How the SAGE Development Began

GEORGE E. VALLEY, JR.

Events leading to the adoption of voice telephone lines for air-defense operational messages are described. This process paved the way for the use of operational data lines in the sAGE (Semi-Automatic Ground Environment) system. The paper describes the early considerations leading to the use of a digital computer in sAGE, and how Whirlwind was chosen to be that computer. The context of the development of magnetic core memory is illuminated. The attitudes of engineering professionals toward digital equipment are reviewed. The author reveals how the name "Ground Environment" was created.

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Foreword

I am very pleased that the *Annals* has the opportunity to publish George Valley's memoir of how sAGE came to be. When we were putting together the *Annals* issue on sAGE (Vol. 5, No. 4, October 1983), of course, I asked George to participate in the discussion and also to write something about sAGE if he so wished. It turned out that he was unable to join the discussion, but he did agree to write an article. George never does anything by half, so he began by collecting documents and papers and then wrote the following extensive memoir. Unfortunately, by the time he had the memoir in a form with which he was satisfied, the deadline on the sAGE issue had passed, and the memoir could not be included.

The wait has been worthwhile, however. I find the memoir lucid, readable, and entirely fascinating. George Valley played a leading role in SAGE—without

Author's Address: 607 Main Street, Concord, MA 01742. © 1985 AFIPS 0164-1239/85/030196-226\$01.00/00 Valley there would have been no sAGE. He was there, and he tells us how he saw it and how he felt about it. So much of what we read these days has had all the emotion and humanity squeezed out of it in the name of cool factuality. Once in a while we come across something that sounds like it was written by a real human being—something that has the juices left in, that tells us not only about what happened but also about the person who wrote it. George has given us one of those.

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Introduction

This memoir is for any young scholar who may be tempted to engage in a nonscholarly enterprise. Verbum sat sapienti. It illuminates only the genesis and early history of what became known as the SAGE system. It is not a complete history of the early days of the MIT Lincoln Laboratory, nor of early work on aircraft control and warning systems. A blow-by-blow account of the activities inside Project Charles and Lincoln Laboratory has been reserved. I have read the papers about SAGE published in the October 1983 issue

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of the Annals of the History of Computing, but this account was prepared quite independently of them. Therefore I give no references to any of those papers, although a figure has been borrowed from one of them. I am indebted to Louise S. Meyer of the Mitre Corporation and to the late Margaret M. Bateman of Lincoln Laboratory for supplying me with various documents of the period about which I write.

1. Postwar Prelude

Standing one foot on the running board of the first cab, the famous Cal Tech relativist waved to me from the Pentagon taxi line. Professor H. P. Robertson beckoned, and I ran regardless of the stifling heat. "George," he began as I was getting into the cab, "I'v lately been briefed about the state of the radar system that the Air Force thinks it's setting up for air defense, and it's scandalous! It's disgraceful! Why don't you do something about it?"

"Me?"

"Yes, you're on the Electronics Panel, and if you ask they'll let you look into it." Then he continued, giving examples of the poverty of the ground-control stations, how even the most rudimentary supplies were unavailable—only the tools taken from jeeps to repair the radar sets—no petty cash even to buy light bulbs, and numerous other deficiencies.

I was happy to catch a ride with Bob Robertson because he could explain questions about relativity in a way that I could understand, and I had a puzzling question. I wasn't prepared for his charge to investigate the air defense, however, and my response was cooler than he might have expected. I did promise to look into the air-defense radar stations, although on that day in 1949 it didn't seem very urgent. No one expected a potentially hostile country to possess nuclear bombs for years to come.

Those who don't remember the towering prewar reputation of German physics do not understand how people could have misjudged the Russian ability to make a nuclear bomb. Almost every important fact of modern physics, if not discovered by a German, had been explained by a German. But the Germans failed to produce a bomb during the war, and therefore the Russian physicists, who had hardly any reputation at all, were not judged able to succeed where the Germans had failed. Few listened to the small minority who weren't impressed by this argument. Cocksure and arrogant, most of us were fooled.

In 1945, within days after the surrender of Japan, those scientists who had worked on the Manhattan Project, as well as some like me who had refused to work on it, had banded together to make nuclear energy into a force for peace, not doom. I had lobbied to Congress against the May-Johnson bill that would have placed nuclear energy entirely in the control of the Department of Defense; I had made innumerable speeches to lawyer's clubs, to doctor's clubs, to chambers of commerce, to Rotary Clubs, to Lions Clubs, to the League of Women Voters, to anybody who would listen.



George E. Valley, Jr. (S.B. Massachusetts Institute of Technology 1935; Ph.D. University of Rochester 1939), worked as a lens designer at Bausch & Lomb Optical Co. in 1935– 1936, and was then a graduate student and teaching assistant in physics (artificial radioactivity) at the

University of Rochester, 1936-1939. He was a Research Fellow at Harvard in 1939, and a National Research Fellow (nuclear physics) in residence at Harvard in 1940. He was a senior staff member at the MIT Radiation Laboratory 1940–1946, and editor of the Radiation Laboratory Technical Series in 1947. He joined the MIT faculty in 1947 as assistant professor and has been professor of physics since 1957 (now emeritus). From 1950 to 1952 he was chairman of the Air Defense System Engineering Committee of the USAF Scientific Advisory Board. He was assistant, then associate director of the MIT Lincoln Laboratory from 1951–1957 and was head of Division 2 from 1951–1956. He served on various SAB committees and panels and was Chief Scientist to the USAF Chief of Staff in 1958. He is a Fellow of the American Physical Society and of the Institute of Electrical and Electronics Engineers, and a member of the Society of Sigma Xi. He has been awarded a gold medal for excellence in mathematics by Flushing High School, a U.S. Army Certificate of Appreciation, a President's Certificate of Merit, and U.S. Air Force Exceptional Service Medals (1956, 1958, 1964).

Editor's Note: The usual style of the *Annals* is to use no titles for individuals, and to refer to them simply by last name. The author has elected to use combinations of titles, first names, last names, and even no names at all to convey how he felt about the various persons at the time.

But nuclear bombs continued to be made, and none of the business people—none of the lawyers, none of the doctors, nor the clergymen who were in attendance at all the meetings—showed any sign of doing anything about the bombs. There were a few MIT and Harvard scientists, and there was Mr. Henry B. Cabot who had interested himself in nuclear politics without our urging; that was the totality of those who were active in the Boston area.

International politics also became highly discouraging after the suicide (murder?) of the highly respected Czech statesman, Jan Masaryk, and the deposing of Eduard Benes during the period when the U.S.S.R. overran Eastern Europe.

Then the plan for the international control of atomic energy proposed by the United States and other powers failed in the United Nations (Carnegie 1946). Although some felt that many U.S. politicians were halfhearted in their support of these proposals, the United States would have had difficulty in backing out, had the proposals been accepted. But Russia, represented by Andrei A. Gromyko, had rejected international control on the grounds that it would infringe on the sovereignty of the U.S.S.R. He put forward an alternate proposal consisting of promises with no method of enforcement. The result was that after months of discussion the United Nations Atomic Energy Commission adjourned permanently on May 17, 1948. Short and clear demonstrations of the clashing views are contained in two papers published in the Bulletin of the Atomic Scientists (Vavilov et al.; Einstein 1948).

This event had the effect of causing me to drift closer to the Air Force and to become more active in its Scientific Advisory Board (SAB), on whose Electronics Panel I served. Thus, when the Russians did detonate a nuclear bomb, I was emotionally primed to respond; the more so perhaps because I realized that my almost-completed new house was vulnerable to the blast wave of the first bomb to hit Boston.

2. The Air Defense System Engineering Committee (ADSEC)

I arranged to visit a radar station, one of those installed by the Air Force's Continental Air Command (CONAC) for the direction of fighter planes in interception of enemy bombers. I didn't see much, because there wasn't much to see: mostly equipment brought back from the theaters of war, not well suited to the current need, and operated by crews that obviously lacked a suitable doctrine for the accomplishment of the air-defense mission. The site resembled one of those army camps of the Indian wars that you see in the late-night movies—except that Quonset huts substituted for log cabins, jeeps took the place of horses, and the officers didn't wear slouch hats. All the operational messages were sent by low-powered field radios operating in the high-frequency band; because these were long-distance messages, their transmission could be hindered by variations of the ionosphere.

I asked why they didn't use the telephone, and a grizzled officer replied with a sermon. He started with the customs of Pharaoh, went on past Ashurbanipal and Darius the Persian to the Battle of Marathon, and paused for breath at the fall of Rome. Then he quoted from Napoleon, from various Civil War generals, and wound up by reciting from the official investigation of the Pearl Harbor attack. His lesson from history was that a military man must never entrust his lines of communication to civilians.

I had collected other information about the state of our air defense. The staff of the Air Force Scientific Advisory Board (SAB) had supplied me with a number of evaluative reports, all disquieting, and with several budgets, all optimistic.

John W. Marchetti was then the civilian director of the Air Force Cambridge Research Center (AFCRC). This laboratory inhabited an old factory building abutting the MIT campus, and Marchetti's office was within easy walking distance of mine. Many of Marchetti's staff had been recruited from the MIT Radiation Laboratory and the Harvard Radio Research Laboratory at the end of the war. Marchetti cultivated those faculty members of Boston-area colleges who might be useful to the Air Force. He wanted to build AFCRC into a large government research center, preferably located near the airport at Bedford, Mass., where he had already established experimental facilities.

Thus it was easy to ask Marchetti to find answers to my detailed questions about air defense, and he cordially did so. He also showed me some of the experimental efforts in his laboratory, where I first saw John V. Harrington's apparatus for transmitting radar pictures over voice telephone lines. This development represented a pass-band compression of over 1000, and was the first practical demonstration of the application of information theory that I had seen. There, too, I first saw a "light gun," or "light pencil" as it is now known. These developments impressed me more than many of the wonderfully expensive items that the Air Force was paying its private contractors to invent in 1949. The afternoon that Marchetti showed me his laboratory was not the last time that I was to notice skilled and clever civil-service

engineers working as unsung heroes in government laboratories.

Marchetti was able to fill me in about the technical details of the air-defense deficiencies. He also proposed solutions, and many of them seemed good. On the whole he confirmed what I already knew, and I in turn could now confirm the impressions of Professor Robertson.

But the final impetus was that sermon about Pharaoh. It was still rankling when I phoned Dr. Theodor von Karman, the chairman and founder of the SAB. He and his aide, Major (later Lieutenant Colonel) Teddy F. Walkowicz, seemed unusually interested in what I had to say, and insisted that I write them a letter explaining my thoughts about air defense in detail.

It took me about a week to find the time to compose a suitable letter, but I finally sent off three pages on November 8, 1949. Since this letter has been mentioned by several authors, among them Kent C. Redmond and Thomas M. Smith in their book *Project Whirlwind* (1980),¹ I will quote from it.

A key paragraph of my letter appeared on the second page, following an abbreviated recital of the inadequacies of the air-defense system as it then stood.

I therefore propose to you that the board set up an Air Defense Committee to consist of members from several of its panels. The work of the Committee would fall into two phases, the implementation of the second phase to depend on the results of the first.

I suggested 10 subjects to be investigated during the course of phase I. I then proposed that if the committee found unsatisfactory answers to many of these questions (I already knew that the answers would be unsatisfactory) in phase I, it should continue immediately to implement phase II—namely, "To find the best solution to the air defense problem."

The Committee would have at its disposal several ground radars and crews together with a squadron of interceptor aircraft, and a free hand to operate these facilities as it willed. The site for this should be near a large city, so that the final product could be used to defend that city as well as to serve as a model for the other installations.

I proposed that the Boston-New York area would be suitable, and that the majority of the committee members be chosen from that area so that they might find it convenient to meet at weekly intervals. I would suggest that the Committee be composed of persons interested in *basic research*, as follows:

From the field of Physics and Electronics From the field of Aerodynamics From the field of Guided Missiles	2
	1
	1

It is important to note that I envisage no large contracts or expenditures; the Committee would consult as private members, held together by their mutual respect, and would make use of existing Air Force facilities.

In the weeks following this letter, I talked to the secretariat of the SAB several times, and on November 28 I attended a special meeting of the SAB Executive Committee, which was addressed at some length by General Muir S. Fairchild, the vice-chief of staff. Fairchild knew a lot more about the air-defense system than I had been able to ferret out. I was impressed by his frankness. He asked the SAB to help fix the system. He made a special point of reading from my letter.

On November 29 the SAB proposed two committees to him: (1) an Air Defense Policy Committee (this committee was not formed); (2) an Air Defense Technical Committee, which was essentially the committee that I had proposed.

On December 15, 1949, General Fairchild signed a staff letter to me, saying:

Regarding the Air Defense Committee, the Chief of Staff has directed that it be organized immediately, and it is planned that this group will be functioning within the next few weeks.

Also on December 15, General Fairchild signed another staff letter, in which I was requested to accept the chairmanship of the proposed committee. He enclosed a list of persons who had been asked to serve on the committee.

- George C. Comstock, an experimental physicist who had worked on blind-landing equipment during the war in the MIT Radiation Laboratory. He was at this time a vice-president of Airborne Instruments Laboratory, Inc. (AIL).
- Allen F. Donovan, an aerodynamicist, a member of SAB, and vice-president of Cornell Aeronautical Laboratory in Buffalo. Every Friday morning he flew his private plane to Boston for our meetings.
- Charles S. Draper, an SAB member, was professor of aeronautical engineering at MIT. At this time he was engaged in his pioneering work on inertial guidance.
- Henry G. Houghton, an SAB member, was head of the MIT Department of Meteorology.
- H. Guyford Stever, an SAB member, was professor of aeronautical engineering at MIT.

¹ The letter is mentioned in Chapter 11. My records and memory contradict at least 10 questions of fact, and a smaller number of interpretations, in Chapter 11. Nevertheless I believe that the remainder of the book, and even a part of Chapter 11, is reasonably accurate.

William R. Hawthorne, an SAB member, was professor of mechanical engineering at MIT and also at Cambridge University. He was a prominent expert on jet engines.

I was associate professor of physics at MIT and principal collaborator to Professor Bruno B. Rossi, the well-known investigator of cosmic radiation and the inventor of vacuum-tube gates, which we physicists called "coincidence circuits" (AND gates), and "pulse adders" (OR gates) (Rossi 1930).

On December 15, 1949, I wrote a four-page paper for the SAB entitled, "Tentative Remarks on the Task, Organization, and Program of the S.A.B. Committee on Improving Air Defense." It elaborated and brought up to date the contents of my letter of November 8 to Dr. von Karman.

A short introduction to the activities of ADSEC is provided by the following quotation from the official history of the SAB (Sturm 1967).

The SAB formed the Air Defense System Engineering Committee (ADSEC) and assigned it the task of developing "equipment and techniques-on an air defense system basis—so as to produce maximum effective air defense for a minimum dollar investment." The committee also set out to "help determine quantitative, factual data concerning current and future operational techniques and equipment" and, hopefully, suggest means that "would help improve the operational effectiveness of the existing Air Defense Command." Since ADSEC would work closely and frequently with an experimental unit of the Air Force Cambridge Research Laboratories, the SAB staffed it with "eminent scientists who could conveniently assemble regularly and on short notice, at that facility." Subsequently, Dr. George E. Valley accepted chairmanship, with Dr. Allen F. Donovan, Dr. Charles S. Draper, Dr. Houghton, and Dr. Stever as members. Two non-SAB scientists, Dr. John Marchetti and Dr. George Comstock, joined them.

Beginning their work in December 1949, the Valley Committee "worked diligently and with considerable success" for the next two years. At the peak of their labors, members met every Friday with government and Massachusetts Institute of Technology scientists at the Cambridge facility. As Dr. Valley described their operations, they functioned as informally as possible, making most of their recommendations verbally to the Air Force officials who sat with them. Their recommendations were then "translated into action by the Air Staff and pertinent field commands through the coordination of the SAB Military Secretary." After the Air Force and MIT, acting on ADSEC and SAB recommendations, created the Lincoln Laboratory there was no further need for the committee and on Dr. Valley's recommendation, the SAB formally dissolved it in January 1952. (Sturm 1967)

John Marchetti was most important in the functioning of ADSEC: his staff did all the housework, and he handled all the financial affairs with the aplomb of an experienced civil servant; he also served as my sounding board between meetings. Together we planned the agendas, determined whom to invite, and decided what to buy. He arranged meetings for me and furnished secretarial and security services, so that I was able to keep almost all of my ADSEC affairs outside of my MIT office.

3. How the Air Defense Command Came to Use the Telephone

The first task that ADSEC took on was a thorough investigation of Air Defense Command operations and equipment. The officers that met with us were completely open and frank—they told all. It became clear to us that the least expensive means of reliable communication were rented telephone lines, together with some special handling procedures well within the capabilities of AT&T to provide, and that not as many officers objected to using civilian-operated facilities during battle as I had earlier feared. My efforts of persuasion, by gossiping with staff officers about the advantages of sending air-defense operational messages over the phone lines, were paying off. But there was an even bigger job: repairing and setting in order the various aspects of the radar stations-taking care of all those faults and deficiencies that Professor Robertson had mentioned to me.

An existing Air Force contract with AIL was reoriented

in the direction of "general consulting, equipment adjusting, and furnishing of good test equipment," and led directly to the formation of the CADS enterprise. The AIL work led by Dr. Comstock resulted in an intensive spot analysis of all the failings of the then current system. These findings were forwarded to the Air Staff early in the fall of 1950. There followed a period during which ADSEC formulated the job that needed to be done to repair the electronic capabilities. (Division 2 1952)

Our attention then turned toward the telephone company. We had begun to think that the nature of the operations at a radar station, and the duties that it needed to perform, required the kind of labor study for which the Western Electric Company had become famous. Its operation of the Sandia Corporation was thought to be a precedent for asking it to take on the immediate air-defense problem. Here perhaps I should explain that the Western Electric Company (The Western) and the Bell Telephone Laboratories (Bell Labs, or The Laboratories, or BTL) were subsidiaries of the American Telephone & Telegraph Company (The Bell System), and that if you wanted "The



The caption on this "official U.S. Air Force photo": Secretary of the Air Force Donald A. Quarles presented the Air Force Exceptional Service Award to Dr. George E. Valley, Jr., in his Pentagon office, November 30, 1956. The citation read:

"Doctor Valley distinguished himself by exceptionally meritorious service to the Department of the Air Force from 1945 through 1955. He served first as a member of the AAF Scientific Advisory Group, under the chairmanship of Doctor Theodor von Karman, which prepared a report, 'Science the Key to Air Supremacy,' for General H. H. Arnold in 1945. Upon formation of the Scientific Advisory Board in 1946, Doctor Valley accepted an appointment to the Electronics and Communications Panel, and he served continuously as member or chairman of that panel through 1955. During these years, Doctor Valley devoted pioneering effort and exceptional ability to defining and solving problems of our continental air defense. In 1950 he played a major role in establishing the Air Defense Systems Engineering Committee of the Scientific Advisory Board. The work of this committee, under Doctor Valley's chairmanship, provided the basis for technical development and present operational capability of our air defense forces. The singularly distinguished accomplishments of Doctor Valley, in fields of scientific research related to air defense operational problems, have earned him the gratitude of the United States Air Force."

Western" to take on a managerial project, you first got "The Laboratories" to bless your project and interpret it to them.

In the fall of 1950, following Dr. Comstock's analysis, I began to operate somewhat independently on this particular project, serving more as the agent of Major General (later Lieutenant General) Donald L. Putt, the military director of the SAB, than as chairman of ADSEC. There seemed not much more that ADSEC could do to get the present system into order. Thus, with the backing of ADSEC and General Putt, I called my old friend Donald A. Quarles, vice-president of BTL and effectively its second in command. I told him enough of what was on my mind to encourage him to meet with me.

At that time the active leader of BTL was Mervin J. Kelly, "Iron Mike," as he was called. I did not find it easy to talk with Dr. Kelly, whereas I felt a filial affection toward Mr. Quarles, who was a kindly man. As I waited in his office in the old Telephone Building on West Street in lower Manhattan, I marveled at its Gilded Age magnificence. Although the room's lofty ceiling, its heavy furniture, and its marble fireplace all testified to an old-fashioned world and a stuffy society, there was nothing in the least old-fashioned or stuffy about Don Quarles. Both he and M. J. Kelly were aggressively forward-looking executives.

Quarles was a quiet, tidy man, and as he came in he smiled and softly shut the door, picked up the phone, and said there'd be no calls. Then he crossed the big room and greeted me with considerable warmth. He seemed already to know a lot about ADSEC, and I had the impression that while I was reciting the pedigrees of its members, he was comparing what I said with what he already knew. He questioned me at length about Marchetti, and eventually invited me to say what was on my mind. He nodded as I made each point, and grinned slightly when I said that there wasn't as much opposition to the use of the phones as I had been led to expect some months before. He said that he'd have to talk it over with M. J. Kelly and Fred Lack (president of the Western), but that what I proposed seemed to him to be appropriate for the Bell System to undertake in the national interest. I had expected that the size of the potential customer might interest him, and his agreeable response did not surprise me. As we were winding up the conversation, he casually asked what I thought should be done for the future. I then told him a little about our plans to send automatically generated radar codes over the lines. At this his face became radiant, and he was smiling broadly when he picked up his phone and ordered "the Cadillac to take Professor Valley to La Guardia." It was my first experience of the executive perquisites of a big corporation.

A week or so after my visit with Don Quarles, a mild-mannered representative of Western Electric showed up to brief ADSEC on the capabilities of the telephone company as they pertained to the current air-defense system. None of us had ever heard about telephones from inside the company before, and we were all charmed by the speaker, Clair W. (Hap) Halligan. A decade later, he became the first president of the Mitre Corporation. A few days after he spoke, I was informed that Halligan might be designated to lead an effort to improve the air defense, if AT&T were to be asked by the Air Force to undertake that task.

General Putt phoned and said the next step was for me to sell the Western Electric Continental Air Defense System (CADS) project to Lieutenant General Ennis C. Whitehead, commander of the Air Defense Command. General Whitehead's headquarters were in Long Island, about 30 miles east of New York; they were soon to be moved to Colorado Springs.

I was unhappy with this assignment because I was scared of General Whitehead. I'd heard so much about his terrible temper and his ruthless personality. His reputation was even more forbidding than that of M. J. Kelly, and I had little confidence in my ability to convince either of them of anything. But General Putt, in his kindly way, insisted that it would go smoothly, and so I went.

General Ennis Whitehead stood about five feet, seven inches tall and was bald. He wore gold-rimmed glasses and the standard "command personality"—an air of regal dominance combined with tough-guy arrogance that can be assumed by commanding generals when on active duty. It has been excellently portrayed by George C. Scott in the movie "Patton."

General Whitehead and his staff listened to what I had to say, asked only a few polite questions, and then escorted me to a sumptuous luncheon. While we had cocktails and appetizers. General Whitehead proceeded to tell me that he also did research. "Darkter," he said in his gravelly cigar-ravaged voice, "my research is on the subject of blood." Thinking that he would be telling something new about blood banks, or blood-pressure control for interceptor pilots, I smiled and nodded, while avoiding a refill of my martini glass. The shrimp had been flown in that morning from Louisiana and were delicious. "Darkter, my research tells me that when you have bled a nation white, you have it at your mercy!"

I offered my glass for a refill, and assumed an interested look, which I didn't feel. "Yes, Darkter, when you've killed 10 percent of their population, that means nearly 20 percent of their men, and twice that many wounded. All they have left are old men and boys, and they have to give up." After about 10 more minutes of this, complete with statistics from the Napoleonic wars, the Civil War, and other wars, I began to perceive from the expressions on the faces of the other officers that I was being hazed. I made play of cutting up my roast beef, put too much of it in my mouth at once, let a little juice dribble down my chin, and said, "General, that is the best piece of military research that has been done since Clausewitz." He looked me in the eye, glanced at my chin, grunted, and his aide and I discussed racing cars until coffee.

After lunch we stood around; soon General Whitehead reappeared and asked when Don Putt and I intended to start this operation, which couldn't start soon enough for him, he said. I answered that it would be as soon as the Air Force and Western Electric could make a contract. "Well, Dark, you tell them down there to harray up. Pleased to have metcha." He left, and I left—slightly in a daze. It had been so easy!

General Putt received the news of this meeting as though he'd been expecting it. John Marchetti said, "George, what did you expect? They may be stupid, but they're not THAT stupid!"

In a few weeks, while General Whitehead moved his headquarters to Colorado Springs, and the various officials ironed out the terms of a contract, General Putt suggested that I escort the Western Electric people out to Colorado, where Dr. Kelly could meet General Whitehead and the contract could be signed. So one day early in 1951 we all got into an Air Force Constellation, about half a dozen from the Air Staff, another half dozen from AT&T, and I.

About halfway there, one of the generals suggested that since I knew Dr. Kelly, I should brief him on what to say to General Whitehead. I asked what I should tell him to say, and was told, "Oh, the usual thing." Dr. Kelly was seated alone in the front row, engrossed in Rebecca West's book, *Black Lamb and Gray Falcon*. In his characteristic way, he was consuming cigarettes while he read.

Dr. Kelly's way was to stick the cigarette, once lit, to his lower lip and allow it to smoulder; he breathed in the fumes if any came near his nose, and allowed the ash to fall on his shirt front. When the cigarette grew so short that it burned his lips, he replaced it with a new one, which he also ignored. He usually did this when in conference, but there it was also his habit to sit with his eyes closed. He was especially impressive when he talked, for he frequently talked at you by making speeches, often with moral overtones, and sometimes sententious to a degree. Occasionally I wondered if he might be thinking about something completely different when he was making one of his eyes-closed, cigarette-ash-dribbling speeches; I came to suspect that he soared over crowds of boring people while on autopilot. In spite of the impression that his habits gave the unwary, he was one of the country's truly great administrators.²

Dr. Kelly intimated that he didn't want to be briefed; while continuing to turn the pages of his book, he made a speech at me.

The long journey in the noisy propeller-driven plane ended when we landed near Denver. As the plane came to a halt, a line of cars assembled beneath its wing. A covey of colonels greeted and individually escorted us to our quarters. After each member had admired the bottled abundance in his room's refrigerator, our delegation regathered.

We were escorted to a standard VIP Air Force reception, the kind that has a long table of food presided over by some swans carved out of ice with dark red roses frozen in them. Before they bring the food on, you have drinks and watch the swans melt. Dr. Kelly stood by himself, his eyes closed, cigarette ashes dribbling down his vest.

The aide approached me with General Whitehead, who was wearing a big black stogie as well as his command personality. "Glad to see yar again, Dark," he announced cordially. I led him over to Dr. Kelly, who slowly unglued the cigarette from his lip, opened his eyes, and thinly smiled.

"General Whitehead, may I introduce Dr. Mervin J. Kelly, executive vice-president of the Bell Telephone Laboratories. Dr. Kelly please meet...."

Kelly cut me off, "General, I want you to know that we're here to put your operation on the same dependable basis that has made the Bell System's worldwide reputation." He paused, and the general broke in.

"Well-ll..., is tha-at so? Does that include the two long distance calls I made this morning and I got the wrong number both times?" He took a pull on his cigar, and flashed his command personality. I knew they had to agree eventually, and all I could do by standing there was to become the butt of their common dislike—so I silently went over to the bar and talked with Halligan, who was taking it all in with the expression of a man reading the comic strips for the first time. Next day they signed the CADS contract.

Halligan's work, which included installation of a great many telephone lines for transmitting messages

about military operations, was the first important event in the history of what came to be known as the SAGE system. If the Air Force had not started to use phone lines for operational purposes, our proposal to use them as SAGE data lines—again for operational purposes—would have had to contend with enough additional prejudice that SAGE might not have been accepted. SAGE was not accepted easily.

4. ADSEC, the Fun Part

During the first several months of ADSEC's existence, its members learned about the air-defense problem, studied the histories of the RAF and Luftwaffe, and responded to these stimuli with new ideas. Fun, but it didn't last.

I will first mention the topics discussed at the earliest meetings, and then illustrate their content by briefly describing a few interesting ones (Cella 1950).

On January 20, 1950, we discussed: new X- and Lband radar projects, and the Raytheon proposal for a ground-wave radar; two devices to assist fighter aircraft to make interceptions, one an analog computer, the other a radar; free (unguided) rockets; a supersonic interceptor proposal from Douglas Aircraft; how to write the ADSEC charter. Major Richard T. Cella was our secretary, and we agreed not to write any reports. There was one visitor.

On February 1 we met at USAF Headquarters and reviewed our charter; we decided to invent an earlywarning device complete with communications and "an alarm bell to ring at the headquarters"; we discussed the possibility of correlating the data from many small radars by means of a computer. Project Whirlwind was mentioned; we decided to foster the testing of a combined radar-analog computer system in a jet fighter; we talked about how we could help the Continental Air Command (CONAC) improve its present capability (see Section 3); we stated a firm need for rocket-firing data; we made a crude budget, one of whose items was "rent of Whirlwind at MIT." We were briefed by Major General Gordon P. Saville and the Air Staff in Washington, then we went to Mitchell Air Force Base and were briefed by General Whitehead and the CONAC staff. All the ADSEC members began to understand the dimensions of the air-defense problem. There were three visitors.

On February 17 we heard a lengthy presentation by Marchetti about the use of CW (continuous-wave) radars together with a digital computer; Jay W. Forrester described Project Whirlwind and confirmed Marchetti's calculations; he also stated that Whirlwind was capable of 10,000 arithmetic computations

² An appreciation of Kelly's career appears in the *IEEE Spectrum*, December 1983.

per second, and that it would be ready to use in June 1950. ADSEC decided to make an early test by hooking up one of Marchetti's radars at Bedford to Whirlwind, using Harrington's phone-line apparatus. It was agreed to take over an ANDB (Air Navigation Development Board) contract between Whirlwind and the USAF's Watson Laboratories (Air Materiel Command in Red Bank, New Jersey-air-traffic-control studies), and reorient it to the air-defense problem. We discussed cruciform wings to give fighter aircraft the needed increased maneuverability; the status of turboprop versus turbojet engines was reviewed. Dr. Comstock discussed some immediate air-defense problems and some possible solutions. There were four visitors, three from Whirlwind; ADSEC extended a permanent invitation to the Whirlwind directors.

On March 17 ADSEC discussed the following: the serious limitations of the new generation of ground radars; currently proposed solutions to the data-handling problem; over-water ducting of radio waves; various new proposals for making a situation display; the forthcoming industrial competition for the electronics to be mounted in the new USAF "1954 fighter" (Professor Stever volunteered to become our expert on this). There were five visitors, including Forrester from Whirlwind.

On March 24 Forrester discussed the problem of combining data from three or more radars, and mentioned the need for storage tubes; Harrington described terminal equipment for pulse radars that were to be used with a computer; Dr. Roydon Sanders of Raytheon described CW radars, and so did Dr. Armig G. Kandoian of the Federal Telecommunications Laboratories. There were eighteen visitors, including five from Whirlwind.

These extracts from the ADSEC minutes illustrate both the diversity of ADSEC interests and the fact that the problem was conceived as being largely soluble by electronic means. They also record the dates on which Project Whirlwind personnel participated in ADSEC discussions.

ADSEC deliberations were along the following lines.

1. We started with a survey of the properties of airplanes, and the consensus was: (a) the range of any aircraft was greatly reduced by flying it at low altitude, and therefore Russian bombers would fly most efficiently and farthest at high altitude; (b) one bomber would be able to carry enough nuclear explosive to erase at least two large cities, if it were on a one-way mission to the United States; (c) if supersonic flight became possible, as expected, all interceptors would fly supersonically, but the next generation of bombers probably would remain subsonic; (d) short-range rockets would always be designed to go higher and faster than manned vehicles; (e) jet engines were inherently so reliable that future airplanes would be able to remain aloft, with aerial refueling, until crew fatigue brought them down.

2. The airborne interceptor radars ("AI sets" in the jargon) failed when looking down at a very low-flying bomber. The technology to remedy this failing did not exist in 1950, and therefore if bombers did fly low, they must also be attacked from low altitude whenever radar-guided interception was required: at night or in dirty weather.

3. The ground-control radars were seriously inadequate: (a) although they worked well at long ranges against high-flying aircraft, they were spaced so far apart that they could not detect low flyers, which could get a free ride to their targets; (b) the problem of ground echoes, while partially solved by the movingtarget indicators developed during the war, was still troublesome, and in hilly country could be forbidding.

We drew the following conclusion: while it was most efficient to fly high, high was also where antiaircraft homing missiles were going to work best, and these must eventually force the bombers to fly either low or not at all. ADSEC didn't know which of these eventualities was most probable, and therefore we needed to find out if it was possible for bombers to attack at low altitude. (Marchetti and I had already convinced ourselves that bombers could and would fly low; hence our interest in CW radars—see Section 5.)

Al Donovan volunteered to make the calculations, and at the next meeting he was able to show, to the satisfaction of the pilots and aerodynamicists present, that a bomber, flying in over the north polar region at high altitude, could always detect the ground radar before the radar detected it; it could thereupon descend under the radar beam and continue undetected at low altitude. To attack most of the northern cities of the United States, such a bomber would have to fly low for only about 10 percent of its journey, and therefore its range penalty would be small. If, in addition, aerial refueling were to be employed in the vicinity of the arctic circle, the entire United States would be vulnerable to low flyers: with the radars sited as they then were, low fliers would find easy paths to most cities throughout the United States.

Thus it was clear from the beginning that ADSEC must be concerned with the low-altitude threat, regardless of what any air force officer or airplane designer might think. In the years to come, my continuing insistence on furnishing sufficient capacity to counter the threat of the low-flying bomber was a handicap to the acceptance of the digital computer system, since almost all aerospace industry executives and almost all aviators were entranced by their newly found ability to fly faster and higher. (During the 1960s the Air Force designed the B70 bomber, which flew supersonically, but only at very high altitudes, and which had a huge radar cross section. That bomber was not put into production. The B1 bomber currently in production is designed to fly at both high and low altitudes, as are the cruise missiles whose tests are so often publicized on television. It is now generally conceded that a very good way to penetrate a modern antiaircraft missile defense is to fly low, under the radar beams.)

My belief that low-flying bombers would be the future threat was responsible for the conception of the SAGE system: the earth's curvature meant that hundreds, if not thousands, of radars would be required to detect low-flying aircraft. These low-altitude radars would require (as we thought early in 1950) moderately complex calculations to transform their signals into aircraft position and velocity data. There was no conceivable way in which human radar operators could be employed to make these calculations for hundreds of aircraft as detected from such a large number of radars, nor could the data be coordinated into a single map if the operators used voice communications. The individual computations were straightforward enough, and anyone could combine the data on a map if he had time enough. It was doing all that work in real time that was impossible.

4. The ground-to-air communications by voice radio were slow, imprecise, and easily jammed, but to make a fast, precise, hard-to-jam system was already within the state of the art. Therefore ADSEC decided the Air Force could take care of that problem on its own.

5. Interceptors were still being armed with machine guns, which were less and less effective against similarly armed bombers. Few people expected that the then-current generation of air-to-air guided rockets, each with its hundreds of tiny vacuum tubes, would prove practical for years to come, and unguided rockets were chiefly useful only against ground targets. Since setting up a system without potent weapons didn't make sense, ADSEC members much desired to find worthwhile interceptor armament for use in the immediate future.

Donovan remarked that the tail structures of airplanes are invariably more fragile than their wings. He said that if two identical airplanes collided so that the wing of one struck the tail of the other, the tail would be demolished, but the wing would suffer only minor damage. After each of the ADSEC members had understood the potential application of this fact, Donovan was encourged to go home and make some quantitative estimates.

The following week he showed calculations and preliminary drawings for a dart-shaped interceptor, whose leading edges were all made of steel, whose orifices could be momentarily shielded by steel shutters, and which was intended to be run through the tails of bombers, one after the other, until it required refueling. Donovan said he could make stress-scaled models of bomber tails, and collide similarly designed models of ramming interceptors with them, for a few tens of thousands of dollars. After all the pilots and aerodynamicists present had agreed with Donovan, Marchetti went to get the money.

A few months later, Donovan showed wind-tunnel movies and data from his tests. These were remarkable: the model interceptor, armored with thin sheets of steel, sliced through the aluminum model tails with scarcely a scratch.

Unfortunately, this elegant solution, although it was supported by mature pilots who volunteered to test it against old radio-controlled bombers, became confused with the repugnant Kamikaze suicide aircraft of the war. Of course, the proposed interceptors were in no sense suicide planes; indeed they were thought by all the experts to be safer to fly than interceptors armed with the then-available weapons. The public seemed unable to believe that, and the idea died. (It is my opinion that it took at least another 20 years of development to make air-to-air guided missiles dependable enough to obviate the need for the ramming interceptor.)³

6. The ground radars worked mostly at higher frequencies than did the associated IFF (Information, Friend or Foe) equipment, which resulted in the radar beam being considerably the sharper of the two. The radar beam was also effectively sharpened because the radar both sent and received, whereas the IFF only received. The result of these reinforcing effects was that whenever a friendly aircraft identified itself, its IFF signal tended to obscure a large sector on the radar scope. This was technically unnecessary, and was entirely due to the exigencies of the late war. We could have gotten the radar frequencies changed, but the IFF was such a tangle of private and corporate interests that it was effectively untouchable.

³ This topic created much discussion when presented to the main body of Project Charles a year later. In 1952 an attempt to revive the idea was made by Edgar Schmued, the chief engineer of the Northrop Aircraft Company, but he did not get any support for it either.

7. Dr. Louis N. Ridenour, who was then the chief scientist to the USAF Chief of Staff, proposed that unattended microphones be set up at intervals of about one mile, to substitute for ground observers. He thought that a useful ground observer corps would cost far too much to train, and that microphones connected to a central point would be much cheaper. Dr. Harry Nyquist of BTL volunteered to study the question; he concluded that even if the effects of birds and insects sitting on the microphones and the effects of ice and snow were to be overcome, the extensive communications network required would make the proposal uneconomical. People were still cheaper than microphones (Nyquist 1950–1951; Ridenour 1950*a*).

5. We Discover a Need for a Digital Computer

When John Marchetti had shown me his laboratory in 1949, I'd seen the specialized digital apparatus that John Harrington and his group had devised to display radar data received over the phone lines. I do not recall that it had then occurred to us that a generalpurpose digital computer might profitably be used to manipulate these data.

The few people who thought of doing real-time problems with digital computers seemed at that time to expect data to appear as servo outputs, or to be furnished by keyboards, or to be digital inputs of unspecified origin. Almost all the groups that were realistically engaged in guiding missiles (a group at Columbia University, under Professor John R. Ragazini, was studying interceptor guidance) thought exclusively in terms of analog computers.

I came upon the simple notion of connecting a digital computer directly to the sensory devices, so that they would function as one instrument, by an oversight, through a series of back doors.

Almost from the day that I'd become aware of the air-defense problem, I had been concerned about the threat of low-flying bombers, and had hesitated to get involved in air defense until I discovered an answer to that problem. The main difficulty was that pulse radars tended to be blinded by the much stronger signals from earth structures, out to ranges determined by the height of the structure and the earth's curvature. The radar signal from a low-flying airplane (say, at an altitude of 500 feet) could be swamped by "ground clutter" thousands to millions of times more intense.

A partial remedy had been found by recognizing that the returning echo from an airplane was slightly changed in radio frequency from that of the stationary

ground clutter. This Doppler effect was well understood and could be used to detect low-flying airplanes, but only if the ground-clutter signals were not more than about a thousand times stronger than those from the airplanes. (There were many parts of the United States, however, where this wasn't good enough, by far.) This moving-target indicator (MTI) apparatus compared the transmitted and received signals by means of a mercury delay line. E. J. Barlow of the Rand Corporation observed (Barlow et al. 1951) that the delay-line apparatus functioned like a comb filter, albeit not a very good one.⁴ The core of the problem was to make a comb filter whose characteristics were matched to the statistical nature of the ground-clutter signals. Such filters eventually proved to be impractical with the technology of the 1950s. The use of continuous wave (CW) radars would obviate the need for such a filter, and therefore the low-flying airplanes might be detected with much more certainty. The undesirable feature of a CW radar was that its signals allowed one to infer the radial velocity, but not the radial position of the target. Everyone familiar with radars knew these facts.

One evening late in 1949, while nodding over a set of exam papers in which no student had made an interesting mistake, I started a doodle about several CW radars connected together to a common observation post. I quickly discovered that three CW radars, emplaced at different known points, could provide signals from which the position of a target seen by all of them could be computed. I tried the computation and discovered that it must be done more precisely than I had expected. Since I couldn't imagine a crew of GI's poring over seven-place logarithm books or running high-precision slide rules while a battle raged overhead and atom bombs were dropping, I muttered, "Nuts," and finished correcting my papers.

A few days later I showed this configuration to Marchetti, still thinking it to be impractical. He carefully went over the calculations and a quarter of an hour later said, "Say, I think you've got something there." Marchetti had worked on radars before the war, and I thought he knew a good deal about them, so I was startled by his response.

"Yes, but those calculations...," I replied. I remember we simply stared at one another for a long time. Then I said, "Hey, it's easy to count frequencies; we could send the raw Doppler signal over phone lines and maybe Harrington's gadgets...." Marchetti con-

⁴ E. J. Barlow and his associates had discussed this problem at the Thursday evening seminars sponsored by ADSEC during the summer of 1950.

tinued to stare darkly, and I added lamely, "Well, maybe we could feed the output of the scaling circuits into a digital computer, and...."

"Now you're talking, George!" He smiled. It was a Saturday morning in December. By noon we had figured out block diagrams and how much such radars would cost if spaced every 10 miles on telephone poles. We had a list of CW radar contractors and another of companies that might build us a computer. We did some phoning the following week, and made a list of people who were already building computers, mostly in universities, many supported by the Office of Naval Research. The name of Whirlwind was not mentioned by any of my informants, and I didn't get to see it for several more weeks.

Marchetti called those companies that seemed likely to be able to build a digital computer to our order. Their replies were uniformly discouraging: too much time and too much palaver, not to mention the astronomical prices they quoted. We would have paid the huge price if a machine had truly been available, but what we wanted was a machine off the shelf, at a time when even the shelves had yet to be invented.

While telephoning around to other physicists and engineers who had already become computer enthusiasts, I rapidly accumulated a store of the thencurrent speculations about the potential usefulness of computers and their current problems. Some people wanted to do purely mathematical problems, of which matrix inversion seemed most important; others wanted to manipulate lists. Relatively few wanted to connect computers to the real world, and these people seemed to believe that the sensory devices would all vield data. In fact, only some sensors—such as weighing machines, odometers, altimeters, the angle-tracking part of automatic tracking radars-had built-in counters. Most sensory devices relied on human operators to interpret noisy and complex signals. The radars that scanned the airspace for the purpose of creating a dynamic map were prime examples of devices that needed skilled observers. These were vital to air defense.

The Raytheon Company's Hurricane computer looked promising. The company was actually studying real-time air-defense problems, although only for the guidance of short-range missiles for fleet air defense. This was not a purpose of great interest to me, since the fleet was unlikely to be defending cities. Hurricane was a parallel computer having a 35-bit word length, and it used 36 delay lines for its memory (Raytheon 1948; ERA 1950). The Hurricane people were also speculating about tying a network of theodolites, or of automatic tracking radars, into their machine, and using the system to track missiles. Like other computerists, they assumed that all sensors yield data.⁵

J. V. Harrington's group was the only one I found that addressed the task of automatically converting radar signals into digital radar data. (The "trackwhile-scan" tests that had been carried on during the war at the MIT Radiation Laboratory had employed analog techniques.)

6. Whirlwind

The more we examined the CW radar idea, the better it looked, and others in Marchetti's laboratory also became interested. It did seem to be an elegant solution, so we continued our search for a digital computer that we could use to count Doppler frequencies and compute bomber positions.

We had even begun to think about building a computer ourselves, a chore that I regarded as too uninteresting to bother with if I could find someone else to do it. In January 1950 I ran into Jerry Wiesner in the hall at MIT. He wanted to know what was new with me, and he told me all about the ground-wave radar experiments at Raytheon. I told him about connecting radars and computers, and that I could get money to make a test if I could find a computer whose proprietors weren't too crazy or too busy. He immediately replied that one was up for grabs, right there on the MIT campus.⁶

Wiesner mentioned Jay Forrester's name, and I remembered having heard about a huge analog computer that had been started years before; it was an enterprise that I had carefully ignored, and I had been unaware that the Forrester project had been transformed into a digital computer. He told me that Forrester now occupied the Barta Building, about halfway between my office and Marchetti's. But Wiesner didn't tell me anything about the troubles that Whirlwind was then having, or of the criticisms that were being leveled at it; nor did he tell me why he thought it might be available.

Marchetti called his opposite number in the Office of Naval Research to find out whether Whirlwind would be available to us, and why ONR might not

⁵ The automatic tracking radars developed for aiming antiaircraft guns during the war did yield angular position data directly, but the range readings still had to be monitored by an operator. These radars were not useful for the much bigger problem of bomber detection and interceptor control, although they were essential parts of gun-aiming systems, including the fire-control systems mounted on interceptors.

⁶ Redmond and Smith (1980, p. 174) give an overblown account of my conversation with Wiesner.

want to continue supporting it. I called other physicists whom I knew to be interested in computers to find out why they hadn't already told me about Whirlwind. (They all thought I already knew about it.) All these people gave us reports about Whirlwind that differed only in their degree of negativity. They had many reasons for warning against Whirlwind, although not nearly as many as have been recited by Redmond and Smith (1980).⁷

After analysis of all the explicit criticisms of Project Whirlwind, we concluded that they could be separated into three categories: (1) it was unnecessarily fast, and therefore too expensive, and besides there were no useful problems for a machine with a 16-bit word; (2) it wasn't designed according to the informants' ideas, which was to say that it didn't look like other machines, and it was therefore too expensive; (3) reasons that seemed mostly based on emotions and bruised toes. Beneath these pejorative characterizations were also tacit but consistent hints of basic faults in the Whirlwind design.

We thought that we had suitable problems for a fast 16-bit machine, and that we could get some stout shoes, so we tended to eliminate many of the criticisms; and of course Whirlwind, being already under construction, couldn't cost any more than some of the astronomical prices that John Marchetti had from industry. The possibility that it might not be well designed needed to be looked into, however. Since Whirlwind was being built at MIT with Navy money, we decided that I should make the first visit.

I had learned about pulse circuits in the MIT Radiation Laboratory and had edited several of the Radiation Laboratory Technical Series of electronics books.⁸ I had since been principal collaborator with Professor Bruno B. Rossi, who was known for his investigations of cosmic radiation. My research apparatus involved gating and counting circuits, and I was accustomed to handling poorly shaped pulses that arrived at highly irregular intervals. I considered computers to be child's play because in them you could manufacture nice square pulses on one side of the room and count them on the other. People more experienced with computers than I then was may smile at this.

During 1948 and 1949 both the MIT Radiation Laboratory and the Los Alamos Scientific (now National) Laboratory had published long series of books about electronics. They described in tutorial detail the theory, the approved methods of construction, and the operation of almost all the new circuits used for weapons developed during the war. The design and construction of pulse circuits of all kinds were covered in minute detail. The original source documents for the Radiation Laboratory books had been preserved by MIT and were regularly consulted by scientists and engineers throughout the northeastern section of the country; other such collections were maintained on the West Coast and in other regional centers. Because I knew that some of the people designing computers had been to MIT to use this collection, it seemed that Whirlwind too must have profited from these papers. Therefore I had no real worry about the quality of Whirlwind engineering, and I tended to discount unfavorable reports about its technical excellence.

Before I approached Whirlwind, Marchetti mentioned that he'd learned that the USAF's Watson Laboratories had a small contract with Whirlwind, and if I approved, he could take cognizance of that contract. In that way we could quickly get an interest in the machine and start its people on the air-defense problem immediately. I determined to find out the details of the Watson Laboratories contract.

Although Marchetti's friends in ONR had told him they intended to reduce their share of Whirlwind's budget, we didn't realize how close to extinction the project actually was (Redmond and Smith 1980, pp. 151–155). Consequently I was surprised by the warmth of my reception at Whirlwind.

Jay Forrester and Bob Everett showed me everything, and they had the machine running. One of its critics had told me that Whirlwind was constructed on a huge scale. Whirlwind was that, for sure: it occupied more space than any collection of vacuum tubes that I had seen until then. The tomes these people had read must have included some with which I was unfamiliar. But my impression was powerfully tempered by the fact that the machine was functioning: it was calculating a freshman mechanics problem and displaying the solution on a cathode-ray tube. This demonstration, plus the additional fact that I only wanted to use Whirlwind for an experimental test of feasibility, swayed me toward Whirlwind. Besides, there wasn't any other computer to be had.

⁷ See pp. 60, 68, 69, 74, 75, 80, 82, 101, 126, 131, 150, 151, and passim.

⁸ Cathode Ray Tube Displays, ed. by T. Soller, M. A. Starr, and G. E. Valley; *Electronic Time Measurements*, ed. by B. Chance et al.; *Vacuum Tube Amplifiers*, ed. by G. E. Valley and H. Wallman; *Waveforms*, ed. by B. Chance et al.; all published by McGraw-Hill, New York, 1948. These are a few of the Rad Lab books, not all of them edited by me, that were pertinent to computers; a reader interested in the full list of the Radiation Laboratory Series will find it printed in any of the books. In the Los Alamos series, a notable book was *Electronics: Experimental Techniques*, by W. C. Elmore and M. L. Sands, McGraw-Hill, New York, 1949.

The most complex problem that they could compute at that time was limited because there were only 5 words of RAM and 27 words of PROM (these acronyms hadn't been invented at that time), which seemed sufficient for the ADSEC feasibility test: entering data from a pulse radar into the computer via a phone line and Harrington's apparatus. When the new memory had been installed, the machine would be capable of running at least one interception, and it would also be capable of the calculations involved in the use of CW radars.

I told Forrester and Everett of these possibilities, and both men seemed very interested. They told me about the problem of air navigation and traffic control that they were studying for the Watson Laboratories and gave me over an inch of reports about that study. We parted with the understanding that we would probably see more of one another.

When my MIT duties next permitted me to visit Marchetti, he immediately wanted to know if Whirlwind was as much of a balled-up mess as we were being told. I replied that while Whirlwind gave the appearance of being mechanically overdesigned, and also looked like something guaranteed to set the teeth of experienced pulse-circuit designers on edge, it nevertheless seemed to work, and it was available. Since we only wanted to use it to prove a point, I thought we should consider it like any other piece of surplus military equipment and exercise the government's privilege to take it over before it was scrapped. Thus if ONR truly wanted out, we might get the machine and its crew on favorable terms. Marchetti said he could have some costs for the February 1 ADSEC meeting, and we decided to take a chance on Whirlwind. We decided to have it more thoroughly inspected during the coming weeks. As I continued to describe details of Whirlwind's construction to him, Marchetti finally exclaimed, "That I have to see!" We agreed to pay a joint visit to Whirlwind in a few days.

I spent the following Sunday going over the sheaf of Whirlwind reports they'd given me and found the following: (1) three quarterly reports on the Watson Laboratories project (Servomechanism Lab 1949), and (2) several memoranda by Gordon Welchman (1949) and David Israel (1949) also having to do with the air traffic control project. Some weeks later I was given another sheaf of reports, mostly by Welchman, and also a copy of Israel's proposal for a master's thesis (1950) on the subject of air traffic control. These papers made the same assumptions that I have already discussed: that radio and radar signals are equivalent to data. One, dated January 10, 1950, is notable for the use of the acronym AGE, but alas it only meant "Airborne Guidance Equipment." The papers by Israel seemed insightful.

Project Whirlwind's quarterly reports to the Watson Laboratories were as follows. The first report reviewed programming already accomplished and presented codes for such tasks as sorting, linear interpolation, series summation, and square root. The codes were given and were the first that I had had the opportunity to study. The second quarterly report described what the group had learned about the air navigation and traffic-control problem. The third report, covering the period from July 25 to October 25, 1949, was mostly devoted to the problems involved in blind-landing aircraft at the rate of two per minute, and it contained somewhat longer codes to accomplish some of the tasks necessary to land aircraft.

Although they had only a tangential bearing on air defense, these papers were intelligently written and showed that the Whirlwind people wanted to learn. I was encouraged.

I will now restate the operational jobs that I had in mind for a digital computer if the initial Whirlwind feasibility test was successful: (1) the use of the computer to tie together a group of CW radars, and to extract position data from their signals; (2) the more general problems of maintaining a dynamic map of the airspace and of directing interceptors and missiles to their targets.

The air-defense system and the air navigation and traffic-control system are superficially similar, but basically they are qualitatively different. The air-defense system deals with uncooperative aircraft that try to hide from it, whereas pilots using the trafficcontrol system are only too glad to tell it where they are and what they intend. Moreover, if the traffic system tells a commercial pilot to make a certain maneuver, he'll obey, whereas one doesn't attempt to tell an enemy bomber pilot anything. Thus the airdefense system has a much harder job of gathering its data—enough harder to make the problem qualitatively different, for the computer as well as all the other equipment.

It might seem that having solved the air-defense problem, the same equipment could be used in traffic control. This statement is only partly true, because the requirements for safety are very different: one collision between commercial airliners is too many, whereas one missed interception can in principle be rectified. Not only are the operational requirements different, but the standards of operator performance are also different.

It was my opinion at that time that a solution to the air-defense problem would be in part a solution to the air navigation and traffic-control problem, but that the reverse was not true: the best of solutions to the civil problem were less likely to be of value for air defense. Nevertheless, I did not discount the Whirlwind group's experience on their Watson Laboratories contract, for they had learned about aviation. I was also encouraged because neither Forrester nor Everett seemed to think he knew all the answers.

The realization of the grave difficulty of the airdefense problem had caused many groups to devise "advanced" systems, each one resembling all the others. They all tried to improve the current manual system by simply equipping each of its operators with a labor-saving device, usually an analog gadget.⁹

When Marchetti took over the Watson Laboratories contract, I expected to hear screams from the civil servants who had been connected with it. To the contrary, the pleasant and very competent project engineer, Herbert Sherman, far from being angry with us, asked for a job. He became a good friend and eventually joined the Lincoln Laboratory. We reoriented the Whirlwind work toward the more immediate task of receiving Harrington's radar data from a phone line.

When word spread, in the following months, that I had gotton into bed with Whirlwind,¹⁰ a number of busybodies warned me it was a grave error. The same kind of spiteful talk started up again after Lincoln Laboratory was founded to carry on the air-defense work, and the gossip sometimes damaged our relations with people whose help we needed. Early in 1950, however, I was able to pacify such critics by assuring them that I intended to rent Whirlwind for only the one year. That was indeed my intention, for I no longer thought a part-time committee like ADSEC could manage or direct a larger experimental program than would be involved in making a proof of principle. I thought we should try to finish the tests, make our recommendations to the Air Force accordingly, and go home.

Other people also disapproved. Following a meeting on March 5, 1950, at which I agreed that the Air Force would support Whirlwind's budget, I found myself snubbed in the halls of MIT by a personage very high in its administration.

In concluding this section, I think it only fair to say that in the coming years I grew to respect Whirlwind's elephantine ruggedness. Whirlwind always worked when needed. Other groups of computerists might have produced a more sophisticated and smaller, perhaps even a faster, machine. But given the properties of vacuum tubes, I doubt any other group would have designed more reliable machinery, so well suited for the military need. It is a pity that this great accomplishment can no longer be appreciated, because so few now remember what it was like to use flip-flops as big as your hand, and to have blisters on your fingers from pulling hot tubes out of sockets that gripped like tiger's jaws.

Much credit is also due the Whirlwind staff, who all fell in with the new project of air defense. In the following years, Jack A. Arnow, Stephen H. Dodd, David R. Israel, John F. Jacobs, Kenneth H. Olsen, William N. Papian, Norman H. Taylor, C. Robert Wieser, Charles A. Zraket, and their co-workers carried out their share of the task willingly, loyally, and with great imagination and effectiveness. Under the pressure of our frequent exercises, they invented many devices that are important in all computers today.

The Korean war started during the summer of 1950, and was the prime cause of my own continued involvement with air defense, and also of Whirlwind's.

7. CW Radars

Sometime in the spring of 1950, it was noticed that the original elegant notion of a net of simple CW radars, with omnidirectional antennas all working into a digital computer, would yield ambiguous data if there were more than a single target. We all rushed to invent more complex configurations, in the naive hope that computers could rectify the fault. (Computers couldn't: ambiguity in, ambiguity out.)

After we had become convinced that the telephonepole radar wouldn't work, given the then-current state of technology, we began an intensive hunt for a new and better way to fill the low-altitude gaps in the radar coverage. I began holding Thursday-evening radar seminars to which mathematicians, engineers, and physicists from industry as well as universities and Air Force installations were invited.

A number of papers proposing and analyzing new systems were presented. S. B. Welles and J. W. Marchetti of AFCRC each presented a paper; Philip Franklin of MIT (a consultant to the Whirlwind

⁹ See Project Charles (1951, paragraphs 4010 through 4022). The systems were: the British Admiralty Comprehensive Display System (CDS); the RAF System; the U.S. Navy's version of CDS, its Project COSMOS, and its Mark 65; the U.S. Army's Project 414A (SYS-NET); the USAF's version of CDS, its BOMARC Test Phase Ground System, and its Watson Laboratories Ground Reporting System. Project Charles recommended that all the engineers engaged in these almost identical systems be employed on just one of them in order to make it work. This didn't happen.

¹⁰ Redmond and Smith (1980, pp. 155–157) describe how I promised \$500,000 of USAF money to support Whirlwind in March 1950. I could have gotten much more, but that's all they seemed to want at the moment.

group) presented five papers; representatives of the Federal Telecommunications Laboratories gave a paper; I offered two of them; H. P. Stabler of Williams College (ex-MIT Radiation Laboratory) wrote one paper; Dr. Harry Nyquist from BTL was very active in analyzing these proposals, and contributed one paper and was probably the author of two handwritten manuscripts that survive as photostats.

Among those who actively participated in the long discussions and analyses, and who also presented papers orally, were other members of the Rand Corporation; Dr. Royden Sanders and his associates from the Raytheon guided missile group; Louis D. Smullin and Robert Fano from MIT; Robert V. Pound from Harvard; Jay Forrester, Robert Everett, and others from Project Whirlwind; and John Marchetti, John Harrington, and others from AFCRL. All these people struggled to invent a new solution to the groundclutter problem.

These seminars went on for several months, and many new radar configurations were considered, most of them conceivable only if a digital computer were a part of the data-handling system. None of these ideas stood up. One good idea, put forth by Barlow (1951), was that because the then-used MTI delay line could be viewed as the transform of a comb filter, a real comb filter might be substituted for it and might yield superior MTI performance.

We also concluded that phased-array radar antennas would be the best type to use with a digital computer. Marchetti already had built a small-scale model of a phased array, and it promised remarkable performance. Its difficulties were its inordinate complexity and profligate use of vacuum tubes in sensitive analog circuits.

A result of these discussions, and of similar ones during the course of Project Charles (see Section 9) and also stemming from the work directed by S. N. Van Voorhis in Lincoln Laboratory—was the stimulus given to exploring the use of the radio-frequency phase displacements in radar signals. Numerous inventions made in many laboratories and a huge number of journal articles have resulted from this work.

8. ADSEC, Not So Much Fun

Almost from ADSEC's start, the Air Force pressed money on it, and by the February 1, 1950, meeting, a tentative list of expenditures had been approved: development of CW radars and ground-wave radar; rental of Whirlwind at MIT; terminal equipment, simulators, etc.; a control center at Bedford Airport; rental of telephone lines. All these together came to about a million dollars, of which about half was for taking over Whirlwind from the Navy.

ADSEC began to be tedious when the Air Force insisted on a progress report before we had made any progress—in order to support our budget, they said. ADSEC's members were already working longer hours than the normal one day per week expected of academic consultants, and preparing a report would be a considerable extra load. Nevertheless, I spent a weekend writing a draft and several two-hour sessions with staff officers revising it. An official USAF copy of the report is dated April 6, 1950 (Valley 1950*a*).

ADSEC's renown began to grow, and in July 1950 I was invited to a Pentagon briefing, where I sat with 21 generals, a colonel, Dr. M. J. Kelly, Don Quarles, and an assistant secretary.

Mingling in such high society apparently gave me notions of grandeur, for also in July I wrote a letter to General Gordon P. Saville telling him that while I had the technology of air defense in hand, he would have to do something about the human factors involved, which I described in unnecessary detail. Time was, if you said things like that to a general, you'd be called out at sunrise. But General Saville and I became good friends. The remaining ADSEC meetings of that spring and summer were occupied by briefings and travels, and by listening to proposals of such little merit that only a short hearing was given to each. Commercial representatives began to hound me, having heard that I was a naif who could hand out government money.

Upon returning to the Cosmic Ray Group's office one morning, I found an impressively suave gentleman, wearing a waxed mustache and a sharkskin suit. He briefly touched his manicured fingers to my chalky paw, while explaining that he represented the American Totalisator Corporation. War, he continued, was very much like a horse race, and therefore if the Air Force needed digital apparatus, it needed totalizators, which had been proven to be real winners. "What is a totalizator and why is it not called a totalizer?" I inquired, in starched pedantic tones. That set his vocal cords into fibrillation, because having always sold totalizators to racetrack operators, who already knew, he'd never had to learn what one was.

In September Harrington and the Whirlwind people got their flip-flops lined up, and they demonstrated to ADSEC and its observers that radar data could indeed be received from a phone line, manipulated by a digital computer, and then displayed on a cathode-ray tube. To my mind this was proof of principle, and sufficient for ADSEC's purpose, which was to tell the Air Force what to do, not to actually do it for them. To be sure, it was a meager demonstration, since Whirlwind still had only 5 words of RAM and 27 words of PROM. But it seemed to me that the most obtuse person could easily see that only some very obvious extensions of the existing system were needed to operate on more complex data, in more complicated ways.

This demonstration as well as the ever-increasing amount of trivia thrown at us by salesmen caused me to wonder if ADSEC was running down, reaching the natural end of its life. I began to imagine the writing of a final report by the end of 1950. Unfortunately for my dream, the Korean war had started in July, and three former division heads of the MIT Radiation Laboratory had accepted temporary jobs in Air Force Headquarters. These men, with all of whom I was on friendly terms, now demanded and received a demonstration of ADSEC's "achievement," as they called it. Following their visit to Cambridge, they began to talk about a new laboratory, to be an Air Forcefinanced successor to the long-disbanded MIT Radiation Laboratory. The proposed laboratory's job would be to improve the air-defense system, using the digital techniques outlined by ADSEC.

On November 20, 1950, Dr. Louis N. Ridenour, the Air Force's chief scientist, sent a memorandum to General Saville, who was then the deputy chief of staff for development: "Proposed Augmentation of ADSEC Activities" (Ridenour 1950). Most of this two-page memo was devoted to describing the air-defense problem and what ADSEC had done about it. The following were some of the operative sentences:

It is now apparent that the experimental work necessary to develop, test, and evaluate the systems proposals made by ADSEC will require a substantial amount of laboratory and field effort.

All concerned agree that the necessary effort might be made available by negotiating a research contract with a suitable institution in the Cambridge area. It is important to have this work centered in Cambridge, in order to provide continuing close contact with ADSEC and AFCRL.

A very tentative exploration of the matter with MIT has indicated that they would consider taking such a contract as that proposed.

The memorandum then estimated a total professional staff of about 100 and a budget of about \$2 million per year. (The Lincoln Laboratory's budget during the 1950s was well over \$20 million per year.) This memorandum is the first document leading to the formation of the MIT Lincoln Laboratory, where SAGE was developed, and which was to have so profound an effect on MIT.

I did not favor the idea of an air-defense laboratory until a second group of Radiation Laboratory veterans, with whom I regularly worked at MIT, decided to oppose the setting up of a new laboratory unless they could run it. Because they knew even less about air defense than the first group, I wasn't very eager to help them, either. The second group then began to attack ADSEC and me as incompetent. They also began to attack the whole idea of using digital computers: they were big expensive toys, useless for any practical scientific work. These statements did strike home to me, for several of these men were my seniors at MIT, and one of them had helped recommend me for tenure.

A few weeks later, on December 15, 1950, while in the Pentagon on SAB business, I received a note asking that I have lunch with Dr. Ridenour. After dazzling me with a lunch at the Chief of Staff's table in the Secretary's Mess, he coaxed me into drafting a letter to be signed by the Chief. The letter requested MIT to set up an electronics laboratory to develop the air-defense ideas originated by ADSEC. I completed it in about an hour, and Ridenour spent another 15 minutes recasting it into appropriate general officer's diction. Then he had it typed, and by four o'clock it had been signed by General Hoyt S. Vandenberg, Chief of Staff, and was on its way to Dr. James R. Killian, Jr., the president of MIT (Vandenberg 1950).

This two-page letter contained the following statements.

The Air Force feels it is now time to implement the work of the part-time ADSEC group by setting up a laboratory which will devote itself intensively to air defense problems. We think it would be best to do this in the Cambridge area, since we intend this laboratory to have the continuing advice and guidance of ADSEC, and because the new laboratory must work closely with the existing Air Force Cambridge Research Laboratories.

MIT was to quote this famous "Vandenberg Letter" more than once during its long struggle to preserve the independence of the Lincoln Laboratory.

Lord Acton's famous remark that "power tends to corrupt and absolute power corrupts absolutely" suggests that corruption may be a monotonic function of power. If so, then extrapolating backward, one surmises that he might have agreed that a mere taste of power may infect with a mere tinge of corruption. So it was with me, and well-meaning officials had little trouble tempting me further down the garden path: Marchetti found it possible to convince me to make a proposal for actually operating the new laboratory. On December 19, 1950, I sent a letter to MIT elaborating on General Vandenberg's letter (Valley 1950b). I described in some detail how large the laboratory should be, and how it might be organized. This estimate was consistent with Ridenour's plan, but the budget was raised to \$6.6 million per year, at Marchetti's urging; also with his advice, an item of \$1.65 million of annual overhead to support MIT's management services was mentioned. Marchetti helped me considerably in writing other parts of the letter as well.

MIT turned this down, to the dismay of the Air Staff, which seemed to see me as its champion in the great physicists' jousting tourney, which now began.

After what must have been an enormous effort, the MIT administration avoided having to cut the baby in half, and in Solomonic wisdom prevailed on Dr. F. W. Loomis to organize an air-defense study group, to be called Project Charles. Wheeler Loomis had the affection of all the physicists and engineers who knew him, and so we all agreed to join Project Charles. Other technologists, from industry as well as from universities, also joined.

On January 19, 1951, the chancellor of MIT, Professor Julius A. Stratton, met with the Scientific Advisory Committee to the MIT Research Laboratory of Electronics (Sayers 1951). These Army and Navy officials were fearful that their influence with MIT was about to be eroded as a result of General Vandenberg's proposal. Chancellor Stratton said MIT would proceed in three phases: I, Project Charles; II, the Valley program; III, a \$10 million per year laboratory. It was understood that phase II was necessary so that the "Valley program" would be sustained while Project Charles deliberated on its worthiness, whereafter it would be a part of the phase III laboratory.

During all the hubbub, hardly anybody mentioned digital computers or Whirlwind, but I noticed that I was out of the MIT administration's doghouse.

Thus was the MIT Lincoln Laboratory conceived.

9. Project Charles

Project Charles started early in 1951 under Loomis's direction. It occupied the upper floors of a building that had just been acquired by MIT for its School of Management. The full-time members included seven from the MIT faculty and five from other universities; four members were also members of ADSEC; the remaining members were from industrial and government laboratories. Marchetti was the only civilian representing the Air Force. There was a total of 28 full-time scientists and engineers.

In periodic attendance were members of a group called "consultants." Of the 16 members of this group, 5 were MIT faculty, and 3 were professors from other universities.

A group of officers also attended Project Charles. They represented the three services of each of Great Britain, Canada, and the United States. The senior officer was Air Commodore G. W. Tuttle, RAF; Lieutenant Colonel Peter J. Schenk was the only USAF officer present. Colonel Schenk, a protégé of General Saville, helped me by running an informal intelligence service; he faithfully reported the ploys and strategems of the opposition. Had Margaret Mead attended Project Charles, she might have written a sequel to her well-known book: *Growing Up Among the Physicists*, she might have called it.

A librarian, two business managers, a mechanical engineer, and two assistants were on the project, as were 18 secretaries, one of whom was Jean S. Holden who became my secretary in the Lincoln Laboratory. She was unusually intelligent, shrewd, pleasant, and loyal; I am much indebted to her, and she remains a valued friend.

Of the Project Charles scientists and engineers from other universities, Professors F. W. Loomis, Ragnar R. Rollefson, and S. N. Van Voorhis joined Lincoln Laboratory. No member of Charles from MIT who wasn't already a member of ADSEC or Whirlwind or of the group that had originally opposed the laboratory joined. Squadron Leader Ronald G. Enticknap, RAF, also joined me. He became a prominent member of the Lincoln Laboratory staff.

Project Charles closely examined the findings and proposals of ADSEC, and I wrote them all into its final report (Project Charles 1951). During April of 1951, Harrington and the Whirlwind people put on a demonstration in which a training plane was vectored to intercept a slowly flying Beechcraft. (Whirlwind had by then been equipped with its first storage-tube RAM.) This demonstration finally overwhelmed the opposition of the second group of Radiation Laboratory veterans, and much of the credit goes personally to Jay Forrester: each day, after enduring hours of Project Charles, he nursed his balky storage tubes late into the night.

Project Charles did not make any proposals to improve the ADSEC recommendations, but Dr. Edwin H. Land proposed an interesting projector-camera to help in the then-current manual tracking of aircraft. The camera was adopted by Project Charles, and also backed by Rollefson; it was developed by Polaroid with Rollefson's support, and later installed under my direction as the "Quick Fix." It worked quite well, but unfortunately some of the airmen operating it complained that they were made ill by radiation from a peripheral ultraviolet lamp. (This lamp was not a part of the Land camera, but was necessary for the operation of the system of which it was a part.) As is thought to be the case today with complaints about video terminals, these complaints were probably emotionally motivated. They were discouraging just the same.

Because of these complaints, and for other reasons, the Air Staff ultimately decided not to install the Land camera-projector.

What Project Charles did achieve was to drive me into accepting a heavy commitment, and to force us to predict the future capacity and size of the necessary digital computer, at a time when there were simply no data on which to base such predictions. Nevertheless, both Jay Forrester and John von Neumann separately tried, and they reached somewhat similar conclusions.

Here is Forrester's estimate, from the summary of Appendix IV-6 of the final report of Project Charles (Forrester 1951).

A rough analysis is given of the problem of processing, by digital computer, reports on 1000 targets received from 70 radars. As a basis for discussion, the capacity and machine design are given on the basis of the present WHIRLWIND computer at MIT, with changes that might be made in such a design. It appears that, by the addition of auxiliary drums and special operations to facilitate sorting and coordinate conversion, a WHIRLWIND-type computer should be able to handle the problem, including interception calculation, within a 15-second scan time.

This estimate was orders of magnitude different from the reality that developed five years later—the fault only of the turbulent atmosphere of Project Charles, which forced the estimate to be made on the basis of almost no practical experience at all.

The final episode of Project Charles was a dinner given by Nathaniel McL. Sage, the director of MIT's Division of Industrial Cooperation, to celebrate the signing of the contract for the operation of the MIT Lincoln Laboratory. Thirty or forty scientists and military men attended, and Nat Sage encouraged everyone to order the best that Locke-Ober's could serve. Most of us accordingly enjoyed Clams Casino followed by Lobster Winter Place, liberally washed down with beer and martinis. When the waiters began to yawn, Nat invigorated everyone by ordering a bottle of Napoleon brandy; I forget now whether it was 1807 brandy that cost \$177, or 1777 brandy that cost \$180. Everyone got a thimbleful to toast the new enterprise, and we all staggered home.

10. The Lincoln Laboratory

Far and away the most important result of Project Charles was the setting up of the MIT Lincoln Laboratory under the directorship of Wheeler Loomis. At MIT's insistence, Lincoln was not supported by an Air Force contract alone, but was "Tripartite"; in practice the three services contributed to the Lincoln budget in roughly the following proportions: Army, Navy, Air Force: 1, 1, 10.

In this way, the Air Force was allotted most of the services of the new laboratory to work on the ADSEC recommendations, as General Vandenberg's letter had requested. The opposition group of scientists were granted a dominant influence in the laboratory, because most of its directorships went to them, as well as control of the Army and Navy money. Lincoln and MIT told the external world that all the funds were "triservice" and would be spent without regard for their origin, but that tended not to happen.

The Whirlwind computer could now feed on a budget larger than the entire computer budget of ONR, from which it had so recently been removed; its money hunger could now be assuaged. The price of this money was Whirlwind's independence: the MIT Computer Laboratory now became simply "Division 6. Digital Computer" with Forrester as its division head. From time to time there were signs that some of the Whirlwind group thought this a Faustian bargain, but they all worked loyally. Most of the criticisms now fell on me, and I was the Lincoln official primarily responsible for SAGE in the eyes of both MIT and the Air Force. I shielded Forrester and the Whirlwind people from the critical storms, as Forrester had previously done when Whirlwind was an ONR project (Redmond and Smith 1980, pp. 47, 76).

One of the principles to which everyone in Lincoln agreed was that there would be an absolute minimum of red tape. We had experienced red tape in the first days of the Radiation Laboratory and had rebelled against wasting time filling out forms while tyrants overran the earth. In 1940 it had been decided that the amount of property that might be stolen would never pay for the cost of one ship lost because we were a day late, and in consequence we could walk into any Radiation Laboratory stockroom and take whatever we needed. Once a day they refilled the bins. We adopted the open-stockroom system and similar principles for other expenditures in the Lincoln Laboratory. Perhaps this openness can no longer be tolerated, but trusting people certainly makes them work faster than burdening them with red tape.

SAGE was as successful as it was because I firmly adhered to the principle that real tests were to be made at regular intervals, and made sure that simulations and paper studies were quickly followed by the real thing. I had been educated to regard people who did problems only by thinking about them as doomed to the same failure experienced by the classic Greeks in their technical efforts: some of them guessed right,



and some of them guessed wrong, and all that came of their lucubrations was argument. True science requires constant reference to nature, and I, an experimental physicist, enforced that doctrine. I made sure that we flew real bombers and intercepted them with real jet interceptors, on the basis of signals from real radars.

As assistant director of Lincoln Laboratory, I supervised the heads of its Division 6; I was also the head of its "Division 2, Aircraft Control and Warning," which was charged with carrying out the ADSEC recommendations, principally the computerized aircraft control and warning system.

Division 2 contained: a group that studied how to remove radar ground clutter (under S. N. Van Voorhis); a group that developed radar data-transmission devices (under J. V. Harrington); a group that fostered the Land camera-projector and the associated "Quick Fix" project (initially under R. R. Rollefson); and a large group that set up the Cape Cod Air Defense System. The latter's task was both to attack and to defend Cape Cod and the Boston area at approximately weekly intervals. In order to make possible these mock attacks, the Air Force honored my original request for aircraft to be under the control of ADSEC. The 6520th Aircraft Control and Warning Squadron (Experimental) eventually comprised three B29 bombers, six jet interceptors with fast-alert hangars, and a company of airmen to guard the Lincoln gapfiller radars. The considerable influx of Air Force personnel created problems for the local school system and radically changed the nature of Hanscom Field. This National Guard airport in Bedford now became a regular airbase with 7000-foot runways, repair facilities, and all the other trappings including an officers club, a PX, and the branch of a local bank.

The Cape Cod System (see the figure) included not only the Air Defense Command's operational radar at Truro on Cape Cod, but also a network of about 12 smaller gap-filler radars. They were located at distances of 20–100 miles from Boston and could detect all low-flying aircraft in that area (when the groundclutter problem permitted). Data from all these radars were transmitted to Cambridge by telephone lines, and the Whirlwind engineers labored, valiantly and well, to insert the data into their computer. They had to invent many new techniques to do this, and many of their innovations, developed under the spur of necessity, have since become computer standards. Opening a new field is always an exciting experience, and morale remained high.

Dr. Howard W. Boehmer was the leader of the group that installed and operated the Cape Cod radars. His many duties included some not normally expected of young physics professors, such as mollifying congressmen, convincing groups of town fathers that radars in their towns would not endanger or deface them, acquiring real estate, and planning bombing attacks. He also directed a full technical program and interested himself in scientific problems of importance. Howard did all these things in a way that immensely helped the overall effort, and he richly deserved the thanks and appreciation of us all.

Among other members of Division 2 (and of Lincoln Laboratory more generally) whom I remember to have made significant contributions to SAGE were: Paul Rosen, Ernest W. Bivans, F. Robert Naka, Frank A. Rodgers, Jerome Freedman, Jean S. Holden, Herbert Sherman, Ronald G. Enticknap, Joseph A. Vitale, Lou Coonrod, Richard H. Baker, Margaret (Maggie) M. Bateman, Irwin L. Lebow, V. Alex Nedzel, Bernard (B. J.) Driscoll, Paul B. Sebring, Roger S. Walen, Leo C. Wilber, Henry W. Fitzpatrick, Harris Fahnestock, and Paul V. Cusick. Cusick, Fahnestock, and Fitzpatrick served as fiscal officers of different ranks at different times; each was a member of the Lincoln Steering Committee, and each was invaluable to me.

In 1952, Professors Loomis and Rollefson left the laboratory, to my chagrin. At that time I had two substantive duties as a division head: to carry out the ADSEC program, and to foster the installation and testing of the Land camera-projector. In my capacity as a director of the Lincoln Laboratory, my duty was also to restrict the spending of money by Whirlwind.

This last task was given me by the MIT administration, and the charge was repeated approximately every two months. Because I was an alumnus, and had recently received tenure, I tried to obey the charge, but soon discovered that I would have even less luck at tightening the purse strings than had the Navy administrators. After all, they hadn't needed Whirlwind, and so they eventually began to turn it off, as described by Redmond and Smith (1980, pp. 154–156).

But I did need Whirlwind, and any restrictions that I might make in its budget might result in an apparent decrease in its performance. Clearly, considerable finesse would be needed to obey my charge from the MIT administration. If I confronted the problem squarely, by technically auditing the Whirlwind budget, I might have to reorganize Whirlwind.

I rather liked Jay Forrester and Bob Everett, and I had no desire to see them go. That the tumult of a Whirlwind reorganization might be seen by others as an opportunity to reorient the entire Lincoln Laboratory program was also a restraint.¹¹ Therefore I tried to foster a second, competing, computer group. This abortive effort convinced me that a competing computer group would require lots of my time, including time to sit down and study computer design. But that kind of calm thinking was just what was denied to me. It is an ironic thought that had the tensions within the Lincoln management been resolved either way, Forrester would not have had the money to develop the core memory. Whirlwind would either have been junked by those scientists who opposed me and my notions about computers, or else while obeying the MIT administration, I would have driven Forrester out of Lincoln by limiting his budget. (By 1953, people no longer fretted about the Whirlwind budget: it had become insignificant compared to the huge expenditures necessary for the IBM machines, the Cape Cod radars, and other devices that we hadn't even dreamed of when we started.)

Not too long after Lincoln Laboratory started operations, that summer of 1951, it became obvious that the Whirlwind storage tubes weren't going to cut the mustard. Even if a man as skilled as Forrester were to sign on as their amah, they weren't going to yield reliable, round-the-clock service. The RCA Selectron tubes, which at that time comprised the RAM of the JOHNNIAC (Goldstine 1972^{12}), didn't seem to be any more reliable, and some of us suspected they might be less. But that computer was not intended for real-time problems: it didn't have to lie perpetually in wait for Tupolev 104s droning in with their fearsome cargos. It was clear that if we couldn't find a RAM that could function for longer than an hour at a time, the squabbles inside Lincon could be forgotten: Lincoln Laboratory would evaporate, Whirlwind would disappear for good, Jack Harrington could return to his previous civil service job at AFCRC, and I could get back to cosmic rays. This knowledge gave me an Olympian perspective of the Lincoln Laboratory and strengthened my resolve when dealing with the more prestigious of my obstreperous colleagues.

I do not think anyone else would have developed core storage, had Forrester not done it. The people in other organizations who said they were doing it seemed too dilatory. The history of computer memories would probably have been that transistor storage followed directly after storage tubes.

When I first learned that Jay Forrester was seriously considering magnetic-core storage, I inquired among some of the obvious experts. I asked the director of the MIT Insulation Laboratory, a well-known solid-state physicist, about the possibility of making magnetic material with the desired hysteresis characteristics. I mentioned we'd like to have it in a form that would allow us to string wires through it. He looked at me as though I were a particularly stupid freshman, and said that for a hundred thousand dol-

¹¹ In a letter to the president of MIT, dated April 10, 1952, I wrote: "Lincoln at present is guided by a group of men characterized by magnificent determination and an unparalleled capacity for being surprized by the turn of events."

¹² Goldstine's book is written in a straightforward, very informative style. I hope it will serve as one of the models for any history of the Lincoln Laboratory or of SAGE that may be written.

lars he'd grow a ferrite crystal and for another eight thousand he'd drill a hole in it.

Scientists at the Bell Laboratories, where many of the new and superior magnetic materials then in use had been developed, were also working on the problem of computer memories. They seemed deliberately to have shunned magnetic materials in favor of ferroelectrics.

Other experts were also skeptical. Years later, when I had lunch with the director of the Philips laboratory in Eindhoven, he said that when the first commercial IBM machine had arrived with its core memory, the truth of what until then had been discounted as an "American exaggeration" had been confirmed, and Philips scientists then knew they were no longer the foremost experts in ferrite technology.

Magnetic cores were simply not favored by the smart money, but Forrester, not knowing it couldn't be done, went looking for someone to do it, and found him (Redmond and Smith 1980, pp. 182ff). Then he spent millions finding out how to manufacture the cores dependably and reproducibly. The core memories took so much money to develop that it seems unlikely that perfecting them would have been regarded as commercially profitable. I got the money from the Air Force; if I hadn't, this paper would be unnecessary.

The magnetic-core memory is acknowledged to have been a crucial development in the history of the modern computer. Its history is a classic story of luck and pluck: a true epic out of the nineteenth century. The cores developed while I watched, and like a boy reading a Horatio Alger novel, I was inspired. After I'd seen the first satisfactory cores, my attitude toward the Whirlwind people changed. I began to take them seriously and to regard them as worthy of respect.

11. Opposition

"The race is not to the swift nor the battle to the strong."

By the time Lincoln Laboratory had weathered the worst of its internal storms, we discovered that most of its original sponsors had departed Washington. Almost all of the early enthusiasts had either left government service or been transferred to other posts. The change from Democratic to Republican administration also caused changes in the ranks of those who gave ultimate approval to our budgets. We were not without friends, but they were far from being as influential as in the early days. This weakening of support had the effect of casting us in the role of just another defense contractor; we now had to compete. Besides deploring the loss of friendly generals and government scientists, I missed the steady hands of William A. M. Burden, who had been special assistant for research and development to the Secretary of the Air Force (see Burden 1984), and of Thomas K. Finletter, the Secretary of the Air Force. Both gentlemen were urbane, Olympian, unflappable, and coolly rational. They saw through nonsense without making too much of it. They were followed into office by Trevor Gardner, Roger Lewis, and Harold Talbott (see Beard 1976¹³).

To make our situation even less comfortable, the computer air-defense system, or the "Lincoln Lab System," or "that crazy system that they try to tell you will track hundreds of targets simultaneously" (as one Air Force officer misidentified it), had become identified as one of a large number of controversial enterprises of the new "high technology."

To set the stage for what follows, here is a partial list of 1950 development projects whose success the majority of scientists and engineers doubted: supersonic aircraft, short-range guided missiles, intercontinental ballistic missiles, artificial earth satellites, reliable high-speed digital computers, nuclear fusion bombs, nuclear fusion power, helicopter cranes, satellites for any useful purpose, VTOL aircraft of high disk loading, superhigh-velocity guns, materials of extremely high strength-to-weight ratio, inertial guidance, radiation weapons.

Many of the scientists and engineers who worked on a particular project doubted that the others would succeed, but in fact only a few failed. Needless to say, I, and most of those working on air-defense problems, didn't believe that intercontinental ballistic missiles would ever be practical. "Imagine," we'd say to one another, "a skyrocket as tall as a 15-story building!" and we'd laugh. ("Imagine a black box with ten thousand tubes in it controlling airplanes!" they probably said to one another, laughing.)

But every item on the list had its dedicated supporters, and over the country you could find from one to a dozen groups hard at work on each. The collectivity of all those groups comprised the majority of scientists and engineers that doubted. Each group tended to doubt the others. This widespread web of doubt caused unease among those administrators who were asked to approve one or several of these projects, but who could understand none of them. (This unease particularly affected executive attitudes toward SAGE,

¹³ Beard's book describes some of the men who came to be Air Force secretaries during the Eisenhower administration. I hope it will join Goldstine's book to serve as a model for histories written about the Lincoln Laboratory and SAGE.

Massachusetts Institute of Technology **Project Lincoln** LEXINGTON, MASSACHUSETTS **IDENTIFICATION CARD** BADGE NO

Valley's badge (number 4) at the newly formed "Project Lincoln" (later renamed Lincoln Laboratory).

which was introducing a radically new technique against the advice of most electronics experts, who felt comfortable only with analog computers.)

The result was that each group tended to be perpetually on the defensive. How defensive depended on how many people opposed the particular project because they thought they knew something about it, or because they felt their interests would be threatened if it were successful.

For example, few people understood helicopters, and no maker of mobile cranes believed that even if they could be made to work, helicopter cranes would ever be much of a threat to his business. Thus there was comparatively little opposition to that project. But every scientist and engineer knew something about computation, and a large number of analog computer experts felt threatened by digital computers.

World War II, with its emphasis on automatic pilots and remotely controlled cannon, fostered the analog computer-servo engineering profession. The art is firmly based on research about feedback amplifiers and servomechanisms done at MIT (Bush and Caldwell 1945)¹⁴ and at BTL during the 1930s (Nyquist 1932; Black 1934); its triumphant glory was revealed by H. W. Bode's historic treatise (1945) on feedback amplifiers (written during the war at BTL and studied in manuscript by workers in the MIT Radiation Laboratory). Many analog computer engineers were around following the war, but so great was the newly realized demand for control devices that the colleges began training increasing numbers. The rise and fall of this profession is a poignant story—of expectations that came true for other people, of ruined careers, of competent engineers pushed down to technician level.

It was natural for the times that the feedback principle was firmly attached in most peoples' minds to analog apparatus; only a relatively few servo engineers were able to make the transition to digital machines.

The feedback principle, with its power to transform shifty vacuum-tube circuits into stable instruments, and its applications to remote control, was correctly viewed as the tool of the future. Projects for robots, involving their use for what is currently called "flexible manufacturing," were freely predicted and planned. In 1945, as the war ended, we confidently expected that factories would have become softly humming hives of selsyn motors, amplidyne generators, and analog computers by the year 1960.

Even before the war ended, feedback theory was applied to broader social questions. On more than one evening, while taking a nightcap with other MIT people in the club car of the Federal Express to Washington, I listened to George Philbrick read from his manuscript. Sometimes he explained congressional action by feedback theory; other times he described the responses of the electorate as a feedback signal, or showed us how the concept of the free market could be rationalized by feedback theory. Clubs, factories, corner stores—all fitted his model. (After the war Philbrick founded a highly successful company that made top-quality components for analog computers. I don't know whether his book was ever published.¹⁵)

An analog computer was an assemblage of servos driving nonlinear potentiometers, or a flock of operational amplifiers with "integrating" or "differentiating" circuits in their feedback loops, interconnected ("programmed") to simulate the system under study. It would typically contain about as many slope and zero-set adjustments as it did feedback loops. Unlike the screwdriver adjustments on the back of a television, which require resetting only once or twice a year, the adjustments of an analog computer had to be touched up every day, or whenever the problem was changed. Analog engineers became skilled at this task and tended to cherish their skill. (Although problems remain today that can best be attacked with analog computers, the main reason they were superseded even in simple real-time systems was that digital computers required no screwdriver controls.)

¹⁴ Bush and Caldwell's 1945 paper reviews earlier work on analog computation as it was done at MIT. Some of the cited papers also describe servomechanisms. See H. L. Hazen, J. J. Jaeger, and G. S. Brown, "An Automatic Curve Follower," *Review of Scientific Instruments* 7 (September 1936), 353–357.

¹⁵ Editor's Note: In 1955 George A. Philbrick Researches, Inc., published A Palimpsest on the Electronic Analog Art; see P. A. Holst, "George A. Philbrick and Polyphemus," Annals, Vol. 4, No. 2, April 1982, pp. 143–156.

Thus analog machines and their design took on quasi-religious attributes: a firmly based doctrine tended to become an established dogma, and skilled professionals tended to act like proficient acolytes. We in Lincoln were cast, by some, in the role of heretics to a state religion, and when we criticized analog devices and refused to employ them, we were regarded as unrepentant sinners. It was as though the Christian martyrs had refused to go into the arena because the lions were mangy. People became enraged by wellintended actions that we thought innocent.

During 1952 and the early part of 1953, the Lincoln computer system was attacked by many and defended by only a few. Most of the nontechnical defenders simply trusted Lincoln and MIT people on the basis of personality judgments; other defenders were longsighted executives of big electronics firms.

The longlines engineers of the Bell System suddenly found a group of strangers from Lincoln Laboratory making the kinds of measurements on their system that they were accustomed to making in private, and these strangers were finding numbers of little closets, each with its tiny skeleton. Most of the defects discovered by Lincoln people were harmless as long as you only wanted to talk over the lines, or send a slow code, such as teletype. But Lincoln was intent on sending at as high a rate as the lines would allow, and finding out that the highest rate wasn't as high as expected. The Bell engineers rarely showed any irritation to us, but we didn't make them happy. We ought to have kept our mouths shut more tightly when we found what seemed to be a laughable fault. I should have seen to that, not only to make my own job easier, but out of respect for the people who had laboriously built the world's finest telephone system.

The real problem for the longlines people was that they were faced with a new market, for which they had to prepare, and in so doing reorient themselves to the new world of the digital computer. SAGE served the Bell System well, for it forced the Bell people to prepare before they were inundated by customer demands. Even so, years passed before 1200-baud transmission was generally sold, and the SAGE system worked a little faster than that. I suppose I served, in the plans of the AT&T management, as an outsider who demanded a revolution they had seen coming. I was for them what is now called by some management experts a "change agent."

Scientists and engineers in smaller companies, whose jobs depended on analog servos and computers, opposed us tenaciously and ingeniously. My basic principle was that I was trying to defend the United States, and that people should take their selfish interests elsewhere, but the notion in the heads of some lovers of analog computers was that I was a dangerous visionary. Many engineers of the aerospace industry, more friendly toward us, sincerely didn't believe digital computers could be reliable enough to be trusted with control of an important system; they felt that as long as we stuck to bookkeeping and computing logarithm tables we were all right. Some analogers allowed that the SAGE computer could probably keep a list of airplanes satisfactorily, but that when it came to putting a missile on target, or actually directing the flight of a missile, the choice of a digital computer would be a grave mistake.

In 1952 the Air Force requested Harrington to test an analog device for compressing radar data to send over phone lines. It comprised a cathode-ray tube (with a long-persistence phosphor) and a moving photocell; like all analog devices it proved to be difficult to keep in adjustment.

FAA engineers and their contractors who had an interest in the control of civil air traffic could see as well as anyone in Lincoln that a dynamic map of the airspace, once established by SAGE, would show the civil as well as the military aircraft. If SAGE could make interceptions, obviously you could use the same machinery, with different programming, to avoid collisions. FAA personnel felt threatened by this possibility. Of course, the civil system eventually did come to use digital computers, and just recently something called the ADGE system, which combines the data from both the military and the FAA radars, has been deployed. Possibly by the third millenium the complete cooperation I once hoped for will have been achieved.

The analog groups who were contractors for missilecontrol systems gave us the most serious trouble. One of them came close to stopping the SAGE system before it was even named. We also tended to believe that some of these groups were responsible for the seemingly endless parades of investigators who passed though our halls.

We were investigated about every six months by the U.S. Senate, or by the Bureau of the Budget, or by a committee for the Secretary of Defense, or by somebody else. Those investigators who came from high places in the government were uniformly intelligent and well informed, but it often seemed that every ignoramus who had a secret clearance and time on his hands came to investigate Lincoln. People spent days with us, mostly to discover what we were doing with all that money, and why we didn't have more red tape inside the laboratory.

Of the competent investigations, the earliest was that led by Dr. M. J. Kelly at the behest of the

Secretary of Defense. Testimony before the Kelly Committee revealed that the Lincoln system was far ahead of its competitors as far as hardware and field testing were concerned, and the Kelly Committee's report, issued in the spring of 1953, was favorable to the Lincoln Transition System, as it had been dubbed by then (Division 2 1953). This report kept for us the already-won confidence of many civilian executives and gained us the confidence of a few more Air Force officers. It also helped us by quenching a few fires inside Lincoln, but our main problems remained.

Here I must also admit that there may have been reason for some of the less-competent investigations. Recall that in 1951, Forrester had estimated that a Whirlwind-like computer, with some additional features, could accept data from 70 radars and could track 1000 planes (Forrester 1951). At less than annual intervals thereafter, Division 6 found it necessary to revise the estimated cost and size of the computer upward. These revisions were sometimes accompanied by a significant decrease in the estimated number of radars or planes that could be handled. I kept a record of some successive numbers of estimated vacuum tubes, and when these are plotted on semilog paper, they reveal a tube-count doubling time of 18 months. Starting with 8000 tubes in mid-1951, the estimated tube count was 60.000 by January 1955. Such increases were no doubt responsible for some of the investigations. The propensity of the Whirlwind group to make precise underestimations of its job had already been manifest before it was assigned the air-defense task (see Redmond and Smith 1980, pp. 22, 43, 57, 63, 74, 80, 92, 93, 108, 118, 146, etc.). When I had first surveyed Whirlwind, its actual cost had not disconcerted me. If air defense had been initially estimated to require 100,000 tubes, that too would have been accepted by me, and by the Air Force. But a doubling time of 18 months made people uneasy, fretful, and dubious. After a while I grew inured to these unpleasant surprises, although they tended to lower morale in the rest of the Lincoln Laboratory. (The radar datatransmission equipment also grew greatly in size and cost, but because it was much smaller and there had been no estimates, there was little criticism of its growth.)

The managers of the U.S. Army's NIKE project, an antiaircraft missile for defense against high-flying bombers, were all veteran Bell System engineers, and some had experience in the longlines department. They felt that their missile should not become another planet in the SAGE universe, and so they found it hard to cooperate when we suggested that the same machine that directed interceptors ought also to put antiaircraft weapons on target. That was obviously the easiest way to avoid shooting down our own planes, but they found it hard to agree. Army officers immediately interpreted the proposal as another Air Force grab. Although the NIKE computers were analog, the NIKE engineers felt that their experience as telephone engineers gave them the expertise to criticize anything that used the phone lines. So they periodically reinvented everything for us. Because they were smart fellows, we sometimes got a good idea from them, but often we would spend a day explaining that a digital computer really wasn't the same as a cross-bar telephone exchange, even if it was as big as one and you could analyze it with Boolean algebra. Although we sometimes showed our irritation, and Dr. Kelly and I more than once shouted across a table, I believe we respected each other's competence and goodwill. The Bell System people were good technologists who believed in what they said, and they could be convinced by reason.

Following his committee's acceptance of SAGE, Dr. Kelly began to notice me at cocktail parties. "Here is Valley," he would announce; "Valley has come up through nuclear." Then, gently nudging my elbow, he would say, "Valley, shake hands with the president of Galactic."

12. Controversy

"Neither bread to the wise nor yet riches to men of understanding nor yet favor to men of skill." Eccles. 9:11

Military officers are hired to win fights. Although today we have officers educated in computer science, in the early 1950s the average field-grade officer had served in the war and been educated by it, and he was familiar only with analog instrumentation. If he had taken any technical refresher courses since the war, they had most likely been about nuclear explosives. If you said the word *computer* to most officers, you implied an analog computer with its characteristic limitations, even though you might actually have spoken the words *digital computer*.

Real-time systems based on analog instrumentation have serious limitations from which digital ones are free. Whereas it is easy in a digital system to have several subroutines among which to switch with no loss of time or precision, such flexibility is difficult to attain with analog apparatus. The number of displays possible for a digital system has broad limits, but is constrained in an analog system. Many sources of input data can be used in a digital system, but only a few in an analog one. (For these and other reasons, analog computers, when used today, are often coupled with digital computers in hybrid systems.)

So when we were asked by an officer, "How many operator positions does the *Lincoln system* require?" and we replied, "We haven't determined just how many the *air-defense problem* requires; what is your estimate?" we were simply not communicating with him. The officer was likely to conclude that we didn't know very much about computers.

Moreover, an analog computer is a simulator, and if you use it to simulate a problem successfully—say, a bombsight design problem—all you have to do is to make a similar assemblage, but smaller and lighter, and you have designed a bombsight.

At Lincoln we reversed all that; we said that it was important to fly real airplanes, and to use real radars and real phone lines. But instead of praising us for our realistic approach to the problem, some visiting officers tended to think we were just using a particularly expensive and clumsy way to simulate, and for them this was a mark against us, as compared with our chief competitors at another university.

I didn't fully grasp these attributes of the military until sometime in 1953. My ignorance was encouraged because the Air Force had initially assigned several of its most intelligent and best-educated officers to Lincoln. They knew about the different kinds of computers and understood what we were doing. If it hadn't been for men like Colonels J. Day Lee, Mike Ingalito, E. F. (Ed) Carey, Jr., John L. Lombardo, and Peter Schenk we'd have gone down the tubes. Those officers proselytized for our cause. Because they had outstanding combat records they were listened to, but they didn't have the opportunity to convince all the other officers in Air Defense Command. (Years later, when I served as chief scientist and spent all my time with officers, a lasting solution to the problem of meshing the mental habits of the military and computer people seemed to be the creation of a new organization. The late Major General James McCormack, Jr., USAF (Ret.), then a vice-president of MIT, named this organization the Mitre Corporation. Hap Halligan, who had shown superior ability to cooperate with workinglevel military officers on a day-to-day basis, was made its president.)

A person unfamiliar with the military might think that once we had the office of the Secretary of Defense behind us, we needn't worry about what all the majors and colonels and generals thought, but persons experienced with the Department of Defense will not think that way, for they will recognize that new weapons cannot easily be shoved down the military's throat. Those officers who are competent to walk a project through the Pentagon are also the ones smart enough to know a gone goose when they smell one. If a project isn't pushed by a competent operational type, it will not necessarily fail, but it will flounder, experience errors of procedure, and suffer delays; the weapon will be unlikely to see service use. Therefore a real problem remained for us after the Kelly Committee report had been issued and accepted.

What we ought to have been doing was, first, see that Air Force officers were widely indoctrinated with elementary notions of the power and flexibility of digital systems. Second, we ought to have found out what kinds of things officers normally have on their minds, instead of treating them all like General Jimmy Doolittle, who has a doctor's degree in aerodynamics. What they were mostly thinking about was how to demonstrate that they could command in an exemplary manner: handle people and bend them to their will. Officers would judge electronic devices as tools that might aid them in their principal purpose: to win fights. A cynical observer said that we needed to hold the officers' hands and convince them that we cared. We needed a sales department. Thus, while there was a cadre of highly intelligent majors and colonels who advised and supported us, there was a greater number of officers who were suspicious of digital computation, however it might be applied. The latter group of officers was fair game for any laboratory that might desire to compete with Lincoln.

The staff of a competing university laboratory, sponsored by the Rome Air Development Center (itself at loggerheads with the Air Force Cambridge Research Center), performed their social duties correctly. The staff of that laboratory paid weekly visits to Colorado Springs, and there they and the concerned officers "designed" the future air-defense system. This "designing" consisted of long discussions about such topics as how many sergeants would be required to operate the new system, the location of the general's command post, and the human engineering of displays and devices for entering data into the system. Little attention was paid to the development of new hardware; as far as we could determine, their technical studies were confined to trying different variations on a large analog simulator.

As a result there was a group of operational officers who found the competing system comprehensible and its proponents friendly and understanding. Meanwhile at Lincoln we were flying real bombers and intercepting them with real fighters, improving our radars and our computer, inventing core memory, revising longdistance telephony, and writing the world's largest computer program for its time. When some officer would ask us where the general was going to sit, we were likely to tell him more news about electronics. Unwittingly, we were implying that all the things he knew how to talk about, and wanted to talk about, were trivial and beneath our notice.

We easily convinced people who knew about the potential advantages of digital apparatus that once we had solved the technical problems, the operational problems on which the other laboratory was concentrating would practically solve themselves. M. J. Kelly, Thomas J. Watson, Jr. of IBM, Robert C. Sprague, president of Sprague Electric, and other powerful executives of the industry supported us. The generals at the very top of the Air Force, who respected those men, also respected and supported us. But you can't win a battle with generals alone.

One day several of the friendly Air Force officers came into my office and said that they had been instructed to wise me up to the facts of life—which, for several hours, they did. After that lesson, I coaxed some of my associates into fleshing out the details of a rough draft of what became known as the Transition System Report, TM20.¹⁶ This report from Lincoln showed a hypothetical Air Defense Direction Center with its operations rooms and other facilities. It was but one of a variety of layouts for the air-defense mission that could be based on a digital computer. TM20 took the heat off for a while.

Following, not necessarily in calendar order, are some of the notable events that resulted from these very different policies of the two laboratories.

1. A general summoned the IBM management to the Pentagon, and ordered them to cease cooperating with Lincoln and to start helping the other laboratory. He said it wasn't fair that the group that needed help the most didn't get any from IBM, while the group that needed help the least got it all. This concept apparently didn't agree with the predilections of the IBM managers. They stood up and said "NO!" Fortunately that general didn't outrank everyone in the Pentagon.

2. IBM stationed two sales executives at Lincoln for several weeks. One of these men mixed with our staff, but the other sat mainly in a wooden chair, which he moved around from day to day. We would find him sitting silently at the end of a hall, or opposite someone's office, or staring at the arithmetic unit of the computer, or by the front door—or someplace else. After they'd gotten to know as much about us, and we about them, as was needed to be frank with one another, they called Forrester and me together. Their parting words were: "George and Jay, in our business we've discovered that it is necessary to give the customer a little of what he thinks he wants, in order to maintain oneself in a position to give him what he really needs." I got the point, but the flow of events was already strongly away from us.

3. Following the report of the Kelly Committee, considerable pressure must have been exerted on those generals in Air Materiel Command and in Air Defense Command for whom the majors and colonels supporting the competing system worked. A round-robin tour of Lincoln Lab and of the other laboratory was laid on, and several generals, their aides, and a few of the majors and colonels in support of each system made the trip.

At Lincoln we first showed them the Quick Fix, and the scientists and engineers involved with it gave them all kinds of operational details, thus showing that there were actually people in residence at Lincoln who did know what life was all about.

The following day we showed them live interceptions as run by Whirlwind. These could be followed on the face of a cathode-ray tube, and we could listen to the pilots' radios as they completed the interceptions. We also showed them through the Whirlwind computer, which by now occupied much of two floors of the Barta Building.

Then we all traveled to the other laboratory, where we witnessed several "interceptions" made on a huge pen-and-ink plotter equipped with two pens: the "bomber" and the "interceptor." The "interceptor" was stated to be the Air Force's pilotless interceptor then under development. (This project was to be an airborne torpedo designed to fly, under guidance of the AC&W (Aircraft Control and Warning) system, to the neighborhood of an enemy bomber, there to home in on it.) Lots of operational details were explained to the officers. The demonstration was run by a large analog simulator. I was astounded to see that this trivial exercise in preprogrammed curve plotting had impressed the majority of the officers as much as had the real thing shown them at Lincoln.

The performance culminated in an afternoon session at which the Air Force's decision was stated: the Lincoln Lab's Quick Fix would no longer be supported, but the Lincoln Transition System and the competing system would both go on as before.

A major explained to me that both systems had big, expensive computers, but the competing system also had the advantage of being able to direct the pilotless

¹⁶ A Lincoln Laboratory quarterly report (Division 2 1953) says: "With the Cooperation of Division 6, the concept of the Transition System has been written down and published as Technical Memorandum No. 20. Plans for implementing the system are well under way."







One of Valley's duties was to be photographed at the experimental SAGE consoles. In the first picture the photographers adjust their lights. Then a discussion is posed. Finally, he pretends to use a light gun.

interceptor. Of course, we offered to direct these missiles as soon as there were any to direct, but— "Lincoln folk tend to think that their computer can do everything." Besides, I was informed, the generals thought the competition's plotting board was superior to Lincoln's cathode-ray tube and that we should adopt it. I was able to keep my temper and not ask how the hell we were supposed to show the tracks of hundreds of aircraft on a pen-and-ink plotter without so messing up the paper that you couldn't see anything.

This defeat riled me for weeks, until I chanced to pick up my childhood copy of Mark Twain's A Connecticut Yankee In King Arthur's Court, and Lo, there was the Air Force! Sir Galahad, and Sir Kay, and Clarence, and of course Arthur himself. Since then, whenever I have come in contact with military officers, I have typecast them for places at King Arthur's round table.

Following this episode, I noticed some hesitancy in furnishing us with the special services that we'd become accustomed to, and some of the good officers began to talk about being transferred to other posts. Those were ominous signs.

4. In the fall of 1953 the RAF invited most of the same generals, the director of the other laboratory, and Dr. Albert Gordon Hill and myself from Lincoln Laboratory to the south of England for an air-defense conference.

One afternoon of the conference was devoted to aircraft control and warning systems. We Americans all hung on the Britishers' words, remembering how their radars had directed the outnumbered RAF to victory in the Battle of Britain. Alas, they had no new ideas of consequence.

Then the Americans were invited to speak. The director of the competing laboratory spoke first. He was obviously awed by all the brass, especially by the British superbrass, those sirs and milords who were also professors, air vice-marshals, and so on. He had not learned, as I had learned from General Whitehead, that in such a situation you stuck a cigar in your face, blew smoke at the intimidating crowd, and overawed the bastards. During the ensuing discussion I realized that he had only the vaguest ideas about physics, electronics, aerodynamics-apparently anything technical. I became increasingly concerned that our side would be shown up for a bunch of scientific illiterates before the British scientists, who had begun to whisper and giggle among themselves. I began to lose my temper.

"Shouldn't it be the other way around?" I asked in a strained voice following one of his blunders. He looked at me uncertainly, and I got up and corrected the blackboard for him. "Shouldn't it be this way?" I asked, and sat down again. If he'd accepted this correction, all would still have been well, but he began a line of patter intended to impress the officers: a series of disjointed sentences that had no technical meaning whatever.

I blew my top. "SHUT UP! SIT DOWN!" I thundered, and then sat down myself, abashed by the sound of my own voice. To my astonishment, he did sit down, whereupon the British scientists fell to having a loud argument among themselves—out of politeness I suppose.

Eventually I was invited to speak. Fortunately, I had brought two cigars.

When we had all talked the subject to death, and were walking down a long corridor, I chanced to overhear the commander of the Air Defense Command say to the commander of the Air Research and Development Command, "I'm sure glad you and I picked the right system."

From that time on, the Lincoln system was truly accepted, but if anyone thinks that SAGE was accepted because of its excellence alone, that person is a potential customer for the Brooklyn Bridge. It was accepted because I shouted an impolite order at the leader of the competition, and he obeyed me. We were at the court of King Arthur, and I had prevailed.

Several months later General Earle E. (Pat) Partridge first addressed me as "Dr. George," the GS15 who'd ushered me into his office having announced, "General Pat, Dr. George and I would like to brief you on—." Thereafter I was on a first-name basis with one-, two-, and three-star generals, and was privileged to address four-star generals as "General Nickname." The delicately antebellum coloring of this locution charmed me for years.

13. How SAGE Got its Name

Although the Lincoln Transition System was now accepted by the military and civilian staffs of the Air Force, in 1954 it had yet to be promoted to the status of a full-fledged production enterprise, which was deemed necessary before all the preparations for installation could be started. Although I was satisfied with the name our system already had, a name more descriptive of its function was thought to be necessary. At that time, everyone accepted as axiomatic that this new name should include the words *air defense*. The problem then arose that all the acronyms containing the initial letters of these two words spelled pejorative adjectives: BAD, MAD, SAD, and so on. A few unrepentant officers, still against Lincoln, approached this problem with gusto. Their favorite name was the Semiautomatic Air Defense System, which they officially called SADS, but privately referred to as the "Sad System." I thought all this too petty to bother about, until a staff officer told me that no general would go to the Capitol and brief Congress on spending several billion dollars for something called SAD. This problem was solved by a wise young officer on General Putt's staff.

One afternoon John D. W. Churchill, my office manager, poked his head in my door and said with a big grin on his face, "Johnny Lombardo has them all in the small conference room; they're finalizing the name."

"What's it going to be?" I asked. John didn't know; we agreed that he'd listen in and report back to me. I asked him if he knew why all these officers needed to make the decision while seated in the Lincoln Laboratory. He replied that they thought the atmosphere would be conducive to creative thinking, which John interpreted to mean that some of them hoped I would come in and shout the way I had shouted in England. I didn't do that.

Toward day's end, a friendly officer on the air staff came in and asked permission to sit down. I nodded, and he opened his envelope and read: "The name of the system now under development at the Lincoln Laboratory is hereby declared to be the Semi-Automatic Ground Environment." He went to the blackboard and wrote "S.A.G.E."; then he sat down and looked at me, his eyes triumphant.

"Where did THAT come from?" I exclaimed.

He explained how one of the SAD advocates had been doodling all afternoon while the others argued, and had gradually worked himself into such a mood of frustration that he drew a cartoon of a figure being hanged. Then he slowly penciled beneath it, "G E VALLEY"; he seemed to like this so much that he then drew a figure whose head was being cut off by a gigantic sword, and under it he also lettered, "G E VALLEY"; he was starting on a third figure when the officer telling me this noticed him, and was fascinated. "Without thinking very much, I mentally added your name to what they had been pushing all day, and then all the extra letters dropped away and I had SAGE. Then I fitted words to the G and the E to fill out your initials, and tossed them Semi-Automatic Ground Environment. They took it."

"I don't think we need to tell that story around," I said.

"But I'll have to tell the General," he protested. "Let's stop it right here," I insisted.

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