.

Not only did all U.S. direction centers have to be integrated with SAGE, but a plan had to be developed to deal with the integration of both the Canadian radars and the Ottawa direction center which Canada was contributing to the U.S. effort. A joint project, called CADIN (Canadian Air Defense Integration), was established to connect the Canadian radars to the sectors within the U.S. The NORAD connection with other combat centers and the new evolving ADC operation center at Colorado Springs also presented integration problems which required centralized design control.

C H A P T E R 3 2

AIR FORCE REACTION AND THE BEGINNING OF MITRE

The Air Force reacted to the integration pressures and problems with a series of organizational changes. The first of these changes was the creation of a group called SWIG (SAGE Weapons Integration Group). This group was composed of representatives from the Air Research and Development Command and engineers from the weapons contractors' organizations. SWIG's job was to specify the changes required in SAGE because of new surveillance, weapons or sensor systems, and to make it possible for them to work with SAGE. In 1957, SWIG was tentatively established at Hanscom Field under Colonel Richard S. Carter. Colonel Carter was suffering from glaucoma, and he was afraid of deterioration of his vision. He had genuine difficulty in doing preparatory reading, so he depended on one of his captains, Brian Hastings, to run his organization. Colonel Carter set himself up in an SDC Butler Building with Captain Hastings and a secretary. I spent time with him estimating what he would need to do his job for SAGE. The gist of my message to him was that without technical help and without real rapport with the Air Defense Command planners, he didn't have a chance to carry out his objectives. I think that I offended him, but at the same time convinced him that he didn't have the organizational clout to make a go of it.

There was a strong feeling among those involved with SAGE that it was impossible to modify the system from outside; that weapons integration was a natural part of the continuing SAGE systems engineering job; and that SAGE would be under constant evolutionary change. It was generally agreed that there ought to be a central organization that coordinated and ultimately signed off on these changes. This organization would require more than just ARDC participation, and would, in fact, require participation of the ADC and AMC as well.

After less than a year, it became apparent that SWIG was unable to establish itself or to get the needed technical support from the weapons contractors. SWIG was replaced with an organization called the Air Defense System Management Office (ADSMO), which consisted of representatives from three branches of the Air Force, and was headed by

Colonel C.A. Thorpe of AMC. Thorpe was a good-natured, hard-working man who had little experience in the development business. His conception of the management office was that of a coordinating agency for all the proposals made by the organizations involved in integrating with SAGE.

Thorpe was a real character. He was likable and energetic. He would give long convoluted speeches about his coordination solution to the integration problem. On one memorable occasion, Thorpe invited the supervisors of the Lincoln Laboratory people who were supporting him, plus some RAND people, to his command post room, which had a U-shaped table facing a large motion picture screen. He proudly proclaimed, as he commanded the screen to roll and unroll by remote control, that he had all the tools, and he was ready to take on the problems of air defense integration.

The ADSMO mission was not very well integrated with the missions of other parts of the Air Force, so Colonel Thorpe set about the task of informing the other ARDC divisions of the ADSMO mission. I was invited to accompany him to the Eglin test range in the Florida panhandle to help him spread the word. Thorpe and I had worked out a pitch where Thorpe described the mission of ADSMO and I described how the Lincoln technical support was structured and what it did. The two of us would field questions, presumably about conflicts that might arise from the division of the work. Thorpe was a pleasant companion: good-natured, loquacious, folksy. He liked to talk about retiring to his native Kentucky and whiling away the hours under his walnut tree.

I was not thoroughly satisfied with the understanding of the parts that the two of us played in this "show and tell" exercise. I had a mild cold, so I was not as sharp as I would like to have been. We arrived at Eglin near evening, and were assigned quarters at the base. Because of my equivalent rank, we were assigned to a general officer's quarters. There were two beds in the suite, separated by a partition. I was tired, and because I had signed up to take a helicopter in the morning from headquarters at Eglin to Santa Rosa Island where the BOMARC launch pads were set up, I wanted to get to bed early. Thorpe was a night person, so I got to bed later than I thought I should. My situation was exacerbated by the fact that Thorpe was the noisiest sleeper I have ever encountered. He had a repertoire of snorts, gurgles, and chain-saw imitations. It was impossible to get to sleep. By early morning I couldn't stand it anymore, so I tried to have him roll over. I woke him up, but before I could tell him what was wrong, he jumped out of

bed, fully refreshed, and wanted to go to breakfast. I didn't see any point in pressing for more sleep because I had lost the skill. I sat bleary-eyed through breakfast and took the shuttle to the helicopter pad. I had never ridden in an open helicopter before and the noise numbed all my ear nerves. I stumbled around the BOMARC launch pad and watched them erect the BOMARC for firing, all the time being aware that I couldn't hear a thing, and climbed back into the helicopter. When I got back, I found Thorpe in the room that had been assigned for our briefing. The commander at Eglin said something which I couldn't hear, but I gathered it was a polite introduction from the practiced smile that he gave to Thorpe when he introduced him. I didn't hear what Thorpe said either, so I missed my cue and was dozing off on one of the front seats facing the commander. I gathered from Thorpe's gestures that it was my turn, so I got up, only to find that a small speech was to be made by the local chief scientist. I was in a panic, and didn't want to cause any more embarrassment, so I made sure that I was to speak next before I got up again. I couldn't hear a thing. I was stopped by hand signals every once in a while when someone asked a question. Thorpe fielded the questions since I couldn't read lips. I finally went through all of the points that I wanted to make and sat down. Thorpe came over and shook my hand. I could make out from his reaction that we had made a hit with the Eglin folks, and we were then whisked off to the airport to an old DC-3 which was fitted out to be used by colonel-level commanders for their transportation.

Thorpe apparently sincerely believed that I had done a good job, because he made a point of praising my performance whenever the topic of organizational integration came up. It was because of incidents like this that I felt that I had lost my ability to evaluate my own performance, and I decided no one needed to know how confused I had been. I decided that success was not necessarily related to performance and I took credit for this fortunate accident. It balanced off the times when my performance was good but the audience didn't like it.

About the time ADSMO was created, negotiations were taking place between James J. Killian, Jr., then president of MIT, Julius A. Stratton, MIT provost, and James H. Douglas, Jr., secretary of the Air Force (assisted by George Valley), to find an organization to take over the system engineering for the SAGE system.

The Air Force had tried a number of techniques for providing technical support for the integration problem. They had talked about

industry combinations for support or about giving the task to a single company, and to eliminate conflicts of interest, restricting what that company could manufacture for the system. Under Douglas, the Air Force approached Bell Laboratories and Western Electric to see if they would take over the job. Bell and Western declined the offer but did agree to continue with the work they had been doing with ADES. They felt, however, that the central design and system engineering role ought to remain with Lincoln.

Misgivings were expressed by those organizations that had been approached by the Air Force for the job, among them RCA. While the manufacturers were willing to do the job, they were not willing to accept the restrictions that went along with it; that is, the exclusion from bidding on the hardware that would result from the SAGE modifications. Also, the idea of expending their best people on a job with so little financial leverage did not appeal to them at all.

While these possibilities of private industry sponsorship were being explored by MIT and the Air Force, Bob Everett and I were asked to talk with organizations that were interested in applying for the task. I remember a meeting we had with Jordan Baruch of Bolt, Beranek and Newman, one of the profit-making companies, about Jordan's conception of how the job would be done. He described a concept in which participating organizations would assign their most valuable engineers to a group. This elite group would continuously receive motivational training which would supposedly keep it unified and concentrating on the job at hand. The notion struck Bob and me as weird and inoperable.

Given the overall lack of interest by profit-oriented industry in completing the SAGE engineering job, MIT and the Air Force began thinking in terms of setting up a non-profit organization. The job of defining such an organization was given to Jim McCormack, then vice president of MIT, who had recently retired as a major general from the Air Force. McCormack was fresh from setting up the Institute for Defense Analyses, so establishing what would eventually become the MITRE Corporation was a natural assignment for him.

A West Point graduate and a Rhodes Scholar, McCormack had been active in atomic weapons development while in the Air Force. A heart attack had forced him to retire. At MIT, he had taken Admiral Edward Cochrane's place as the vice president in charge of the defense work that MIT was pursuing. He was known for his striking resemblance

to Edward VIII, the Duke of Windsor, who abdicated the British throne in 1936. McCormack had a tremendous capacity for consumption of martinis at lunch. I had been at luncheons with him where he consumed three or four martinis without any visual effects. He could also ad lib summaries of complicated conferences. At one point, there had been a space symposium at a Boston hotel, organized around approaches articulated by Werner von Braun. Jim sat through the presentations, and after lunch on the last day of the symposium, he was asked to summarize what had gone on — this, after having consumed his martini ration. He got to his feet, and, without notes, delivered a thorough yet brief summary of most of what had been said. I remember telling him that I had enjoyed his summary, and I asked him how he had decided what to say. He responded by telling me that he didn't know what he was going to say until he said it.

During this period there were several challenges to the concept of developing a not-for-profit organization; most notably, the examples of other companies which had been established for this purpose and had eventually run into trouble. On the other hand, there were successful not-for-profit organizations like RAND, and the newly formed System Development Corporation (SDC). SDC, in fact, felt that they were the natural choice for the SAGE system engineering and integration job. They had responsibility for the master program and were connected with ADC through both the training and programming products. McCormack had several discussions with the SDC/RAND board, but decided that it would be important to include in the new organization the people who had worked on system engineering at Lincoln, since they would have background on all facets of the system. As a step in that direction, I was asked to prepare the first draft of the work statement for the contract which defined, in general terms, the function of the not-for-profit organization.

It finally came down to a choice between the not-for-profit SDC and creating a new not-for-profit company. The Air Force, growing increasingly concerned about the SAGE integration problem, felt that Lincoln people, because of their familiarity with the program, should be involved. In May, 1957, Brigadier General I. L. Farman and Colonel Gordon T. Gould of the ARDC System Management Office at Wright-Patterson visited Carl Overhage at Lincoln. General Farman had gotten the word that Secretary Douglas favored the use of Lincoln people, and had come to find out from Overhage if Lincoln would do it. Bob Everett and I were both at this meeting. It was determined that another meeting should

be held, at which the choice between SDC and a new not-for-profit organization would be made.

The follow-up meeting included Bob and me, Overhage, Colonel Gould and other colonel-level people responsible for electronic systems at Wright-Patterson, and M. O. Kappler from SDC. Colonel Gould arrived early to make arrangements with Overhage to use a Lincoln conference room for the meeting. We were all assembled except for Colonel Forrest G. Allen from Wright, who arrived a little late. Colonel Allen was a spit-and-polish colonel who smoked cigarettes using a foot-long cigarette holder which he carried like a riding crop. He strolled into the meeting and draped himself across several chairs. His arrival was a signal to begin the meeting. During the first part of the meeting, Everett was asked to describe the potential organization. He emphasized that the organization should include both hardware and software as well as enough research and development capability to verify designs and to increase the validity of projections. The proposal essentially described the operating concept used at Lincoln.

We broke for lunch. Kappler didn't eat anything in order to be sharp when he was scheduled to speak. After lunch, when we returned to the conference room, Kappler made his pitch that the system could be controlled by the designs of the computer programmer, and that hardware and design verification could be done by others. Following Kappler's speech, there was a disjointed conversation which avoided direct reference to the selection. Colonel Gould said something about how productive the meeting had been. Colonel Allen smoked his last cigarette and stuffed his cigarette holder into his briefcase. Overhage made a noncommittal speech, declaring everyone was happy. Bob and I looked for a consensus but were unable to find one. We went away feeling empty. Sometimes it happens that way.

Kappler was counting heavily on getting the SAGE system engineering job. As president of SDC, he was responsible for employing a large number of system programmers, and he desperately needed work to follow the SAGE programming job. He took the position that if he couldn't be given the job sole-source, he would have to compete with private profit-making companies, which in fact he began to do soon after the meeting. He eventually pitted himself against his own board on that subject, refusing to limit himself to what could be given him sole-source by the government. He tells the story about meeting Bill Golden, his board chairman, on

what he thought was a routine trip from the airport. He was given the news by Golden that he was to be replaced by Wesley Melahn, and was given a termination settlement. Within a year or so, the board concluded that Kappler had been basically right, and that SDC ought to become a profit-making corporation and compete for work like any other profit-making operation. When this information got back to Kappler, he told me that his first reaction was to hire a sky-writer and have him write out, "I told you so!" directly over Golden's office in Manhattan.

The board eventually felt that Wes Melahn did not have the necessary experience in the private sector. The story was, that acting with typical insensitivity, they advertised for Melahn's replacement in the *New York Times*, which was how he learned of their decision.

Between the Air Force headquarters (including the secretary of the Air Force) and MIT management, a decision was finally made to establish a new not-for-profit company to carry out the tasks spelled out in the work statement that Bob and I had prepared. As a peace-making gesture to SDC, several members of the SDC/RAND board would be invited to become board members of the new company.

As part of the package negotiated by MIT, it was agreed to upgrade ADSMO and to put it under the direction of Major General Kenneth P. Bergquist, with an ADC component headed by Brigadier General Loren McCollom and AMC's Major General Clyde H. Mitchell. In the interim, Lincoln Division 6 would provide temporary support to this newly formed Air Defense Systems Integration Division (ADSID).

General Bergquist came from ADC headquarters, and had had considerable experience in air defense. He was in Hawaii as part of a radar squadron, and it was his radar that should have provided tactical warning of the Japanese attack on Pearl Harbor. He had been through the post-mortem Pearl Harbor investigation, and didn't want anything like that to happen again. He had a deputy for engineering, a Colonel Wilfred H. Tetley, who would have liked to have been a scientist, and whose office blackboard was always covered with Greek symbols and equations with which he tried to defend himself from the Lincoln crowd. Tetley was given the job of interfacing with MIT management, and from his point of view, what MIT did was subject to his direction. He had been in Hawaii with General Bergquist, and had been traumatized by the investigation. I was his MIT counterpart, and he used to come around and complain that he was being ignored by Lincoln. The Lincoln people were used to ignoring

bureaucratic reporting procedures and Tetley used to try to get my attention by bending my ear on the mathematics that were the basis of the SAGE radar and computer operations. When he found that I was not responding with proper enthusiasm, he would mix his lectures with descriptions of the horrors of being a retired colonel who could at any moment be jerked out of retirement to attend his own court-martial! I always felt that Tetley should have been a mathematics teacher.

ADSMO was a short-lived organization which devoted much of its effort to the planning rather than to the implementation of its functions. Its original charges were to oversee the integration of the various components of the air defense system and to plan for the future evolution of the system. With ADSMO receiving this comprehensive charge, the ADES office, originally designed to be the coordinator for the various organizations involved in the air defense task, saw a reduction in its scope: ADES, combined with the Electronic Defense System Division, was now called the 216L Project Office and was to handle only the ground electronic portion of the air defense system. It was placed under ADSMO, rather than under the Air Force Cambridge Research Laboratory. Without any link to what was going on at Lincoln, ADES was at a technical loss. Thus, ADES requested and received, with much appreciation, a cadre of knowledgeable SAGE people from Lincoln (under Jim May from Division 6) collocated in New York.

At the same time, the definition of ADSMO's role and the scope of its concern continued to expand to the point where many Air Force people thought it should report to Air Force Headquarters rather than to the ARDC. They thought it needed this higher-level connection to cover the management, development and procurement of weapons and systems as well as of the electronic ground equipment. This thought was given some emphasis as negotiations proceeded. The model for this special connection with headquarters was drawn from General Bernard A. Schriever's Ballistic Missile Division on the West Coast. But General Schriever's bypassing of ARDC headquarters had caused enough friction in the Air Force so that people were hesitant to do it again, especially since by now General Schriever himself was a three-star general and head of ARDC (later called Air Force Systems Command). He had responsibility for ADSMO as well as for the Ballistic Missile Division and the Aeronautical Division at Wright Field. Schriever took over many AMC functions, among them development and systems acquisition.

Schriever was opposed to placing ADSMO directly under Air Force Headquarters control, and ultimately, ADSMO was placed under Air Force Systems Command (AFSC). Schriever was a man who got his way. His strong physical presence (he had a well-built, well-tanned, immaculately groomed 6'4" frame, and was an excellent golfer), combined with his political expertise and connections, made him a formidable adversary at the internecine competitions, where he won most of the contests. Under AFSC, ADSMO became ADSID, headed by General Bergquist. In the course of the negotiations regarding ADSMO's reporting level, Bergquist had argued very strongly for the connection to Air Force Headquarters, and because it was at cross-purposes with what Schriever wanted, Schriever placed his own man, Brigadier General Charles H. Terhune, as the vice commander of ADSID. Terhune later became deputy commander of ADSID's successor, the Command and Control Development Division (C²D²). C²D² became the Electronic Systems Division, and as a major general, Terhune was its second commander.

Bob Everett and I were at this point the senior technical people left on the job. In mid-1958, Overhage and McCormack nominated "Hap" Halligan as president of the new not-for-profit corporation. He was the director of military engineering at Bell Laboratories and had been on the CADS program and on the Bell/Western portion of the SAGE task. Halligan was an experienced Bell Laboratories man in his late fifties. He had been a source of support to the SAGE system in his ADES role, and Overhage and McCormack chose him because of his maturity as well as his Bell Laboratories background. Hap was endorsed by the initial MITRE board under Rowan Gaither, who was to die of cancer prior to taking any effective role in the management of MITRE.

Everett and I were asked to become the technical director and associate technical director of the new company in October, 1958. We both accepted. The other 270 people from Lincoln Division 6 were targeted to join in January, 1959. In order to get the company started, it was decided that MITRE, as the company was named, would be given a \$13 million startup subcontract from MIT for the first six months of operation. Bob Everett and I drafted the work statement for this subcontract, which was based on the proposal we had drafted earlier.

C H A P T E R 3 3

HALLIGAN

When Halligan was named president of MITRE, it was well known that Jay Forrester opposed the selection. Halligan invited Jay to his office, and also invited Bob and me to sit in, to talk over Jay's objection. Jay, in his usual straightforward way, said he felt that whoever took on the job of president would have to be totally committed to it, and he questioned Halligan's dedication, since Halligan had taken only a leave of absence from Bell Labs to become president of MITRE. Halligan noted the fact that he had a stake in Bell Labs' retirement fund, and protested Jay's objection. He did not see why this would be an impediment. After some minutes of reiterating their respective views on the subject, Halligan asked Jay if it had been his desire to have the job, thinking Jay might have felt it ought to have been offered to him. Jay, in his characteristic manner, told Halligan that the industrial dynamics program he was creating at MIT was infinitely more useful than anything the new MITRE organization might create. Jay did not mention the looming legal battle with IBM for the patent to the random access core memory — and the time and effort he knew it would take to ensure a favorable outcome.

Halligan was a good-natured, easy-going man of good reputation who was used to managing in the conservative style of the Bell System. He tended to favor the status quo and avoided controversy, especially in dealing with the MITRE board and with the Air Force. At Bell, Halligan was used to having the power of the organization above him. But at MITRE, the power of the organization was below him, and those holding the power were on the average of 10 to 20 years younger than he. It was awkward for him to delegate without delegating his whole job, especially since he was bedeviled by criticisms of the stubborn, independent "Lincoln clique." In an effort to bring in new blood, he hired a retired Air Force lieutenant colonel, Peter Schenk, as executive vice president. But he never gave Schenk any real authority. This situation lasted about a year, until it became clear that Schenk could not justify his position, and he eased himself out. MITRE used him as a consultant for a little while.

I believe Hap felt he was being supportive of the organization

he inherited from Lincoln, but couldn't find a solution to relieve the pressure of the criticisms. I liked him, but was always distressed by what came across to us as a lack of support. My own relationship with Halligan was colored by the apparent attitude he held toward us — one of resignation rather than encouragement. With me, it centered on my communication skills with outsiders, particularly briefings to the Air Force. All of my activities were devoted to developing a program with the Air Force and planning projects that met their needs. I had a natural tendency toward stage fright, of which I was aware, but which I had overcome, or so I thought. My communication with large groups was good, I felt, but I needed the encouragement of our management, which Hap withheld. I would prepare speeches that were good in my opinion, but dry runs with Hap would cast a cloud of doubt. Hap would say, "I don't like it," but wouldn't explain. This resulted in my being overwhelmed by stage fright, and caused me to fumble the first few lines of my talks, only confirming Hap's judgment. It got to the point where a professional speechwriter and communications expert was hired to teach us, especially Everett and me, not to mumble or otherwise give bad speeches. We had come from a culture that was content-oriented (MIT), where use of communication aids, such as view-graphs or slides and slick presentations, were somehow suspect, and where speaking from notes scribbled on the back of an old envelope was considered a virtue. Hap and I never resolved this difficulty, which only got worse with time.

Needless to say, Everett's response to Halligan was as the subordinate who held all the chips. This put Halligan in a position where he would have to justify changes in organization or direction, rather than allowing himself his prerogative of rank — namely, the right to be arbitrary. This isn't to say that Everett was disrespectful or insubordinate, but he required too much justification to be brushed off, and Hap didn't have the energy for the work that would be required to resolve the issues and take personal control of the situation. From outward appearances and socially, they were quite compatible, but repetitions of these encounters prevented Halligan from endorsing Everett by recommending him for membership on the board. He did, however, change Bob's title from technical director to technical vice president.

To his credit, Hap managed as MITRE's first president for eight years, during which time the company developed and adapted to the new problems with considerable success. He broadened the reputation of

the company in the communications field, where he drew upon his knowledge of the Bell System, and he attracted many competent and experienced people. Hap also succeeded in providing a mature front for the largely less experienced and less polished Lincoln staff. In retrospect, Hap may have been the right man for the job, and he probably did more for MITRE than my own biased experience with him would admit at the time.

In spite of the problems that he had in dealing with us, Hap established a corporate structure that was well-balanced and thoughtfully developed. In particular, he initiated a formal "policies and procedures" book that set the limits on the authority and scope of various parts of the organization, and he assigned a number of administrative and finance people to set the procedures down and coordinate them. Although there were some differences of opinion about these policies and procedures, the overall effect was to force the management to be open and explicit about MITRE operations.

Shortly after the formation of MITRE and the Air Force's Electronic Systems Division (ESD), MITRE realized that its job had drastically changed from a one-project (SAGE) task to a many-project task. This required a new orientation, and a new way of working with the Air Force system program offices. There was in particular a problem of dividing MITRE effort among a large number of ESD projects, and at the same time, ensuring that each project received the best mix of skills. The embryonic ESD, in trying to find its niche in the Air Force world, was particularly concerned about the nature of support that MITRE would now provide. Largely due to Hap's effort, MITRE negotiated a memorandum of agreement with ESD's commander, Major General Charles Terhune, in December, 1962. The agreement spelled out the classes of work that MITRE could be called upon to do, and defined system/subsystem engineering, task engineering, and research and experimentation as major categories. In addition, it dealt with the technical direction authority, which was a joint ESD/MITRE product. It also outlined the parallel lines of authority which the MITRE project leaders and ESD project officers were expected to follow, especially in the case of disputes, and it defined the Technical Information Report (TIR), the formal vehicle for transferring MITRE designs and technical direction to ESD. The memorandum of agreement would be modified in later years to adapt to changing situations, but it provided the framework of the MITRE/ESD contract.

At about this time, the Holifield Congressional Subcommittee on

Military Operations was investigating federal contract research centers (FCRCs), a group of contract research organizations that had been established as not-for-profit corporations (including MITRE, SDC, RAND, Aerospace, Lincoln, IDA and others) which supported primarily the Air Force in its system development programs. One of these, Aerospace, and its president, Ivan Getting, had drawn attention to themselves by having the government pay for transporting Getting's yacht from the East Coast to the West Coast, and by Getting's salary of about \$90,000 per year, which was double the salary of the congressmen who were now investigating Aerospace. Among other things, the investigation resulted in a rule that no officer of any FCRC would be paid a salary higher than a congressman's salary (which was \$45,000 at the time). At the hearings in Washington, all the FCRCs took their turn in describing the nature of their work and their relationships with the government. The effort Hap had put into the memorandum of agreement with ESD found its place at the hearings, where Hap, in a polished, confident and cool presentation, went over these matters with members of the committee. We were proud of our appearance before that committee.

After the challenge to the use of FCRCs, Major General Ben Funk, commander of AFSC's Space Systems Division, which had employed Aerospace, was given the assignment of defining the appropriate role that FCRCs should play, and the numbers and kinds of staff that ought to be employed. This job was referred to a committee set up by Funk, which was known as the Funk Committee. It consisted of Halligan and the other presidents from all the FCRCs. Each FCRC tried to define the proper role it should play, and tried to define the way the Air Force program offices should use them. The Funk Committee report did that to the best of its ability, and the book was used as a platform for discussion, seeking to make the process of allocation and assignment less subjective. The Funk Committee report became the major reference for the many reviews that would be made of MITRE's productivity.

As part of setting up the organization to work on many projects instead of one, it was necessary to establish a way of charging directly those funds for the specific projects. The major expense was the charge for MITRE staff time, and some way had to be invented for allocating charges among the projects. Hap assigned me the task of designing and effecting an appropriate time allocation system. Needless to say, this was an onerous assignment, in view of the staff's long tradition of opposing any kind of a

time card system. We finally settled on a system in which the allocation of staff by project was installed, and each week, the staff member would record hours spent on a project and the supervisor would endorse it. It was for me one of the most frustrating tasks I had taken on at MITRE. Hap didn't have anything to say about the job I'd done, which I took to mean that he liked it.

CHAPTER 34

LEAVING LINCOLN AND JOINING MITRE

In establishing the new organization, McCormack and Overhage faced the problem of finding people who both knew the SAGE job and would be willing to join the new company. It was logical to them that a major part of Division 6, or all of Division 6, ought to form the nucleus of the organization. There had already been a sort of rift between Division 2 and Division 6 on the subject of the control of the SAGE system design, and there seemed to be some sentiment on the part of Overhage and the rest of the Lincoln Steering Committee that it would be desirable if Division 6 could be split off from Lincoln so that Lincoln could continue its research in its own way.

By the time of the rift, Division 6 had evolved its own main groups. There was a group devoted to operational mathematical specifications; a group that did programming; a group that did testing of ESS and Montgomery, and a Systems Office. There was also a research group, under Bill Papian, which was then concentrating on research on the TX-0, an all-transistorized advanced computer, which was expected to lead the trend in computer design. While the rest of Division 6 opted to transfer to the new company when it was formed, Papian's group chose to stay with Lincoln.

Prior to the transfer, Division 6 had been working with ADSID for about a year so the transfer was not traumatic. Those who were already working for the Air Force were to continue to work in the same capacity (and in the same Lincoln office space) after the transfer. I felt no pangs about making the transfer. I thoroughly believed that the new arrangement would make it easier to do the integration and overall systems engineering job. But, as in many major changes, there remained a certain amount of hostility between Lincoln and the part of Lincoln that went to MITRE. It took the form of disparaging remarks about the meaning of the acronym MITRE. Among the most humorous remarks were these: that MITRE stood for "MIT Reject Engineers" or "Must I Trust Robert Everett?" or, in reference to the fact that there were a lot of Italians in the support groups, "Many Italians Trying to Run Everything."

To celebrate our leaving, the Steering Committee had a dinner for us at the MIT Faculty Club. After the dinner, Carl Overhage, director of Lincoln Laboratory, spoke. Overhage's favorite way of handling this sort of toastmaster job was to characterize people who were leaving by making reference to classical works. This time he referred to Shakespeare, and made me Hamlet. I took his remarks to mean that, like Hamlet, I was diffident and often paralyzed when the situation called for action. Usually I enjoyed Overhage's farewell addresses, but this time I didn't. Although I was leaving to join the new company, I believed, as Bell and Western believed, that Lincoln could have picked up the continuing role of system engineering on the air defense system. I made what I now consider an indelicate response to Overhage's talk. I chose to tell a story, a true story, about a friend of mine who was an anchorman on a television news show in Poland Springs, Maine. He commuted to work every day in a carpool. One day, driving particularly fast, he encountered a moose wandering along the highway in his lane. Because there was a car coming in the other direction and he could not stop in time, he hit the moose. The moose slid over the hood, smashed through the window on the passenger side where his carpool mate was sitting, and sprayed "moose juice," so to speak, all over my friend's passenger. When the car came to a stop, the passenger looked over at my friend who was laughing about the scene and he said, "My, Charlie, you're taking this wonderfully well." I concluded my remarks by saying, "My, Lincoln, you're taking this wonderfully well." As soon as I had finished the story I was sorry I told it. As a farewell gift to all the departing Lincoln Steering Committee members, we were given anodized aluminum trays, the borders of which had a number of Lincoln-head pennies with the mint year corresponding to each year spent at the MIT Lincoln Laboratory.

During the period of the transition, Bob Everett spent quite a bit of his time ensuring that the retirement benefits which we had accrued at Lincoln would be held in effect and could be vested at a later date. During the course of that negotiation, Everett managed to get at odds with Joseph Snyder, who was vice president and treasurer at MIT, and Bob's behavior, which was characteristic of him, was tenacious and logical, if not insubordinate. Ensuring that MITRE people would get these benefits was an additional incentive for joining the company. Bob won the point, but in the process alienated Snyder. The retirement issue was one of many similar issues too numerous to mention that would have to be dealt with, and

and provided adequate opportunity for friction between the new MITRE management and MIT. All in all, the friction increased concern about giving Everett the position I believe he deserved as an officer of the newly-formed MITRE. Instead of becoming Halligan's vice president, he was named technical director.

* * *

One of the outstanding things about the Digital Computer Laboratory and Division 6 of Lincoln Laboratory, and I think to a large extent the other divisions of the laboratory, was the esprit de corps — the spirit that pervaded the operation. Everyone had a sense of purpose — a sense of doing something important. People felt the pressure and had the desire to solve the air defense problem, although there was often disagreement as to how to achieve that end. Energy was directed more toward solving individual problems, such as making a workable high-speed memory or a usable data link, than it was toward solving the problem of the value of the finished product. It was an engineer's dream. The responsibility for achieving that end was liberally distributed among the participants. I know that I felt the need to do something useful at every level of responsibility that I held.

Today, Jay Forrester feels the reason for this spirit was the reward/punishment system that was operating. In most organizational situations, there is a high penalty for failure and a low reward for success. In the Digital Computer Laboratory and Division 6, the opposite was true: there was a low penalty for failure and a high reward for success. People were willing to try things out and fail, and try again, without the devastation that comes as the high penalty for failure. It was an ideal climate for bringing out the best and the greatest number of ideas in working groups. Not only was this relationship between reward and success operating, there was also the feeling that each engineering problem should have one or two backups to ensure the success of the enterprise. Typical of this was the core memory backup for electrostatic storage. There was also a lot of attention paid early on in the development of any part of the system to its

eventual upgrading and replacement. This attention actually sped up the process of acquiring new and extended capabilities for the system.

In addition to this philosophy, a high degree of confidence in the MIT people was shown by the administration. We were often told about Nat Sage, the principal man in the MIT administration who shaped MIT contracting policy. He set up regulations and procedures that were deliberately ambiguous which could be interpreted by the contracting officer to meet the needs of the situation. Sage would insulate the people working in the laboratory from the nitpicking of the customer.

The transition from Lincoln to MITRE occurred in stages from 1958 to 1960. The biggest group transferred on January 1, 1959. The Lincoln people generally believed that the success they had at Lincoln was due in large part to the fact that they could take the initiative in making technical decisions. They wanted the same freedom at MITRE. They wanted to be able to tackle systems problems as a whole as they had done while at Lincoln — to decide how much planning and analysis ought to be done, how to allocate their resources, and the like.

This need for autonomy on the part of the MITRE staff was at cross-purposes with the desire of the Air Force to take on the responsibility for the design and management of procuring the system. Much effort was expended by ADSID people to get control of the MITRE program. In oversimplified terms, the desire of people at the System Program Office (SPO) was to obtain manpower from MITRE they could not otherwise acquire through military and civil service sources. In the extreme, the SPO wanted to be able to individually select and integrate into their organization MITRE people for the project.

Doing design verification — building breadboards and brassboards — was another area of disagreement between MITRE and ESD. MITRE people thought it important to be able to do design verification to prove the feasibility or utility of designs of which they were not quite sure. Design verification also fulfilled another need: it kept the staff current in the state of the art so they could make judgments about the proposals the industrial contractors prepared. The System Program Office most often looked to other contractors to provide these proofs of performance. They wanted MITRE to do primarily administrative work. From their point of view, the same budget could provide for more people to do administrative work than hands-on work because of the cost of the facilities required. Nevertheless, MITRE managed to get an agreement that at least 10 percent

of their work would be hands-on. The MITRE people thought a better ratio would have been a third or more, depending on the particular problem they were addressing.

There was also a resistance to overplanning the use of the MITRE staff, since it was a belief at both Lincoln and MITRE that problems cannot be predicted accurately before trying out the design, and that it is important, especially in system engineering, design control and design verification, to be able to accommodate problems as they might arise. For a time, MITRE people refused to divide a job into subtasks, on the grounds that it would allow for more rigid allocation of resources and thus preclude the adaptation which would be required to meet the real problems as they came up. However, MITRE did agree to reporting, after the fact, on the cost of the actual jobs which were done. Eventually, MITRE and SPO came to terms with plans that were broad enough to accommodate the necessary changes as time progressed, but sufficiently defined to outline what MITRE was to accomplish during that period.

The concern on the part of MITRE people continued to manifest itself in such things as resistance to the introduction of time cards and the use of stickers on car bumpers which were a pass to the Hanscom Air Base, because of the implication that the Air Force was fine-tuning its control over MITRE resources. In some parts of the Air Force, there was a drive to govern precisely what should be done on a MITRE project, and to monitor MITRE expenditures. This monitoring eventually led the Air Force people who were responsible for MITRE contract management to do helicopter surveys of the MITRE parking lots at various times during the day to verify attendance. We were used to the MIT philosophy that quality of time on the job was more important than attendance. We agreed, after considerable harassment, to do the monitoring ourselves. We also agreed on a standard time sheet that reported an individual's time on a project-by-project basis. We finally settled the issue by suggesting to the Electronic Systems Division that those Air Force people who were stationed in the MITRE buildings, and whose attendance was less than perfect, be treated in the same way that MITRE treated its staff. We proposed that the military people, as well as MITRE people, use a standard time sheet. We said that when MITRE staff members came in late, they would be asked to make a visit to the Division Office to explain their lateness or lack of time spent on a project. The military people, at the same time, would report in a similar way to their deputies. We proposed that this was only fair, and that the

symmetry of the proposal would prevent attendance information from being used by either the military or MITRE people as leverage in local squabbles. This broke the back of those who were bent on fine-tuning the contract control. After we presented our “symmetrical” proposal, the Air Force dropped both the surveillance and the subject.

The attitude that led to these situations was pervasive, and prompted an Air Force investigation of the situation by a Colonel Hunn. Hunn naturally felt that there ought to be a SAGE budget that broke down into areas of work and their respective costs and schedules. It was my job to convey to him the MITRE philosophy — which was that we would tell him the budget after the fact. Although some officers were incensed by the MITRE position, Hunn took a more permissive attitude, and he listened sympathetically to our explanations and the crude way in which we handled the SAGE budgeting. His patience paid off in our eventual restructuring of the contracting policy, and in the institution of the TO&P — Technical Objective and Plans — a document that spelled out the boundaries of the projects but that allowed for trade-offs among them.

C H A P T E R 3 5

FROM SAGE TO BUIC

During the time that MITRE was getting established, several things were happening that were lowering SAGE priority. Internationally, there was the growing awareness that the Russians were concentrating on ballistic missiles rather than on aircraft. This concentration on ballistic missiles for strategic offense by the Russians was underscored by the launching of Sputnik in 1957, and the growing perception that there was or may have been a missile gap. The Air Force emphasis changed from defense to offense (deterrence) and from aircraft to intercontinental ballistic missiles, and the budget for these missiles competed for funding across the board.

At the same time, SAGE was being criticized as overexpensive, and the charge was levelled that SAGE was “gold-plated.” The cost of SAGE had escalated far beyond the early estimates of the project, and for the first time, the costs of the project were seriously being questioned. It had seemed that the cost of the system had increased greatly during the course of its development. However, the Air Force claimed that it was a “bookkeeping” increase. They maintained that the increases were due to the broadened scope of SAGE, from its original definition of computers, data processors and digital communications to be added to an existing air defense system (on which the original estimates were based) to the entire air defense system, including radars, ground-air communications, all point-to-point communications, command headquarters, and so on. But the President’s Scientific Advisory Committee, set up to examine air defense, was seriously critical of many air defense expenditures, and the newly formed Deputy Director of Defense Research and Engineering (DDR&E) under Herb York and his deputy Hector Skifter began to question the cost-effectiveness of the whole operation. Also during this period, a committee headed by John Klotz was formed by the Department of Defense and DDR&E to investigate the SAGE system. The Klotz Committee was essentially a data-gathering organization. It was obvious that its charge was to cut down the scope of SAGE by examining all aspects of the system.

Shortly after the first SAGE sector became operational in 1958 at McGuire, SAGE began to lose much of its support. As a consequence, the overall distribution of SAGE sectors was continually being decreased, and the plans continued to include manual sites as part of the U.S. air defense program. The SAGE plan of an attrition system of defense, which would provide uniform coverage over the continental U.S., began to slip back into the perimeter system of defense, which relied on radars primarily along the northern border and the East and West Coasts.

Another criticism of SAGE was that the direction centers, located on SAC bases, would be bonus targets in the event of an attack on SAC. Around 1957, Lincoln had been developing the concept of a system known as the Super Combat Center (SCC). This system concept included placing the direction centers in deep, underground facilities which would be even more centralized and have a broader area of coverage than the area direction centers. To make this system more palatable and more cost-effective, the plan included the idea that air traffic control in the United States would be merged with the air defense and share the radars of both systems. At least one facility was built, in Montana, where the en-route traffic control center was integrated in the same building as the air defense direction center. The SCC concept was also broadened to include some of the functions that had previously been thought to belong to the National Military Command System, such as strategic warning. Also included in the SCC concept was a plan to upgrade the computers; the FSQ-7 was expected to be replaced with the FSQ-32 computer, a transistorized machine.

In 1959, the SCC plan was turned down. Critics of the system had pointed to its vulnerability to nuclear attack, as well as to the overly conservative design of the system, which required back-up on all critical communications and processing, the tremendous air conditioning load, and the special, expensive lighting arrangements required for the cathode ray tube displays. When the SCC program was cancelled, the FSQ-32 was modified for SAC use in the SAC control system.

The first connection between air traffic control (ATC) and SAGE was established before SAGE was born. In 1949, the Air Force's Watson Laboratory signed a contract with the Digital Computer Laboratory to investigate the feasibility of using a digital computer as the control element in an air traffic control system. The work under this contract led to the connection between AFCRC's MEW Digital Radar Relay and Whirlwind,

and to the first track-while-scan processing of digitized radar data by a digital computer. These techniques and experiments were the early foundation of what later became SAGE's radar processors and trackers. In 1957, the government requested a proposal from Lincoln for an air traffic control system that would use many of the same kinds of equipment and techniques as SAGE. Elwood P. Quesada, chairman of the Airways Modernization Board and later to be the first administrator of the Federal Aviation Administration, visited Lincoln to follow up on the proposal. Shortly after his visit, James T. Pyle, administrator of the Civil Aeronautics Agency (CAA) visited to express his interest in integrating military and commercial air defense and air traffic facilities.

We relied on Dave Israel in this area, as Dave was our technical person on air traffic control. He had continued his interest in air traffic control since his thesis and 1949 Air Force study days. Since air traffic control had much in common with air defense, we persuaded the Air Force, and later the Air Force in cooperation with the FAA, to carry out research and planning in this integration. Dave was assigned to the task, and he in turn set up a three-man team, including Howard Kirshner, Paul Stylos and David Bailey, to pursue the problems. They began to plan a series of experiments that became the CHARM (CAA High-Altitude Remote Monitor) project after MITRE was formed and took over the work. CHARM aimed at demonstrating the possibility of SAGE-ATC integration, using the Boston Air Route Traffic Control Center at Logan Airport, the ESS radars at South Truro and Bath, and the Whirlwind computer in Cambridge (in order not to interfere with SAGE tests involving the XD-1). This was a diversification into non-military systems work that would generate MITRE's single largest steady source of non-defense related funding. CHARM demonstrated that such integration was feasible, but, for many reasons — financial, doctrinal, etc. — it would be difficult to make it practicable, especially the use of joint command and control centers.

Eventually, the FAA recognized MITRE as its systems engineer, and the three-man team was built up in number and transferred to Washington. Israel stayed at Bedford, while Kirshner, Bailey, and Paul Locher moved to Washington to continue MITRE's ATC work for the FAA. Kirshner was effectively the project leader, although he reported to Israel in Bedford who was the nominal project leader. Howard had been a part of the Cape Cod System design group. He was tall, pleasant and articulate,

and he had a lively sense of humor that drew people to him. One could always count on him to lighten up meetings.

Although Israel was the most knowledgeable ATC person we had, he tended to run roughshod over the FAA staff who had responsibility for air traffic control. I remember being called into the office of the senior FAA man in Bedford, who insisted that Israel be fired, or at least be taken off the project. I explained that we had every confidence in Israel, and that the FAA was free to find another company to support it if they chose to. Fortunately, he changed his mind, and our ATC group grew and matured until it became a significant part of the support for MITRE's future Washington Operations.

Even before the idea of the SCC was proposed, alternative ways to decrease the vulnerability of the system were being considered. Shortly before the formation of MITRE, the Air Defense Command and NORAD were beginning to function as a full-fledged command that included a strong Deputy for Planning. Under their charter, they had the responsibility for generating an operational plan for the defense of North America. This plan was called the North American Air Defense Operational Plan (NADOP). These plans tended to be overly ambitious — the commands were charged with the defense role, but were unrealistic in that they did not match the budgets granted by Congress. When faced with this situation, the Department of Defense had developed an automatic response: set up a study to advise the Secretary of Defense on what he should do about these plans. These studies were performed by senior volunteers from industry and academia.

One particular such study was assigned to the Weapon Systems Evaluation Group (WSEG). WSEG was supported by the Institute for Defense Analyses (IDA). The director of research of WSEG was Al Hill, who had left the directorship of Lincoln Laboratory to become vice president and director of research for IDA. I had been assigned as a Lincoln member of this study. I reported to Hill and found that he was pleased that I had been assigned the task. Colonel Lee, however, was upset that I had been assigned this task because he felt it was detracting from the SAGE software design for which I held responsibility.

I remember sitting in Hill's office while a steady stream of other industry and university participants dropped in to pay their respects. I found that many participants were from the special interest groups protecting their weapon or computer or whatever it was that their companies

were producing. I saw this as the main stumbling block to trimming the air defense plans to fit the available monies. I hadn't much experience with studies of this sort, so I spent most of my time trying to understand what people were doing and believed that I was a disappointment to Hill, even though I contributed considerably to the design of the second generation of BOMARC. The upshot of the WSEG study was a pared-down version of the ADC's original NADOP plan. When I left the study, it was nearly a repeat of the time when I had left RLE — i.e., Hill didn't care.

Lincoln had encouraged and participated in an ADC planning study of decentralization as a means of decreasing vulnerability. The system that evolved from this study became known as BUIC, for Back-Up Intercept Control. The BUIC system was located at radar sites, with connections to other radars in the sector. It would be used on a standby basis, in the event of an attack on a direction center. BUIC could perform SAGE functions, but with limited capacity. Nevertheless, BUIC ensured that air defense functions could be carried on if the SAC bases were hit. The BUIC system was approved in 1961. In the late sixties and early seventies, air defense continued to be important enough to keep some of the SAGE divisions operating, but not important enough to make it effective over a broad range of likely scenarios. The low-altitude problem, for example, was left unsolved.

From 1949 to 1963, the conception of air defense of the continental United States had changed dramatically. The initial idea was of an area defense system able to cause significant attrition on any bomber raid up to 1000 raiding aircraft. As ballistic missiles gained emphasis and demanded a greater share of the defense budget, the scope of the air defense system decreased, and the conception of the role changed from area defense to air space policing, warning, and surveillance. This evolution was to continue at a limited rate, so that all of the SAGE direction centers had to readjust their areas of responsibility, and the arguments for defense systems conceived by Lincoln were driven from area defense toward perimeter defense.

C H A P T E R 3 6

THE WINTER STUDY

The ADES Project Office in New York was the first system program office to be organized around electronics systems as a separate entity, rather than as support subsystems to the individual weapons they controlled. These electronic systems were centralized and served a variety of weapons and sensors and supported many functions previously carried on by human operators. These systems, later to become known as command and control systems, tended to centralize the control and provide management support to upper-echelon command and control functions. They were evolutionary, constantly in the process of growing and adapting to the changing force structure. They also accommodated new and old subsystems and thus their management required contact with the upper echelons of the various operating commands and had to adapt to changing command personnel. There was always a struggle between those people who thought of them as serving the sensor-effector terminals and those who believed they should substitute for or enhance the management functions of the command. This difference was recognized for the first time in the New York Project Office (ADES) and was sharpened in definition by the needs for integration first between SAGE and its weapons (and radars) and then by the need for automation at command headquarters such as North American Air Defense Command (NORAD) and National Military Command System (NMCS).

The concept for the National Military Command System, which grew out of the discussions of these matters, led also to the idea that these systems ought to be treated differently during the R&D phases. It was about the time of this recognition that General Schriever had been named head of Air Research and Development Command. He had taken command about the time that these things were being reviewed at Air Force Headquarters in a study called the Becker Study (after the man who ran it), and had insisted upon being given a chance to carry on the work within his new ARDC structure which included the formation of the Command and Control Development Division (C²D²). (In 1961, ARDC and portions of AMC were consolidated into a new command, Air Force Systems

Command. Schriever was put in charge and was given his fourth star. The Electronic Systems Division of AFSC was activated at the same time.)

A study called the Winter Study had been proposed by ARDC in late 1959 to look at all systems that could be called command and control systems, identify their weaknesses and propose solutions. It was a given that these systems would come under the control of ARDC. I was designated executive director of this study and was teamed with Lieutenant Colonel John L. Lombardo, who was then deputy for planning at C²D². I spent a lot of time on defining the organization and on operation of the study, and was assisted by people drawn from MITRE, SDC, and Bell Laboratories, as well as from Air Force commands and laboratories. Hundreds of people were involved. We started the study in January 1960. About a month after we began, Lombardo and I were summoned to Boston's Logan Airport to meet with Jack Ruina, an influential official in the Department of Defense. He informed us that the study required the guidance and approval of a high-level steering committee and that a prestigious study director was to be named. I was annoyed by this turn of events because it tied my hands waiting for these entities to materialize. The steering committee was formed under the chairmanship of Al Hill, who had been creeping into and out of my life since I joined MIT. Gordon N. Thayer, a vice president of AT&T, was named study director and I worked with him.

Prior to Thayer's arrival on the scene, however, I had to work with Al Hill who was then a patient at a local hospital. Needless to say, this was not a satisfactory situation for either one of us. In addition, I was substituting for Thayer, whom I didn't know, and I didn't feel free to negotiate for him. In spite of the awkwardness of the situation, the Winter Study did establish a strong connection between the various command and control systems, criticizing some and recommending others, and recommended that all should be treated as a class rather than as separate entities. It also demonstrated the value of a central system engineering and laboratory support facility collocated with the system program offices assigned by the Systems Command. This need for laboratory and technical support endorsed the concept on which The MITRE Corporation was based. It defined in general terms the structure and content of that technical support.

At the conclusion of the study it was recommended that MITRE be changed from an organization that provided support to one system (SAGE) to an organization that supported most of the electronic systems

being developed at that time by the Air Force. Thus, as the SAGE system was coming to the end of its development cycle, it was augmented by the North American Air Defense System (NORAD), the Air Force Command and Control systems, the Tactical Air Control System, the Strategic Command and Control System, all topped off by the National Military Command and Control System (NMCCS). In addition, those systems supporting intelligence operations at the commands were added to the class, as were weather and air traffic control systems.

Not everyone was happy with the study or the fact that it was going on. Any criticism of what was being done, especially on the command systems, was bitterly resented by the command in question. I remember visiting General Thomas Power, now a four-star general and commander of SAC, with Gordon Thayer. Power was cut from the same bolt of fabric as was General LeMay, the dynamic, disciplined leader of the most powerful strategic force in the world. Our visit was in Power's office. The chairs had been set up in a horseshoe arrangement in front of Power's desk. Thayer was prepared to report on the status of the Winter Study. Power didn't allow him to speak, but insisted that one of his deputies, a colonel, do the speaking. This colonel had the most nerve-racking job in SAC and showed signs of stress and fatigue. He used part of the time we had to criticize the Winter Study for its evaluation of the developing SAC system. The rest of the time was spent in a monologue by General Power stressing that SAC Headquarters could design the system without help from a committee. Thayer, who had had great experience, chose to shut up and listen. In spite of my natural aversion to confrontation, I was wishing that he would more aggressively promote the Winter Study findings, but I think in retrospect he made the right decision.

During the course of the study, we met with the Steering Committee several times. Some of us felt that Hill had not been able to serve as effectively as we had hoped he would in his role as chairman of the Committee. However, considering the complexity of the problem and the lack of pertinent experience of the Steering Committee members, it should not have been surprising that this was so. The last episode in the Winter Study story was the report by Hill to the U.S. Air Force Scientific Advisory Board, a group of scientists from industry, academia, and the military. Hill delivered a report that did not, in my opinion, reflect fully the findings of the study, and which seemed, rather, to be drawn largely from his own biases. I also remember this SAB meeting as my introduction to

Eugene G. Fubini. Fubini was a bantam rooster of a man, who seemed to view the Pentagon as a cockpit where it was necessary to meet every challenge with a combination of harassment, upstaging, undercutting, and finally, outclassing the rest of the roosters. The remainder of the meeting was largely spent placating Fubini. The findings of the Winter Study were lost in the shuffle. In spite of the first impression I got of Fubini from this meeting, he went on to become the most effective Deputy Director of Defense Research and Engineering, and his antics were part of the process he used to gain the attention of those in charge of electronics-based developments. The report of the Winter Study was never officially accepted by the Air Force. Nonetheless, it served Schriever's purposes and prevented further attempts to attach ESD to Air Force Headquarters.

Although it was frustrating, many good ideas were put forth, and the study forced the Air Force and its support people to look at the bigger picture. The Winter Study identified many weaknesses in ongoing programs that were subsequently corrected, using subcommittee recommendations. It set up a management scheme that included participation of the operating command headquarters in the design process. It also set a pattern for the use of a system engineer, backed up by a laboratory structure. Much of the post-SAGE structure of MITRE was influenced by the findings of the Winter Study.

CHAPTER 37

THE MYSTIQUE OF SYSTEM ENGINEERING

An Allegory

Once upon a time, there was a prince who ruled over a small principality beset by dragons. He had a small band of knights who fought bravely against the dragons — frequently at enormous odds. Because they were valiant knights they frequently won, but because the dragons were very fierce, sometimes the knights would lose.

Often the prince thought about the great King Arthur and his famous Round Table of invincible knights. If only he were King Arthur, he wouldn't have any trouble with dragons! Then, one day, the prince heard that a knight of the Round Table had killed a dragon with his bare hands. The prince, excited, determined to travel to Camelot to hear of this wonder first hand.

When the prince arrived at Camelot and had exchanged greetings with King Arthur, the conversation turned to a discussion of dragons.

"Is it true that a knight of yours killed a dragon with his bare hands?" the prince asked.

"Quite true," replied Arthur.

"Surely, that knight must rank first among all the knights of the Round Table," the prince opined.

"Not so," said the King. "In fact, that knight ranks 173rd. The knights who rank foremost among the knights of the Round Table are those who write the guide books for dragon slaying. As a matter of fact, the guide books say that you are supposed to slay a dragon with a sword, so killing one with bare hands shows an improper understanding of the correct procedure and doesn't get very much credit."

"Perhaps these guide books are what my knights need," mused the prince. "Do they help your knights kill dragons?"

"They don't really help my knights," Arthur observed, "because I can't afford to equip them with the expensive kinds

of swords that the guide books specify. As a matter of fact, my knights haven't killed many dragons lately — there was the one we were talking about that the knight killed with his bare hands, and there was another dragon who ate a guidebook by mistake and died of indigestion. Not many dragons, to be sure, but it doesn't really upset me because, after all, writing guide books is a far more noble occupation for a knight than slaying dragons."

The prince heard all of this with wonder, and soon he departed to return to his principality, no longer envious of the famous King, but beset with a new worry. If his knights were to emulate the prestigious activities of King Arthur's noblest knights to share in their fame and glory, who would there be to slay the dragons?

This allegory, written by a Division 6 engineer, Ed Bensley, shows how the attitude toward system development changes as you move from first-generation systems to subsequent generations. First-generation systems are always fraught with major deficiencies at the component level — so all efforts are taken up with making things work, at the expense of standardization and integration. Subsequent generations are faced with the integration and standardization problems left unsolved because of the first-generation approach, and there is always a trend toward trying to attack the problem as a whole. This process comes to be known as the system engineering process. The “bare hands” approach had been used on Whirlwind I and on the Digital Radar Relay; swords were defined for the later phases of SAGE.

As we progressed through the component/subsystem development in SAGE and finally the overall system development, people gradually gained an understanding of “the system.” They tended to stop thinking about vacuum tube characteristics and to start thinking about air surveillance. This happened to a majority of the Division 6 personnel. In my case, I had had early experience with radars, sonar, and other position-location devices in the Navy, but I didn't think about these as “systems”; rather, I thought of them as devices. Also, on Whirlwind II I worked on components, maximizing performance of the basic functions, such as the arithmetic element. As I moved into the Systems Office, however, I was forced to treat these devices as subsystems, and the total of these

subsystems as a single system. The need for the broader approach was driven home for me by the Systems Office experience and by the development of the master computer program, which required the integration of many functions.

In the mid-sixties, a similar second-generation effort was taking shape in the ballistic missile system area. Minuteman, a second-generation system, was alleged to be a top-down system development, and Ramo-Wooldridge was the system engineer. Sold on the success of Minuteman, the Air Force decided to write the processes into a series of regulations, known as the 375 Series. These were based on the assumption that if the Air Force were to do another missile, they would go through the same procedures and processes, and come out with as successful a product. But our experience on SAGE indicated that each system was different enough that to be successful one could not regulate the entire process. This was especially the case when one turned to command system development. In these systems, such as the NMCCS, integration was achieved through a communication process that was extremely complex and that could not easily be regulated. The command systems had to communicate with a variety of other systems, the weapons system being just one. And not only did one have to integrate with new systems, one had to accommodate old systems that were never retired. Regulating the process had the effect of destroying opportunities for innovation on the one hand, and on the other, including unnecessary steps, simply because they were steps that had been taken on the original Minuteman. This led to the frustrations aptly alluded to in the allegory. In spite of our negative feelings about the Department of Defense's regulatory approach to systems engineering, many very competent people were involved in the thinking that went into the regulation process. Those system program offices that were successful treated the system regulation for what it was, relying on the pertinent parts, but not overlooking the need for deletion and innovation in the system under development.

The concern with systems was influenced by the operational research studies carried out by organizations such as RAND and IDA and by system studies initiated by industry, academia, and government agencies. By the late 1950s, system engineering became an obsession whose manifestation took the form of defining the system engineering process. One could always expect that the first part of almost every presentation to the military by industry would be a definition of system engineering,

which, if applied, would make every prime contractor a system engineer. This, along with the application of the 375 Series regulations, made it almost impossible to separate MITRE efforts from those of the prime contractors. A number of attempts were made to structure the processes with enough detail so that one could distinguish the more abstract elements of the design from the more concrete.

A typical experience with this phenomenon was to watch the eyes of the people who were being briefed on a prime contract proposal. When the definition of system engineering came up, as it inevitably did, the eyes of the listener would glaze over and he went into a coma until that part of the presentation was finished. There were attempts to distinguish between system engineering and “general” system engineering, the latter being what was done by the government in concert with the MITRE staff assigned to the system project. In my role, I was caught up in this semantic labyrinth, and had to be as precise as one could be in order to plan and execute MITRE’s role. I remember constructing a briefing on this, for which a number of us had designed a system development flow chart. I thought it was crystal clear, and expected to receive the kudos of my colleagues. I presented it to Halligan, who rose to the occasion and said straight out that he didn’t like it.

Eventually, the SAGE project reached a point at which the aggregate of the subsystems could be integrated, and the definition of system engineering was replaced by a description of the work. In later years, enough experience had been accrued so that a general understanding about the need for treating problems of the larger system would be attacked at the same time that the problems of subsystems were undertaken. With the introduction of integrated circuits, there was enough standardization so that top-down design became the rule, rather than the exception.

C H A P T E R 3 8

SUMMING UP

In the process of solving the problems of the SAGE system, there were certain fundamental problems that were solved for the first time. These problems occurred at all levels. In the computer design area, the implementation of a working real-time control computer dedicated to a military mission was successfully demonstrated and implemented.

Several inventions were realized, among them random access core memory, which was by far the most important component to be developed. This included not only the concept but also the materials, the structure and fabrication techniques. The application of marginal checking under machine control to minimize unscheduled downtime was implemented for the first time. The practical data link using phone lines to facilitate computer-to-computer and radar-to-computer information transfer was implemented on a large scale.

Time sharing of a common data base and computation aids were both used extensively for the first time. Other innovations included the first automatic initiation and tracking of aircraft using radar data; the first interactive system employing CRT displays with alphanumeric symbols; the first extensive use of computer graphics; the first interceptor direction program employing a choice of final phase tactics; the first use of overlap coverage to raise the data rate and simplify the handover problem; the first dual arithmetic element; the first duplex computer system; one of the first radar beam splitters; the first use of the light gun or pencil, and the first extensive use of index registers.

There is also a list of management innovations that are attributable to SAGE. The ADES program office was the first Air Force office set up to manage electronic systems as distinct and separate from weapons systems. Prior to SAGE, these systems were subordinate to the weapons they controlled; SAGE grouped together all of the electronics elements under ADES. This would lead to a broader concept, the Worldwide Military Command and Control System (WWMCCS), which grew out of the SAGE super combat center idea. SAGE represented the first time that the Air Force set aside a special team, in this case the 4620th Wing, to

define, structure and integrate the higher-level command and control functions for the Air Defense Command. This approach would become the rule rather than the exception. Finally, the Air Force established the Electronic Systems Division to handle the ground-based information systems of all Air Force weapons projects.

SAGE spawned a number of other management innovations. The Systems Office, which I established, led to what is now called “configuration management,” although at the time the concept of configuration management of command and control systems did not exist. The idea of having a system engineer in an associate contractor role was also new. SAGE was the largest scale system engineering job of its time. The system engineer — first Lincoln, then MITRE — was in a prime contractor role, tasked to pull together all the elements of SAGE.

The SAGE experience was in its way unique. There were few systems of such complexity and scope that were challenging the engineering community in the U.S. during the 1950s. The missile programs and the space programs would come as close as anything to this complexity and scope. The ingredients that came together and made the SAGE experience what it was seemed to include the following: first, there was an agreed upon, generally perceived threat. The Russian bombers were real and the atomic bombs were real. The hostile intent of the Soviet Union was also clearly demonstrated by its action. In the Lincoln organization, there was a second factor. Lincoln was part of MIT and MIT’s reputation and policies isolated those who worked on the SAGE design from the interference of the government bureaucracy. It made it possible for those to whom the problems were presented to efficiently initiate action to solve these problems. Another factor was the youth of the organization — our ages averaged in the late twenties or early thirties. It was said we were too young to know that we couldn’t do what we did. Ken Olsen claimed that naivete is a virtue in a revolutionary venture, or in any creative endeavor.

We found that people made a difference. Engineers are equipped by training to make compromises, to make things work, and to improve on design. We found that the people who had been responsible for making things work, such as the military-trained technician, contributed substantially to the progress. We found that software was as large and complex a problem as the hardware. We found that there was a payoff in having computer aids to programming, and that SAGE contributed a lot to the development of these aids.

The Lincoln staff were mostly engineers, and engineers solve engineering problems. The criterion for success is whether or not the system works, and is only loosely connected to the elegance of the solution. Thus, there can be a high payoff to conservative design and even to brute force design. We often said that we were like road builders. We didn't care where the road took us as long as it supported the traffic. This conservative design philosophy contributed to the opinion held by some members of the government establishment that we were gold-plating the system and not taking into account, sufficiently, the cost.

In addition to being engineers, the Lincoln staff became systems engineers. They started to work on components and subsystems, switching their focus as one level (such as basic circuits) was completed and understood, to larger and larger aggregates of subsystems, until they were focused on the U.S. air defense system in all its breadth and complexity.

During the late fifties, one of George Valley's seminars featured Sy Ramo, who had played a very important role in the development of the Atlas and Minuteman missile systems. His talk was on systems engineering and it struck a chord with me. He talked about two kinds of systems engineering — systems engineering in the small and systems engineering in the large. In the small, he included all of the engineering solutions required to connect together and hook up a system (make the electrons flow, make the data streams coincide with each other, proper sync pulses, etc.). By systems engineering in the large, he referred to not only the technical and physical network design, but to all of the sociological, financial, budgetary, and other such considerations that go into a system that consumes as much of the national treasure as did the SAGE job. I think that the Lincoln staff who participated in the SAGE design were provided a unique opportunity in that they were able to work in both of these areas.

My own experience tended toward systems engineering in the large, where the objective of the things I did was to bring the various constituencies together to rationalize and agree upon the best design, taking into account all of these factors. SAGE allowed each of us to seek our own functional level and appropriate area. The unity of the program depended heavily on each person's ability to integrate his pieces of the puzzle. This puzzle, which I had come across by chance, dominated my professional life for more than a decade, and was the foundation of the rest of my career.

EPILOGUE

Where is it written that the computer field is evolving so rapidly that each new innovation leads, within a short time, to the obsolescence of its predecessor? In 1982, a group of old SAGE hands visited the air defense direction center in Fort Lee, Virginia. We were surprised to find that the FSQ-7 computer was still operating as reliably as it did during its first years of operation. The commander at the base was pleased to show us around the installation. Although it had been operating for twenty years, the computer showed no signs of neglect, and its outstanding reliability was still its major feature. The master program, however, had evolved considerably from the initial version first prepared by Lincoln. SDC had continued to provide computer program maintenance.

Those of us who participated in the initial design of the system were moved by the enthusiastic way the sector personnel spoke about the machine, even though they were well aware that the full installation required as much as three megawatts of power, and that a new machine would require only a minute fraction of the power and space consumed by the rack upon rack of vacuum tube circuits. I believe that this longevity is a record for large-scale digital control computers.

During the course of the development of SAGE, each of 24 such direction centers were equipped with two FSQ-7s as well, most of them along the Canadian border and the East and West Coasts. In January, 1983, six were still running, and in January, 1984, all the SAGE computers were shut down, to be replaced with more modern machines. Portions of the FSQ-7, as well as of Whirlwind, are on display at the Computer Museum in Boston, and at the Smithsonian in Washington, D.C.

The companies and laboratories that were founded initially to play a part in the SAGE program have since adapted to live in the computer-dominated world. SDC, which was founded to provide the computer program for SAGE, was converted to a profit-making company, with strong ties still to the software business which they dominated in the 1960s. It has been acquired by Burroughs and kept as a separate division. Lincoln Laboratory, which was set up to solve the air defense problem, has switched its focus to satellite communications and anti-intercontinental ballistic missile work, and other research problems. The MITRE

Corporation shifted its focus from SAGE to other information system-based programs at all levels of the military establishment. It has grown from the 270 employees who transferred from Lincoln in 1958 to around 5,000 employees today, and from a single-project company to an organization administering some 400 projects.

IBM utilized the SAGE experience by adapting components and subsystems developed for SAGE to their product line. They adopted the core memory, for which they endured and lost a lengthy court battle with Jay Forrester and MIT over the patent. AT&T profited tremendously from the SAGE data link, which evolved into their A-1 data service and which opened for them a new market in data transmission. The Digital Equipment Corporation is the most famous case of a group of people from the staff setting up a new venture; there are many cases where individuals and groups of Lincoln and MITRE personnel who participated in SAGE formed profit-making companies.

The SAGE experience affected many people, and in many cases, including mine, was the foundation upon which careers were built. Most of the people mentioned in these memoirs have had successful careers, partly attributable to the SAGE and Whirlwind experience. Jay W. Forrester participated in the final settlement of the IBM suit for the core memory patent, and then, as a professor at MIT's Sloan School of Management, developed a new field, industrial dynamics, which has attracted international interest. Bob Everett, one of the founders of MITRE, became its third president in 1969, and held that title until his retirement in 1986. George Valley spent a term as chief scientist for the Air Force, and returned to MIT as a professor in the Physics Department, where he concentrated on evaluating the undergraduate curriculum by taking all the undergraduate courses. He is currently writing his own memoirs of SAGE. Al Hill became vice president of IDA and later returned to MIT to head its Center for Naval Studies, and most recently, served as chairman of the board of the Charles Stark Draper Laboratory. Charlie Zraket stayed with MITRE and succeeded Bob Everett as president. Dave Israel left MITRE in the late 1960s and is chief engineer of the Defense Communications Agency. Halligan retired at 66, after a short term as chairman of MITRE's executive committee. He died in 1975 of emphysema.

Moose Walquist left Lincoln and became vice president in charge of TRW's satellite communications efforts. Jack Harrington also left Lincoln, to become head of MIT's Center for Space Research, and

most recently, senior vice president of research at COMSAT Corporation. Bob Wieser became director of Advanced Weapons Programs at McDonnell Astronautics Company, and is now a private consultant. Arnov left Lincoln to found Interactive Data, and later Advantage Systems, Inc. Steve Dodd was one of the few who remained at Lincoln until his retirement. Norm Taylor left Lincoln to become president of Scientific Engineering Inc., and later vice president of ITEK. Most recently he headed his own consulting firm.

I took a disability retirement after having served as senior vice president of MITRE. Pretty good for a country boy from Yellow Jump, North Dakota!

THE ROMANCE OF PROGRAMMING

A Speech for
the SAGE Symposium
8 November 1956

Illustrations by the author

INTRODUCTION

To begin, I would like to say a few words about the title of this talk, “The Romance of Programming.” Isn’t that a good title? When you read it on the announcement, didn’t it make you want to come? Actually, this is going to be a technical talk. The first title we chose for it was, “The Application of Heisenberg’s Uncertainty Principle to the Design of Computer Programs for the SAGE Direction Center.” Now you *know* that nobody would come to a talk with a title like that, even if it were compulsory. So we tricked you — and here you are, waiting to see movies of guided missiles, and we’re going to talk about paper shuffling. Later on, we’ll be flashing slides showing partial differential equations and all that rigamarole that is the substance of a technical talk. But before we do, there’s a rumor that RAND and Lincoln are producing programs for SAGE ... that the RAND organization and the Lincoln organization are all jumbled together in one homogeneous mess — er, mass ... that you can’t tell a Lincoln programmer from a RAND programmer except that the RAND programmer has a Santa Monica suntan.

Let me tell you right now that these stories are true, and furthermore, that every one of us should get a real understanding of what is going on. I have added this precautionary note because of other rumors which have been circulated. Like this: “We hear that you have cut out monitoring in the initial master program. We hear the program won’t control weapons. We hear that crosstelling has been eliminated.” These rumors are not completely untrue. It is a known fact that there are about three women to every man in the United States, but I advise extreme care in interpreting this data. Of course, we have modified the concept of the program as we understood it two years ago; isn’t this the normal procedure for a healthy development program? The steps in any development between the conception of the idea and the completion of the system are like learning to play the piano.

Melodie in F

The first system of musical notation consists of a grand staff with a treble clef on the upper staff and a bass clef on the lower staff. The key signature has one flat (F major). The melody in the treble clef is marked with fingerings: 2, 1, 2, 2, 1, 2, 5, 1, 2, 5, 4, 1, 2. The bass clef accompaniment includes fingerings: 1, 5, 1, 5, 1, 3, 1, 4, 1.

Moderato

Piano

The second system of musical notation is marked "Moderato" and "Piano". It features a grand staff with a treble clef and a bass clef. The melody in the treble clef is marked with fingerings: 3, 2, 1, 3, 2, 1, 5. The bass clef accompaniment includes a flat sign (b) and fingerings: 5, 1, 3, 5. The instruction "col pedale" is written below the bass staff.

Moderato

Piano

The third system of musical notation is marked "Moderato" and "Piano". It features a grand staff with a treble clef and a bass clef. The melody in the treble clef is marked with fingerings: 3, 2, 1, 3, 2, 1, 5. The bass clef accompaniment includes a flat sign (b) and fingerings: 5, 1, 3, 5. The instruction "con pedale" is written below the bass staff.

033-030

Slide 1

(SLIDE #1) You can see that the melody is the same, but it gets damned complicated, even if it does use the same 10 fingers and keeps the same time as it goes along. Early in the development we had to come up with a concept of what we were going to produce. We recognized, of course, that much of our data was incomplete. Some was not even available, but we had to get started on a system in order to have something to modify and improve. As time went on and as our system began to take shape, we found things we had overlooked, other factors we didn't understand, and still others whose magnitude we couldn't predict. For example, we couldn't predict the number of instructions required in the final program from the two or three pages of description of it contained in the Operational Plan. It would be like predicting from his sketches the number of rivets required for Frank Lloyd Wright's "Mile-High" building, although there is a feeling among non-building types that builders can do this. As each new hurdle was taken, our original system concept changed; sometimes slightly, sometimes in major ways.

In the early days, we had a sort of group delusion. We thought our computer was a giant brain. Some people told us it was a computer, but we didn't believe that. It turned out that it was a computer, and what's more, it only had 96,000 registers of drum storage, and 8,000 registers of core storage. We learned as we went along. There are things which two or

three years ago were matters of opinion, which are now matters of fact. There was an opinion that our Operational Specs were too complicated to program within the time schedule — this is now a matter of fact. Some of our opinions were wrong. We thought that we could produce the program with 10 or 15 programmers, so we continued modifying as we learned, as in the case of the monitoring function, but we are not eliminating it. We are changing our ideas on crosstesting, but we are not eliminating that either.

Sometimes we are brought up short when we try to answer the question of how much can we pay in terms of computer instructions and program time for, let us say, one percent additional refinement in systems performance. It is very much like the budget — many more items are desirable, but you have a fixed amount of dollars to work with, so you ask yourself how necessary these items are, desirable though they may be. If I'm looking for transportation, should I spend \$10,000 for a Lincoln

Continental with air-conditioning, or should I buy a Ford? The costs are different, but the utility is about the same. What we have been doing is going over our program design and substituting Fords for Continentals, 17-inch screens for 24-inch screens, mouton for mink, and computers for “giant brains.” To make this point clear, I will trace through the history of the program production and introduce you to some of the people who contributed to it, and tell you what kinds of jobs they do, and show you what we learned as we went along.



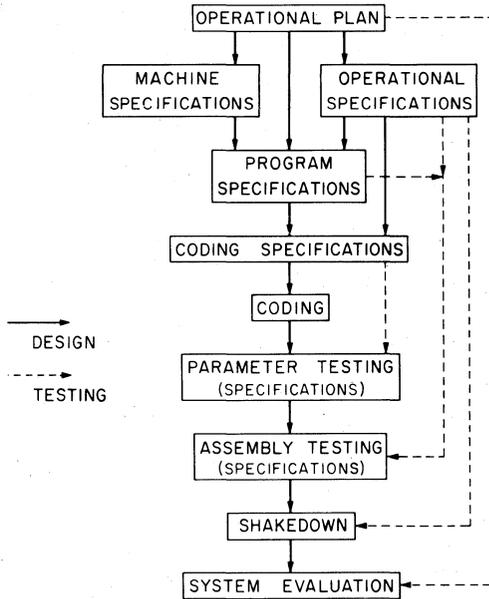
060-263

Slide 2

One of the difficulties in preparing this presentation has been finding interesting ways of showing programmers at their work. We thought we would like to follow the excellent example set by Dr. Overhage in an earlier symposium. In his presentation, as you may remember, the people in his division were shown at their work in interesting settings. As I remember it, one of the fellows working on the Bath radar was shown perched precariously 100 feet in the air on a 150-foot radar antenna. This was part of his job. There was another shot of a man doing his work inside the leg of a Texas Tower.

We have examined the work postures of our people (SLIDE #2), and we find that what they do is sit behind desks, hunched slightly forward, chin in hand....thinking!!!!

PROGRAM PRODUCTION

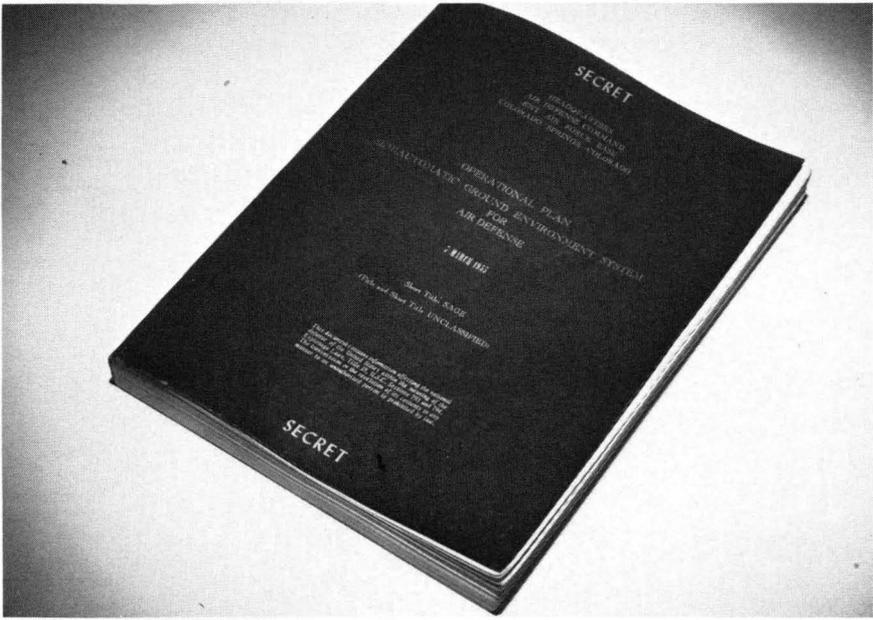


Slide 3

THE PHASES IN THE PROGRAMMING JOB

Now we get into the technical part of my talk.

(SLIDE #3) In this slide, you will recognize the Visual Aid for a Technical Talk. It has black boxes and it has lines of flow. We were going to use our general-purpose flow diagram instead of this slide, but someone has marked it Top Secret and I don't have the clearance to use it. I'm going to give you a few seconds to read the slide and make something out of it. It starts at the top and ends at the bottom ... finished?



Slide 4

In the summer of 1954, Colonel Oscar T. Halley and his merry men flew in from Colorado Springs, briefcases in hand, and sat down with us and wrote a book about Air Defense in the United States. They called it SAGE. The book (called the Operational Plan) said that SAGE would find airplanes in the air and shoot them down with the aid of men and computing devices. For some of the functions, the book specified how this might be done. This is a picture of this Operational Plan (SLIDE #4) — not very exciting to look at. It has the usual plot — scientist solves problem; engineer and programmer implement solution after terrific struggle. Hero lives happily ever after. We are not to that ever after part yet.

We were a confident crew fresh from finishing the Cape Cod '53 and '54 systems, who, with the help of the 4620th Air Defense Wing, wrote a set of specifications which defined the things people were shown by the computer, what they could do when they saw these things, and what would happen in the computer when they did it. We spelled out the means by which these people and the computer gathered and displayed a picture of what was going on in the air space around us, and how they calculated the direction in which the interceptor would have to go to catch the enemy.

This was our Lincoln Continental period. We added air-conditioning, jacks on all four wheels, twin exhausts, aluminum heads, push-button steering, and fur-lined seat covers. We wrote 20 operational specifications covering such matters as radar inputs, tracking, height finding, and raid forming. These were supplemented by 13 mathematical specifications for the equations and logical decisions which must be made in order for the program to do its job.



Slide 5

This slide (SLIDE #5) shows what red-blooded American boys and girls with a little vigor and imagination can produce — three volumes of ops specs and two volumes of math specs.

About this time, we moved in a crew to worry about the design of the computer program and set them down between the five-foot shelf of ops and math specs, and another five-foot shelf of machine specs, and told them first of all to get familiar with the mechanics of our giant brain, with its 100 consoles, 60,000 switches, 32 display categories, 90 display assignment bits, 64 light guns, 96,000 drum registers, 8,000 core memory registers, and while they were about it, find out what it was that we wanted the machine to do. They broke the program into pieces so that many people could work on it and it could be operated in the machine, and they made an outline of the timing and the table storage. These became the beginning of the program specifications.

One of the troubles with any technical talk is that it lapses into the jargon of a particular discipline. The words have no meaning to people outside the field. In the next section, I was going to give you a short description of the job of producing the program, coding specifications, coding, assembly test, and the rest, but it wouldn't give you the feeling for the jobs. After all, these jobs are being done by people and people have feelings; you can't get the feeling for the job of our programmers unless you yourself have read the ops specs, and studied the machine specs,

designed your program and checked it out. We have searched for a way of sharing the programmer's life with those of you who are not programmers, and since we have only a few minutes in which to do it, we have turned to the arts. When I was a boy on the North Dakota prairie, John Masfield's poem, *Sea Fever*, gave me a good feeling for the sailor's life. Remember?

*I must go down to the seas again, to the
lonely sea and the sky,
And all I ask is a tall ship and a star
to steer her by,
And the wheel's kick and the wind's song
and the white sail's shaking,
And a grey mist on the sea's face and a
grey dawn breaking.*

Can't you just see that sailor on the deck of his ship, chin high, drinking in the sea and the sky, loving the challenge of it, full of dignity, in tune with his universe? We searched among the programmers for a poet — we found bank clerks, jet pilots, school teachers, geologists, psychologists, Doctors of Philosophy, Russian language teachers, Marine Corps chaplains — but no poets.

I never saw a Coding Spec
I never hope to sight one,
But I can tell you anyhow
I'd rather sight than write one.

WAC

Slide 6

One of our people, who shall remain anonymous, wrote this (SLIDE #6). It proves that the minstrel died with the Middle Ages.



Slide 7

Finally, we found it — the thing that would communicate to you the hopes, dreams, aspirations; the sounds, sight, and feel of the programmer life — and we bring it to you now. Let me set the scene: Our programmer came to work with us nine months ago, finished the IBM programmer training course, was given a program to produce, sits by himself in his room ... thinking (SLIDE #7)...

* * *

(Background music begins.)

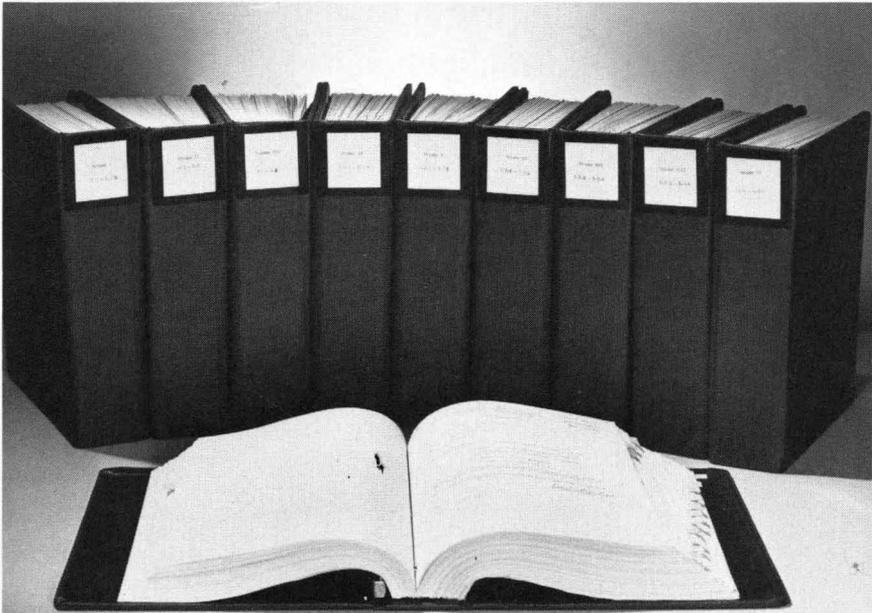
Is somethin' botherin' you, sonny? You say that you've been readin' them books for months, an' they tole ya they all been changed, an' you can't find out what this ops spec means, an' the fella who wrote it switched jobs and won't talk to ya anymore, an' somebody gave all your coding sheets to the man from the clean-up campaign an' you ran after him and pulled 'em out of his bucket, an he bit your finger? An' somebody lost your executive deck in the card room an' you went down there an' they were cutting it up with a great big scissors? An' you tole them it took you a week to get it ready an' they slammed the door in your face an' you heard them laffin' in there, an' you been waitin' for six weeks to get on the computer to test your program and when you get on, somebody spills coffee on your cards and they won't go through the card reader an' the machine hangs up on drum parities? An' you dropped a reel of tape an' it goes lickety split down the hall reelin' out the tape as it goes, an' people step on it? An' when you get back to the office your supervisor tells you they're cuttin' out your program anyway, an' you take your supervisor down and shove your test deck down his throat, an' he's at the hospital havin' his stomach pumped, an you're waitin' for the police to take you away ... is that's what's botherin' you, ole timer?

Well, keep your head up high, take a walk in the sun, remember that hard work will overcome obstacles, a penny saved is a penny earned, and life is just a bowl of cherries ... *(Martial music drowns out the last two sentences.)*

* * *

We hope that this helps you to identify yourselves with us.

The writing of the coding specifications was a time of occupational therapy for us, and marked the end of our Lincoln Continental period. At this point, we faced up to the fact that “there was a Ford in our future.”



Slide 8

In the coding specification, the programmer specified the inputs, the outputs, and the complex logical structure connecting them for a given program. There are 70 or so of these subprograms in the master program, and a coding spec was written for each. This slide (SLIDE #8) shows the coding spec files. More than 1,000 documents have passed through these coding spec files. The programmer then writes the sequence of computer instructions to make the computer do what the coding specs say it should do. This process is known as coding and ultimately results in a pile of punched cards to be fed into the computer.



Slide 9

200

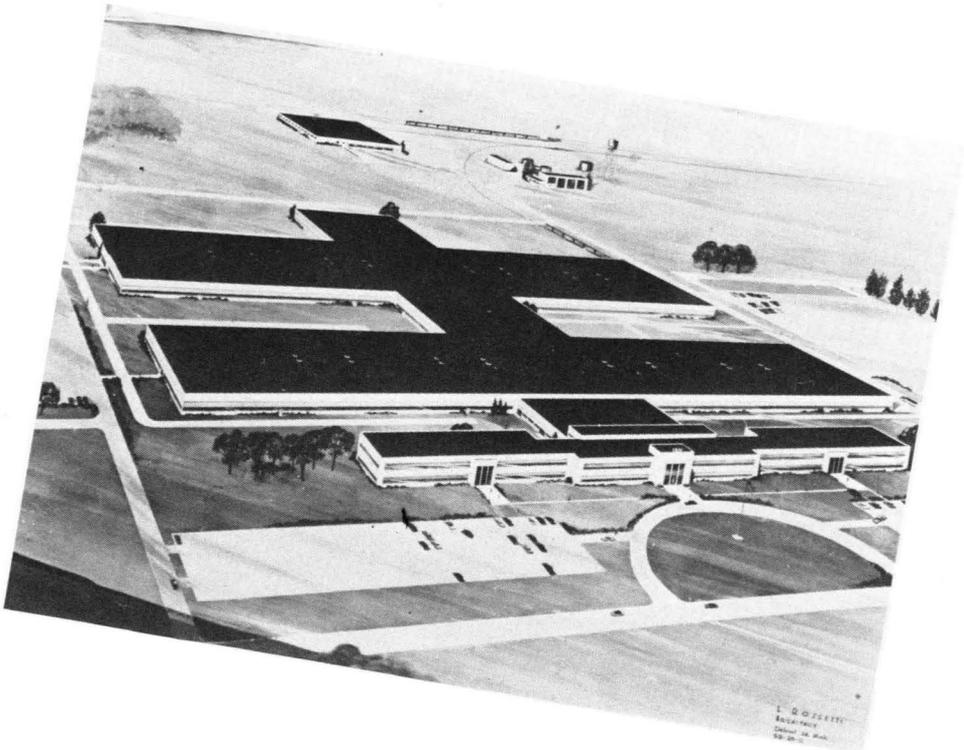
(SLIDE #9) This is a stack of 60,000 cards shown next to Madeline Carey — as many as in our master program. Placed end to end, these cards would stretch from the ground to an airplane flying at 30,000 feet. Each of the subprograms must be tested to see if it does, in fact, provide an accurate communication link between input and output. We make up a group of input tables from live and simulated data and we write out predicted output results. When the program and its input tables are fed into core memory, the computer goes to work and produces an output which is compared to the predicted output. This is “parameter testing” the program. After the program is parameter-tested, the program’s individual subprograms are assembled and tested together at various steps. This testing of the assembled set of subprograms is called “assembly testing,” and it consists of running the assembled subprograms off of prepared simulated tables — keyboard inputs — and simulated radar data. The end of assembly testing is to show that the program meets the current idea of the operational specifications.

After assembly testing, the program is integrated with the computer and operators who run the system. This is called “shakedown.” When the parts (the program, the people, and the equipment) are all working together as a system, evaluation will begin.

This has been a quick review of the job. To place a time scale on it, the operational plan was completed in the spring of 1955; the operational specifications were completed in the fall of 1955; the program and coding specifications were completed in the summer of 1956; the parameter testing and assembly testing are well along. We expect that a portion of the program called the Air Surveillance package will be delivered to shakedown in December, and the rest of the program will be delivered to shakedown and will be installed in the field in June of 1957. Evaluation should begin in the fall of 1957, but more of this later.

DESCRIPTION OF THE PROGRAMMING PROCESS

I have noticed that many of you in the audience have been straining to get the meaning of this technical presentation — many of you are asleep. To relieve the strain and build up interest, I am going to tell you about us. There are about 360 of us: 280 RAND programmers, and 80 Lincoln programmers. We occupy space — three wings in Murphy General Hospital, the bottom floor of Building C, the basement of Building F, rooms in the field station, rooms in the McGuire Direction Center, and two motels painted pink in Kingston, New York. We use almost all of the machine time in Building F, we use 14 hours per day of machine time at Kingston.



L. G. FLETT
RECAPITAL
DORSET, N. H.
19-20-21

Slide 10

204

(SLIDE #10) This is a view of the IBM Kingston plant — our motels are off to the right — we land the Wellesley Apache on the airstrip in the foreground.



Slide 11

(SLIDE #11) This is the maintenance console of one of the machines in the Kingston test cell where we work.



Slide 12

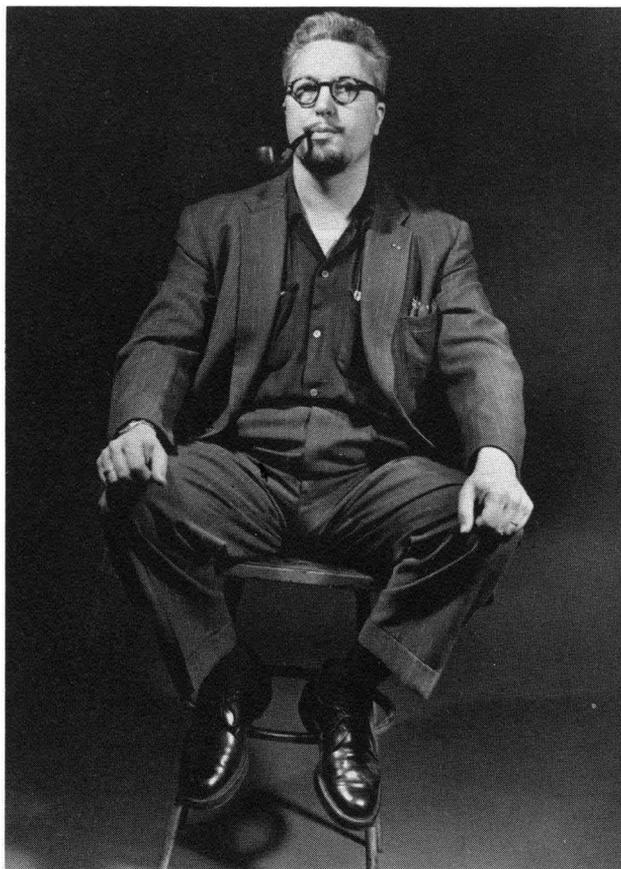
We have eight hours per day of computer time at the McGuire Direction Center. (SLIDE #12) This is a picture of McGuire from the air.

Now, to get to us. Again, we run into the problem we had before: how can we tell you about us programmers so that you get the feel of it? Let's try it this way ...



Slide 13

(SLIDE #13) This is the top view of a programmer. The function of the hairy spherical structure closest to you is to think.



Slide 14

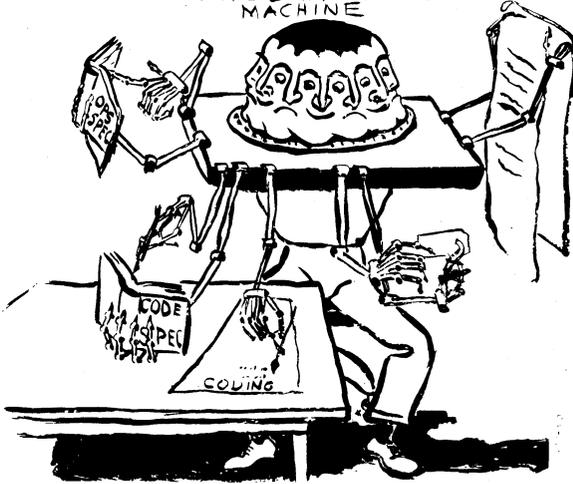
(SLIDE #14) This is the front elevation of a programmer — the hair on the bottom side of the sphere as well as that on the top side are not necessary to the function of the thinker, but resulted from the whim of his industrial designer. Some programmers come without either hair.



Slide 15

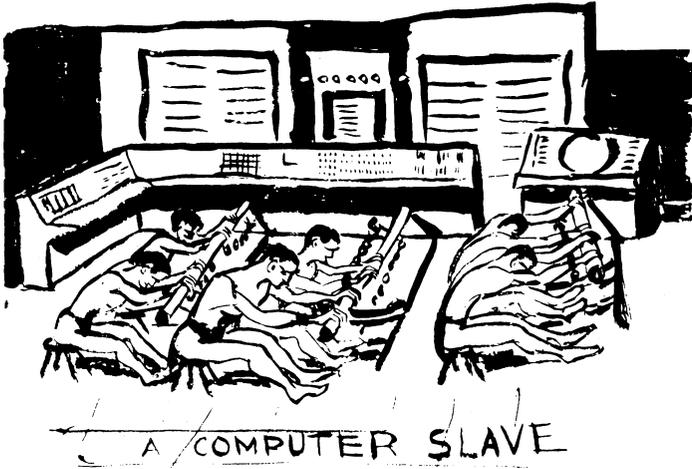
(SLIDE #15) This is the side elevation of a programmer. He is held to that chair by a force that increases with the reciprocal of the fifth power of the distance from it — this force is subject to his will — and some have been known to keep this power on for as many as 14 hours per day.

A PROGRAMMING
MACHINE



Slide 16

But of course, this is not the way to tell you about us programmers — to know us, you must see us as those who know us see us. To our supervisors, we are many-talented, tireless workers, turning ideas into reality (SLIDE #16).



Slide 17

To our fellow workers, we are part of a team — bound together by our common objectives (SLIDE #17).



Slide 18

To the computer maintenance man, we are a friendly advisor,
working with him to find machine troubles (SLIDE #18).

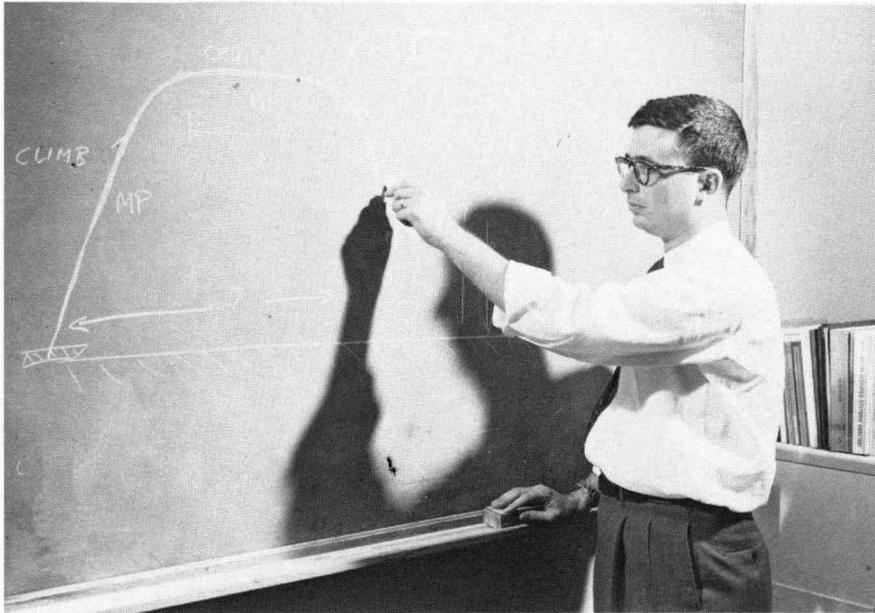


Slide 19

And to our wives, we are very much like any other guys to their wives (SLIDE #19).

Now we dive back into our technical talk. Let us pick an example of one of the functions which is performed by the program, and show how it grew through the years — how this function is an example which illustrates my opening statements that as we learn more about the problems, we must modify our initial concept of what it should be.

We will select as our example the *digital display*. The Operational Plan contains only a few references to digital display. It indicated the format that the digital display would have, and how it might be used in the performance of some of the SAGE functions. In all, there were two or three illustrations, and probably a page or two of meaningful discussions of what the digital display would be used for.



Slide 20

(SLIDE #20) This is David Robinson Israel, who wrote most of the section that pertains to the operation of the SAGE system. He did it all with his left hand.

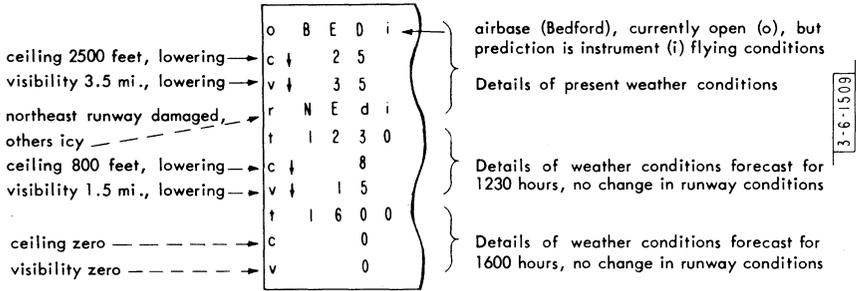
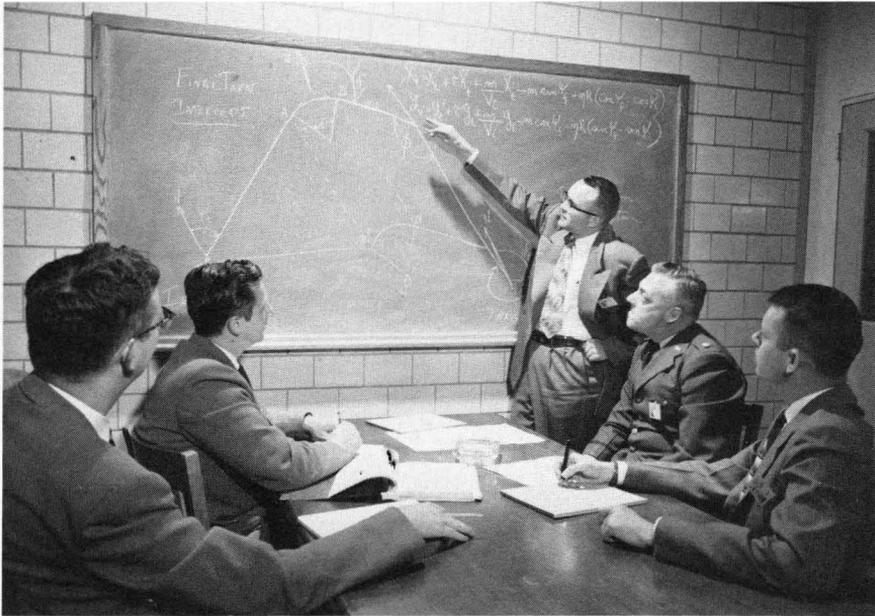


Fig. 64. Detailed Weather for Individual Bases.

(SLIDE #21) This is Figure 64 from the Operational Plan. It shows a typical kind of digital display. The Operational Plan says that detailed weather and forecasts for each base used by the subsector will be available to the senior director, senior weapons director, weapons director, intercept directors, and other personnel. Display will be selected by base. The early estimates of the cost of the digital display function were made from the Operational Plan and were based on the experience that we had obtained in Cape Cod and what we knew of the FSQ-7. It was not possible at the time to estimate with certainty the size this program would be. The reasons that we could not were: no one had programmed for the FSQ-7; the operation had not yet been specified in detail and agreed to by the Air Defense Command; we could not predict our own capability for producing these programs efficiently. These things made the estimate at that time uncertain by factors greater than two or three; however, there was no better estimate. The only way that we could improve the estimate was to actually do the work. The work of defining the operational and mathematical specs was divided among some 14 people who had previously participated in the '53 and '54 Cape Cod programming job. Decisions about what should go in each of the specs was made at meetings such as are illustrated in the next slide.



Slide 22

(SLIDE #22) Here we see Jack Cahill, Steve Hauser, Charlie Grandy, Major Chesler and Dave Bailey reviewing the first drafts of the Operational Specification. We argued, fought, screamed at each other, disagreed, and finally, agreed on what represented an acceptable specification for each of the SAGE functions. We spent eight months writing, arguing, defining what the operation should be, and when finally we were bleeding and exhausted, we froze the specification so that the next step in the production could proceed.



Slide 23

This slide (SLIDE #23) shows how concurrence was reached with the 4620th Air Defense Wing. Here we see Charlie Zraket, Major Janek, and Lieutenant Colonel Stevenson going over in detail the operational specifications and agreeing on what should be in them.



Slide 24

This slide (SLIDE #24) shows the work sheet picked up after one of those meetings. Oh! That's not the right sheet!

In addition to the above mentioned frame counts, a smoothed frame count will be computed for all OFI radars and for all LRI radars after masking, as follows:

$$\bar{D}_n = \frac{3 \bar{D}_{n-1} + D_n}{4} \quad n \neq 1$$

$$\bar{D}_1 = D_1 \quad n = 1$$

where \bar{D}_n = smoothed frame count, present frame

\bar{D}_{n-1} = smoothed frame count, previous frame

D_n = actual frame count, present frame, computed from all subframes

D_1 = actual frame count of first frame after a set returns to the ON or OVERRIDE status after having been in the OFF or UNAVAILABLE status.

When D_n is EXCESSIVE (see Section V below), it will not be used to adjust \bar{D}_n . \bar{D}_n must never be allowed to exceed the corresponding capacity limit in Section III above. D_n is the data count used for the Radar Data Input digital displays described in GM-3774-1.

V. EXCESSIVE Data

The number of returns received from a radar during any scan approximately equals the number of aircraft seen by that radar during the previous scan. The difference between these two numbers will be caused by non-unity blip-scan ratios, non-aircraft returns, multiple returns, and aircraft entering or leaving the radar's coverage.

If the number of radar data from any set, after masking, received in any subframe causes the sum of the most recent three consecutive subframe counts (frame count) to exceed the smoothed data count

how often computed?
(Cross to give capacity for the sections)

What if the smoothed count is ON?

What if ...

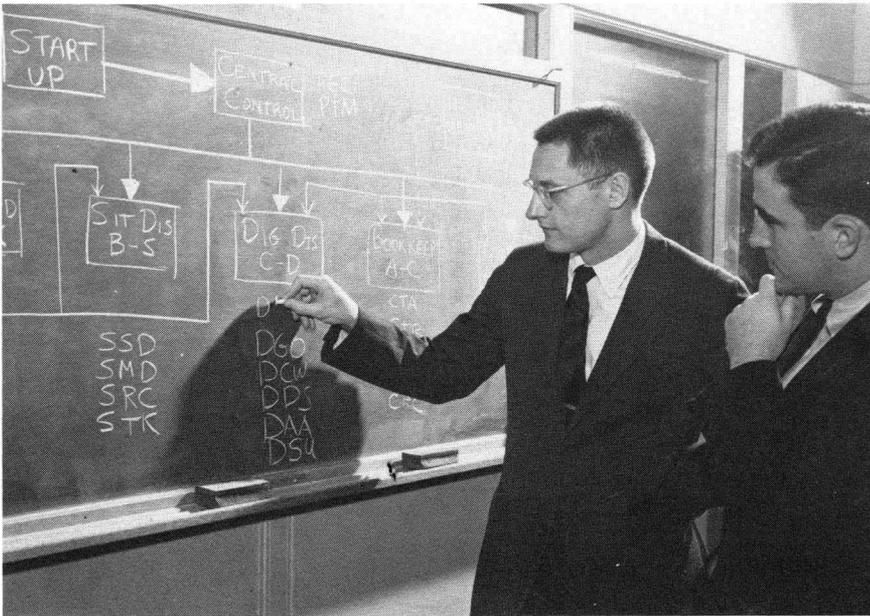
Is there a limit on the number of returns?

What if ...

17.13

(SLIDE #25) There.

Each of the people who defined one of the air defense functions, such as weapons assignment, track initiation, radar inputs, or intercept direction, drew up requirements of his need for digital displays, and these were summarized without filtering into another document which we called an Operational Specification for Digital Display.



Slide 26

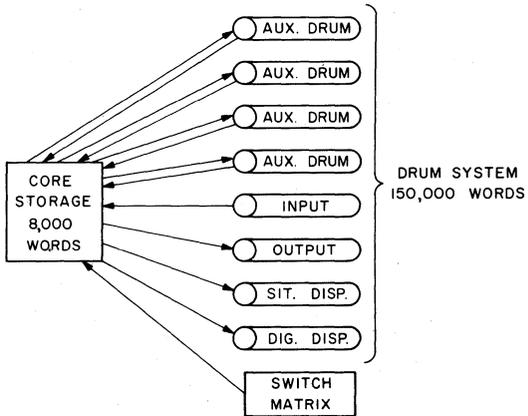
In this slide (SLIDE #26), we show two of the major contributors to the program designs, Al Shoolman and Herb Benington.

Part of their job was to divide the program into parts so that it would work with the machine and so that it could be divided among a number of programmers. I would like to spend a few minutes giving you a background for what this means.

To the programmer, the FSQ-7 consists of devices for:

1. Storing data (tapes; drums and core memory; cards).
2. Performing arithmetic and logical processes on it (arithmetic element).
3. Transferring data around among the storage and processing devices (drum reading and writing system, etc.).
4. Bringing in new data and transferring out processed data (buffer drum; card machines; display system; output system).

There are 48 storing, transferring, and processing operations built into the control mechanism of the FSQ-7, and the sequence in which they are performed is specified in a program of instructions to the machine. The job of the programmer is to specify this sequence of instructions.



DRUM SYSTEM

Slide 27

This slide (SLIDE #27) shows the storage arrangement of the FSQ-7.

Because each of the 48 operations takes about 12 microseconds to perform, the program must be held in a storage device which can supply instructions in sequence to the arithmetic element at about 12-microsecond intervals, or time will be lost while getting the instructions. Each of the instructions contains the location of the data to be processed, and specifies what operations should be performed on it. Because instructions are performed at 12-microsecond intervals, the data which is to be operated upon must also be available at 12-microsecond intervals. The memory device in the FSQ-7, which supplies instructions and data six microseconds apart, is the high-speed core memory which in the present machine contains 8000 registers. Because our 65,000-instruction program cannot fit in core memory, it must be broken up and brought in in pieces called subprograms (containing 500-3000 instructions each), along with the data on which this subprogram operates. The programs that are concerned with bringing in the subprogram, as well as the data on which it operates, into core memory must be permanently stored in the core memory at all times. Thus, three categories of programs are held in core memory:

1. The subprogram;
2. The data on which it operates;
3. A program that specifies the sequence in which subprograms and their associated data are brought in and sent out (which we will call here the "program environment control program").

Ideally, the high-speed core memory should be big enough to hold the whole program and all of the data used by the system. Thus, one would reduce the problem of breaking up the program into pieces, and of shuffling it in and out of core memory.

The whole 65,000-instruction program and the data upon which it operates is maintained on the auxiliary drums, and is called into core memory in blocks specified by the sequence control program. These are blocks of registers arranged in sequence around the periphery of the drum. When the program environment control program calls a block of these registers into core memory, some time is required for the first register in the block to come up under the drum-reading heads. This time on the average is 10 milliseconds, or time for about 1000 instructions to be performed. The FSQ-7 is designed so that the arithmetic processes can be performed during this interval; part of the designer's problem is to arrange the sequencing of the program so that this time is not lost.

The problem of designing a program, then, is the problem of (1) dividing up the whole program into parts; (2) designing tables of data that can be used by the program and its parts; (3) working out the logistics to insure that each subprogram and its associated tables are

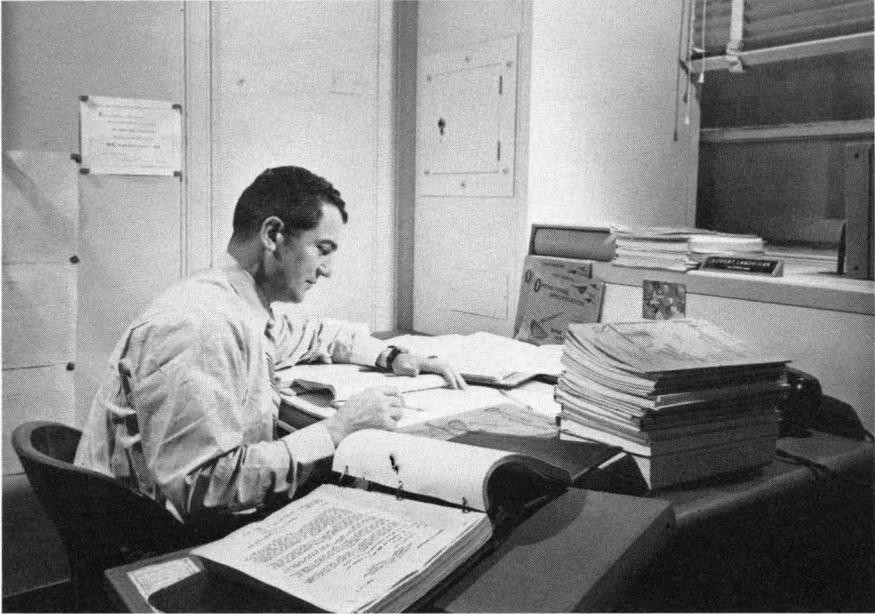
brought in and out at the proper time, and (4) making sure that it is possible for the designer who cannot get into the machine to understand “what’s going on in there.”

A broad-brush design was made by Benington and Company, and they decided that digital display would be treated as a separate function in the computer program. Up to this time, about 30 people had contributed in some way or another to the specs for the digital displays, and because of the magnitude of the problem of producing the program, it had to be divided among 10 different people. I would like to give you a feel for the kinds of things that people did while they developed the program from the ops and math specs. To do this, I will introduce you to the people and show them at the various phases of the work that they did.



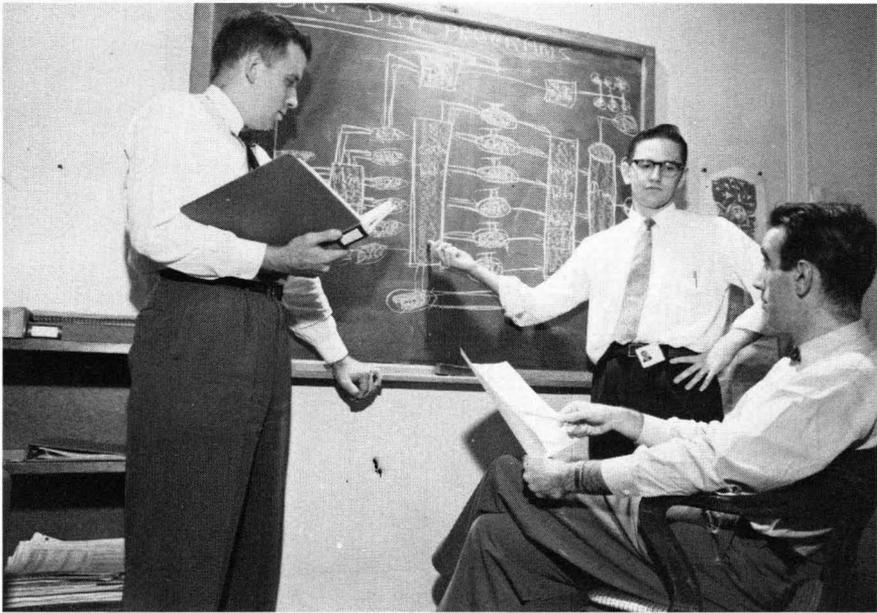
Slide 28

(SLIDE #28) This is Howard Briscoe of Lincoln, whose subsection was assigned the responsibility for producing the digital display programs.



Slide 29

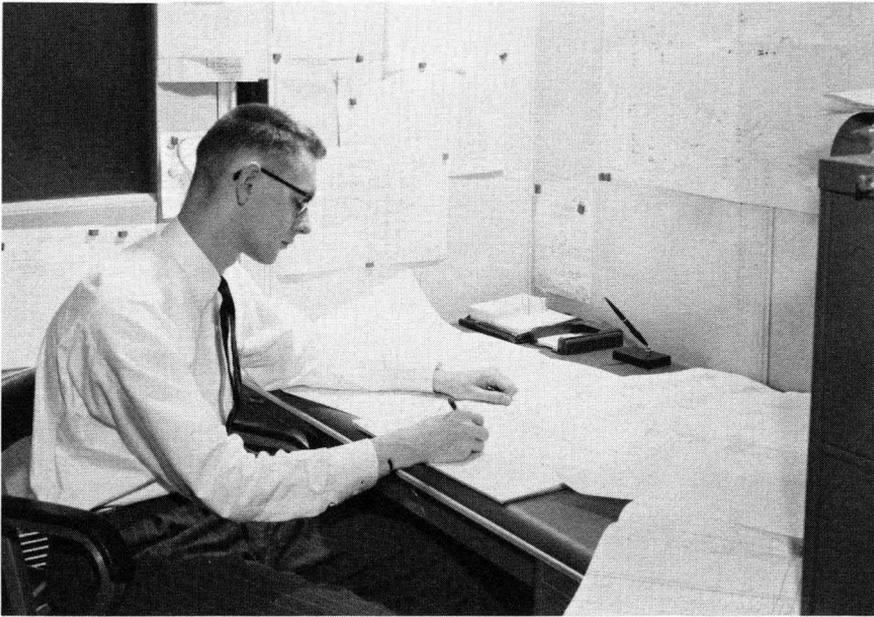
The first step in the process is illustrated by Robert Landrigan of RAND (SLIDE #29), who is digging through the operational and mathematical specifications. He is looking for things that apply to digital display. This digging gave him the knowledge in the form of personal notes, annotated operational specifications, and operational modification requests which formed the framework in which he could specify what his program should do. The operational modification request became an addition to the operational specification and corrected the inconsistencies and filled in the gaps.



Slide 30

(SLIDE #30) This slide illustrates part of the second phase in the production of the program. This is the writing and coordination of the coding specifications. In the picture we see Robert McGill, Howard Metcalfe, and George Sioras, all of RAND, coordinating. They have on the board the flow diagram for the whole digital display program.

The inputs to the digital display program are the central tables and switch inputs. The switch inputs hold the direct requests for digital displays from the consoles and the central tables hold those conditions under which a display is forced on an operator. The digital display program has subprograms which search the central tables and the switch inputs for conditions that require a display be shown to an operator. These are called "control subprograms." The output of these programs is a control table from which six digital display make-up programs are controlled. There is a make-up program for alarms, for tracks, for monitoring, for geography, for summaries and for miscellaneous digital displays. The output of the digital display make-up program is a drum image table which is periodically transferred to the digital display drum. The digital display system takes it from there. The product of this phase was in the coding specifications. For each subprogram (and there were sixteen of these), there was a separate coding specification that detailed the inputs and outputs in terms of the tables needed and affected by the subprogram, and the detailed design of all the communication and all of the sequence requirements of the program.



Slide 31

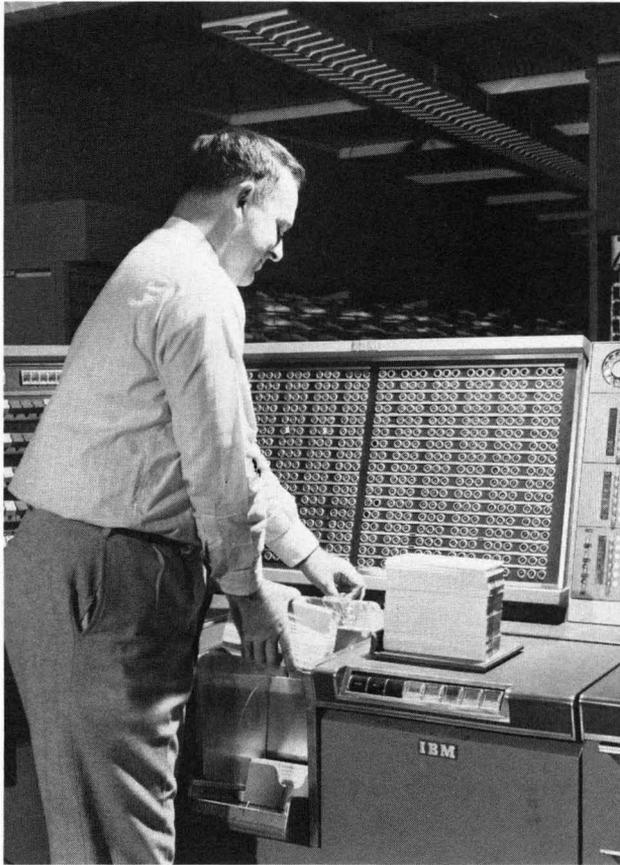
The next phase in the operation is illustrated by Robert Meyer of IBM (SLIDE #31). Here we see him writing the code that translates the coding specification into a sequence of coded instructions. He has prepared a detailed flow chart for a better understanding of the program, and he uses it to help him code. The product of this phase is the manuscript of annotated code and a completely detailed flow chart that describes what the program does.



Slide 32

(SLIDE #32) This slide shows Arthur Doll of RAND having his cards punched, in the card room. He turns in his handwritten manuscript (as illustrated in the preceding slide) along with a card request form. The card room prepares a set of cards, one for each instruction (in Hollerith code), and runs these cards through a tabulator, which prints the contents of the cards so that they can be checked for agreement with the original manuscript. The product of this phase is an accurately punched instruction deck for the program and a list of instructions and annotations from which the programmer can work.

Although the program itself requires some 60,000 cards, as we illustrated in an earlier slide, about 10 times that number, 600,000 cards, must be punched while producing the program. The total area represented by these cards could cover all the floor space in the Lincoln buildings and the RAND buildings. The punchings from these cards would be enough, when burned, to boil the 5,000 gallons of coffee that have been consumed during the production of the program.



Slide 33

The next phase in the program production is illustrated by Jack Meyers of RAND compiling his program at the maintenance console (SLIDE #33). In this phase, he checks to see that core storage allocations for the program and tables are correct. The product of this phase is a stack of cards punched in machine language (20 instructions per card) called a binary deck, and cards showing where the program is in core storage during parameter testing and where it should get its tables.



Slide 34

(SLIDE #34) This is a picture of Inez Hazel of Lincoln, preparing parameter testing plans for her subprogram. As we said before, the program was divided into subprograms so that it could be brought into and operated from core memory. The test of the subprogram is parameter testing. During this test, the programmer operates her subprogram with simulated tables of input data, and she must bring out not only the results of the operation, but an idea of what happened at each step along the way so that she can find and fix the trouble. To do this, to bring out a history of what happens as the program operates, we use an instrumentation program called the "checker," which records the contents of specified registers at specified steps in the operation, so that in the event of a failure during the testing, the programmer can, by perusing this record, determine where the failure occurred. Mrs. Hazel in the slide is preparing a parameter checkout plan for testing her program. She specifies the simulated data required by the program and the expected results from testing the program on a data matrix. She also specifies the procedures in checking the program and an executive deck for telling the checker program what to record.



Slide 35

This picture (SLIDE #35) shows Mary Kresge of RAND at the printer in the control room in Building F, beginning a parameter test. In this phase, the parameter test planning is executed in the machine. The program is operated by the checker, and at specified points along the way, data is collected and printed out.



Slide 36

The printout from a typical program is several yards long, and in this slide (SLIDE #36) we see Russell White of RAND analyzing the results of the parameter tests.

The product of this phase is a final correct binary deck, which has passed the parameter test and is ready to be assembled with the rest of the subprograms.

I suppose you've been wondering why I told you this shaggy dog story. Perhaps you feel there is a point to it. Perhaps you think I'm not finished. We could go on and tell you about the secret ceremonials that the programmers hold at midnight at the maintenance console, the coming of age ceremony when the programmer finishes his first parameter test, the graduation to opinion leader, the dance of the section chiefs. All of this would be interesting and you would learn more about us from it, but this story does have a point and the point is this: we didn't know how much we would have to change the concept of our program until the coding was far enough along so that our estimates were sound.

In our example of the digital display, the people doing the work we described produced a program with 14,000 instructions in it, and this was just one of the program functions. The other functions were radar inputs, tracking, height-finding, situation displays, switch interpretation, etc. Digital displays had 16 out of 75 or so subprograms. The other program functions grew in the same way, and when we totalled the instructions up, we found that we had some 85,000, plus 22,000 registers for tables, plus 10,000 instructions for instrumentation, plus 5,000 instructions for forward telling, standby duplex, and automatic crosstabling. This totalled about 120,000 instructions. Now the auxiliary drums in the FSQ-7 have about 96,000 registers, so our program design was about 25,000 instructions bigger than it should have been. Did we cry? Did we tear our hair? You're damned right we did. But this is not too bad a miss in an estimate that had so many uncertainties in it. Obviously, something had to be done, so what we have done is to go over our program, as I told you in the beginning, and we have taken off the jacks on the four wheels, we have taken off the seat covers, we have made do with one cigarette lighter. In short, we are buying a Ford.

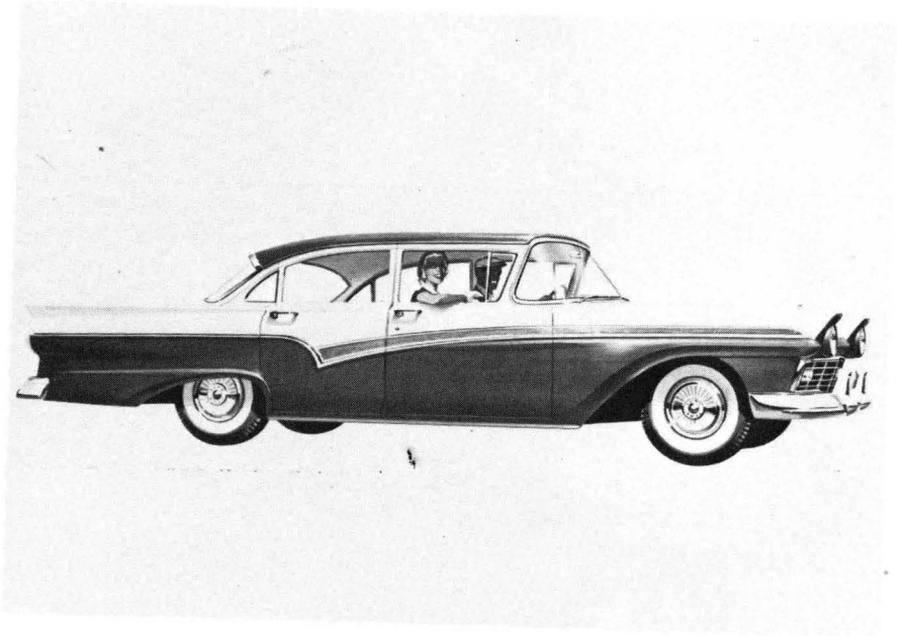
EFFECT ON PROGRAMS

<u>PROGRAMS</u>	<u>PREVIOUS LENGTH</u>	<u>SAVINGS</u>
<u>DD CONTROL PROGRAMS</u>		
CONTROL REQUESTS	1672	610
CONTROL FORCED DD's	2000	530
SLOT ALLOCATION	425	250
<u>DD SUBROUTINES</u>		
ALARM LIGHTS	79	NONE
CONFLICTING DD's	584	500
FORCED DD's	100	20
ILLEGAL ACTION CHECK	30	30
CONTROL WORD MAKEUP	250	50
PARTIAL CONTROL WORD MAKEUP	37	NONE
<u>DD MAKEUP PROGRAMS</u>		
ALARM DD's	1638	1000
GEOGRAPHY DD's	1703	310
TRACK DD's	1940	400
SUMMARY DD's	1395	680
MISCELLANEOUS DD's	1566	500
MONITOR DD's	646	646
<u>CENTRAL TRACK BOOKKEEPING</u>		
CTB	698	500
	<u>14,763</u>	<u>6,026</u>

Slide 37

This slide (SLIDE #37) will give you an idea of where we made the savings in digital display.

The control programs and subroutines, as I said before, are those programs which sense for conditions under which a digital display is shown to an operator. The savings that were made in these programs were due to a simplification and standardization of the information content and the rules for routing displays to particular stations. The digital display makeup programs, as we explained before, collect the data and make up the displays to be shown at particular stations. In alarms, the savings were due to a simplification and standardization of all alarm digital displays in the direction centers. The remaining savings in the makeup programs were due to the elimination of digital displays with limited utility and the simplification of other displays. For example, the tote that contained a summary of the number of tracks of each identity, and the tote that contained a summary of the weapons assignment status of the hostile tracks in the direction center were combined into one tote, serving both purposes, effectively eliminating one of the totes. We eliminated the tote that gave the detailed information on raids and groups (how many aircraft, mean speed and mean altitude, etc.), because all of this information is easily available on the situation display. And so we will continue to go through the program, standardizing, eliminating redundant information wherever we can, improving the efficiency of the operation itself. We expect that at some point when we have the time, we will be able to get a substantial saving by reprogramming in the light of what we have learned in the first pass through the program. We can't do much of this, however, because it takes time to go through this process for each program, so most of our changes will be in the form of elimination.



Slide 38

CONCLUSION

Thank you for coming over and listening to this speech. We never did get around to the differential equations and the technical talk that I promised you, but I think that we have made our points. We are doing what we must do.

(SLIDE #38) Note that she is as roomy as the Lincoln Continental, she can go as fast on the highways, and we can get her in our garage.

THE END

The SAGE air defense system was the first military system to be substantially controlled by a large-scale, real-time digital computer. Among the SAGE computer firsts were:

- Random-access magnetic core memory
- Real-time operating system with task sequencing
- Computer networking
- Digital transmission over telephone lines
- Computer-driven displays
- On-line terminals
- High-reliability computation
- Duplex computers
- Marginal checking
- Software debugging tools
- Automatic data description language (COMPOOL)
- Computer graphics
- Digital signal processing
- Digital track-while-scan
- Digital real-time system simulation
- Light gun input