

MANUSCRIPT EDITION

PROJECT

# WHIRLWIND

CASE HISTORY

KENT C. REDMOND  
THOMAS M. SMITH

**PROJECT WHIRLWIND**

**A Case History in**

**Contemporary Technology**

**Kent C. Redmond and Thomas M. Smith**

**Reproduced by**

**The MITRE Corporation**

**Bedford, MA**

**November 1975**



## **FOREWORD**

In the beginning, MIT begat Whirlwind. Whirlwind begat SAGE; SAGE begat Lincoln Laboratory; Lincoln Laboratory begat MITRE. Lest our lineage be forgot, we publish the Whirlwind History.

The Whirlwind History was written in 1967 by Kent Redmond and Tom Smith on a grant from The MITRE Corporation. It was intended for publication by the Smithsonian Institution as part of a series on the history of computer development, but when the idea of the series was dropped by the Smithsonian, the manuscript lay fallow for a number of years. Frequent requests for copies were honored by photocopy of photocopy, with the result that legibility was poor and there were delays in production.

We managed to locate an original copy and have reproduced a few copies in the interest of preserving this well done piece of the computer story for future scholars and historians.

Robert R. Everett  
President, The MITRE Corporation



## TABLE OF CONTENTS

<u>Chapter</u>		<u>Page</u>
1	The Beginning	1.1
2	Computing Problems Emerge	2.1
3	The Shift to Digital	3.1
4	Preliminary Design Efforts	4.1
5	Pressure from ONR	5.1
6	Problems of Federal Assistance	6.1
7	Breaking New Trails	7.1
8	R&D Policies and Practices	8.1
9	The Collision Course of ONR and Whirlwind	9.1
10	ADSEC and Whirlwind	10.1
11	Internal Storage Problems	11.1
12	Magnetic Cores and R&D Progress	12.1
13	In Retrospect	13.1



## Chapter One

### THE BEGINNING

Since the Second World War there has been growing recognition in the United States of the practical value of science for the defense and welfare of the American people. Along with this appreciation of the usefulness of applied science there has been also a growing apprehensiveness at the rapid and profound changes being wrought in our society by the multibillion-dollar scientific technology we have created. Government, industry, and institutions of higher education have pooled their dollar and manpower resources with great ingenuity to provide awesome weapons of war and magnificent production, transportation, and communication facilities, and it has all happened so fast that neither the experts nor the common citizens are always sure what we are doing, where we are going, or which direction we should be heading as a society when putting our scientific technology to work for us.

The immediate impact of our scientific and technological achievements has not been hard to imagine and anticipate, although sometimes the force of the impact and the rate and scale of the direct consequences have surprised us.

Of greater import and harder to foresee have been the second-order and third-order effects, the tumbling-domino consequences, and the cumulative alterations that threaten to move us into patterns of living we do not like, do not understand, and do not want.

This historical study of one research and development project takes a look at an example of what we are doing and where we are going on a small scale, and to these ends it examines a project of our scientific technology, Project Whirlwind, in some detail in order to cast some light--again on a small scale--in one direction we are heading, that of the information revolution via the research and development road. Whether this particular project more fittingly provides information and examples of how the business of modern scientific technology ought to be conducted, or whether it offers instructive, cautionary lessons in what should not be done is for the reader to decide, although the authors offer their own judgments and conclusions.

This story is written not for the technical specialist or the management specialist or the funding specialist but for the thoughtful layman who believes, as do the authors, that if we gain a clearer understanding of what we are doing and what we can do, this will help us decide where we want to go and how to get there, in those affairs that require the use of scientific technology as our obedient servant.

The men in this story were engineers. Their aim in 1944 was to design and build an aircraft simulator, but their achievement by 1953 was as different as it was unforeseen. Instead, in the course of a decade of pioneering electronic and radar experience, they had acquired a thoroughgoing mastery of the basic concepts of integrated system design; they had built "Whirlwind," a high-speed, prototypal, digital computer that became uniquely appropriate for a brief, mid-century strategic mission in the defense of the nation; and they had created a small group of experts who by their contributions then and later were to seed American computer technology with a know-how that, in the reckoning of some observers, transformed the computer overnight from a limited instrument intended primarily for mathematical and scientific computation to a device of wide and practical social potentiality.

In accomplishing this preliminary transformation, these men vaulted the technical computer state of the art a decade ahead of where it otherwise would have been, according to this view. Their contributions immensely strengthened the sinews of the emerging computer technology for the tasks that lay ahead. While it was true that their actions unexpectedly accelerated the onset of the unanticipated information revolution for which this century appears likely to be remembered by later generations,

nevertheless the net gains in ability to identify, define, and offer solutions to social problems could be expected to more than compensate for the stresses and uncertainties which that revolution and the general onrush of human affairs would impose.

On the debit side, in the view of others, the men of Project Whirlwind extravagantly spent some five million dollars of public money in five short years. They pursued impetuous, risky, and unrealistic research and development practices in peace time, practices of the sort that prudently can be sustained by a nation only on a short-term basis, in time of war and extreme crisis. Their project was able to flourish in special and unique circumstances, like an experimental hothouse plant in forced growth, and such favorable circumstances are quite unlike the conditions that normally prevail in the conduct of research and development affairs.

Consequently, this project was not typical, not representative, and not an exemplar to be followed. It was at best a lesson in fortunate improvisation, and it offered a clear warning to all how runaway tendencies can dominate the enthusiastic pursuit of research and development when business-as-usual restraints are absent. According to this view, Project Whirlwind provided not a lesson in how the efficient and expeditious conduct of research and development might be achieved as a new norm, but a

demonstration, by its obvious malpractices, of the essential wisdom of traditional procedures.

It succeeded rather than failed, according to this argument, because of unusual and unexpected circumstances beyond its control. The project had become an engineering-development project without a practical mission until these circumstances, involving a potential shift in the very balance of international power in world affairs, had intervened. Not only was the project not a business-as-usual enterprise, but it took nothing less than a looming national military and political crisis to come to its rescue. Had Project Whirlwind been conceived in the beginning or shortly thereafter been modified in anticipation of this crisis, then its importance, its priority rank, and its conduct of its own affairs would have developed naturally. Instead, one could argue, it had been fiscally hell-bent to develop a fantastic machine for which virtually no one except its enthusiastic builders could see any use.

While pure scientists might be excused for spending modest sums for their traditionally impractical investigations of the unknown, even though the ultimate practical payoff was not visible, engineers setting up expensive development and experimental-prototype projects must not be allowed to proceed without an explicit, agreed-upon, practical goal in view. Pure science could afford to leap-frog ahead into the unknown, because if some of its

enterprises did fall on their faces, the national loss in dollars and manhours would be minimal and tolerable. Not so, where multimillion-dollar engineering projects such as Whirlwind were concerned, and Whirlwind had just missed, by perhaps a hair's breadth, falling on its face. Even its magnificent, internal, magnetic-core storage had emerged as a desperate, risky, ad hoc engineering solution to the nagging problems of unreliable electrostatic-tube storage. Project Whirlwind, when all was said and done, had been lucky.

Or had it?

It is possible to take a position of praise or a position of censure or one that is a mixture of the two. But whatever the convictions of the observer and whatever the verdict, this research and development project offers a significant history for the thoughtful observer of the research and development process. The genesis and development of the project were characterized by a mixture of elements that were traditional and elements that were novel. Thus, the men did not work in the relatively independent entrepreneurial isolation that characterized, for example, the creative efforts of the Wright brothers. Instead, they worked under an institutional aegis. Nominally the aegis was that of higher education, for it was provided by the Massachusetts Institute of Technology. But actually it was a peculiar combination of educational and governmental,

tinged with industrial, and it was made possible only by the unprecedented exigencies and modes of activity created by the Second World War.

While MIT furnished a physical plant and the technically trained intellectual resources, the United States Navy during the earlier period and the United States Air Force during the later period furnished the necessary funds. National-defense tax dollars thus entirely underwrote the cost of this enterprise in twentieth-century scientific research and engineering development.

That this should be the case was a natural consequence of the historical circumstances that had impelled the United States into the war. Well before 1944, prosecution of the war against Nazi Germany and Japan had brought numerous technical problems and their solutions to the attention of American engineers and scientists. To an extent previously unknown, American engineers and scientists were providing essential technical leadership while cooperating with the national military establishment and American industry. Through such agencies as the National Defense Research Committee and the Office of Scientific Research and Development, some 30,000 engineers and scientists became, in the words of OSRD Director Vannevar Bush, "full and responsible partners for the first time in the conduct of war." Approximately

one-half billion dollars was expended by these agencies in the search for new weapons and new medicines.<sup>1</sup>

The Japanese attack upon Pearl Harbor in December, 1941, caught both the armed forces and industry unprepared for the vital responsibilities thrust so abruptly upon them. Presidential efforts to make of the United States the "arsenal of democracy," given substance in March, 1941 by the enactment of Lend-Lease legislation, had, it is true, encouraged the efforts of American industry to increase substantially the production of arms and other military equipment for the beleaguered forces of the United Kingdom, but such efforts were a trifle compared to the needs created by the precipitation of the United States into the conflict. American entry demanded the immediate and total conversion of the national industrial, technical, and scientific complex to the development and manufacture of the weapons and ancillary equipment which were to become the instruments of victory. Since the nation's colleges and universities were the repositories of much of its scientific and engineering talent, the Government turned to them to obtain "large-scale assistance . . . mainly for military applications of nuclear energy, communications, control systems, and improvements in propulsion." These institutions responded to the call, adapting themselves to meet the vital challenge and rendering such aid and leadership as they could.<sup>2</sup>

Among these institutions was the Massachusetts Institute of Technology, recognized as a leading educational and research center in science and engineering. Even prior to American entry in 1941 MIT, as part of its contribution to the war against fascism, had enlarged its areas of scientific research and engineering development by the addition of programs and facilities designed specifically to seek solutions to technical problems arising from the need for new and improved weapons. One of the facilities added was the Servomechanisms Laboratory of the Department of Electrical Engineering. The special competence developed by the Laboratory and its personnel, coupled with the technical resources of the Institute, had formed by 1944 a combination of talent uniquely qualified to undertake for the United States Navy a project that ultimately was to make a major contribution to computer technology.

As originally conceived, the project would provide a common solution to a twofold problem, that of the flight instructor and that of the aircraft designer. It was taking far too much time and money to train flight crews to man the more complex, newer models, and it was taking far too much time and money to design projected high-performance airplanes. A possible solution worth investigating had been suggested by the recent successful development of flight trainers.

The massive trained-manpower needs of World War II confirmed the inadequacies of contemporary methods and

equipment for the training of crews of military aircraft. Both British and Americans had sought to eliminate this major weakness by the initiation of research and development programs designed to create superior equipment and methods. The programs led to the operational flight trainer which, without ever leaving the ground, simulated the flight characteristics of a particular existing warplane. Such trainers had proved attractively useful in training flight crews at inestimable savings in time, money, and lives. The early British trainer, the "Silloth," was pneumatically operated. The Americans subsequently applied the same technique, but when further investigation disclosed that temperature and humidity variations affected pneumatic operation too drastically to permit satisfactorily realistic operation of the complicated systems required in the trainer, they turned to electrical networks and motor circuits, and obtained the greater reliability and versatility they desired.<sup>3</sup>

Following the example of the flight trainers, if a mock airplane cabin or cockpit could be put through the sort of motions that wind-tunnel tests and calculations indicated a new and untried design might exhibit, the responses of a pilot at the mock-up controls would provide valuable data regarding the promise of the untried design and, when integrated with further wind-tunnel tests

and calculations, could effectively accelerate the development and production of wholly new and superior airplanes. At least, such was the reasoning of MIT engineers when they joined in active discussions with United States Navy personnel in 1943 and 1944.

The Navy planners approached the problem from a more practical military view. They saw it as an opportunity to reduce the increasing cost in dollars and man-hours of providing a new and different flight trainer for each warplane model in combat use. Instead, a protean, versatile, master ground trainer would be developed that could be adjusted to simulate the flying behavior of any one of a number of warplanes. Such a prototype trainer would provide, they realized, the configurations and specifications to which cheaper individual-model trainers might be built in desired numbers for their flying schools.<sup>4</sup>

So Navy and MIT engineers, for their separate but mutually reinforcing reasons, made common cause and in 1944 embarked on a common project utilizing Navy funds and MIT technical competence: the development of the Aircraft Stability and Control Analyzer (ASCA).

As events turned out, ASCA was never built. A series of consequences that no one foresaw intervened, Whirlwind I appeared instead, and it was put to a wholly different use involving the aerial defense of the continental United States.

The key figure to set these events in motion was the Naval Officer who, more than any other one man, brought the ASCA project into being, Captain Luis de Florez, director of the Special Devices Division of the Bureau of Aeronautics. Captain de Florez was one of those who in the very early days of World War II had decided to forsake a lucrative civilian career in order to serve the national cause. An engineer with an international reputation for his work in aviation and oil refining, he joined the Navy in 1939. There, until his return to civilian life in 1946, he pioneered in the development of "synthetic" training devices, some of which one Congressional subcommittee report called "little short of miraculous."<sup>5</sup> In 1944 he received the Robert J. Collier Trophy of the National Aeronautics Association for his contributions to the preparation of combat crews during the Second World War.

In 1940, after flight training at Pensacola, Captain--then Commander--de Florez was brought to Washington as special assistant to the head of the Bureau of Aeronautics, Vice Admiral John Towers, a friend of long standing. By April of 1941, the Commander had won recognition for his advanced training concepts. Later in the same year he went to London to study British developments in synthetic training devices, and it was presumably upon that occasion that he had the opportunity to study the British "Silloth" trainer.

Returning to the United States just before Pearl Harbor, he was placed in charge of a section in the training division of the Navy. Subsequently, he was promoted and granted authority to establish the Special Devices Division with an initial appropriation of \$50,000. Before the year's end the figure had been increased to \$1,500,000; by the end of the following year, 1942, it had reached \$10,000,000. As of November, 1944, the Division was well established with a staff of some 250 technical officers and 150 enlisted men and civilians.<sup>6</sup>

For Captain de Florez the trainer-analyzer was a logical and proper extension of existing operational flight trainers which the Bell Telephone Laboratories had developed for the Navy, notably trainers for the PBM, PB4Y2, and F6F aircraft. These simulators permitted the reproduction of typical operational flight conditions by means of instrument readings. The instruments within the cockpit of the trainer were fed data by an "electro-mechanical computing system" which responded to both simulated aircraft performance and crew reaction. Sufficient realism was attained to familiarize the flight crew with the operational characteristics of the type of aircraft for which they were preparing.

Operational flight trainers were expensive, but they had proved a technical and practical training success. It seemed only natural to Navy and Massachusetts Institute

of Technology planners, prodded by Captain de Florez, to extend the concept "into the generalized field of aircraft simulation" by investigating the feasibility of a "universal trainer into which constants for various types of aircraft could be set."<sup>7</sup>

During the fall and winter of 1943, Captain de Florez discussed the dual-purpose simulator with members of his technical staff and also with representatives of the Bell Telephone Laboratories and the Massachusetts Institute of Technology. Bell's involvement was the obvious consequence of its contemporary work in operational flight trainers. The Institute's involvement stemmed from its own personnel's interest in the problem and from the reputation of its impressive technical resources. The latter made the Institute a source of advice and guidance which Captain de Florez as a graduate found quite natural and easy to tap. Initially, he had anticipated using the Institute as a consultant only; the actual engineering development would be performed by the Bell Telephone Laboratories.<sup>8</sup>

While engaged in discussion with Captain de Florez and his staff, the officers and professors at the Institute proceeded to expand their own investigations into the matter. On the eighth of December, N. McL. ("Nat") Sage, director of the Division of Industrial Cooperation at MIT, sent off an official letter to Captain de Florez,

notifying him that the Institute had appointed Professor John R. Markham as Project Engineer for research on an "Airplane Stability and Control Analyzer."<sup>9</sup> While the Special Devices Division investigated the dimensions of the enterprise that was taking shape, examining projected costs and identifying industrial laboratories that might be willing to develop such a trainer-analyzer, Markham, together with Joseph Bicknell and Otto C. Koppen, made a study of more detailed technical aspects of the problem. They drew up a report the following April on what they called "a proposed method of ensuring satisfactory handling characteristics of new airplanes," and circulated it to interested parties. Of particular significance for the dawning Whirlwind story is their assertion that "a specialized calculating machine could be built that could be set up for a particular airplane according to data obtained by experimental means, and the pilot's control motions could be fed into the system by actually having the pilot fly the resulting airplane."<sup>10</sup>

The MIT study was incorporated into the Navy program as the result of a conference called in January, 1944 to discuss the feasibility of using the PBM-3 trainer then under development at the Bell Telephone Laboratories as the basis for the proposed dual-purpose simulator. Agreement was reached at the conference to defer further discussion until specifications had been prepared by the

MIT group. Once this had been done, discussions would be resumed, after which the recommended specifications would be forwarded to the Western Electric Company for a proposal to be drawn up on the required engineering work.<sup>11</sup>

By mid-April the MIT report was completed and sent to the Special Devices Division. It contained the reasoned conclusion of aeronautical engineering specialists Markham, Bicknell, and Koppen that it was practicable to design and construct an aircraft control and stability analyzer. The success of existing flight trainers, they noted, permitted the assumption that "a similar mock-up and calculating machine could be used to develop the flying characteristics of a projected airplane." Save for the construction of a flying prototype, the proposed simulator, they opined, "should provide the best means of determining flying characteristics of large airplanes whether the design be conventional or unconventional." They warned, however, that adoption of the proposed simulator would require the expansion and improvement of existing wind tunnel techniques and equipment in order to secure more and superior data.<sup>12</sup>

A copy of the report was sent directly to Captain de Florez with an accompanying letter from Professor Jerome C. Hunsaker, head of the Department of Mechanical Engineering at the Institute. Professor Hunsaker expressed his conviction that the proposed simulator offered "a

new tool of very great research significance," permitting for the first time, if the details could be worked out, "the controlled motion (handling characteristics) of an airplane" to be estimated prior to construction. The heart of the simulator, the analyzer, would be difficult to design and build, he acknowledged, but the Bell Telephone Laboratories possessed the ability if they would make the effort. If the Navy undertook the proposal, the Institute would for its part enthusiastically continue to cooperate and assist by making available the facilities of its Wright Brothers Wind Tunnel for the development, at Institute expense, of the equipment required to determine "the unusual aerodynamic coefficients needed to feed into the analyzer."<sup>13</sup>

Once the concept had been endorsed by the findings of the MIT study group, the Special Devices Division proceeded during the following month to establish a formal program for its implementation, identifying the proposed simulator as "Device 2-K, Aircraft Stability and Control Analyzer."<sup>14</sup> On August eleventh, specifications for both the computer and the cockpit were published, and the procedures for the selection of a qualified contractor were instituted. Curiously, the specifications contained no reference to the use of the simulator as a master, operational flight trainer, but described it as a means "to obtain quantitative measurements of the

stability, control, and handling characteristics of large multi-engined aircraft" prior to construction, permitting the distinct inference that if the MIT engineers had not prepared the specifications, their recommendations had been most influential.<sup>15</sup> The omission, however, was in no way a reflection of any change in purpose on the part of Captain de Florez, for during the years that followed he continued to regard the proposed device as the prototype of both a master operational flight trainer and an experimental-aircraft simulator.

Captain de Florez initially had anticipated that the project would be undertaken jointly by the Bell Telephone Laboratories and its manufacturing parent, the Western Electric Company, but ultimately the task was given to the Massachusetts Institute of Technology. All in all, some twenty-five commercial and industrial organizations were considered in the original canvass, but these either were eliminated or withdrew for various reasons.<sup>16</sup> Apparently, both Bell and Western Electric were reluctant to undertake the program lest it interfere with Navy contracts of greater immediacy.<sup>17</sup> Furthermore, by the fall of 1944, victory was visible over the horizon, and it is possible that the two companies preferred not to commit their facilities to a long-term military responsibility rather remote from their primary peacetime missions of servicing the needs of their parent organization, the American Telephone and Telegraph Company.

Intended or not, the selection of the Institute was logical and natural. MIT possessed the interest and the requisite technical resources, and it had participated in the project from the very beginning. In addition, Navy negotiators anticipated a substantial reduction in cost, since the Institute as a non-profit corporation had lower direct costs and overhead than private industrial organizations.<sup>19</sup> Whatever the reasons, the Special Devices Division was authorized in November, 1944 to undertake in conjunction with MIT a preliminary investigation into the trainer-analyzer.<sup>20</sup>

Captain de Florez's course of action did not go unchallenged. From the very beginning of his Navy career his advocacy of technical innovation met criticism, opposition and even outright hostility, but it is to be remembered that the history of innovation is also the history of resistance to change, especially where institutional officers and custodians are involved. Since institutions exist to preserve what men value, they draw some of their strength and substance and vitality from tradition as well as from innovation. The opposition to innovation had generally been sincere, finding its roots, as Elting Morison has noted, in adherence to the traditional, to the familiar, to the fear of change and of the impact of change upon one's career if not one's very way of life.<sup>21</sup> When exercised in a military institution and carried to an extreme, it can,

as history shows, confound statecraft and endanger the very security of a modern nation.

Captain de Florez was neither the first nor the most conspicuous to encounter such resistance while encouraging technical progress. The First Sea Lord of the British Admiralty, Sir John Fisher, had encountered opposition and hostility in the decade preceding the First World War when he pushed through the dreadnought construction program and spent millions on the submarine. Proponents of the German U-boat as an offensive weapon were unable to win the support of the guiding genius of German sea power, Admiral von Tirpitz, and thus Germany neglected to realize the potential of the weapon which might have brought her victory in the First World War.<sup>22</sup> Opprobrium was heaped upon the Board of Ordnance and Fortification of the United States War Department for wasting its limited funds on Samuel Langley's unsuccessful experiments in heavier-than-air flights.<sup>23</sup> Admiral William S. Sims, one of the creators of the modern American Navy, was in constant difficulty because of his support of innovation.<sup>24</sup>

The opposition to de Florez's proposed trainer-analyzer was sharp and articulate. It was given voice by Captain W. S. Diehl, Chief of the Aerodynamics and Hydrodynamics Branch of the Bureau of Aeronautics, who, acting under oral instructions, had investigated the feasibility and value of the proposed trainer-analyzer. In his report

to his superiors, Captain Diehl was bitterly negative, describing the projected device as "essentially a physicist's dream and an engineer's nightmare." The claims made for the simulator were technically unsupportable and fallacious, Diehl argued. Furthermore, the proposal was both inappropriate and redundant, since it encroached upon work already in process under the aegis of the National Advisory Committee for Aeronautics. These views, Diehl asserted were shared by other engineers within both the Navy and the National Advisory Committee for Aeronautics.<sup>25</sup>

To counter the adverse criticism voiced by Diehl, de Florez marshalled his forces within both the Massachusetts Institute of Technology and his own organization, the Special Devices Division of the Bureau of Aeronautics. The counter-arguments from the Institute study group reiterated the initial conclusions that the trainer-analyzer was technically feasible, valid, and of great promise. Professor Jerome C. Hunsaker, than on leave from the Institute to serve the National Advisory Committee for Aeronautics as its chairman, responded in that capacity, rejecting the charge that the proposed program would encroach upon the Committee's work. Instead, Hunsaker encouraged the Navy to proceed with the project not only because of its great practical promise, but because the research was important for itself.<sup>26</sup>

For his own part, Captain de Florez replied that the proposed generalized trainer was a natural outgrowth of the operational flight trainer. It would eliminate about 80 per cent of the work required for the design and construction of a specific flight trainer. A substantial reduction in cost would result at the same time that a means would be provided to accelerate the successful design and development of new airplanes. The criticism voiced by Captain Diehl, he implied, was just as invalid and unsubstantial as had been earlier criticism of the projected development of the now successful operational flight trainer. He recommended, therefore, that his Division be authorized to continue with the project in cooperation with the Massachusetts Institute of Technology.<sup>27</sup>

Captain de Florez was persuasive. His arguments were undergirded by a record of demonstrated accomplishment. On the twenty-eighth of November, Rear Admiral D. D. Ramsey, chief of the Bureau of Aeronautics, granted the requested permission.<sup>28</sup>

Anticipating that approval to continue with the project and to enter into contractual negotiations with the Massachusetts Institute of Technology would be forthcoming, representatives of the Special Devices Division had met in mid-October with technical and administrative representatives of the Institute for preliminary discussions.

Present at this conference was another who played a key role in the Whirlwind story, N. McL. ("Nat") Sage. In his capacity as director of the Division of Industrial Research, Nat Sage was responsible for the negotiation and administration of externally-sponsored research and development projects conducted by the Institute.<sup>29</sup> From that office he was to serve as a sympathetic and protective liaison agent between the project and its Navy sponsors, as well as between Project Whirlwind and the MIT administration. Considered by his peers to be an excellent judge of men, Sage was more apt to support the man than the project, in the belief fortified by his experience that a good man meant a good project. His support of Whirlwind and its leadership was a reflection of his willingness to aid younger men who had gained his confidence and respect.<sup>30</sup> It is extremely doubtful whether Whirlwind could have survived the stormy years of 1947-1949 had not Nat Sage given it his unswerving and resourceful support in his dealings both within the MIT community and with the Navy.

Nat Sage's influence extended beyond the Institute. His was a strong, dynamic personality. His policy views helped mold the pattern of the relationships that evolved during the war years between the federal government and MIT. These relationships were not peculiar to MIT but were representative of those which developed between American educational institutions and the government in

the wartime research and development effort. Sage was a shrewd and penetrating observer who understood well the attitudes, the institutional commitments, the frailties and foibles as well as the strengths and insights of both the career military minds and the civilian-in-for-the-duration administrators and contract officers with whom he had to deal when representing MIT. Since this wartime cooperation was unprecedented, Sage had a relatively free hand as he charted unfamiliar seas in establishing the procedures and forms which were to guide the contractual relationships between MIT and the Government. The novelty of these relationships, the exigencies of the War, and Sage's experience and resourcefulness cumulatively gave him the power to induce the Government to accept many of his suggestions concerning contractual arrangements,<sup>31</sup> and one consequence of this state of affairs was the broad latitude of options subsequently made available to the new ASCA project in the early conduct of its operations. Indeed, by conservative institutional and corporate standards the project enjoyed greater freedom of operational choice than many responsible executives find it comfortable to contemplate allowing their enthusiastic younger subordinates.

Another influential MIT representative present at the October, 1944 discussions was Professor Gordon S. Brown, the director of the Servomechanisms Laboratory.<sup>32</sup>

Professor Brown's presence at the discussions with the Special Devices Division indicated that if the Institute chose to proceed with the next phase of the ASCA project, the Servomechanisms Laboratory might well be involved. This was understandable, for the nature of the work lay within the competence and experience of the Laboratory. The Servomechanisms Laboratory had been established in December, 1940, under the direction of Gordon S. Brown, assisted by Albert C. Hall, John O. Silvey, and Jay W. Forrester. It was the outgrowth both of a training program for United States Naval Fire Control Officers begun in 1939 in the Department of Electrical Engineering and of arrangements made by the Sperry Gyroscope Company with the Institute to undertake a research and development program that would produce a remote control system for antiaircraft guns on merchant ships.

Effective defenses were needed against Nazi dive bombers, which in the fall and winter of 1940-1941 had become a primary menace to the supply ships approaching the United Kingdom from the United States and elsewhere. Necessary to a particular defense system under development by the Sperry Gyroscope Company was a servomechanism that would link a computing sight to the 37 mm. guns with which merchant vessels were to be armed. Rather than retool to manufacture an already existing British remote-control system, the company had chosen to develop a system which would utilize to the greatest possible extent

components already in domestic production. To this end the Company had arranged with the Massachusetts Institute of Technology to conduct the necessary research.<sup>33</sup>

Within the Institute, the responsibility for the research program was given to the Department of Electrical Engineering because of its experience in servomechanisms; in turn the Department organized the Servomechanisms Laboratory.<sup>34</sup>

From the beginning the new laboratory was a loosely controlled organization, for it played a very special role in Professor Brown's thinking. Believing that the conduct of research and development under very liberal controls was essential, he refused to employ the procedural controls that many would have considered mandatory features of good management practice. From a conservative critic's point of view, Brown provided a dangerously decentralized "every man for himself" environment allowing too great autonomy to be practical and safely business-like. It permitted each project director within the Laboratory to organize the work according to his individual peculiarities and capabilities. Carried to the next logical step, it left to each investigator all the latitude he could wish for in the conduct of his work. If the man's talents were not up to the task to be performed, this latitude permitted deficiencies to become quickly apparent.

From Brown's point of view, it was a matter of finding a good man and backing him by turning him loose to make his own mistakes. In the case of the Servomechanisms Laboratory on the MIT campus, the good man preferably took the form of any brilliant and promising graduate student in electrical engineering who gave indications of being able to avoid the gross mistakes and of profiting rapidly from the small ones. Brown's surveillance was perhaps deceptively loose because he gave his project directors such wide leeway. The more astute students soon realized that this procedure gave them all the rope they needed to hang themselves as high and spectacularly as one could wish, and one effect of this realization was the exercise of prudent caution and more careful planning while being innovative.

The unconventional management techniques and procedures Brown applied were so inconspicuous as to seem almost absent. Some of his own subordinates in the Laboratory became convinced that he really did not know what was going on, so often was his back apparently turned. This apparently casual supervision was deliberate, however, reflecting Brown's philosophy of education and his ideas on the proper conduct of advanced research and development. Brown was convinced that the loosely structured but, for his purposes, highly communicative interchange of ideas and problems

which resulted not only contributed to the growing maturity of the student but also enabled the older faculty members involved to remain more innovative and more critical of their own technical views. A net result would be the more rapid and sound progress of engineering knowledge, for regardless of academic level, professors and students alike were stimulated by their mutual contacts and exchanges of views in this informal research-laboratory environment.

Brown felt then and in after years that successful and original engineering research could more likely be achieved if the research and development problem were pursued by students caught up in an instructional program. It was not enough to provide the intellectual milieu, the intellectual challenges, the new horizons that a first-rate educational and training program could offer. Necessary preliminaries as these were, they were too protectively academic. The harsher, more realistic practical experience of the bona fide research and development laboratory committed to solving non-academic problems was also necessary, nor should such experiences be postponed until after graduate degrees had been obtained. Brown saw no reason why carefully selected predoctoral and premaster's degree students of the caliber that MIT attracted should not be exposed to the novel blend of the sheltered,

academic instructional program and the playing-for-keeps, practical, research and development program that they would encounter during the remainder of their experience as professional engineers. In his direction of the Servomechanisms Laboratory, Professor Brown sought to implement these convictions.<sup>35</sup> The measure of his success was demonstrated not only by the considerable performance of the Laboratory itself, but also by the performances of former students and assistants in later years.

The spirit of the Laboratory was high, in part because it was the product of Professor Brown's inconspicuous leadership, but also in part because other factors operated. One was the élan of the graduate student and research assistant who, having embarked upon his professional career, is determined to demonstrate his creative abilities and competence and to find new worlds to conquer. This élan Professor Brown sought to further and exploit. Another factor, equally strong, was the personal dedication the War evoked. Whether this sense of personal commitment stemmed from pure patriotism or the desire to get a "dirty" job done, it was as much a stimulant to the young neophyte in the Laboratory as it was to his senior mentors and colleagues of the scientific and engineering community. After the Japanese attacked Pearl

Harbor on December 7, 1941, the American people committed themselves wholly to the war effort, and there arose a national mood of determination and self-sacrifice difficult to imagine and reconstruct in all its intensity by those who have not experienced it. It became a force whose impact upon every citizen was not lightly to be discounted, and the response to the nation's call when it mobilized its scientific and engineering manpower to aid the prosecution of the war attests to the power of this mood. The urgencies of the War - to many, the conflict was the very battle for survival of the American way of life- made it difficult if not impossible to adhere to a "business as usual" philosophy.

The operational latitude within the Servomechanisms Laboratory encouraged the exercise of these motivations and enthusiasms. Much of the same psychological atmosphere, the same élan, the same personal response were to be carried over into Project Whirlwind, -- and years later were recalled with longing and nostalgia by those who had been participants.

In the years following its establishment, the Servomechanisms Laboratory had expanded both in programs and in personnel. By the time MIT was discussing the ASCA project with the Special Devices Division, the Laboratory had a staff of approximately 100, including

thirty-five engineers. It had since its creation "developed remote control systems for 40mm gun drives; for radar ship antenna drives; for airborne radar and turret equipment; and for stabilized antennas, directors and gun mounts; as well as having cooperated in a number of other instrument problems."<sup>36</sup> As a consequence, the Laboratory had in its four years acquired extensive experience in the research, design, development, and practical test of that general class of machines, an example of which it was anticipated would form the heart and brain of the projected trainer-analyzer.

During the month which followed the October conference between the Institute and the Navy, both the Special Devices Division and the Servomechanisms Laboratory sought to arrive at an unofficial understanding which could serve as the basis for official contractual negotiations between the Navy and MIT. A tentative proposal was prepared by the Laboratory in early November, providing for a research and development program which would be carried to the "breadboard model" stage over a one-year period at an estimated cost of \$200,000. The construction of the final simulator would be undertaken only after the program had then been re-evaluated and the decision to continue had been made.<sup>37</sup> A conference held on November 15th disclosed, however, that both parties

had come to the opinion that the initial proposal was both too extensive and too expensive. Consequently, they jointly worked out a new proposal, recommending a more modest preliminary study which would cost about \$75,000. This study would provide, they felt, a more accurate appraisal of the feasibility and ultimate cost of the trainer-analyzer.

The terms of this agreement were incorporated in the Special Devices Division's application to the Bureau of Aeronautics for approval of the project. On December 14, 1944, the Navy issued a formal Letter of Intent for Contract Noa(s)-5216. Four days later the Institute officially accepted the Letter, and the program for the development of the Airplane Stability and Control Analyzer was officially launched.<sup>38</sup> None of the participants anticipated a major change in course. In this they were quite reasonable and quite wrong, for no one could anticipate, before the research was undertaken, that the difficulties inherent in realizing the initial purpose would be so profound or that the efforts of both MIT and Navy experts to reach a solution would generate a different enterprise superseding the first. Even the prophetic Captain Diehl, who had called the project "a physicist's dream and an engineer's nightmare," did not allow for a change in course; after all, his solution had been to refrain from embarking on it at all.

NOTES TO CHAPTER 1.

1. Vannevar Bush, Modern Arms and Free Men (New York, 1949), pp. 6-7.
2. James McCormack and Vincent A. Fulmer, "Federal Sponsorship of University Research," The Federal Government and Higher Education, The American Assembly, Columbia University (Englewood Cliffs, N.J., 1960), p. 78.
3. Draft memo, (anonymous, no date). The contents suggest a Navy source prepared it prior to April, 1944.
4. SDD Memo, Oct. 11, 1944, J. W. Ludwig to Capt. de Florez. Cf. Enclosure "D" of BuAer Procurement Directive EN 11-27339-45, Nov. 22, 1944: "Data for OP&M," Nov. 20, 1944, by J. B. Van Duzer and R. I. Knapp of SDD.
5. Quoted in Robert L. Taylor, "Captain Among the Synthetics," The New Yorker, Nov. 11, 1944, p. 34.
6. Robert L. Taylor, "Captain Among the Synthetics," The New Yorker, Part I, Nov. 11, 1944, pp. 34ff; Part II, Nov. 18, 1944, pp. 32ff; see also Luis de Florez's obituary in The New York Times, Dec. 6, 1962, p. 43.
7. Servomechanisms Laboratory, MIT, Project Whirlwind, Summary Report No. 1 (Apr., 1946), pp. 1-4.
8. Memorandum, Rogers Follansbee, Aircraft Simulation Section, to Director, SDD, subj.: "Analyzer, Flight Characteristics," Feb. 5, 1944.
9. Ltr., N. McL. Sage to Capt. Luis de Florez, Dec. 8, 1943.
10. John R. Markham, Otto C. Koppen, Joseph Bicknell, Note on A Proposed Method of Ensuring Satisfactory Handling Characteristics of New Airplanes, (April, 1944), p. 4.
11. Ltr., Rogers Follansbee, Aircraft Simulation Section, SDD, to J. Bicknell, MIT, subj.: "PBM-3 Operational Flight Trainer Data-Forwarding of," Feb. 5, 1944.

12. Markham, Koppen, Bicknell, Note on A Proposed Method. . . . , pp. 1-6.
13. Ltr., J. C. Hunsaker to Capt. Luis de Florez, Apr. 15, 1944.
14. (C. P. Andrade), Memorandum for Files, subj.: "Project Whirlwind," June 13, 1946.
15. Navy Dept., BuAer, SDD, Specifications for Airplane Stability and Control Analyzer, Aug. 11, 1944.
16. Memorandum, Head of Production Branch, SDD, to Director, SDD, subj.: "Project 2-K - Report on Companies Considered and Facilities Available," Oct. 13, 1944; Ltr., L. F. Jones Gov't Development Section, RCA, to R. I. Knapp, SDD, Oct. 18, 1944; Ltr., W. S. Hill, Ass't District Engineer, General Electric Co., to J. B. Van Duzer, SDD, Oct. 11, 1944.
17. Memorandum J. B. Van Duzer, SDD, to E. N. Howell, SDD, subj.: "Sources for Project 2-K, Stability Control Analyzer and F7F OFT's," Sept. 18, 1944.
18. Enclosure "D" of BuAer Procurement Directive EN11-27339-45, Nov. 22, 1944; "Data for OP&M," Nov. 20, 1944 by J. B. Van Duzer and R. I. Knapp of SDD.
19. Memorandum, Luis de Florez to Rear Admiral D. C. Ramsey, BuAer, Nov. 27, 1944.
20. Memo, Director, SDD, to Chief, BuAer, subj.: "Airplane Stability and Control Analyzer," Oct. 13, 1944.
21. See Professor Morison's essay, "Gunfire at Sea: A Case Study of Innovation," published in his Men, Machines, and Modern Times (Cambridge, Mass., 1966), pp. 17-44.
22. A. J. Marder, From the Dreadnought to Scapa Flow, vol. 1, The Road to War, 1904-1914 (London, 1961), pp. 330-335.
23. Mark Sullivan, Our Times, The United States 1900-1925 (New York, 1932), II, America Finding Herself, pp. 557-568.
24. Elting Morison, Admiral Sims and the Modern American Navy (Boston, 1942), passim.

25. Memorandum, Head of Aerodynamics and Hydrodynamics Branch to Chief, BuAer, subj.: "Airplane Stability and Control Analyzer--Comment on," Sept. 5, 1944 (Enclosure "A" to ltr., Director, SDD to Chief, BuAer, subj.: "Airplane Stability and Control Analyzer," Oct. 13, 1944).
26. Memo, Comments on Captain Diehl's Letter with Regard to ASCA (MIT memo), Oct. 2, 1944; ltr., J. C. Hunsaker to Capt. Luis de Florez, Oct. 4, 1944 (Enclosures "C" and "B" respectively, to ltr., Director, SDD, to Chief, BuAer, subj.: "Airplane Stability and Control Analyzer," Oct. 13, 1944).
27. Ltr., Director, SDD, to Chief, BuAer, subj.: "Airplane Stability and Control Analyzer," Oct. 13, 1944. Cf. Enclosure "E" Memorandum, J. W. Ludwig to Capt. de Florez, subj.: "Analysis of Project 2-K Airplane Stability and Control Analyzer," Oct. 11, 1944, ibid.
28. Approval initialled on Memorandum, Capt. Luis de Florez to Rear Admiral D. C. Ramsey, BuAer, Nov. 27, 1944.
29. Memorandum, J. B. Van Duzer, subj.: "Project 2-K, Aircraft Stability Control Analyzer, Conference with MIT representatives," Oct. 18, 1944.
30. Interview, Prof. G. S. Brown, MIT, by the authors, Jul. 6, 1964; Interview, Prof. J. W. Forrester, MIT, by the authors, Jul. 24, 1964.
31. Ibid.
32. Memorandum, J. B. Van Duzer, subj.: "Project 2-K, Aircraft Stability Control Analyzer, Conference with MIT representatives," Oct. 18, 1944.
33. Interview, Jay W. Forrester and Robert R. Everett by the authors, Jul. 31, 1963.
34. Jay W. Forrester, "Hydraulic Servomechanism Developments," MS Thesis, Dep't of Electrical Engineering, MIT, June, 1945, pp. 1-3.
35. Interviews by the authors with: J. W. Forrester and Robert R. Everett, Jul. 31, 1963; Kenneth H. Olsen, June 24, 1964; Charles W. Adams and John F. Gilmore, Jul. 3, 1964; Gordon S. Brown, Jul. 6, 1964.

36. N. McL. Sage, Director, DIC, MIT, to Chief, BuAer, att'n.: Lt. Comdr. E. N. Howell, SDD, subj.: "Proposal for Contract for Development of a Generalized Multi-Engined Operational Flight Trainer," May 22, 1945; draft of ltr. from G. S. Brown to Chief, BuAer, att'n.: Lt. J. B. Van Deusen, SDD, subj.: "Proposal for Contract for Development of Aircraft Analyzer," Nov. 3, 1944.
37. J. W. Forrester, MIT Computation Book, #36, p. 14; draft ltr., G. S. Brown to Chief BuAer, att'n, J. B. Van Deusen, subj.: "Proposal for Contract for Development of Aircraft Analyzer," Nov. 3, 1944.
38. Enclosure "D" of BuAer Procurement Directive EN11-27339-45, Nov. 22, 1944: "Date for OP&M," Nov. 20, 1944, by J. B. Van Deusen and R. I. Knapp, SDD; J. W. Forrester, Administrative notes entered in his MIT Computation Book #36, p. 14; Memorandum, Luis de Florez to Rear Admiral D. C. Ramsey, BuAer, Nov. 27, 1944; Navy Dep't., BuAer, Letter of Intent for Contract NOA(s)-5216, Dec. 14, 1944.

## COMPUTING PROBLEMS EMERGE

A natural change in the course of the investigation occurred as a consequence of the preliminary aerodynamic analyses of the Airplane Stability and Control Analyzer's prospects and problems. It occurred between the time Captain Luis de Florez had initiated preliminary discussions with the Massachusetts Institute of Technology in 1943 and the time the Institute accepted the Letter of Intent over a year later. It was set in motion when de Florez asked the Institute how practical his ASCA project to build a simulator appeared to be from an aerodynamics viewpoint. As we have seen, the response of Professor Hunsaker and the Wright Brothers Wind Tunnel engineers was that the problem appeared by no means insoluble, and the subsequent, more detailed investigation and conclusions of Markham and his associates reaffirmed the reasonableness of de Florez' proposal and indicated it was attractively worth further consideration.

The consequence of these conclusions was a shift in the focus of investigation from the field of aerodynamics and aeronautical engineering to the field of electrical engineering and electromechanical control systems. So the problem passed to Professor Gordon Brown and the Servomechanisms Laboratory. The formal agreement reached in December, 1944 occurred,

of course, after the fact of Brown's involvement, and it betokened not his decision to become involved but his commitment and that of his engineers to pursue the ASCA project and its electromechanical simulation problem further. He had already brought the problem to the attention of one of his assistant directors, Jay W. Forrester, who had managed earlier projects in the Laboratory.<sup>1</sup> Forrester became interested in this provocative engineering challenge, as Brown had hoped, and accepted the direction of the ASCA project in the fall of 1944. It was a responsibility that he was not to relinquish until 1956.

While remaining in charge, Forrester soon brought Robert R. Everett into the project. In a very special way, reflecting the complementary temperaments of the two young men, Everett came to share the responsibility and the technical direction of the project with Forrester. These were the two engineers whose technical and administrative leadership gave the project its basic character during the following decade. There was never any question that Forrester was in charge of the project, exercising administrative authority and technical leadership, and there was never any question that Everett was second in command, exercising continuing technical leadership and administrative authority when Forrester was preoccupied with external affairs.

Linked by a deep mutual respect and understanding, they worked together in unusual harmony, without always employing the same means to reach their common goal.

A native of Anselmo, Nebraska, Forrester had obtained his Bachelor of Science degree in engineering at the University of Nebraska in 1939. In the fall of that eventful year (World War II had begun when Hitler invaded Poland in September, 1939) Forrester came to MIT as a graduate student and research assistant in electrical engineering. He was already on hand when Brown set up the Servomechanisms Laboratory in response to the looming technical demands of the war. The progress of the war expanded the opportunities for original engineering research at MIT by providing the incentive, the needs, and the funds. One of the research and development fields so expanded and accelerated involved the design and development of feedback circuits and mechanical and electrical analogue devices and powerful servomechanisms responsive to remote control. It was a field that saw dramatic technical progress during the war, and since the "Servomech Lab" was in the middle of it, Forrester was one of those who acquired extensive familiarity with the potentialities and the limitations of servomechanisms and with associated problems of integrated

system design and development. Fighter-director radar controls later placed on the USS Lexington were one of the systems that had given him important practical experience. Consequently, when de Florez' trainer-analyzer appeared above Forrester's horizon, he possessed both the technical experience and the administrative organizational experience to set up the project. Because of the unpredictable character of the research and development process, neither he nor anyone else at the time realized what the project would become and what transformations would ensue during the following eighteen months, not to mention ten years.

Everett, born in Yonkers, New York, had received his B. S. degree in electrical engineering at Duke University in June, 1942, six months after the United States became a combatant in the war. In the summer of that year, about a month after entering MIT to seek a master's degree, Everett joined the war effort by going to work for Forrester in the Servomechanisms Laboratory.

Both young men thus were exposed to Brown's way of doing things and to the level of intellectual enterprise maintained by him and his colleagues. Under his eye they developed their respective organizational and administrative talents as well as their electrical engineering expertise. Although they did not try to

duplicate Professor Brown's personal style, it is not surprising that features of the philosophy of management followed by Brown within the Servomechanism Laboratory influenced significantly the organization and administration of the Airplane Stability and Control Analyzer program after it became Forrester's primary responsibility. He and Everett proceeded, of course, to conduct the program in their own style.<sup>2</sup>

After accepting technical and administrative responsibility from Brown, Forrester worked on the ASCA project virtually alone at first and by the end of the first week in November had laid out his plan of attack. Basic units of the complete analyzer would include a simulator "cockpit with controls and instruments, the flight engineer-observer station, and the calculating equipment." While the specifications seemed to have purely electrical analogue computing in mind, Forrester surmised that many of the integrator functions "might well be met through use of a variable-stroke hydraulic transmission." Perhaps a mixed mechanical and synchro data system, although more expensive, might avoid certain design difficulties of the all-electric system. "A combination system of synchro data, voltage data, mechanical integrators for multiplication by constants, and hydraulic transmission integrators for integrating and for multiplication by two variables" might be a suitable compromise.<sup>3</sup>

Obviously, he should study existing trainers, familiarize himself further with the equations embodying the aerodynamic requirements, "discuss the objective of the apparatus with the Navy sponsors. . . with commercial test pilots, designers, and wind tunnel men for detailed information on behavior and accuracy," examine "mechanical and electrical methods of continuous mathematical calculating," obtain engine-performance equations, study the physical details involving "types of signalling [and] types of amplifiers and other components," consider the types of schematic approaches available, and lay out a schematic solution that would "reduce the number and types of equipment as far as possible."<sup>4</sup>

On November 4th and 5th he laid out preliminary schematics "to show the solution of the equations" contained in the specifications. Familiarizing himself further in this way with the abstract statements, conditions, and quantities that the Airplane Analyzer would translate into suitable motions of the simulator cockpit, he considered ways and means of interpreting and restating for engineering purposes the requirements set forth in April at MIT by Markham, Koppen, and Bicknell and in August by the Bureau of Aeronautics.<sup>5</sup> His preliminary survey indicated that there were ninety-two quantities and thirty-three simultaneous

equations involved, just to describe the aircraft response. Further study indicated that, strictly speaking, thirty of the equations described the aircraft response, three related to acceleration and velocity, eight dealt with instrument responses, and six applied to the control forces. So the Analyzer would have to handle at least 47 equations involving 53 variables with respect to time, and none of these took into account the engines and engine controls. Since a multi-engine simulator was what de Florez had in mind, the device would be complex, indeed.

From his preliminary schematics Forrester further drew the regretful conclusion that "the extensive use of synchro position for quantities or of mechanical multiplication seems entirely out of the question." On the other hand, "a-c voltage signals should cause much less difficulty because of the ease of isolating various circuits." Careful engineering, he noted, ought to be able to avoid phase difficulties such as Bell Telephone Laboratories engineers had encountered when designing trainers.<sup>6</sup>

In this manner he proceeded to shape his preliminary assessment of the ASCA problem. It was partly on the basis of this assessment that the meeting of November 15, 1944, between Navy Bureau of Aeronautics personnel and Sage, Brown, and ASCA

engineers Forrester, Everett, and Hugh Boyd of the Institute called to discuss contract arrangements found both the Navy and MIT representatives ready to back away from the \$200,000 bread-board-model contract that had been proposed earlier. As Forrester noted at the time, "BuAer felt from previous projects that the project would not be of such magnitude and also, after the intervening two weeks of study, MIT had a clearer picture of the requirements." It appeared more practical to think in terms of "a 4 to 6 months preliminary study to be covered by an appropriation of \$75,000...."<sup>7</sup>

What had happened was this: MIT, through the informal actions of Sage, Brown, and Forrester, had initiated the limited feasibility and cost study stipulated in Contract N0a(s)-5216 even before the Letter of Intent was issued by the Navy and accepted by the Institute. This was neither the first nor the last time that professional involvement with the engineering problems by the engineers preceded official endorsement by the appropriate administrative and legal officers. Indeed, it was the practical thing to do: assess the problem in a preliminary way before making a commitment to undertake it in greater detail. In a very real sense Forrester's work before December was a feasibility study of the prospect of taking on the ASCA feasibility study.

This arrangement---a not infrequent characteristic of the research and development process---allowed formal fiscal and administrative agreements to rest upon the latest technical thinking and had the merit of placing the entire procedure on a more empirically sound basis attractive to all the parties concerned. In consequence, the legal and monetary relationships became subsidiary means to the end of securing the engineering knowledge and the technical hardware sought. As will be seen, this subordination of fiscal and administrative factors to the engineering factors did not persist throughout the history of the project. But it was a customary way to start a project and quite acceptable in view of the fact that the war was still on. While prudent control of expenditures was always to be desired, cost itself was no object; the imperative consideration was to get on with the job, at whatever the cost in dollars. In such a wartime policy climate (the only kind in which Forrester had accumulated his research and development experience), it was natural that fiscal and administrative policies should be subordinated to the technical needs of those who were getting the job done.

Forrester's investigative techniques were, of course, the product of his experience acquired since at least 1940. They were not intuitive, unexamined

procedures that he was unaware of and could not explain. On the contrary, his was a temperament that took it for granted he should analyze and make as explicit as possible the useful techniques that "came naturally" from his experiences. His was a mind that preferred to know where it stood and why, at all times. It was committed in a very self-aware way to understanding and rationalizing and systematizing the intellectual procedures through which it moved, especially where innovative activity, such as engineering research evoked, was involved. This trait was part of the young graduate student's immense self-possession (that some found presumptuous, if not patronizing, in one so youthful). It helped him to organize his plans of attack, it helped him to carry them out, and although it did not prevent errors in judgment, it provided continuing re-examination of that judgment and helped to minimize errors before they got out of hand. It could not forestall basic policy-level errors, nor was it a remedy for the fact that the fullness of his expert knowledge in the area of mechanized analog computation and the principles of servomechanisms was also the measure of the depth of his contemporary ignorance of mechanized digital computation, resulting in a postponement in his selection of a suitable computer while he endured progressive disenchantment with the ideal device in his mind's eye that he had at first selected.

An example of his self-aware, analytical mode of procedure is to be seen in a report, his master's thesis, which he began in 1941 and finished in November, 1944, as he was taking up the ASCA project. The views he had expressed in 1941 he considered still appropriate in 1944. In discussing the scope of the thesis, he made no apology for the fact that "considerable emphasis is given to the mathematical analysis of the control systems which have been developed." "This has not been done because the analysis is academically fascinating," he wrote, "but because, from an engineering viewpoint, it has proven the surest and quickest way to obtain the desired results and to avoid the pitfalls so often appearing in the trial and error attempt to solve a complex problem." Forrester felt that the analysis of the specific servomechanism discussed in his thesis provided "an excellent example of the philosophy of the laboratory toward remote control theory. "<sup>8</sup>

"It may seem," he continued, "that an undue amount of attention is devoted to the development and design of the early experimental and pilot models. However, it is there that the analytical approach may most effectively be shown, and the brief dismissal of many of the design and engineering problems of later work results not because these problems were easily solved,

but because one with the necessary understanding and respect for the complexity of the operating principles may expect to reach the proper answer."<sup>9</sup>

Forrester regarded it as a telling virtue that a "great deal of time and attention is devoted at the Servomechanisms Laboratory to careful measurements of the characteristics of individual pieces of equipment which are to be placed in a remote control system. These measurements and study yield information on the reliability of the components and make available numerical values of the constants appearing in the equations representing the response of a system. Such an investment of time and effort has returned substantial and satisfying dividends in the reduction of time consumed by 'cut-and-try' experimenting."<sup>10</sup>

Such were the technical background and perspectives that Forrester brought to bear when he opened his investigation of the ASCA problem in November, 1944. By mid-December he had become sufficiently acquainted with both operational flight trainers in general and the proposed trainer-analyzer in particular to articulate the technical requirements for the analyzer, prepare a tentative time schedule, and assemble a list of personnel whom he considered competent to carry out the work needed in fulfillment of the contract's aims and terms.

Among the personnel he sought, in addition to Everett, were three other engineers: Hugh Boyd, Stephen Dodd, and George Schwartz, all of whom had been working on projects in the Servomechanisms Laboratory. As was to be expected, however, some time elapsed before Forrester was able to assemble the engineering staff he wanted. By the following February only Forrester himself and Boyd had been able to devote full time to the project; the others divided their time between the trainer-analyzer and other projects within the Laboratory.<sup>11</sup>

Work on the project did get under way, nevertheless, and at the start it was paper work. Early in March, a five-page laboratory report outlining methods of mounting and actuating the simulator cockpit was given to Forrester by one of his engineers.<sup>12</sup> The cockpit problem appeared to admit more of straightforward solutions than did the prospect of designing the extremely complicated analog computer that the simulator would require. As more problems and subproblems were investigated, even more problems were uncovered, with the result that as the six-month preliminary study period progressed, Forrester became less sanguine than he had been. The problems were proving more formidable than he and his associates had thought in October and November that they would be. Overall, however, the

situation was under control, for the point of such research was to delineate the scope of the problems involved, and the emerging picture continued to indicate that the stumbling blocks were not insurmountable. Forrester, Brown, and Sage remained confident they could achieve a solution, although by May, 1945, they were willing to admit that they had, in their earlier, relative ignorance, justifiably underestimated the development cost.<sup>13</sup>

In a memorandum of May 8th that never went beyond the draft stage, Forrester noted that the problem of developing certain components for the Analyzer was requiring more extensive research than they had expected.<sup>14</sup> Many of the customary electrical and mechanical procedures of solving the appropriate differential equations could not be applied in their usual form but would have to be improved and tailored to the job at hand. Particularly was this true, Forrester felt, where speed of response was critical and where the ratio of maximum to minimum signal was extremely broad. In the former instance, the reactions of the pilot in the simulator cockpit would enter into the statement and the solution of the hypothetical aircraft's stability. His responses to simulated aerodynamic forces acting upon the pilot's controls ought to accomplish corrective actions, and these would have to take effect as promptly in the simulator

as they would in a flying airplane. The scale of allowable response times was limited to the time it would take in an actual airplane. The equipment had to operate within these real response times. "This is especially true," wrote Forrester, "of the integrators which convert accelerations to velocities and of the control-column loading equipment."

Again, both the normal maneuverability of an airplane and its range from smooth, level flight to sharp maneuvers imposed a corresponding range between minimum and maximum signals that no known mechanical equipment having a single scale of operation could embrace unless, by suitable mechanical and electrical means, one incorporated "an automatically self-adjusting scale factor. Details of variable scale-factor devices have been worked out but have not yet been experimentally proven."<sup>15</sup>

Consequently, when Forrester contemplated the end of Phase 1 (the preliminary study period) and the beginning of Phase 2 (actual design and construction of ASCA itself), as these were called for in the contract, he recognized that they could not know where they stood in Phase 1 until the studies and demonstrations then in progress were completed, nor could they provide a realistic answer regarding the practicality of Phase 2 until they knew that suitable components existed. He

felt that the components could be developed "in the next few weeks," but saw the necessity of obtaining additional laboratory and engineering time. Perhaps an extension of time on Phase 1 and scrutiny of the transition period between Phase 1 and Phase 2 would constitute the best course of action. If \$50,000 were added to the original \$75,000 allotted for Phase 1, a sufficient extension of time might be achieved. An interval of two or three months, at \$25,000 per month, might take care of the transition period.<sup>16</sup>

This precise course of action was not taken. The Navy remained satisfied with the rate of progress made during the first five months. It was confident that the "general outlook was promising." When the Institute's Division of Industrial Cooperation--Sage's office--submitted on May 22, 1945 its proposal for extension and modification of the contract, the upward revision of estimated cost that Forrester, Brown, and Sage felt was necessary omitted the complicated phasing of phases that Forrester had toyed with and stated instead that the project could be carried through to completion within eighteen months at a cost of approximately \$875,000.<sup>17</sup>

The Navy's response was to renew and continue the project under Letter of Intent for Contract N0a(s)-7082, dated June 30, 1945. By this renewal the Navy firmly committed itself to the project, for the

new contract not only continued, but expanded the project at a cost which was too large to permit easy withdrawal. Since the aims of de Florez and his assistants and the aims of the MIT personnel were in fundamental agreement--to build an Aircraft Stability and Control Analyzer--, since the Institute had become well committed, first through the efforts of Sage and Hunsaker, then through the efforts of Sage and Brown, and now through the efforts of Sage, Brown, and Brown's competent assistant, Forrester, and since excellent contractual and working relationships on the technical level had been established, there was every reason to go ahead. Of course, there were unsolved problems.<sup>18</sup> Had there been no problems, the Navy would not have had to turn to the Institute, and one of the private manufacturers less interested in advanced research could have taken on the job. As to the immediate future, no one could say how long it would take to finish the war against Japan; the empire the Japanese had begun to build in 1932 might be crumbling rapidly, but no one could be sure how long the successful invasion of Japan itself would take. And if ASCA were developed too late to help in the War, its long-run achievement would still be useful. Eighteen months and \$875,000 would represent a sound prospective investment of Navy research and development funds for 1945 and 1946.

The onset of summer saw increasingly severe technical analysis by Forrester and his associates of the progress they were making and the problems they were encountering. Forrester in particular began to probe insistently the problems of the form the computer equipment should take and the appropriate representation that should be given the test data. To represent nonlinear data by mechanical linkage, for example, appeared in prospect to be neither a flexible nor a general enough method. Although it could be incorporated in an analog system, it posed the possibility that wasteful trial-and-error routines would have to be undertaken each time new data required adjustment of the linkage system.<sup>19</sup>

Forrester discussed his problems with others. Professor Samuel H. Caldwell of the Electrical Engineering Department had suggested in May that the work of George R. Stibitz and his associates at Bell Telephone Laboratories might offer suitable alternatives, but Forrester did not pursue this lead at the time.<sup>20</sup> Stibitz, a mathematician who had obtained his Ph.D. in physics, was then involved in the design of a digital computer using telephone relays for storage of numerical data and for arithmetical operations. The following year the Bell Relay Computer, Model 5, was put into operation.<sup>21</sup>

By the last week of June, 1945 Forrester was notifying Brown that the rough survey of requirements his group had made in anticipation of the Navy's continued support indicated they needed greater manpower. Eight more electrical and electronic engineers and three mechanical engineers (they had none at present) were needed. In addition, a building would have to be designed and built; consequently, an architect should be obtained to supervise construction through the following spring. Forrester went on to outline a schedule of the progress required to carry out Phase 2: research from August, 1945 until the first of the following year, mechanical development and design from August, 1945 to March, 1946, electrical development and design from January to March, 1946, procurement and construction from January to July, 1946, assembly and installation from July to December, 1946, testing and trouble-shooting from January to March, 1947, and delivery of the equipment to the Navy on March 31, 1947.<sup>22</sup>

As June passed into July and July into August during that summer of 1945, Forrester became increasingly disenchanted with the lack of flexibility and versatility of the elaborate servomechanism system that was taking tentative shape. The real-time response problem still defied forthright solution, and unless certain design features were changed, the units of the

Analyzer would remain permanently interconnected in the pattern imposed by the equations of motion of the aircraft. In consequence, the computer portion of the Analyzer would be unavailable for use on other problems between simulation tests. The result would be a grossly uneconomical waste of potentially one of the most powerful such computers to come into existence. Perhaps pre-wired, removable plugboards could be employed, with the result that operating characteristics of the computer circuits might be explored between tests and provide important information on the potentialities of the computer for simulator work.<sup>23</sup>

In August the apparent need to make several changes in the integrator circuits of the Analyzer represented additional problems, while success with experimental tests on a variable-oscillator design suggested a feasible three-phase motor could be developed for a variable-frequency servo application that the Analyzer required. The project was making reasonable progress on some details--Stephen Dodd was studying the properties of the aerodynamic equations, George Schwartz was investigating ways and means of representing aircraft piston-engine performances, and others were examining aspects of radio noise level, cathode follower characteristics, and analyzer component interconnections--but progress with many of the

detailed design chores did not keep Forrester from pondering upon the overall characteristics and limitations of the simulator.

In retrospect it should be noted that the wartime design experiences of the graduate students in the Servomechanisms Laboratory, unusually rich and varied though they had been, had not placed them at the forefront of innovative, analog-computer design activity in the manner that their postwar experience in Project Whirlwind was to give them pioneering competence and pre-eminence in digital design work. Their relatively heavy-handed, brute-force engineering approach to the design of analog computation machinery contrasted with the light touch manifested at the end of 1945 in the analog computer approach taken, for example, by Arthur Vance and his associates at RCA. Expert in designing low-drift amplifiers, they developed driftless, direct-current amplifiers that proved essential to later analog computer development. Here they possessed a degree of experience and competence that the Servomechanisms Laboratory engineers lacked, and de Florez' engineers in the Special Devices Division of the Navy were aware of these differences.

The SDD program managers were also increasingly preoccupied with the dawning missile and rocket technology that German engineers had launched spectacularly

with the V-2 rockets used to bombard London, and they were sensitive to the greater challenges lying ahead for simulator engineers in both the aircraft and the missile fields of design.<sup>24</sup> It would be easy to suggest, in consequence, that the Navy programmers deliberately began to encourage Forrester and his colleagues to explore other design avenues that would avoid the analog computer design problems they were encountering, but the evidence of such long-range master planning is not only lacking but also contradicted by the complex sequence and fortuitousness of related events during the remainder of 1945. Apparently aware that the course of engineering research is not explicitly predictable since it requires innovative intellectual activity if it is to proceed, the Navy engineers rested their confidence upon the already demonstrated innovative abilities of the MIT engineers and encouraged them to go whither their investigations led them, within the overall confines of the Airplane Stability and Control Analyzer problem that had been laid down.

Meanwhile, it was in August, 1945, while Forrester and his associates were seeking solutions to the analog engineering problems besetting them, that Japan surrendered. The war was at last over, and many could move once more to pick up the threads of their peace-time lives and occupations. Forrester had to give some

of his attention to the necessary goings and comings and reorganization of activities that ensued, but the dislocations of the war's end proved to be transitory in their effect upon the project.

At the same time, in the wider technical community and unknown to Forrester, a mathematician at Brown University was making arrangements to call an international conference on computers in October. The end of the war meant that a meeting could be called to take stock of wartime developments, and R. C. Archibald, chairman of a National Research Council committee, was readying notices that would bring together experts from England and the United States for a two-day session at MIT. Archibald and his committee were particularly interested in new "electronic devices. . . which promise astronomical speeds for numerical computing processes."

During the summer, Forrester had found time in the midst of the routine of his laboratory affairs to discuss computational techniques other than those associated with the analog computer and found that the engineering development of none was so far advanced as to be of immediate use to him. From a fellow graduate student in electrical engineering, Perry O. Crawford, Jr., Forrester learned of the intriguing future prospects that some already saw for employing

digital numerical techniques in machine calculation. At MIT during the middle Thirties Professor Caldwell had introduced a course in mathematical analysis by mechanical methods, and Crawford, who had studied as a graduate student and research associate under Caldwell and Vannevar Bush, was well exposed to both analogue and digital machine-computation concepts when they were for the most part still in the conceptual stage, especially where digital techniques were concerned.<sup>25</sup>

In 1942 Crawford had submitted his master's thesis under the title, "Automatic Control by Arithmetical Operations," setting forth one application of digital techniques of computation, that of the automatic control and direction of antiaircraft gunfire. After indicating how recently physicists and electronic engineers had become seriously interested in methods of performing arithmetical operations using such electronic devices as the Eccles-Jordan flip-flop circuit, he restricted his discussion to the problem of predicting the future position of the target and described the sort of electronic equipment that might be built to perform the operations required in automatic calculating: "electronic switching elements, devices for multiplying two numbers, finding a function of a variable, recording numbers, translating mechanical displacements into

numerical data, and for translating numerical data into mechanical displacements."<sup>26</sup> Forrester recognized that all of these operations were required of the Aircraft Stability and Control Analyzer. There was no question that Crawford understood the nature of his problem, although it was not encouraging to hear Crawford express the opinion that the successful application of these new digital techniques to the sort of problem that Forrester's group was attacking lay still too far in the future to be of any help.

These ideas, presented to Forrester in stimulating detail in mid-September, did not slip from his mind.<sup>27</sup> Meanwhile, there was the daily administration of the project to attend to, and the momentum of project affairs kept him busy. When Crawford left the Institute a month later, to go to work for de Florez in the Special Devices Division of the Navy, he spent part of his last day on the campus talking with Forrester about digital calculators and the new breed of controlled-sequence devices then coming over the horizon. These were represented by an elaborate vacuum-tube calculator, the "Electronic Numerical Integrator and Computer," called ENIAC for short, and another known as the "Electronic Digital Variable Automatic Computer," the EDVAC. Both of these were under development at the University of Pennsylvania in Philadelphia.<sup>28</sup>

While neither calculator was in operation yet, the former was nearing completion at the Moore School of Electrical Engineering under the direction of John W. Mauchly and J. Presper Eckert, Jr. Mauchly was a physicist, Eckert an electrical engineer, and both were well aware that theirs was the first enterprise committed to using vacuum-tube circuits to carry out the complex calculations required.

Forrester was on the track of something new. He liked what he saw, and the more he saw, the more he wanted to see. He couldn't put it aside. In after years both he and Everett were to attribute to Perry Crawford the suggestion which they came to take seriously, that digital numerical techniques merited serious study.<sup>29</sup>

Although Forrester had worked more extensively with analog devices, his mathematical and electrical engineering background permitted him to recognize and explore rapidly the prospects of digital calculation. There was nothing novel about the equivalence of the two modes of calculating, analog and digital; he was aware that these were alternative procedures, each possessing its particular virtues and defects. And if the arrangement of some of the electrical components that Crawford called to his attention was novel, the tubes, capacitors, resistors, and elemental

circuits were familiar features that offered no trouble. The novelty thus lay less in the elements than in the system implications, and with these Forrester promptly began to familiarize himself. So intent were he and Crawford, and then Everett, in their contemplation of the prospects, that they paid little heed to the historical background of the state of the art as they found it, and indeed such awareness was not necessary to qualify them to carry out the technical pursuit they then engaged in.



NOTES ON CHAPTER 2.

1. Interview, Prof. G. S. Brown, MIT, by the author, July 6, 1964.
2. Interview, Jay W. Forrester and Robert R. Everett, by the authors, July 31, 1963.
3. J. W. Forrester, Computation Book No. 36, entries of Nov. 2, 1944.
4. J. W. Forrester, Computation Book No. 36, entry of Nov. 7, 1944.
5. J. R. Markham, O. C. Koppen, J. Bicknell, Proposed Method of Ensuring Satisfactory Handling Characteristics in Airplanes, April, 1944; Specifications for Airplane Stability and Control Analyzer, Navy Dept., Bureau of Aeronautics, Special Devices Depot, N. Y., N. Y., Aug. 11, 1944.
6. J. W. Forrester, Computation Book No. 36, entry of Nov. 7, 1944.
7. J. W. Forrester, Computation Book No. 36, entry of Nov. 21, 1944.
8. J. W. Forrester, "Hydraulic Servomechanism Developments," pp. 4-5. This report was submitted "in partial fulfillment of the requirements for the degree of Master of Science from the Massachusetts Institute of Technology, Department of Electrical Engineering, June, 1945."
9. Ibid., p. 6.
10. Ibid., pp. 6-7.
11. J. W. Forrester, Computation Book No. 36, entry dated Feb. 5, 1945, "Program to Date."
12. Servomechanism Laboratory Report No. R-100, Contract No. 6345, March 4, 1945, subj.: "Method of Cockpit Mounting and Actuating Mechanism."
13. Servomechanisms Laboratory, Project Whirlwind, Summary Report No. 1, (Apr., 1946), pp. 1-5; see also memorandum prepared but not submitted by J. W. Forrester, subj.: "Status of Contract NOA(s)-5216," May 8, 1945.
14. J. W. Forrester, draft memo, 5-8-45, subj.: "Status of Contract NOA(s)5216."

15. Ibid.
16. Ibid.
17. Ibid.; ltr., N. McL. Sage, Director, DIC, to Chief, BuAer, subj.: "Proposal for . . . Development for . . . O. F. T.," May 22, 1945.
18. Interview, Everett and Forrester by the authors, July 31, 1963.
19. Ibid.
20. J. W. Forrester, Computation Book No. 39, p. 34, entry of May 26, 1945.
21. F. L. Alt, Electronic Digital Computers (New York, 1958), p. 18.
22. Ltr., J. W. Forrester to Dr. G. S. Brown, June 22, 1945.
23. J. W. Forrester, Computation Book No. 39, supplement to p. 54, entry of June 27, 1945.
24. Interview, P. O. Crawford by the authors, Oct. 25, 1967.
25. Ibid.
26. P. O. Crawford, Jr., "Automatic Control by Arithmetical Operations," "submitted in partial fulfillment of the requirements for the Degree of Master of Science at the Massachusetts Institute of Technology, 1942," pp. 1-2.
27. J. W. Forrester, Computation Book No. 39, suppl. to p. 65, entry of Sept. 18, 1945.
28. Ibid., p. 42, entry of Oct. 16, 1945.
29. Interview, J. W. Forrester and R. R. Everett, by the authors, July 31, 1963.

## THE SHIFT TO DIGITAL

The summer and fall of 1945 found a small but growing interest in electronic digital computers flourishing here and there in the United States and Europe. It was of little consequence that no such electronic digital computers were yet in operation, so far as their attractive potentialities were concerned. What mattered was that western mathematicians and engineers were beginning to be caught up in a classic example of the historical phenomenon of "convergence," in which the embryonic computer technology was assuming its shape and character from the joining together of several diverse machine design traditions and several abstract intellectual traditions. Personal curiosity had combined with historical circumstance to place various individuals at peculiar, strategic positions from which they could take advantage of the opportunities provided by this convergence of traditions. Among these individuals happened to be the young engineers at the Massachusetts Institute of Technology. Their individual exertions had helped to bring them to such positions, and the converging traditions set the boundaries within which their ingenuity would go to work.

The first of these traditions was itself a composite of several machine and device developments, each of which had a long history. They included the odometer and the abacus in diverse forms from ancient times, the slide-rule and the mechanical adder from recent times, and the electromechanical calculator traditions of the last hundred years, culminating in the relay machines of George R. Stibitz and his colleagues at the Bell Telephone Laboratories and the electromechanical "Harvard Mark I" built by Howard Aiken with the assistance of several associates from the International Business Machines corporation.

The intellectual traditions included at least three or four of note -- those of counting and "reckoning," or calculating; other mathematical traditions that had produced the logarithm, the slide-rule, and Charles Babbage's unbuilt and forgotten computer (to name only a few); and the scientific and technical traditions that produced, in one direction, theories about electrons and electromagnetic phenomena, and in the other direction, applications in such forms as the vacuum tube, electronic circuits, and radar.

The technical machine tradition that first exploited computer techniques in the late 1930s and early 1940s was the electromechanical tradition brought into being by prior advances in the technology of electricity, in the technology of controlled-

motion machinery, in the technology of the desk calculator, and in the technology of the developed telephone system. At the hands of Aiken, Stibitz, and others, it provided the first machine generation of computers, but although it spawned refined and improved members of one class of relay, switch, and gear machines, it shortly proved to be a subclass of provocative but sterile computers without lasting issue. The cause lay not in any essential "hybrid sterility," but in the development of a more promising machine form that happened to be just a trifle slower to achieve practical working condition. This second form permitted vastly greater calculating speeds, and it provided a second machine generation of computers mechanically unrelated to the first: the electronic digital computers.

Electronic digital computers came hard on the heels of Aiken's and Stibitz's innovations. The first true electronic computer was the Electronic Numerical Integrator and Computer, better known by its acronym, ENIAC, designed and built under the direction of John W. Mauchly and J. Presper Eckert, Jr., at the University of Pennsylvania for the United States Army. Mauchly as a physicist had found himself (without realizing it) in the same predicament in 1941 that Aiken and then Stibitz

independently suffered: he was sharply aware that great quantities of data needed mathematical processing and that existing techniques were slow, cumbersome, unreliable, and hopelessly unable to meet the challenge. He concluded that electronic facilities ought to be exploited but found no one in electronics at work on the problem. So in the fall of 1941 he joined the Moore School of Electrical Engineering at the University of Pennsylvania, determined to obtain the electronics design assistance he sought.

By 1943 he, Eckert, and others at the Moore School, as well as Army Ordnance representatives, were convinced that a system of vacuum tubes and radio circuits, of standard form and established characteristics wherever possible, should be able to perform lengthy digital computations in the laboratory with the necessary speed and accuracy to provide values extremely useful for further perfecting greatly needed ballistics trajectory data. Their ensuing work on the ENIAC during 1944 and 1945 was carried on under wartime secrecy, but this did not keep it from coming to the attention of the Princeton mathematician John von Neumann, or of some of the electrical engineers at MIT. The machine went into operation in 1946 and was put to work on ballistics calculations as planned.

It would multiply two ten-decimal numbers in less than three thousandths of a second, compared to the three-second interval the Mark I required.

The ENIAC was deliberately designed to be of limited use and was not intended to be a general-purpose machine of wide application, for there were many engineering problems that its designers had to solve without ambitiously extending the capacities of their projected machine. In this respect they resisted the temptations to enlarge and improve that had caused Babbage to fail a century earlier. Aiken's non-electronic Mark I filled a wall, so to speak, fifty-one feet long and eight feet high, and Bell Aberdeen relay computer was housed in a room approximately forty by thirty feet in area. The ENIAC also was large, occupying the walls of a room approximately forth feet by twenty feet in size and including racks of assemblies on wheels in the center of the room. 18,000 radio tubes and 1,500 electrical relays went into its construction, together with plug boards, wiring, power units, and related equipment. Its internal storage was kept small, consonant with the mathematical chores it was expected to perform. The large size of this and other early computers was deliberately stipulated by the designers. Access to the components was what they

were interested in, so that unanticipated repairs and improvements might be made easily in these pioneer machines.

The ENIAC, being electronic, was not an engineering descendant of the Mark I mechanical machine. Instead, its physical equipment tradition traced back into the complex history of electronics in radio and telephony. The electronic computer tradition after the ENIAC rested not only on the ENIAC itself but on wartime developments in the pulsed circuitry of radar, on the well developed state of the art in radio tubes and circuitry, which was subsequently modified by transistors and solid-state circuitry, and on the logical abstractions of a mathematical tradition that included such names as Babbage, Edward Boole, and John von Neumann.

As had been the case with the abacus tradition and the mechanical calculator tradition, so it was with the electronic computer tradition, for this tradition arose out of the accumulation and synthesis of many strands of endeavor associated previously, as well as then and later, with other traditions. Contributing to this synthesis was the abstract logical, but not the equipment, tradition of the automatic sequence controlled calculators. The prototypes of Babbage, Stibitz, and Aiken, the first premature, the latter two within the practical state-of-the-art, had

come into existence as evolutionary consequences of a long and complex historical tradition of their own. When this calculator tradition was joined to the hitherto separate electronic traditions of radio and radar circuitry, their coming together seemed so natural, and the modes of application of circuits to computation and of computational and logical concepts to electronics were so provocative, that the specialists involved did not even wait for the first electronic computer, the ENIAC, to be completed, tested, and put into operation before they rushed on enthusiastically to more ambitious designs.

It was at this juncture of events that the MIT engineers working on the Aircraft Stability and Control Analyzer fell in with those who were engaging in the activities bringing together the calculator and the electronic traditions. It was Forrester's good fortune, as the leader of the group, to find himself standing at the intersection of the two traditions; his was also an instance of the innovative situation that Pasteur once characterized with the remark, "Chance favors the prepared mind." Forrester's mind was indeed prepared by the computer problems the aircraft

analyzer was posing at the time, and he grasped at this attractive prospect of a general solution.

It was also Everett's and Forrester's combined insight and vision to perceive, seize, and exploit the opportunities of joining and applying the abstract, elaborate, logical-formal tradition of manipulating discrete numerical quantities (upon which the mechanical-calculator tradition has been resting) to the engineering tradition of producing radio and radar equipment and, in the course of their enterprise, to expand and develop further both of these traditions. Their intuitive and analytical assessments of the rate at which such reduction to practice (for some reason it is never called "elevation to practice") could be accomplished was one key factor that was to make their particular computer unique in its time. A second key factor was the type of task they designed their computer to perform. In this respect their computer was so unlike all the others that for a while it appeared it would find no use to justify either its cost or its very existence. Then once again a separate tradition over which Forrester and his group had had neither

control nor influence -- this time taking the form of certain highly specialized preparations for waging war -- joined with and made use of the brand-new electronic computer tradition.

But that is getting ahead of the story. At the end of summer, 1945, the prospect confronting the MIT engineers was still that of a cockpit or control cabin connected, somehow, to an analog computer related to the design tradition of the electromechanical differential analyzer developed by Vanner Bush and Samuel Caldwell between 1935 and 1942.<sup>1</sup>

News of the successful operation of the MIT differential analyzer had been withheld during the war lest the enemy learn how useful and practical it was. Instead, intimations were deliberately "leaked" that it had failed to live up to expectations. The end of hostilities permit MIT to celebrate the true achievements of the analyzer at a meeting of computer experts which has already been mentioned -- that which Professor R. C. Archibald of Brown University had called in the name of the National Research Council, acting as chairman of the Council's Committee on Mathematical Tables and Other Aids to Computation.

The meeting took place on October 30-31, 1945 at MIT under the auspices of "Subcommittee Z on Calculating Machines and Mechanical Computation," and the purpose was expressed in the formal title: "Conference on Advanced Computation Techniques."<sup>2</sup> Among those who attended were Perry Crawford and Jay Forrester. The latter was particularly interested in reports of the design activity going on at the University of Pennsylvania. Prepared by his conversations with Perry Crawford and anyone else he had encountered who was knowledgeable, he was keenly receptive to the stated object of the Conference "to familiarize each member of the Group with present potentialities in the field, and to make known future developments." The program of papers to be delivered and especially the roster of expected conferees redoubled both his curiosity and his growing commitment to the new digital tradition then emerging.

Before two weeks had passed, he had visited the University of Pennsylvania to obtain more information and was inquiring into the design details of the ENIAC and its projected successor, the Electronic Digital Variable Automatic Computer (EDVAC).

It is difficult, if not impossible, to say confidently just when the realization struck Forrester that a novel solution -- the electronic digital instead of the analog mode -- had presented itself. Caldwell's suggestion in May that Forrester might find Stibitz's work significant did not precipitate the vital moment. Did the conversation with Perry Crawford in September, then? Or was it their discussion on the day Crawford left in October? In after years, Forrester recalled standing on the steps in front of one of the Institute buildings talking with Crawford, when the latter's remarks turned on a light in his mind.<sup>3</sup> His recollection is that from that time on, he began to consider the digital mode seriously.

Such authentic evidence must be used with caution, nevertheless, for historical analyses of inventive activity have shown time after time that our use of hindsight to reconstruct and evaluate crucial events responds to the logical and esthetic requirements of making sure that what went before fits rationally with what is later known to have followed. However satisfying and consistent such reconstructions are, even though based on first-hand knowledge and participation, they ignore this historical fact: that which came before,

was brought to pass without the knowledge of after events that hindsight subsequently bestows. Whatever the personal theory of the inventive process the innovator himself holds, and whether this theory has been rigorously thought through and made explicit in his own mind or whether it is rough-cut or assumed intuitively without demonstration, this is the innovator's private theory of the historical process -- although it is seldom regarded as the historical process, under such circumstances -- that enables him to reconstruct the past to his own satisfaction.

The intent of these remarks is not, of course, to cast doubt upon the value or honesty of personal recollections but rather to point out how very complex and how independent of both rationality and irrationality is the process by which new things occur. At the heart of the problem of historical reconstruction of events lies the grave risk of generating misunderstanding and confusion by unwittingly co-identifying our assumptions, and consequently our views, regarding the process by which new things occur, with the process itself. Experience demonstrates that most innovators have been too busy in their own fields of thought and action to work out a rigorous analysis of how events proceed. They consider

it a waste of time, if not an affront to their integrity, to consider how they know what they very well remember, or how well their recollections accord with prior and subsequent events in detail. Such considerations remain the professional responsibility of the historian as he seeks to approximate and interpret the past.

In any case, the turn of events that strategically affected Forrester's prosecution of the ASCA project is conspicuously visible in internal developments within the project, in the hiring practices and the efforts to secure the assistance of certain departments within the Institute, in the renegotiation of the Navy contract, and in Forrester's and Crawford's separate and joint efforts to explore the prospective application to other uses of a digital computer sophisticated enough to meet the requirements of the aircraft analyzer.

The months of November and December following Archibald's computer conference were put to use by Forrester in testing the immediate import and implications of the digital concept. It was a period during which he critically tested, revised, examined, and re-examined the value of the insight he had had regarding a solution to his

problem. If that insight were glitter without substance or too wild a dream, then it must be dismissed. If it continued to show promise, the grounds for this promise must be laid out and at least prospected in a preliminary way.

Accordingly, on November 9 Forrester, as part of his effort to shift the attention of his staff, held a conference of his own project engineers on the general subject of techniques in computation. The opening paragraph of the report of this conference, written by one of the project engineers, reveals how fundamental was the reexamination and the reorientation that Forrester was initiating:

Analogy type of computation has been under consideration, but there are other approaches which are highly thought of in mathematical circles throughout the country at present. It is the purpose of the present discussion to consider certain features of some of these.

After indicating the basic modes of calculation of the Harvard Mark I and noting that its mechanical calculating speeds were quite slow, the report turned to the "Pennsylvania technique" of calculating electronically with vacuum tubes. "Even though the machine they are building along these lines has not worked yet, there is

already a proposal to make another one which contains other features of importance to the aircraft analyzer problem," said the report.<sup>5</sup> The latter machine referred to was the EDVAC, which was intended to employ pulses in temporal, linear sequence and store its information "by the use of a thin column of mercury, referred to as a 'tank'" -- a mercury delay line.

A possible method of adding was next discussed, then a principle of operation of multiplying that would take only two or three times as long as a single addition. The report's closing remarks reflect how sharply Forrester and some of his engineers were examining the novel digital design trends: "These digital techniques are fundamentally processes of doing one thing at a time. Techniques for high speed electronic computation have not been worked out, and several months' work will be necessary to properly evaluate the process." Actually, the evaluation was already under way in the Servomechanisms Laboratory.

On November 13th, after visiting the University of Pennsylvania, Forrester requested from the Pentagon access to published technical reports on the EDVAC, pointing out that

his Navy aircraft research contract involved "the simultaneous solution of many differential equations, and the techniques visualized by the University of Pennsylvania on their EDVAC computer show considerable promise."<sup>6</sup>

By the middle of November he was looking for additional specially trained and talented young men to add to the project staff, men who could move with the old staff in the new direction they were heading. He sought men who had records of special competence as excellent graduate students or as well-qualified radar experts. Lip-service to the policy of top-quality work from top-quality personnel would not suffice; he would consider only men of exceptional promise and not waste his time on the run of the mill. Thus, in a letter of November 16th to the MIT Radiation Laboratory he asked for men with a radar design background, "experienced in the generation and handling of low-power level signal pulses, in applications which will require pulses a part of the microsecond long and spaced a microsecond apart."<sup>7</sup> Suitable prospects ought to be between 25 and 40 years old, he specified, "and of doctor's degree caliber although selection will be based on the man's experience, cleverness, ingenuity,

and references rather than on his academic degrees." He closed the letter on a characteristic note: "I wish to stress the need for cleverness and ingenuity in the field of coded pulses."

He was convinced, from his wartime experience, that it was more efficient to pay a higher price for one really good man and then give him his head, than it was to pay less for three average men and have to lead them by the hand. He felt he possessed the experience and the judgment to enable him to recognize the difference, and although he did not expect his every selection of a new man to be error-free, he was adamant about maintaining exceptional standards in choosing design and test engineers and mathematicians. Over the years ahead this policy brought in many promising men, some of whom went on to important industrial, engineering, and consultative achievements in their subsequent careers.

The high standards that Forrester and his associates came to insist upon, with the endorsement of Brown and Sage in the background, inevitably attracted unfavorable comment from some outsiders as time passed, eliciting

off-the-record remarks that the project personnel were arrogantly high-hat and snobbish, working in a building closed by security regulations to outsiders, that they were as unrealistic about what they were doing as they were young and immature, and that theirs was a "gold-plated boondoggle," extravagant in its demands, in its rewards, and in its raids upon the taxpayers' purse.

Unquestionably, it became for the project members a deliberate policy of saving development time and money in the long run by insisting on going "first class." Drawing together as it did young men of ambition, ability and spirit, and reinforced by a habit of daily operations that stressed and, for the most part, obtained intelligently planned and coordinated operations, this policy produced an unusually high esprit de corps. As one project member recalled years later, viewing the operation from the perspective of a personally successful administrative engineering career in the computer hardware business, "We were cocky. Oh, we were cocky! We were going to show everybody! And we did. But we

had to lose some of the cockiness in the sweat it took to pull it off."

Forrester's philosophy of acquiring high-caliber staff members became the project's continuing policy, and representative events that helped bring this about are to be seen in measures he took to enlist the aid of certain departments at the Institute, even as he was beginning to explore in earnest the possibility of using the still thoroughly "new-fangled" digital computer, a successful vacuum-tube model of which had never yet been put into operation nor any other electronic model, for that matter. A week before Christmas he was suggesting active consultative arrangements. "I feel," he wrote to Professor Henry B. Phillips, "that the Mathematics Department can make an outstanding contribution to our work in studying proper set-up procedures and techniques to be used in digital computation," and he suggested that exploratory conferences with specified individuals be arranged to generate momentum.<sup>8</sup> Already, since early December, lecture notes on mathematical analysis by mechanical methods were being provided to ASCA personnel

from a course given by Professor Samuel H. Caldwell of the Electrical Engineering Department.<sup>9</sup>

Forrester's letter to the Physics Department was more detailed than his letter to Professor Phillips, and it revealed his awareness of the prowess a digital computer would possess if it could meet the requirements of the Aircraft Stability and Control Analyzer: "It is our hope that the solution of the equations involved can be accomplished by electronic techniques. If so, the computer also will be capable of solving many other problems in the fields of mathematics and physics." Such a computer, he recognized, would be "of much greater scope than any other now in existence or being considered for the immediate future."<sup>10</sup> He hoped that they might be able to obtain the "active participation of certain men whose primary interest is in physics, but with a secondary interest in our work." He named a couple of graduate students then matriculating for the doctorate as the sort of talent he was looking for, and he indicated that suitable appointments could be made either through the Electrical Engineering Department (with which the Servomechanisms Laboratory was affiliated) or through the Physics Department, as they preferred.

At that time he thought of the program in prospect as comprising two major parts: a six-month "preliminary survey of electronic computation possibilities," and a three-year design and construction phase.<sup>11</sup> This view, which had come out of discussions and personal reflections before the end of November, sifted out the following considerations: If the analogy method of solution were pursued, using a physical representation of the problem and measuring the physical quantities of interest, and if, to this end, the Differential Analyzer type of analog machine were used, it would unfortunately not be suitable for the ASCA problem. To represent the quantities by electrically interconnected mechanical shaft rotations, for example, would require a device of extreme complexity. Further, the length of time to accomplish a solution would be prohibitively impractical, "because equipment will not respond fast enough to give the pilot proper 'feel'."<sup>12</sup>

On the other hand, if ASCA computation were performed by electrical voltage analogy instead of mechanical shaft rotation, as Forrester and his assistants had been thinking

of doing, then one encountered the technical difficulties of insufficient signal range. These along with the factors of the physical tolerances which could be achieved and of the friction which would be encountered, might severely limit sensitivity and accuracy of solution. A change in equations or problems would require elaborate new physical hook-up of the operating components. However, analog solutions ought to be exact, in theory, and the analogy technique was well known and well established for relatively simple problems. Such were the pros and cons on the analogy side of the question.

On the digital side, numerical analysis by arithmetical processes would replace analogous physical quantities and could solve the entire set of ASCA equations, given time enough. If there were not time enough, then there would have to be speed enough. There would be no physical tolerances or friction to contend with, and although digital techniques were not so well established as analog, experience was being acquired with the Harvard Mark I, the Bell Telephone Relay Computer, and the University of Pennsylvania ENIAC then approaching completion. "Most new plans," Forrester observed, "lean toward the binary system" of notation, and

the vacuum tube, he knew, was a more reliable -- and incredibly faster -- binary (on-off) device than a relay.<sup>13</sup>

Considering the prospects of a hypothetical electronic machine, then, one might expect two types of storage, mercury delay line storage and electrostatic tube storage, and suitable computation and control circuitry.

He saw various mathematical and engineering advantages in digital computation, not the least of which were the prospects that construction costs might be less and trouble location easier. Problems could be set up more rapidly and since their solution must progress one step at a time in the digital mode, then "the problems of a large, interconnected, simultaneously operating analog computer network are avoided." Finally, the computer could be "used for many problems other than aircraft analysis."<sup>14</sup>

Nevertheless, the digital disadvantages were formidable. Although construction costs might be lower, development costs might be higher, for digital techniques and devices were not well known or well established. Development would take more time, but construction should take less. Further, he reflected realistically, the ASCA problem "requires pushing the digital technique well beyond anything even contemplated up to the present time."<sup>15</sup>

Although many circuits appeared to be ready for development and use, five or six months of intensive study would be necessary in order to find whether the first promise were as substantial as it appeared. Forrester concluded that such a study must be made. The prospects were sufficiently attractive and enough people were optimistic about the future of digital computation to warrant proceeding a step further. Was this a prudent decision? It certainly was not without risk. "This group at the present time," he wrote, "has no concrete information on which to predict the outcome of an investigation."<sup>16</sup>

By mid-January 1946, the investigations which Forrester and his associates had been carrying on since October gave Forrester sufficient confidence to recommend to the Navy that "numerical electronic methods as applied to the aircraft analyzer be carefully investigated." If the digital computer could be successfully developed, their proposal noted, the rewards would be great; among them would be "more reliable performance, higher accuracy, lower cost, smaller size, and more flexible operation."

In addition, the digital computer would permit the "solution of many scientific and engineering problems other than those associated with aircraft flight."<sup>17</sup> This last comment contained within itself the embryo of the general multi-purpose computer which was increasingly to become the primary goal of the project.

Digital computation techniques and methods, however, were not accepted immediately as a panacea. Extensive research was necessary, with all the ramifications such a program entailed, but the advantages appeared so attractive that the Institute proposed that the new method be thoroughly explored. To this end a contract was proposed which sought the accomplishment of two tasks: (1) the design of a digital type computer adequate to the requirements of the aircraft analyzer; (2) the adaptation of the computer to the analyzer, and the design of required associate equipment. The two tasks would overlap chronologically. Task One would commence immediately and pursue intermediate objectives until December 1949, when the whole project was to be completed. Task Two would phase in around December 1946, but only after a "final decision on the practicability of

electronic computation as related to the aircraft analyzer" had been made. Task One, it was estimated, would cost \$1.910,000; task two \$477,600 -- a total of \$2,388,000 for the completed project.<sup>18</sup> Such was their thinking at the beginning of 1946. If the projected rate of progress was overly optimistic, it nevertheless provided timed goals to aim for.

Simultaneously with the submission of the proposal to the Office of Research and Inventions, Forrester replied to a request made earlier by Lieutenant Commander H. C. Knutson of the Special Devices Division of the Office for "comments on the applications of high-speed electronic computation."<sup>19</sup> Forrester's reply contained much of the substance of ideas that had emerged from previous discussions he had held with Perry Crawford on the subject. The digital computer, Forrester predicted would possess a flexibility not possible with the analogue computer, permitting therefore, the construction of a "Universal Computer" with definite possibilities for military application in both tactics and research. In tactical use, it would replace the analogue computer then used in "offensive and defensive fire control"

systems, and furthermore, it would make possible a "coordinated CIC (Combat Information Center)," possessing "automatic defensive" capabilities, an essential factor in "rocket and guided missile warfare." In military research, electronic computation made possible wide and diversified research programs in "dynamic systems": (1) aircraft stability and control; (2) automatic radar tracking and fire control; (3) stability and trajectories of guided missiles; (4) study of aerial and submarine torpedoes including launching characteristics; (5) servomechanisms systems; and (6) stability and control characteristics of surface ships. Digital computation, furthermore, would allow the "study of both interior and exterior ballistics" and "stress and deflection studies in ship and aircraft structures."

Leaving the areas of possible military application, Forrester turned to a detailed analysis of the implication of high-speed electronic computation for scientific and technical research in general. Here he predicted widespread opportunities in the fields of (1) nuclear physics, (2) thermodynamics, (3) compressible fluid mechanics,

(4) electrodynamics, (5) mechanical engineering, and (6) civil engineering. He further considered its application to statistical studies in both the physical and social sciences. In the latter sciences alone, he observed, it would be of value to government agencies and departments.

He concluded his response with the following comment:

The development of electronic digital computation is only beginning, and considerable effort and money will be expended in achieving the equipment to meet the above objectives. Once sufficient development is completed, however, the cost of duplicating electronic computing equipment will be less than for other forms of computers. Beginning with a suitable basic design, new computers could be built with facilities for a specific magnitude of problem by adding or omitting standardized memory or storage units without requiring significant redesign.<sup>20</sup>

The proposal that had been made in January to the Chief of the Office of Research and Inventions was resubmitted the following March in revised form. Substantively, the revision differed little from the original: it requested that the date of completion be extended to June 1950 and that the total allowable expenditures be increased from \$2,388,000 to \$2,434,000. In addition, a summary of Forrester's letter to Knutson was embodied in the revised proposal.

Four principal tasks were delineated by the Institute:

1. "Research, development and contruction necessary to demonstrate digital techniques of the type required for the final computer."
2. "Design of a computer which is adequate for the aircraft analyzer problem."
3. "Construct and assemble the computer and associated equipment for control and stability studies on aircraft."
4. "Operation of the complete equipment for the solution of aircraft stability problems and application of the computer to other types of scientific computation."<sup>21</sup>

Although the revised proposal submitted to the Office of Research and Inventions was an Institute document, it also reflected the influence and ideas of those at the Special Devices Division who were immediately responsible for Navy administration of the trainer-analyzer project. The rapport between the two groups was sufficient to produce fruitful joint discussions in which a proposal acceptable to the Navy could be worked out. Actually, the Special Devices

Division in February 1946 had recommended to its parent organization, ORI, that the Institute's proposal be accepted and implemented. Hence, it is probable that the March revision reflected from the Office of Research and Inventions on the original proposal. The revision was then transmitted to the Institute by the Special Devices Division. As a consequence of whatever internal adjustments were made, the Office of Research and Inventions incorporated Tasks One and Two of the March proposal into Contract N5ori-60. This contract superseded the earlier Letter of Intent for former Contract Noa(s)-7082 and became retroactively effective to June 30, 1945.

Under Task Order I of the new contract, the Institute was to undertake first the construction of "a small digital computer involving investigation of electric circuits, video amplifiers, electrostatic storage tubes, electronic switching and mathematical studies of digital computation and the adaptation of problems to this method of solution." Second, it was "to design an electronic computer and aircraft analyzer based on Phase 1 of this Task Order."

Phase One was to commence as of the date of the contract and was to terminate on June 30, 1947. The second phase, commencing on July 1, 1947, was to terminate on June 30, 1948. The total cost of the contract was set at \$1,194,420; the first phase would require \$666,000, the second the balance of \$528,360.<sup>22</sup> These costs were in agreement with the amounts set by the Institute in its revised proposal of March 1946. In that month, also, the Navy revised its initial specifications to conform to the changed conditions and goals. Finally, in the revised specifications, the project was given the name by which it was to be known in the future: "Whirlwind."<sup>23</sup>



## NOTES TO CHAPTER 3

1. V. Bush, S. H. Caldwell, "A New Type of Differential Analyzer," Journal of the Franklin Institute, Vol. 240, no. 4 (October 1945), pp. 255-326; S. H. Caldwell, "Educated Machinery," Technology Review, Vol. 48, no. 1 (November 1945), pp. 31-34.
2. Conference program, October 30-31, 1945, entitled: "Conference on Advanced Computation Techniques, National Research Council, Committee on Mathematical Tables and Other Aids to Computation, Subcommittee Z on Calculating Machines and Mechanical Computation; Cambridge, Massachusetts."
3. Interview, Jay W. Forrester, by the authors, July 31, 1963.
4. Conference Note C6, written by Kenneth Tuttle, Subject: "Conference on Techniques of Computation held November 9, 1945," November 14, 1945.
5. Ibid, p 3.
6. Letter, airmail special delivery, J. W. Forrester to Office of Chief of Ordnance, November 13, 1945.
7. Letter, J. W. Forrester to James W. Walsh, November 16, 1945.
8. Letter, J. W. Forrester to Prof. Henry B. Phillips, December 17, 1945.
9. Eng. Memo No. 3, Subject: "Lecture Notes - Part I . . . , " December 5, 1945.
10. Letter, J. W. Forrester to Prof. John G. Slater, December 17, 1945. A carbon of each of these letters went to N. Mel Sage and Gordon S. Brown for their information.

NOTES TO CHAPTER 3 (CONTINUED)

11. Ibid.
12. Conference Note C9, written by J. W. Forrester, Subject: "Outline of Discussion on Digital Computation as Applied to the Aircraft Analyzer," November 28, 1945.
13. Ibid, p. 5
14. Ibid, p. 8
15. Ibid, p. 9
16. Ibid.
17. Ltr., N. McL. Sage to Chief, Research and Inventions, USN, January 16, 1946; see also ltr., A. P. Bencks, Lt. USNR. SDD (Washington), to N. Sage, DIC, MIT, November 27, 1945.
18. Ltr., N. McL. Sage to Chief, Research and Inventions, USN, January 16, 1946.
19. Ltr., J. W. Forrester to Lt. Cmdr. H. C. Knutson, SDD, ORI (Washington), January 28, 1946.
20. Ibid.
21. Memo, J. W. Forrester to N. McL. Sage, February 25, 1946; memo, N. McL. Sage to J. W. Forrester, March 16, 1946.
22. Navy Department, Office of Research and Inventions, Contract Number N5 ori-60, June 30, 1945, "Task Order I -- Constituting a part of Contract N5ori-60 with the Massachusetts Institute of Technology and superseding BuAer Letter of Intent for Contract NOa(s) 7082," (The documents do not explain the apparent \$60 discrepancy between the two totals.)
23. Navy Department, Office of Research and Inventions, Special Devices Division, "Specifications for Project RF-12 known as WHIRLWIND," revised March 20, 1946.

## PRELIMINARY DESIGN EFFORTS

At the end of 1945 the computer was still the tail of the dog, so to speak, and the Aircraft Stability and Control Analyzer was the dog. A year later, judging by events within the Servomechanisms Laboratory, the tail had passed through and beyond the point of wagging the dog and had become the dog. The Analyzer became the tail, and even that was cropped in 1948 when the cockpit was junked.

It could be argued in after years that abandonment of work on the rest of the Analyzer was a serious tactical mistake, for as the Analyzer faded further into the background, so did the once-obvious immediate practical relevance of the untried computer become more remote and more nebulous. The overriding pragmatic question in the spending of military funds was, classically, "What's it good for?" Although the answer became dazzlingly clear to Forrester and his associates in the project and

was perhaps earlier as clear to Perry Crawford, it seemed to become less clear to outsiders almost in proportion to the rising costs, as the months and years passed.

Early in 1946 Forrester at the Massachusetts Institute of Technology and Crawford in the Navy separately saw five years of research and development work ahead before demonstrated success would be perceived and appreciated. To Forrester it became a goal that required the sort of monthly pace a \$100,000-a-month budget would provide.

At the end of the first week in January he was pressing his search for the men he needed. "The general type of man whom we need," he wrote in a letter asking Nat Sage for assistance, "should have originality and what is often referred to as 'genius.' He should not be bound by the traditional approach.... I do not know of suitable prospects...."<sup>1</sup>

While he was beating the bushes for top personnel, he set up ten divisions in the Laboratory - seven to carry on the technical work and three to support these -,

ordered a weekly meeting of each division at a time when he could be present, created a coordinating committee of his divisional leaders, and called for a stepped-up delivery of reports on technical progress and problems. "The Navy expects, and rightly so, to be informed of research and development progress through suitable reports," he pointed out to his staff in an early "Conference note."<sup>2</sup>

By the end of February he had a firm enough prospective schedule, based on discussions with the Navy, to call several tasks and their time schedules to the attention of the engineers on the project. These tasks covered the time period from July 1945 to June 1950.<sup>3</sup> According to the new schedules, the last six months of 1945 had been devoted to completion of studies in analog computation, to preliminary investigation of digital electronic methods, and to plans for carrying on the latter. Characterized in this manner, the project gave the impression of being routinely in command of its situation at all times, and this was an impression that Forrester sought naturally and by design to convey to his

But had they been in command of the situation at all times? This was a question which Navy programmers and administrators were to raise later more than once, not only regarding this particular period in the affairs of the project but regarding later periods as well. There were some critics who came to feel that Forrester was attempting to gloss over the brute fact that the project had had to abandon its first intention to build an analog computer, just as, later, it abandoned the Aircraft Analyzer. Forrester and his associates had made a false start, ran this argument. What was to be gained, then, beyond self-deception and false impressions conveyed, by describing the situation otherwise? And why try to deceive his own engineers, many of whom had been intimately involved, by such statements in his published schedule?

The answer is to be found in part in Forrester's style of conducting his affairs and in part in the character of the research-and-development process. He saw himself as best carrying out his directorial function by shielding his men from potential outside interference that would interrupt their progress and, at worst, demoralize their

enterprise. It was his responsibility to see that the project had what it needed to proceed with its investigations and to not distract the efforts required to proceed by making the personnel of the project privy to external administrative, policy and fiscal problems that they were not qualified to handle, that they were not hired to handle, and that they could do nothing about in any event. Forrester saw no reason to allocate time for his engineers to stand wringing their hands.

Since both he and his staff understood the difference between the known and the unknown and between the predictable and the unpredictable in engineering research, no false illusions were being generated within the organization by putting the best face on the fact that preliminary views and preliminary investigation had yielded unforeseen negative information that stimulated the discovery of an affirmative alternative. As Forrester well knew, an engineering problem was also an engineering opportunity the validity of which could be affirmed only by the finding of a solution. The digital computer offered a challenging and exciting solution, indeed. So he chose to regard the

last six months of 1945 as a period devoted to analogue and digital computation studies rather than as a period of crisis, and although it had been a time when far-reaching decisions were made, these did not constitute a serious crisis, in his view.<sup>4</sup>

He gave his team a year to lay its plans for building a digital computer, another half year on simulator cockpit studies and equipment and on logical designs and bench-test models, a third year to work up final equipment and circuit designs and to begin work on final components, a fourth year to build the prototype computer and receive the cockpit from the Navy as an item of "Government-Furnished Equipment," and a fifth year to finish, test, and deliver to the Navy the completed analyzer. In the middle of 1950 the machine would go into full operation.

He visualized four tasks. The first would produce a small digital computer that could perform the basic functions and would see the accomplishment of basic theoretical work by the middle of 1947. The second would begin before Task I ended, would last for a year, and would lay out the basic designs for the cockpit and

prototype computer. The third task would overlap the second and in a year produce these components of the prototype Aircraft Analyzer, and the fourth task would produce the working, tested Analyzer a year later.

In conclusion, Forrester stipulated a policy of periodic review "because of the indefinite nature of the problem and dependence upon ideas which have not yet been formulated." He also made it clear that the schedules and tasks described were not fixed for all time, that indeed the arrangement simply reflected present thinking.

By the middle of March 1946, Forrester had set up a flow of internal information among the project engineers that he intended would indicate what each investigator was doing every two weeks along various of the following lines of investigation:

Block Diagrams

Computing Circuits

Mathematics

Mechanical, including Cockpit

### Mercury Delay Lines

### Storage Tube Research

### Other Electronic Problems<sup>5</sup>

He took it for granted that this beginning arrangement would be improved upon, and it was altered as necessary in the following years.

Of the lines of investigation indicated, only the "Mechanical, including Cockpit" represented a continuation of an earlier line of inquiry, and even that was affected by the knowledge that devices must be developed to convert the digital, electronic-pulse data into mechanical forces and motions affecting the pilot and the cockpit. Further, the responsive forces generated by the pilot's movements of the mock controls must be converted back into corresponding digital pulse data. These problems were not impossible, but neither did established solutions exist. The digital computer was too new.

Forrester's appraisal of the overall situation with respect to computer design caused him to consider more than one aspect of the storage problem; while mercury

delay lines as proposed for the EDVAC appeared quite promising, so did the use of special radio tubes.

Forrester and his associates began to survey the state of the art in this specialized area.

Computing circuits composed a category worthy of several engineers' attention, for these circuits would carry out the electronic operations which would perform the appropriate arithmetic and calculating operations in the digital mode.

It was visualized at first that the Block Diagrams Group, the Mathematics Group, and the Electronics Group, especially, would construct a symbiotic relationship in which each would create necessary information for the other. But in the state of engineering art as it then existed, so unformed with respect, on the one hand, to an Aircraft Analyzer, and, on the other hand, to a digital computer, their relatively vast mutual ignorance imposed contingent restraints that hobbled them together. Perceiving this, the Mathematics Group sought to work its way out of their mutual predicament of ignorance by considering ways and means of attacking the aircraft equations

that Markham and Bicknell had provided, as modified and extended by L. Bernbaum and Bicknell.<sup>6</sup> "We have decided," reported the head of the group, Hugh R. Boyd, "to work on rather short specific problems and gradually build up sufficient data and experience in numerical methods to enable us to attack the aircraft problem effectively. This preliminary work would also serve to build up our knowledge of other types of problems which our computer would solve effectively."<sup>7</sup>

The Electronics Group became several groups, oriented to the things they were working with such as circuits, pulse transformers, mercury delay lines, and storage tubes. They were component-oriented, and they realized that decisions from the Block Diagrams Group would give them information about more elaborate components and their systemic relationships. A demonstration adder, clock pulse generator, switching arrangements, and electrostatic tubes were among the devices under study and construction during the spring.<sup>8</sup>

The task of the Block Diagrams Group, as described by its head and only full-time member, at the time, Robert Everett, was "in general, to devise a complete

computer system, including definitions of all components, interconnections of these components, [and the] sequence of operations."<sup>9</sup> At the same time that the Mathematics Group would be a source of information about computer requirements, the Block Diagrams Group would be ascertaining machine computing techniques, programming techniques, and component designs for accomplishing computing, storing, switching, and programming.

The hindsight of experience showed the attempted correlations of the responsibilities of these groups to have been at once reasonable and naïve. Had they been mathematicians instead of engineers, the young men involved might have placed the power and responsibility to lead the way in the hands of the Mathematics Group. Here, too, they would have been reasonable and naïve. But they did not, and the Block Diagrams Group became the leader as the months wore on.

The Block Diagrams Group, meanwhile began to analyze possible ways to proceed. By early April, with Everett assigned to the job full time, Steve Dodd assigned 1/5 time, Pat Youtz 3/8 time, and P. Tilton 1/2 time,

the Group found open to it "a great number of system possibilities ranging all the way from the completely serial or sequential method described by Von Neumann, where no two operations are performed at once, to a completely parallel method where all operations are carried out at once, including digit transmission."<sup>10</sup>

The latter method would be equivalent in complexity to an analogy type solution, Everett felt. He saw that the range of possibilities represented "a complete range of solution time and a complete range of complexity and duplication of equipment. Some intermediate complexity must eventually be chosen, the criterion being that the total equipment must be as simple as possible but still provide the required solutions."<sup>11</sup>

Everett went on to point out that three considerations dictated the course of action of the Block Diagrams Group.

(1) The Mathematics Group had to determine the mathematical phrasing and solution procedures of the Aircraft Analyzer equations, in order to know the "maximum expected total of operations required in a fixed time period."<sup>11</sup>

(2) The Electronics Group had to ascertain "the time required for a single operation." (3) The Block Diagrams Group had to acquire a knowledge of components that would "allow the most efficient paralleling of equipment, to satisfy" the requirements the other two Groups were working with. These strictures made it apparent to Everett that "no final system block diagrams can be developed for a long time," although final designs of components could probably be estimated closely enough to provide these elements of the system when they were needed.

Since explicit system parameters were as yet unavailable, Everett proceeded to consider the order of magnitude of data, orders, and solution procedures a computer might be expected to handle when coping with operations of the Aircraft Analyzer. Acting upon the preliminary assumptions that he thus constructed and proceeding in the direction indicated by the ENIAC and EDVAC enterprises of Eckert and Mauchly, Everett envisioned a machine that would have a total storage of about 8,200 words or less, that would accommodate a word length of about 30 binary digits,

that would round off numbers as a fixed policy to begin with, while the problem of errors resulting from rounding off would be taken up later, that would operate its storage tubes serially, that would use a high-speed multiplier, that would perform input and output operations simultaneously, and that would operate as a sequential machine.

Everett suffered no illusions about what he and his Group did not know; a first purpose of their early efforts would be "to learn as much as possible about computer techniques and problems." At the same time, they would be providing the Electronics Group with "preliminary specifications to enable them to better direct their efforts."<sup>13</sup>

Thus the young engineers under Forrester's direction spent the year of 1946 exploring possibilities, selecting from these the arrangements, designs, requirements, practical limits, characteristics, theoretical models, and bench-test items they found promising. Some worked on hardware designs. Some worked on mathematical procedures that would be amenable to machine handling and

machine solution. Some worked on the problems peculiar to creating a machine - the Analyzer and its computer - that, to work properly, must consist of an integrated system of component electronic and mechanical mechanisms and sub-mechanisms. However efficiently and reliably a particular circuit or subassembly might perform its functions when tested by itself, how would it work when interconnected with other circuits? Would an array of these generate sufficient "noise" - residual currents, stray impedances and interference, back emf's, and the like - to cause a theoretically simple arrangement to become inordinately complex as a consequence of making it work in practice? Especially important were the policy-level design decisions that would give the system its basic character. Should a storage assembly acquire its unit of information (technically called a "word") bit by bit or should it acquire it all at once? If mercury delay lines were used, means of inserting information essential to the calculating processes of the computer (interpolating) must be provided, complicating the circuitry. Since pulses of electric current constituted the basic signals

the computer would use, the timing and routing of these must be finely controlled at all times. Since the digital mode of operation meant that fresh signals were used either to alter the character of earlier signals or to alter the character of a patterned arrangement of earlier signals, a "domino" effect was a prevailing feature. If just one "domino" fell the wrong way, if just one signal were mistimed, misrouted, or were to cause a wrong radio tube to operate, or if one tube or circuit malfunctioned and no "back up" component were there to compensate for the failure, then all of the rest of the calculations to follow would be in error. In homely analogy, the computer was an intricate array of "bucket brigades," and if one bucket failed to be passed on, then the entire operation was nullified.

On the other hand, even though for want of a nail a kingdom could be lost, reliability and coherence were practical possibilities because of the "building-block," or modular, construction that was possible. A reliable gate circuit could be inserted wherever it was needed, like a building-block in a wall. The digital computer

would be a more complex piece of machinery than, say, any automobile, yet it could employ the same submechanisms over and over, as a tree does by employing not one leaf but hundreds simultaneously. Unlike the tree, the computer must have its modular submechanisms - its multivibrator "flip-flops," its gate circuits, etc. - interconnected in contingent patterns in such a way that the static hook-up of tubes, wires, resistors, condensers, diodes, and the like could accommodate and effect a dynamic, ordered pattern of flow of radio pulses.

Long before spring, in 1946, the project engineers had passed beyond these simple considerations, which have been represented here in oversimplified language, to the more sophisticated design and construction challenges of working on the detailed technical specifications. Forrester had perceived at the start that although the mercury-delay-line storage principle possessed many attractive features, it might prove slow for the needs of the Aircraft Analyzer, especially when part of a serial, or sequential-pulse, machine. Flip-flops could be used as storage devices, but in a simultaneous-pulse machine,

so many tubes would be involved that keeping them all replaced and operating would be well-nigh impossible.

Electrostatic storage offered an attractive alternative principle. Various investigators in the field of vacuum-tube research were working upon applications of this principle, by which a minute spot on a signal plate could be negatively or positively charged and hold, or store, that charge long enough to be useful. The RCA "Selectron" tube, the Williams tube (named after its British inventor), and an electrostatic tube developed by another MIT laboratory, the Radiation Laboratory, were among applications that attracted Forrester's attention as 1946 wore on, and by autumn he, Steve Dodd, who had been carrying on preliminary tests, and their associates decided to modify the MIT tube design to fit their particular needs.<sup>14</sup>

In the meantime, Everett and his group had found compelling reasons to discard the sequential mode of pulse operation and adopt the higher-speed, simultaneous, or parallel, transmission of digits (pulses) among the circuits of the machine.<sup>15</sup> A high-speed, parallel-digit

multiplier appeared promising for the same reasons, the most important of which was the speed of computation required if the Aircraft Simulator were to work.<sup>16</sup>

Not only did the parallel-signals computer look both promising and feasible, but also all knew that the time was rapidly approaching when the project must "either fish or cut bait." Forrester had pointed out in early June that "we are not yet in a position to decide what must be built by next June until we know the basic principles we are to use as a foundation...."<sup>17</sup> Viewing the performance limits within which the first computer they proposed to build must operate, Forrester pointed out that the contract with the Navy "calls for a model computer which will, at the very minimum, demonstrate operating principles which we plan to incorporate in the aircraft analyzer. At most, it may become a computer which will be useful for solving a variety of other problems."<sup>18</sup> If the latter multi-purpose type of computer were decided upon, then, Forrester advised, it might become necessary to extend the estimated terminal date for Phase One of the contract beyond June 1947.

What technical information might conceivably guide them in establishing the basic design parameters, so that they might proceed to build their first computer? Forrester was ready with a provisional answer: it would depend "largely upon the information forthcoming from the electrostatics field."<sup>19</sup> This policy view of June 1946 had hardened by December into the decision to build a pre-prototype computer, "in view of the probable complexity of the prototype computer which might include some 3,000 tubes...."<sup>20</sup> The pre-prototype, a "simpler experimental computer," would "test the components of the computer and a system made up of them, the system being capable of doing test computations."<sup>21</sup> It would "provide a system in which to test new components or types of operations as they become available." It would "check reliability and evaluate mechanical design and maintenance problems." And it would be operating in six months.<sup>21</sup>

When electrostatic storage tubes had been perfected, these could be substituted for the more primitive storage devices that initially would be provided for test purposes.

Standard electronic and relay racks accommodating removable assembly bases (plug-in chassis) 17 inches deep and ten inches wide, would be used. They were readily available and permitted easy accessibility to and testing of the hardware of which the computer would be composed.

Project Supervisor of the pre-prototype would be Forrester. Harris Fahnestock (who had joined the staff earlier in 1946) would be in charge of Production. Everett would be in charge of the Block Diagrams Division, Leon D. Wilson would head the Computer Division, and David R. Brown would head the Electronic Engineering Division.<sup>22</sup> Within the already existing organization of Project 6345, this redirection of operations was an evolutionary phasing-in of more specialized activity; it did not abruptly alter the entire conduct of affairs. It represented the sharper focusing of operations that Forrester and his associates felt was now possible after a year of engineering research.

The extent and magnitude and detail to which their studies had carried them have only been suggested in

this nontechnical account. Whether they had used their time and energies wisely is difficult to determine. They had worked hard, they had learned a great deal. They had added carefully selected engineers to their staff, as well as bright young graduate students looking for subjects for Master's Theses in Electrical Engineering. When Forrester encountered a "business-as-usual" attitude among government suppliers of surplus equipment his project needed, he waited until he had clear evidence the responses were less than reasonably prompt and intelligent and then used it to clear up the "bottlenecks" and to ensure that, for a while, at least, they would get better service. This was a never-ending battle and a normal one, with private and governmental suppliers; from the point of view of the project workers, they were never long in need of what they required, nor were they continually being held up by lack of funds and materials and technical facilities.

"We got what we needed," recalled one engineer, "and since there was such an extensive exchange of information going on, it was hard to get out of line or to order

something that on one else could imagine why you'd need it. We were given our heads, but we were held accountable. You never knew, in the early days, when Jay [Forrester] or your supervisor would stop by to see how you were doing. There was never any question but that they were there to help, and there was never any question but that they expected you to know what you were doing. Those that didn't, somehow moved on out. Jay was pretty good at figuring out what it was that a man could do that would help the work along. Many of us were going to class and had homework, and once things really got going, we could work morning, afternoon, or evening. You just followed the most intelligent course of action."

Whatever efficiency of the project is attested to by remembered high morale, the fact remained that as the end of 1946 approached, it began to look increasingly as though the six-months completion date of the pre-prototype computer could not possibly be met.<sup>23</sup> The details simply could not be worked out rapidly and reliably enough. In the longer run of affairs, however, the overall progress

of the project hinged less upon any specific practical problem or accomplishment than upon the concurrent investigations of many paths that appeared promising. These investigations, from the spring of 1946 to June of 1948, involved the exploration of both phases of Contract N5ori-60 as outlined in Task Order I. But increasingly the emphasis was placed upon Phase One, development and construction of the digital computer.

In the fall of 1947, following conversations that Forrester and Everett had been carrying on with Navy Special Devices technical personnel at Sands Point, the two young engineers prepared two technical memoranda which were studies of possible applications of the digital computer to naval warfare. The first of these was "a brief study of a simplified version of the anti-submarine problem." The second, issued two weeks later, was more ambitious in its scope and followed naturally from the first. It presented "in rather general terms some possibilities in the arrangement and use of high-speed digital computers for the analysis, evaluation and intercommunication of

information in an anti-submarine naval group.<sup>24</sup> To the best of their knowledge at the time and in after years, Forrester and Everett knew of no earlier practical engineering work on how the logic of computers could be applied to interpret radar data.<sup>25</sup>

The two reports taken together represented an informal proposal for practical military application of a computer the like of which had not yet been built, although Forrester and Everett specifically had Whirlwind in mind. "For the simplified problem selected," they wrote in Report L-1, the Whirlwind I computer is entirely adequate for a problem involving 10 ships, 5 submarines, interconnecting radar and sonar data, and depth charges in any number up to 20 preset units and 20 proximity-fuze units in the water at one time.<sup>26</sup> While the first report was interested primarily in examining how a destroyer could acquire target data and translate these into depth-charge firing orders by means of a computer, the second report was concerned with the all-important details of the sort of communication among the ships of an anti-submarine task group that would provide true combat information and control as the battle situation

was developing. Accordingly, the second report examined the following example in detail: "Five surface ships and one aircraft are illustrated with two targets, one surface, and one submerged. All units collect such information as they are able by the various methods noted. The computation and information system must make use of this total body of information to the best possible advantage."<sup>27</sup> The problem they then set up and explored in detail would require, they concluded, "one-half the storage capacity and one-third the operating time of WWI."<sup>28</sup>

There was no question in their minds that the computer they were getting ready to build would be able to handle such problems with capacity and time to spare. Both they and the Navy Special Devices engineers enthusiastically realized that they were contemplating a revolutionary device which would contribute immeasurably to the efficiency and accuracy of solving target problems in actual battle operations, but they knew also that they could as yet only talk about "paper operations." Actual testing in practice lay in the problematic future, and while they were convinced more than ever, after

these detailed studies, that they had a general-purpose computer of a practical type truly in prospect, their more immediate problems late in 1947 lay in the realm of translating their ideas further into engineering designs and their designs further into working hardware.

As their efforts came to be more and more completely devoted to working out the engineering intricacies of the projected computer, the Aircraft Stability and Control Analyzer assumed a position of lesser importance in their minds. It was but one example of the practical applications to which Whirlwind might be turned, and although it still posed severe development problems in its own right, the amount of funds and the scale of enterprise reflected in Project Whirlwind as well as their innovating engineering predilections produced a "first things first" attitude that reasonably centered their attention upon the computer itself.

Engineering development of the cockpit and its ancillary gear for the Aircraft Stability and Control Analyzer continued until June, 1948, when the decision was finally reached to discontinue that phase of the project entirely. This decision

recognized the course which the program had been following and marked the total preoccupation of the project with the effort to develop a general-purpose digital computer.

Upon public announcement of termination two reasons were advanced for the decision: (1) This phase of the total program had been carried forward as far as possible under the existing state of the art. Further information regarding the conversion of digital quantities to analogue quantities was necessary; however, the research necessary to this end could not be pursued since other phases of the total program more urgently required the engineering and financial resources available. (2) Continuation of Phase One of the total program was really unnecessary, since the pace which had been followed in the design and construction of the simulation equipment would have resulted in its availability prior to completion of the computer.<sup>29</sup>

The decision to discontinue the cockpit phase of the project was not unanticipated. De-emphasis of this phase had been accelerated throughout the winter and early spring of 1948, and the reduction in effort had been brought to the attention of both the Navy and the Institute by Forrester.<sup>30</sup>

The decision was also in accord with recommendations made by Perry Crawford in December of 1947 that the work on the cockpit be discontinued as "not essential to the program" at the time.<sup>31</sup> It was only absorbing money and engineering talent which could be applied with greater benefit to development of the computer. Official naval acceptance of the decision was acknowledged in August, 1948, and its necessity was justified on the grounds that the research effort required to develop the digital computer "for comprehensive real-time simulation for synthetic evaluation was too enormous."<sup>32</sup> The truth of the matter was that the Navy was running low on research and development monies, and Special Devices personnel were well aware of the fact.<sup>33</sup>

The change in emphasis did not go completely unchallenged. During the course of a conference called by the Commander of the Office of Naval Research - which had replaced the Office of Research and Inventions as the parent organization for the Special Devices Division - the question of the initial goal of the project was raised, and some of the Navy participants expressed the hope

that the project would not "deviate too far from its original aim of producing a high-performance facility for analyzing proposed aircraft." In response to these doubts, Forrester explained that the digital computer anticipated had never been intended to be the aircraft analyzer, but rather a working model of the type of computer which could be used in the Aircraft Stability and Control Analyzer. However, he added, it would have limited applicability to the initial device. Throughout his remarks, nevertheless, was the implication that the computer he and his associates were seeking to design and construct would be in truth a general-purpose computer which in addition to scientific calculations could be applied to "limited, real-time aircraft simulation."<sup>34</sup>

The decision to discontinue work on the cockpit prevailed, and in October Navy Special Devices personnel proposed that the cockpit which had been acquired earlier from the Air Force be disposed of as surplus.<sup>35</sup> The following December, after the useful spare parts had been removed, the fuselage, cockpit, and turret were consigned to the scrap heap.<sup>36</sup>

In the meantime, in November, 1948, the opposition fired one more shot in defense of the aircraft analyzer. The Mathematics Branch of the Office of Naval Research, whose head, Dr. Mina Rees, had expressed some reservations concerning Forrester's comments at the September conference, investigated the possibility of realizing the original purpose of the project through the use of analogue equipment being developed under another Navy program. The investigation, conducted by Dr. C. V. L. Smith of the Mathematics Branch, reached a negative conclusion, but it was a qualified negative. Dr. Smith stated in his report that if the equations initially supplied by the Department of Aeronautical Engineering of the Institute were to be used, analogue equipment could not perform the computations necessary in the time required. He then proceeded to question whether the "mathematical formulation of the 'Whirlwind' problem" had not been too elaborate, thereby opening the possibility that more simplified equations might not only meet the requirements of the device, but also permit the use of analogue computational techniques.<sup>37</sup>

There the matter stood.

Subsequent events were to suggest that perhaps this was less the final shot in defense of the analyzer than it was the opening shot in a conflict between engineer and mathematician that was to characterize future relations between the MIT group and the Navy.

In retrospect it would appear that throughout this early formative period in Project Whirlwind's history, the Special Devices Division represented effectively both the Institute's cause and its own as it sought and obtained from higher Naval authority the permission and funds necessary to change and expand the program. It is arguable that the Navy's acceptance of the revised program represented a tacit, although not explicit, encouragement of concentration of effort upon the development of a "universal" computer rather than one peculiar to the aircraft analyzer. If so, it would follow that the investigators engaged in the project would feel justified in elevating the computer research and development phase of the total effort to primacy, subordinating the "aircraft analyzer" to a secondary requirement to be met later if at all.

The policies developed and followed during this early period were acceptable to Institute leadership and to Navy leadership, and so were the improvisations and modifications of these policies. The shift of emphasis from aircraft analyzer to universal-purpose computer was not always destined to receive Navy endorsement, for the times changed, the temper of the times changed, and so did the Navy personnel. Among the factors contributing to a deterioration of sympathetic support were reduction in Navy research and development budgets after the war, appearance of a new philosophy of research and development sponsorship in the Navy, the consequent emergence of the Office of Naval Research, and the inevitable personnel changes in the offices designated to oversee the Navy's role as fiscal sponsor of Project Whirlwind. These factors caused the early rapport between Servomechanisms Laboratory personnel and Navy personnel to be dimmed, if not extinguished. Unfortunately, the powerful operation of these factors could not be checked. They increasingly blurred and obscured the intrinsic merit and promise of the unique Whirlwind configuration of the digital computer.



## NOTES TO CHAPTER 4

1. Ltr., J. W. Forrester to N. McL. Sage, January 7, 1946.
2. Conference Note C-10, by J. W. Forrester, February 11, 1946.
3. Administrative Memo A-1, J. W. Forrester to Engineers of Project 6345, February 27, 1946, Subj.: "Present Status of Contractual Relations with Navy as Regards DIC 6345."
4. Interview, J. W. Forrester by the authors, July 31, 1963.
5. Engineering Note E-8, Subj.: "Cockpit Program;" Mar. 1, 1946; Administrative Memo A-3, subj.: "Electronic Staff," Mar. 5, 1946; Administrative Memo A-7, subj.: "Progress Reports," Mar. 15, 1946; Administrative Memo. A-9, subj.: "Reports, Schedules and Meetings."
6. Report 64, "ASCA Equations," originally dated October 31, 1945, revised Apr. 4, 1946 for Project 6345.
7. Engineering Note E-14, subj.: "Mathematics Group," Apr. 12, 1946.
8. Administrative Memo A-3, subj.: "Electronic Staff," Mar. 5, 1946. Conference Note C-12, subj.: "General Meeting of Staff Members of Project 6345," June 10, 1946.
9. Engineering Note E-13, subj.: "Block Diagrams Group," p. 1, Apr. 3, 1946.
10. Ibid., p. 1.
11. Ibid., p. 2.
12. Ibid., pp. 3-4.

## NOTES TO CHAPTER 4 (CONTINUED)

13. Ibid., pp. 4-5.
14. Conference Note C-19, subj.: "Part I - Report of Lab. Work - S. Dodd;" "Part II, Report of Proposed Storage Tube Program - J. W. Forrester," October 23, 1946.
15. Conference Note C-22, subj.: "Discussion of a Parallel Computer," November 6, 1946.
16. Conference Note C-24, subj.: "A High-Speed Parallel Digit Multiplier," Nov. 20, 1946.
17. Conference Note C-12, subj.: "General Meeting of Staff Members of Project 6345," p. 2, June 10, 1946.
18. Ibid.
19. Ibid.
20. Conference Note C-25, subj.: "Pre-Prototype Computer," Dec. 2, 1946.
21. Ibid.
22. Ibid. Memorandum M-43, subj.: "Pre-prototype Status, Report," Dec. 10, 1946; memo M-52, subj.: "Notes and Block Diagrams for the Pre-Prototype Computer," Dec. 27, 1946.
23. Memo M-47, Subj.: "Pre-prototype Computer Meeting," Dec. 17, 1946.
24. These were the first of the L-Series of reports that the Project began to issue. Report L-1 appeared as Memorandum M-108 but was shortly changed to: Report L-1, J. W. Forrester and R. R. Everett to Director, Special Devices Center, subj.: "Digital Computation for Anti-submarine Problem," October 1, 1947. Report L-2 appeared as: Limited Distribution Memorandum L-2, J. W.

## NOTES TO CHAPTER 4 (CONTINUED)

Forrester and R. R. Everett to Director, Special Devices Center, subj.: "Information System of Interconnected Digital Computers," Oct. 15, 1947. Although entitled "Memorandum," the items in this series rapidly became known as "L-Reports." The "Special Devices Center," referred to above, replaced the "Special Devices Division" in the reorganization accompanying the creation of the Office of Naval Research; as SDD had reported to the Office of Research and Inventions, so did SDC report to ONR.

25. Interview, J. W. Forrester and R. R. Everett by the authors, October 26, 1967.
26. Report L-1, p. 1.
27. Report L-2, p. 2.
28. Report L-2, p. 12.
29. Project Whirlwind Summary Report No. 9., June 1948,  
pp. 12-3.
30. Ltr., J. W. Forrester to N. Sage, subj.: "Amendment No. 4 to Project Whirlwind Contract N5ori 60,"  
Feb. 2, 1948; Ltr., J. W. Forrester to Director,  
SDC, att'n Charles Doersam, Mar. 2, 1948.
31. Memorandum, Perry Crawford, Jr., to Director,  
SDC, Subj.: "Whirlwind Program," Dec. 18, 1947.
32. Proposed Work Description for Whirlwind Procurement, August 16, 1948, corrected by Perry Crawford, August 16, 1948.
33. Interview, C. R. Wieser by the authors,  
June 16, 1965.

## NOTES TO CHAPTER 4 (CONTINUED)

34. Perry Crawford, Jr., Memorandum for the files, subj.: "Conference on Project Whirlwind Held at Navy Department, 22 September 1948," Nov. 2, 1948.
35. Information on the acquisition of the Cockpit is contained in: Ltr., Noel Gayler, Cmdr, USN, to CG. AMC, Wright Field, Sept. 18, 1947.
36. Memorandum, C. H. Doersam, Jr., Computer Section to Director, SDC, subj.: "B-24 Fuselage for Project Whirlwind; Disposition of," Oct. 25, 1948; memorandum, Survey and Surplus Property Review Board to Head, Building and Ground Units, subj.: "Surplus Property, Instructions for Disposal of," Dec. 3, 1948.
37. Memorandum, Code 424 (Fred D. Rigby) to Code 100, subj.: "Report from C. V. L. Smith to Head of Mathematics Branch, subj.: "Recommendations Concerning the Realization of the . . . , " Nov. 18, 1948.

## PRESSURE FROM ONR

It was equally easy to take the view at the start of 1947 that Project Whirlwind was making due progress or that it was falling behind, depending upon the expectations of the observer. In either case, the selection of a proper scale against which to measure the activities of the project remained a complicated, intuitive, highly subjective, and obscure task of judgment, further complicated by the common, joint practice of establishing goals and schedules to be met that the Massachusetts Institute of Technology and the Navy followed. In this respect, Project Whirlwind was like many, if not most, research and development projects. Goals and schedules had been set, providing a time-table for exploring the unknown and the partly known; since the time-table was the product of mixed ignorance and knowledge, it is not surprising that subsequent investigation showed these goals to be less attainable and more remote than earlier had been thought. Revised goals were necessary, and these called for further investigation, further research and development

effort that yielded additional information. Inevitably, some of the new information, in its turn, further modified, transformed, or even destroyed some of the revised goals.

Hindsight in later years might tell whether there had been progress and of what kind, but here, too, the proper scale of measurement is not easy to select. On the one hand, the engineers in the project, their counterpart Navy Special Devices program managers, administrative superiors at the Institute and in the Navy's Office of Naval Research, which had replaced the Office of Research and Inventions during the latter half of 1946, all had the opportunity to contemplate the impressive record of past achievements of MIT, of its Division of Industrial Cooperation under Nat Sage and of its Servomechanisms Laboratory under Gordon Brown. On the other hand, they could survey apprehensively the still-unsolved problems and the new, relatively formless state of the art with which the MIT engineers were struggling in the digital computer realm.

Forecasts which engineers and administrators cautiously generate while they are contemplating troublesome problems rarely agree with those which they optimistically foresee while they are reviewing past accomplishments, and any analysis that attempts to combine the virtues of both runs the risk of appearing to present confusing, vague, and contradictory statements at best and outright doubletalk at worst. The evidence that is most convincing to the insider is least convincing to the outsider. The conclusions resting upon such internal evidence are most persuasive to those familiar with the evidence and least persuasive to those who, viewing it from without, lack the feel of its pulse and the sense of its past and present color that provide good rate-of-progress information.

It was from this sort of predicament that Jay Forrester sought to extricate himself when writing a semi-annual review of the status of the Whirlwind contract as of January 1947. While he and his associates were able to keep their heads above water, the current

of events continued inexorably, slowly, steadily to carry the Project toward certain shoals and reefs that were forming as a consequence of actions taken by the Navy to reorganize its practices and policies for supervising and funding research and development projects.

Mindful of the two Tasks that had been written into Contract N5ori-60 a year earlier,<sup>1</sup> Forrester was willing to admit to Special Devices program managers and their superiors in the new Office of Naval Research that Phase Two, the construction of a prototype computer, would commence not in July 1947, as planned, but in January 1948, in order to allow sufficient development-time for the pre-prototype and thus establish, with sufficient firmness to proceed, the configuration of the Phase Two device.<sup>2</sup>

The computation speeds of the machine would have to be "well above those originally anticipated," and although the general nature of the computer block diagrams had been established under Everett's leadership, much new work involving "the advancement of electronic techniques in the fields of video circuits, electronic switching, trigger circuits, and pulse transformers" lay ahead. Further-

more, since reliability of operation in a computer hooked up to an aircraft simulator was crucial, "checking and trouble-shooting circuits similar to those required in the final electronic computer" should be incorporated, and time should be allowed for this.<sup>3</sup> The Institute felt, said Forrester -- and by this he meant himself and his associates working on Project DIC-6345 in the Servomechanisms Laboratory and implied also Professor Gordon Brown, his superior, and Nat Sage, his administrative supporter and protector, who received a copy of the letter, -- the Institute felt that the pre-prototype ought to possess the operating speeds and approximate circuits the final machine would feature.

Forrester believed that in view of existing circumstances, the design of the pre-prototype could be firmly established by the end of the next eight or nine months, and construction of its many parts could be completed three or four months later (having begun well before October). Such would be the state of the Project at the end of 1947, and it would permit the pre-prototype to be assembled and put into

preliminary operation "early in 1948."<sup>4</sup>

He was careful to exclude the electrostatic storage tubes from this schedule. They might be available in time, but sufficient data were "not now available to make firm time estimates," so manual-switch storage and flip-flop storage would be incorporated until such time as the tubes became available. About 25,000 binary digits of future electrostatic storage were called for in the plans, but they were still only in the plans.

To accomplish the revised, stretched-out schedule, a level of expenditure of \$30,000 per month would be required for the coming year, and most of these expenses would be charged to the pre-prototype of Phase One of the contract. As both MIT and SDC representatives well knew, Phase One had never been intended to "define the nature or extent of this pre-prototype computer." Forrester could nevertheless assure the Navy that "the project is now prepared to embark upon the specific system design of a pre-prototype electronic computer which is the end objective of Phase 1."<sup>5</sup>

Benefitting from the past year's researches, the pre-prototype would employ parallel, or simultaneous, transmission of digits. Block diagrams that Everett had developed for a serial-transmission computer -- inspired originally by the proposed EDVAC machine -- had convinced the engineers that, despite relatively simple and easy-to-maintain circuits, such a device would be too slow. So block diagrams for a faster, parallel computer had been developed. Registers large enough to accommodate 16 binary digits would be employed. "Sixteen digits are considered sufficient for testing and demonstrating electronic operation and for a certain few investigations into the mathematical applications of digital computers," Forrester observed, but realizing how much more useful in mathematical investigation such an instrument might be if it could handle larger numbers, he added that the computer would be so designed as to carry out its operations "in multiples of 16 digits in length," specifically, 32 binary digits.<sup>6</sup>

It was a well-composed letter, and it said more than is indicated here. It was packed with information, presenting the good news with the less than satisfactory and putting the latter softly, so as not to disturb. Nevertheless, it was a letter that said progress was slower than had been scheduled, and when one paused and reflected upon just what sort of slow progress it was, one could see that it was progress to the tune of \$305,000 already spent, progress to the further tune of another \$200,000 anticipated for the next six months, and progress to still another tune of \$528,000 for the year after that -- over a million dollars -- and no assurance when the storage tubes would be ready. It promised ultra-high computer speeds, only 16-digit operation to begin with, and some kind of storage some time. For a million dollars and more!

This state of affairs represented not unreasonable progress to any Navy program manager who had been involved at the start of the engineering project to develop an Aircraft Stability and Control Analyzer, nor was it

cause for special concern to those, like deFlorez, Gratiot, or Crawford, who had entertained a rather visionary and aggressively dynamic engineering philosophy with regard to certain technical developments they considered desirable and feasible. But just as Project Whirlwind's behavior could be accounted for by the very character of the research and development process it was engaged in, so could the Navy's changing attitude be explained by fundamental organizational and policy changes which were taking place within the Navy. These were generated by circumstances that historically and genetically had nothing to do with aircraft analyzers and digital computers, and they were too profound to be affected by the small influence that the Navy Special Devices personnel and MIT's Project Whirlwind engineers could exert. The accumulating impact of these changes the Special Devices Center and Project Whirlwind, put in strongest terms, emasculated the former and drove the latter to the wall. Put in milder terms, these changes inevitably effected a major reassessment of some of the projects in which the Navy and civilian advanced research and development teams were jointly engaged -- and one of these projects happened to be Project Whirlwind.

At the time of the inception of the Aircraft Analyzer program at MIT in 1944, Navy supervision and funding of the program had been a primary responsibility of the Special Devices Division, initially organized as a branch of the Bureau of Aeronautics, but transferred along with other Navy research and development facilities and organizations to the Office of Research and Inventions in May of 1945, and subsequently, in August of 1946, to the newly created Office of Naval Research. Under both the Bureau of Aeronautics and the Office of Research and Inventions, the Division had been permitted a wide latitude of authority and freedom of action. Once under the control of the Office of Naval Research, however, the Division was phased out and its facility at Sands Point, Port Washington, New York was designated as the Special Devices Center of the Office of Naval Research. Until February of 1949, nevertheless, Navy responsibility for the supervision of Project Whirlwind was to remain with the Special Devices Center, thus assuring a continuity of supervision up to that time. <sup>7</sup>

Prior to the appearance of the Office of Naval Research there had developed between the engineers of Project Whirlwind and the engineers of the Special Devices Division a reciprocal confidence and sympathy which was to decrease proportionately to the increase in ONR's exercise of authority over the Center and its programs. There had been occasional areas of disagreement between the two groups, but relations had been basically sympathetic and understanding, the product of a rapport grounded in the engineering orientation of the two groups and in a common parenthood of the Aircraft Stability and Control Analyzer. The relatively harmonious relations which had been established early in the program were given additional strength and substance when Perry Crawford, Jr. left the Massachusetts Institute of Technology in October of 1945 to join the staff of the Special Devices Division. Crawford, who subsequently suggested the use of the digital computer as a solution to the real-time problem which was besetting Forrester and his colleagues, brought to SDD additional familiarity with the Project, but more importantly, he brought with him an imagina-

tive and enthusiastic confidence in the potential utility and versatility of the digital computer. As head of the Special Devices Center's computer section, Crawford was to prove an imaginative, able, and influential ally to Project Whirlwind until ONR took full command.<sup>8</sup>

As SDC became more and more the instrument of ONR, however, the relations between Project Whirlwind and the Center became increasingly strained and critical, to such a degree that in the winter of 1947, Forrester even questioned the Center's competence "to provide the proper administrative, technical and financial assistance to the work and to properly relate the interests of all Navy groups." Undoubtedly, Forrester was becoming increasingly restive under the more critical supervision which was emanating from SDC in response to the increasing pressures generated by ONR. The earlier rapport was being submerged by the tensions which were created as ONR asserted its authority.<sup>9</sup>

The years 1947 through 1949 were difficult years for Forrester and Project Whirlwind, for in addition to the increasing tempo and severity of Navy criticism, Project Whirlwind found itself under closer and more penetrating scrutiny by the Institute's top administration. Project Whirlwind had become a source of contention, caught up in the struggle which accompanied ONR's efforts to implement the authority inherent within its enabling legislation. It was caught up also in the struggle between mathematician and engineer which accompanied the pioneering research and development phase of digital computation. Finally, it was caught up in the struggle over funds which accompanied post-war retrenchment.

Jay Forrester's direction of the Project also became involved in the controversy. Dedicated to Project Whirlwind and determined to secure its success, Forrester aggressively and single-mindedly pursued the course which he believed would most quickly reach that end. Without doubt, his aggressiveness and determination offended many, but without this sense of purpose

behind it, the Project could very likely have failed. His superiors both within MIT and the Navy no doubt were pleased by his determination to do the best job possible on what he considered to be "one of the most important development jobs in the country," for he was convinced that computers promised rewards to the military as great if not greater than radar. But his attitude toward costs could not fail to be disturbing because those which others regarded as expenses, imposing an upper limit, he seemed to consider as productive investments, as means to an end, rather than determinants of level of effort. His apparently cavalier attitude toward costs was doubly disturbing because of his youth and because he apparently failed to communicate effectively his rationale or philosophy, if indeed he had one in that regard, to cost-conscious Navy supervisors compelled to stay within limited, peacetime budgets. They were dismayed, not reassured, by his conviction "that the facilities and funds needed to do a job are subordinate to getting the job done as

quickly as technical progress permits."<sup>10</sup>

The rate of progress Forrester claimed for the program did not go unchallenged. Thus, his letter of January 1947, carefully composed though it was, as we have just seen, nevertheless left the program vulnerable. For it compared progress accomplished to goals set and permitted critical eyes to find the progress wanting. It suggested the goals-- the time schedule -- be modified, but this suggestion raised again the impression that progress had not been satisfactory. And indeed, judged by the goals set earlier, the progress had not been satisfactory. So Navy programmers could ask, was this indeed the case? Had the goals been unrealistic? Or were the capacities of the researchers inadequate? First they were going to build a simulator. Now all that was discussed was a computer, and this wasn't even the computer, or the sort of computer, that the project had set out to build. Were they eager young men who had gotten beyond their depth and didn't realize it --

wouldn't realize it -- yet? Two and a half weeks after Forrester sent in his letter, the head of the Naval Research Advisory Committee, a civilian scientist, spent an hour and a half on an inspection visit.

Dr. Mina S. Rees, Head of the Mathematics Branch of ONR, was of the opinion that the "consensus of visitors to the project is that there is too much talk and not enough machine." To the mathematician who visited the Project and who lacked understanding of the engineering problems involved, this comment seemed only too self-evident and accurate. Also, criticisms voiced of Forrester and his project could on occasion be extremely harsh and extreme, reflecting as one observer noted, "the personal animosity which is widespread in the computer development field and especially as regards Mr. Forrester." It is not impossible that such criticisms, even when discounted for their exaggeration, were influential even though not sufficient by themselves in shaping Mina Rees' view of Project Whirlwind and its staff.<sup>11</sup>

Without doubt, Forrester and his associates were operating in a very competitive field and one in which the mathematician was a powerful if not a dominant influence. Young, inexperienced, and unknown engineers, they were matching skills and abilities with men of known stature and status such as John von Neumann of the Institute for Advanced Studies, Howard Aiken of Harvard, J. P. Eckert and J. W. Mauchly of the University of Pennsylvania, G. R. Stibitz and S. B. Williams of the Bell Telephone Laboratories, and M. V. Wilkes of Cambridge, England, just to mention a few.

The electronic automatic sequence control machine was in its early conceptual stage. These young engineers were seeking not to refine an already existing device, but rather to design, develop, and construct an entirely new one. In short, they were converting a concept into an electrical system embodied in a piece of tangible hardware. If the Whirlwind engineers had not been operating within the protective womb of MIT, it is altogether conceivable that the Project would have been terminated by the Navy, particularly after ONR had

assumed primary responsibility for Navy research and development. The mathematicians of ONR, enamored of the computer as a scientific instrument of rapid calculation, failed to recognize its potential as a command and control center as early advocated by Forrester and Crawford. The engineers of Project Whirlwind and SDC, concerned primarily with application to military needs rather than development of theoretical concepts, saw it as an instrument primarily adapted to facilitate human control of events in the physical world and only secondarily intended as a mathematician's tool.<sup>12</sup>

The misgivings expressed by Mina Rees were not hers alone, for they were shared by her colleagues within the Mathematics Branch. The mathematicians were concerned because they believed that neither Forrester nor any of his associates actively engaged on the Project possessed the "mathematical competence needed in the design of a new type digital computer."<sup>13</sup> Such misgivings were not a sudden development, nor were they allayed by Forrester's semi-annual review submitted at the end of

January, 1947. They led in February to a visit by Warren Weaver, then Head of the Naval Research Advisory Committee, to the Servomechanisms Laboratory at MIT to investigate the Project. Later Weaver was also to visit SDC at Sands Point. After visiting the Institute, Weaver in his comments to Mina Rees expressed no major criticisms, or praise either, of the Project or the personnel engaged upon it, but he did raise some very penetrating questions without providing the answers. Included among the questions which were concerned primarily with the nature and purpose of the Project was one pertaining to the quality of the mathematics in the program. Weaver wondered if it matched their "excellent physics and engineering?" Subsequently, after visiting SDC and writing with greater retrospection, Weaver observed that neither achievement nor progress could be measured by a single visit. His conversation with Forrester had left him, he observed, with the belief that there was some confusion whether Whirlwind was really "a simulator or a general-purpose

computer," a belief caused by Forrester's description of Whirlwind at one point in the conversation as a general-purpose computer. But when pressed by Weaver to explain how it would handle certain scientific calculations, Forrester evaded a direct answer by describing it as a "fire-control" computer. As Weaver put it, was the Project failing to be good biscuits by trying to be cake? Crucial here were the value judgments to be applied; it was easy indeed to make invidious comparisons of engineering simulators and biscuits, on the one hand, with scientific mathematical machines and cake, on the other, and Weaver was wary of rendering such judgments even while phrasing the problem in perhaps suggestive terms. The strongest impression he gained from his visits to MIT and to SDC was that both Forrester and Crawford were extremely competent and able, and that the Whirlwind staff was "well organized, enthusiastic and hard at work."<sup>14</sup>

Weaver's visit to the Servomechanism Laboratory at MIT and his subsequent visit to SDC may have been purely coincidental or part of a general study he was making of the Navy's research and development program, but coming on the heels of each other, they strongly suggest that he was investigating the Project within its total context, seeking to determine not only the implementation of the program at MIT, but also its direction by SDC. Mina Rees and her colleagues were concerned about SDC, its relations with ONR, and the guidance and direction it was providing Project Whirlwind. This concern, the Computer Section of SDC, understandably, felt led to an improper interference in its area of authority, but the balance of power was shifting to the Mathematics Branch.<sup>15</sup> During the course of a discussion over the establishment at Sands Point of a "simulation facility," using Whirlwind II (the projected second-generation computer) as its information and control center, Mina Rees while expressing approval had some reservations lest she was "relinquishing some responsibilities that properly belong to the

Mathematics Section." In addition, in an aside to Perry Crawford, she questioned if the Center were not engaging in "empire building."<sup>16</sup>

If Mina Rees and her colleagues had hoped through Weaver's visits to obtain evidence which would support their efforts to curb the Project -- or even destroy it, as some of the junior members of Project Whirlwind charged in retrospect -- they were disappointed. On the other hand, his comments did not still their apprehensions.

It is doubtful, however, if Mina Rees and her associates sought to destroy the Project. Certainly, they sought to bring it under firm control, to orient it properly, for they were seriously concerned about the program which, they believed, had merit but lacked direction and purpose. In the fall of 1957, Mina Rees believed that the Project possessed real and tangible possibilities, particularly for "scientific" computation, and even if it failed to attain complete success, "a

substantial contribution to the art" would have been made and "the money invested . . . worthwhile." The money was also, one might add, considerable in amount, a fact which seriously disturbed ONR, as events were to prove. Between the inception of the program and the assumption of control by the Mathematics Branch of ONR, the estimated costs had more than doubled and threatened to continue to mount, and the schedule had slipped by some twelve months, yet the original purpose of the program contractually remained the same.<sup>17</sup>

The pot continued to simmer, even if it did not boil, the discontent of the mathematicians providing a steady source of heat. They continued disturbed by the Project's lack of that which they regarded as competent mathematical talent essential to a well-ordered, properly organized computer program. For them the ideal electronic digital computer program was the one at the Institute for Advanced Studies under the direction of Dr. John von Neumann. Persistently, they compared the two programs, asking how

Whirlwind differed from the IAS computer. If the two devices did not differ significantly, then why was Whirlwind costing so much more? Persistently, also, they asked why Whirlwind was being designed and built as a general-purpose computer if its primary application was to be simulation. These specific questions were raised at an ONR conference in October 1947, accompanied by the charges that Project Whirlwind lacked essential mathematical competence, that no effective analysis of the functions of Whirlwind had been prepared, that the status of the storage tube program had been exaggerated, and that even within the MIT community, the Project was under fire for lack of interdepartmental cooperation and for its unsatisfactory progress rate.<sup>18</sup>

Responding to these specific questions and charges, which obviously contained the implication that SDC had been remiss in its direction of Project Whirlwind, Crawford recommended that Professor Francis J. Murray of Columbia University be retained to evaluate the

"mathematical competence indicated by the work to date" and to make a comparison between Whirlwind I and the computer von Neumann was developing at Princeton. In addition, Project Whirlwind's directors should prepare "detailed information concerning the components designed for Whirlwind I and the design of the Whirlwind I system." Until the information requested was furnished and the decision was reached that the program was indeed valuable, he recommended that no further consideration be given to the financing of Whirlwind II. Crawford's last recommendation may have accurately reflected his own annoyance and misgivings, but certainly it mirrored the opinion of some within the upper echelons of ONR, and implied that the Office was threatening the use of its ultimate weapon -- the power of the purse -- to bring the Project into line.<sup>19</sup>

In order to dampen the heat persistently emanating from ONR, Forrester followed two courses. To meet the chronic objections, he prepared with his staff, upon the recommendation of Captain George M. O'Rear of SDC, a twenty-two volume administrative and technical

summary of Project Whirlwind since its inception in 1944, setting forth in detail the changes made in the purpose and nature of the program and the reasons for them.<sup>20</sup> This report, he hoped, would explain away ONR's objections and serve as a compendium to provide answers to any future questions the Project's critics might ask. The questions and charges which had been made at the October conference at ONR were answered separately and in specific detail.

Comparing Whirlwind I to the von Neumann computer, Forrester argued the former was faster, more applicable to Navy needs, and further advanced in design and construction. Comparative costs could not be determined, he noted, since von Neumann had no cost estimates for his finished device; however, because of "final design refinements and the more finished packaging," Whirlwind's final costs would probably exceed those of the IAS computer by a margin greater than the two-to-one ratio forecast by ONR. His critics, he suggested, evidenced a real

lack of understanding of "the simulation and control field and . . . the meaning of a general purpose computer" when they sought to make Whirlwind one or the other, for the complexities of simulation demanded a flexibility which permitted a wide variety of uses. The storage tube development program was difficult and complex, but one which had always been frankly and candidly discussed without exaggeration.

In denial of the charge that interdepartmental cooperation was lacking, Forrester cited instances in which other departments had cooperated by making either personnel or facilities available. Within his own department, Electrical Engineering, a separate research program -- supported by the Rockefeller Foundation -- in digital computation had been discontinued to permit consolidation of the two staffs in order to make the total effort more effective. All in all, Forrester argued, because of the immense importance of electronic digital computation, MIT had rendered more aid to the Project than the Navy

had a reasonable right to expect, and furthermore, this assistance had been given despite heavy teaching and research commitments.<sup>21</sup>

Despite Forrester's disclaimers, supported as they were by cited cases, there was a continuing feeling that cooperation, if not lacking, was limited. Forrester, belatedly perhaps, had requested assistance from other departments, but the indications were that they had not responded enthusiastically.<sup>22</sup> Beyond the usual obstacles -- other commitments, lack of interest, etc. -- one significant impediment to cooperation was without doubt the classified nature of the Project, a barrier which Forrester found to be a continuing problem.<sup>23</sup> Professor Samuel Caldwell, who had been working on the research program supported by the Rockefeller Foundation, refused to work with Project Whirlwind so long as it was subject to military security restrictions. He would work only "on research concerning electronic computing that will freely serve all of science," a view which was shared by many of his colleagues.<sup>24</sup>

Nevertheless, Forrester did have a measure of assistance and cooperation from the Mathematics Department. Professor Philip Franklin of that department was dividing his time between departmental and Project Whirlwind duties at the time that Crawford called for an inspection visit by Professor Murray of Columbia. Together with two full-time members of the Project, Franklin constituted its Mathematics Section. The effort put in by this group and by others working on mathematical problems in the Project "would represent a larger staff than available for the entire engineering activity of the Institute for Advance Study computer" if it included all who performed mathematical functions within the program, Forrester pointed out. Both he and his critics knew that this was an organizational procedure, as well as a legitimate way of interpreting program operations, that was not restricted to the Whirlwind Project.

Although the Project was not emphasizing mathematics as much as ONR felt was necessary, it was pursuing research in pure and applied mathematics related to the computer.<sup>26</sup> At the same time, mathematics that was not directly pertinent to the engineering development of the hybrid, practical, general purpose, science and engineering instrument that Forrester and Everett visualized tended to be subordinated. Forrester felt sufficiently vulnerable to ONR's criticisms to be goaded into further defensive action following Murray's visit, which occurred on November 8; four days later, Project Whirlwind coincidentally published a memorandum by Franklin surveying in some seven pages of single-spaced typescript the Project's mathematical program, both accomplished and planned.<sup>27</sup> In addition, within the month Forrester was planning to enlarge both the mathematics staff and program, subject to Navy approval indicated by adjustment of the contract "to cover continuing basic research programs."<sup>28</sup>

The Project's activities were by no means confined to responding to the external pressures generated by ONR's persisting and sceptical scrutiny. Indeed, Forrester shielded his engineers, so far as he was able, from the outside alarms so that they might continue their research activities with as little interruption as possible. In the fall of 1946 they had begun looking actively for building space to house the project pre-prototype computer.<sup>29</sup> By March the firm of Jackson and Moreland, Engineers, headed by Edward L. Moreland, Frank M. Carbert, and Ralph D. Booth, had estimated that the accommodations specified would cost about \$770,000 if incorporated, as proposed, into the projected Navy Supersonic Wind Tunnel Laboratory on the campus by extending the office section of the Laboratory "three additional floors, making this building a four-story building in order to house the Servo-Mechanism Laboratory."<sup>30</sup>

Forrester allowed himself a growth factor in a report to SDC in April on the matter. The Supersonic Laboratory accommodations requested would include

enough space, he felt, "for development and operation of the final Whirlwind computer."<sup>31</sup> He could not yet make a report, he said, on the alternative of "purchase or rental of an existing building."

The growth factor assumed more explicit form in a letter to Perry Crawford near the end of April, in which Forrester confirmed earlier verbal discussions, for the record, in a way typical of the degree of cooperation that had become characteristic of relations with SDC and that soon was to disappear as the Mathematics Branch of ONR assumed greater authority. Phase 1 would be extended to June 1948 because "a reevaluation of progress and time schedules" indicated that more research and development time would be needed "prior to design of Whirlwind 1."<sup>32</sup> This was not a "stretch-out" representing reduced effort, however, because "the scope of the Whirlwind I computer is considerably more extensive than originally planned and will require an additional six months' time. Since the computer will be more

nearly like Whirlwind II than originally anticipated the design of Whirlwind I will appreciably ease the design and construction problem of Whirlwind II." Looking ahead, Forrester drew attention to developments that both MIT and SDC viewed at that time as reasonable projections: "It is anticipated that Task II involving the construction of Whirlwind II will overlap somewhat the end of Phase 2 of Task Order 1 covering the system design and that steps will be taken as soon as possible to formulate Task II." A different future was in store, however. Whirlwind II was never built, and in its place appeared a more elaborate machine than anyone was then planning on, the ANFSQ-7.

As a matter of policy, Forrester deliberately stopped referring to the "pre-prototype" in external correspondence that spring; as a matter of custom, "pre-prototype" yielded to "Whirlwind" in the Laboratory as the summer wore on. In the meantime, further investigation by Jackson and Moreland

revealed that the earlier estimate of building costs had been too low, and the Supersonic Laboratory became less attractive as time and cost schedules indicated an already-existing building might be more feasible. Before the end of August the Barta Building, located on Massachusetts Avenue close by the MIT Campus came under serious consideration, and it was the Barta Building that became the home of the Whirlwind computer.

The technical appraisals undertaken by Forrester, Everett, Fahnestock, Boyd and other engineers in the Project during 1946 had indicated that problems of engineering reduction-to-practice were least troublesome in the areas of information input and output and most troublesome in the area of storage. The Project leaders became convinced early that fast internal storage organized in easy-to-add-onto units was essential, and they devoted their efforts particularly to electrostatic storage.<sup>33</sup> Input and output problems they were willing to let the Navy Special Devices Center

contract for separately, and by autumn of 1946 Eastman Kodak was involved in providing "equipment for the preparation of input films from a manually operated key board as well as output recording devices and mechanisms for reinserting output data into the input of the computer."<sup>34</sup> As matters turned out, the Eastman equipment, using minute clear or opaque spots on 35 mm. film to represent binary digits to be implanted or read by cathode ray tubes and associated photosensitive tubes, was never perfected for Project Whirlwind, and other input-output techniques brought forward by the industry-wide advancing state of the computer art were employed instead.

During 1947 the quota of graduate students employed as research assistants rose from eight to twelve and then to fifteen. By the end of October, Forrester was asking for twenty for the next year.<sup>35</sup> Most of these were working towards their Master's Degree and carried

out or assisted in special investigations that added to the Laboratory's pooled knowledge in a modest and detailed way while providing the subject for a Master's thesis or occasionally a doctoral dissertation. The practice of bringing students into the Laboratory continued as long as it remained on the campus and geographically separate from the MIT subsidiary it later joined, the Lincoln Laboratory located in nearby Bedford. Not only were Forrester and his assistants continuing Gordon Brown's policy with regard to students, but also they found the campus relationship invaluable in providing a small but growing pool of first-class engineering talent which in later years was to spread out into the growing computer industry. While these students gave their best efforts to the Project, often continuing on the staff after obtaining their degrees, the Project in return gave them the experience that put many of them a professional jump ahead of their contemporaries.

While technical work proceeded apace, as Warren Weaver, Professor Murray, and other visitors observed, the Project leaders presided over the expanding activity, moving from details to overviews to analysis of how the work was proceeding on many fronts, and back to details. While Everett, for example, spent more of his time on the complex problems of logical circuitry and attended to the details of creating and maintaining an integrated system of working components as research phased into advanced design and design into projected hardware, Forrester occupied himself with internal and external organizational details and with building and maintaining a high-spirited, hard-working organization. Supported by Nat Sage's office, he selected a subcontractor to fabricate the hardware, the racks, the panels -- the form and substance itself -- of Whirlwind I. Sylvania Electric Products Company of Boston took the contract with MIT during the latter half of 1947 and went to work building the items to the requirements and specifications of the Whirlwind staff.

The Mathematics Branch of ONR endorsed these developments even while preserving its apprehensions over the basic direction and purpose of the project. That there was much activity and increasing amounts of money being spent at Project Whirlwind was no guarantee, after all, that the money was being wisely or well spent. What was really going on in the Servomechanisms Laboratory? The Mathematics Branch could never share SDC's confidence. The twenty-two volumes of Summary Report Number Two, for all their impressive and informative detail, were but another manifestation of the peculiar style in which Forrester's operation proceeded to go its own way, have its own way, and -- for all Mina Rees, C. V. L. Smith, and their associates could tell -- be heading for a spectacular fall in its own way.

ONR, consequently, welcomed Crawford's suggestion that Francis J. Murray of Columbia University be asked to look into the situation and deliver a report free of the modulated yet enthusiastic

bias to be expected in the Project's own Summary Reports.

Murray, as has been remarked, visited the Project on November 8, 1947. He was an associate professor of mathematics who possessed both classroom and laboratory experience in computers.<sup>36</sup> He had agreed to undertake the task Crawford proposed in his memorandum discussing the ONR conference of October, and accompanied by representatives of SDC, including Perry Crawford, he conferred with Forrester, Everett, and Philip Franklin of the MIT Mathematics Department. Both professional courtesy and official responsibility required Professor Franklin's attendance at the conference. His presence also served to counter the ONR charge of inadequate attention to mathematics, a consideration Forrester was not likely to overlook. A week later the Cambridge conferees, minus Franklin, travelled to Princeton to meet with Professors John von Neumann and H. H. Goldstine for a discussion of the IAS computer program. Within the following week, Murray had finished his report and submitted it to the Director, SDC.<sup>37</sup>

In his report, Professor Murray evaluated Whirlwind I in the context of the environment he had seen and heard interpreted and portrayed by Forrester and his associates during the Cambridge conversations.

Whirlwind I, they had explained, was to be used primarily for simulation, but since "no single use of the digital computer would justify the development cost," it would consider two other types of problems: control and scientific computation. Again Forrester, on this occasion supported by Everett, evaded typing Whirlwind to a particular application. To both men the question of application was academic, for Whirlwind was adaptable to a variety of uses, of which ASCA was only one, even if by contract the primary one.

The report contained no direct criticism of the Whirlwind program, but Murray did give evidence supporting the ONR charge of insufficient attention to the mathematical needs of the program by noting that no mathematical analysis of the operations of Whirlwind I had yet been made and any existing plans for one were inadequate. Such analysis was essential; it should be

performed within Whirlwind, not by a separate group, and should be included "as a component of the device." There was no need for the mathematical analysis to await availability of the computer, for it would not interfere with or delay engineering development. The two could and should proceed concurrently.

In comparing the two programs at Cambridge and Princeton, respectively, Murray concluded that although they had a "common logical ancestry," they were "distinct to a remarkable degree." The application of digital computation to simulation and control required the "engineering development" of Whirlwind, a requirement not imposed upon the IAS computer which was at liberty to follow "direction of interest to its own objectives," namely, the consideration of "purely scientific problems." Hence the emphasis upon engineering development was proper, for engineering development was "absolutely necessary," and to delay it would "delay the use of digital computers in the type of problem" with which

Whirlwind was concerned. He implied that since Whirlwind was being designed for future manufacturing, it had to follow more rigid engineering standards than did the IAS computer, an implication which von Neumann later rejected.<sup>38</sup>

Once the Murray Report had made its way from SDC to Mina Rees's office in Washington, a copy was forwarded to von Neumann at the Institute for Advanced Studies at Princeton for his comment. Accepting Murray's definition of Whirlwind's purpose as "precise and authentic" and agreeing with the importance of "a thorough mathematical analysis," von Neumann mildly rejected Murray's observations concerning the differences between the two programs. The contrast had been drawn too sharply, he felt, yet he rejected the implication that because Whirlwind had a definite application in mind and was being designed and developed with intent of industrial production, it need be more "reliable and maintainable" than the

IAS computer which was intended for "general scientific purposes."<sup>11</sup>

Von Neumann also questioned Murray's assumption that the difference in objectives had caused the differences in design and plan. These resulted, rather, from the differences in people. If the objectives were exchanged, the courses followed would have remained the same, for "the subject is new and it is the rule rather than the exception that two groups who work independently towards very similar or even identical objectives may come out with rather different conclusions. I need not say that I consider this very desirable. The subject is so new that it is quite reasonable to try a variety of approaches and not to place all bets on the same chance.<sup>39</sup>

Von Neumann's observations and judgments were moderate and restrained and in a vein not unlike Warren Weaver's of ten months earlier. Unfortunately for Forrester, they were not strong enough to allay

suspicions in ONR. Instead, the issue was only just beginning to be well joined between MIT and ONR, and it was the sort of issue that many years later was to provide grounds for the remark, "We're not going to let it become another Whirlwind!" - a policy view that could be taken as a stout assertion of control by a determined administrator or that, again, could be regarded as a subtle failure of administrative nerve where the vigorous prosecution of research and development might be demanded.

## NOTES TO CHAPTER 5

1. See End of Chapter 3.
2. Ltr, J. W. Forrester to Director, SDC, Subject: "Semi-Annual Review of Contract N5ori-60," p. 2, January 28, 1947.
3. Ibid., p. 1.
4. Ibid., p. 2.
5. Ibid., p. 1.
6. Ibid., p. 4.
7. Letter of Intent for Contract NOa(s)-5216, December 14, 1944; Task Order No. 1, Contract N5ori60, June 30, 1945; Amendment No. 4, January 21, 1948 and Amendment No. 6, September 29, 1948, Task Order #1, Contract N5ori60; Directive, Chief of Naval Research to Director, SDC, February 8, 1949.
8. Interviews by the authors: J. W. Forrester and R. R. Everett, July 31, 1963, G. S. Brown, July 6, 1964.
9. J. W. Forrester, Computation Book No. 45, November 27, 1946 to December 10, 1948, pp. 89-91 and 134-5, see also the following memoranda by Perry Crawford, Jr.: Confidential Memorandum, Subject: "Project Whirlwind," November 4, 1947; Memorandum to Director, SDC, Subject: "Whirlwind Program," December 18, 1947; Memorandum to Director, SDC, Subject: "Report on visit to MIT on 9 January 1948," January 12, 1948.
10. Memorandum, Perry Crawford, Jr., to Director, SDC, Subject: "Report on visit to MIT on 9 January 1948," January 12, 1948; Memorandum, Subject: "Report on conference with Dr. Mina Rees and Dr. John Curtiss at Sands Point, 15 September 1947," (author anonymous --

## NOTES TO CHAPTER 5 (CONTINUED)

found in ONR files); J. W. Forrester to Capt. D. P. Tucket, ONR, July 23, 1948; interview, Norman Taylor by Howard Murphy and K. C. Redmond, August 8, 1963.

11. Memorandum, Subject: "Report on conference with Dr. Mina Rees and John Curtiss at Sands Point, 15 September 1947,"
12. Ltr, J. W. Forrester to Lt. Comdr, H. C. Knutson, SDD, ORI (Washington), January 28, 1946; Report L-3, by J. W. Forrester, Hugh R. Boyd, R. R. Everett, Harris Fahnstock, R. A. Nelson, Subject: "Forecast for Military Systems using Electronic Digital Computers," Servomechanisms Laboratory, MIT, September 17, 1948; R. R. Everett, The Whirlwind I Computer, revised text of a paper presented at the joint AIEE-Institute of Radio Engineers Conference, Philadelphia, Pa., December 10-12, 1951.
13. Memorandum, Perry Crawford, Jr. to Director, SDC, Subject: "Discussion of Project Whirlwind at ONR conference on 28 October 1947," October 29, 1947.
14. Memorandum, J. W. Forrester to N. McL. Sage, Subject: "Warren Weaver, Visit to Laboratory, February 15, 1947," February 19, 1947; J. W. Forrester, Computation Book No. 45, p. 27; ltr Warren Weaver to Chief of Naval Research, att'n, Mina Rees, February 20, 1947; ltr., Warren Weaver to Chief of Naval Research, att'n Mina Rees, June 26, 1947.
15. Memorandum for the files, (author anonymous), August 26, 1947.
16. Memorandum, Subject: "Report on conference with Dr. Mina Rees and Dr. John Curtiss at Sands Point, 15 September 1947."

## NOTES TO CHAPTER 5 (CONTINUED)

17. Ibid.; Task Order No. 1, Contract N5 ori60, June 30, 1945; Amendment No. 6, T. O. No. 1, Contract N5ori60, September 29, 1948.
18. Memorandum, Perry Crawford, Jr. to Director, SDC, Subject: "Discussion of Project Whirlwind at ONR conference on 28 October 1947," October 29, 1947.
19. Ibid.
20. Project Whirlwind, Summary Report No. 2.
21. Ltr., J. W. Forrester to Director, SDC, att'n. Capt. G. M. O'Rear, November 21, 1947.
22. Ltr., J. W. Forrester to H. L. Hazen, Math Dep't., March 10, 1947.
23. Ltr., J. W. Forrester to Director, SDC, att'n. Capt. G. M. O'Rear, April 23, 1948.
24. Ltr., Warren Weaver to Chief of Naval Research, att'n. Mina Rees, February 20, 1947.
25. Ltr., J. W. Forrester to Director, SDC, att'n Capt. G. M. O'Rear, November 21, 1947.
26. Memorandum No. 94, W. S. Loud to J. W. Forrester, R. R. Everett, P. Franklin, Subject: "Suggestions for Further Work," August 6, 1947; Memo M-124, Philip Franklin to J. W. Forrester and R. R. Everett, Subject: "Location of Target from Combined Observations," October 21, 1947.
27. Memo M-160, P. Franklin to J. W. Forrester, Subject: "Mathematical Work of Project Whirlwind," November 12, 1947.

## NOTES TO CHAPTER 5 (CONTINUED)

28. Project Whirlwind, Summary Report No. 3., 3 December 1947; ltr., J. W. Forrester to C. O., Boston Br. Office, ONR, Subject: "Participation of Mathematicians and Scientists in Project Whirlwind Activities," January 30, 1948; J. W. Forrester Computation Bk No. 45, pp. 86-87.
29. Memo No. M-41, Subject: "Floor Space Required . . .," November 29, 1946; ltr., J. W. Forrester to N. Sage, November 29, 1946.
30. Ltr., Jackson and Moreland to N. McL. Sage (copies to Profs. Forrester and Steves), March 11, 1947.
31. Ltr., J. W. Forrester to Director, SDC, att'n Mr. H. C. Knutson, April 14, 1947.
32. Ltr., J. W. Forrester to Director, SDC, att'n Perry Crawford, April 28, 1947.
33. See: J. W. Forrester, Computation Book No. 44 (October 10, 1946-March 14, 1948); Summary Report No. 1, to SDC April 1946.
34. Conference Note C-14, Project Whirlwind, October 9, 1946.
35. J. W. Forrester, Computation Book No. 45, p. 29 (entry of March 17, 1947), p. 52 (entry of August 29, 1947) and p. 79 (entry of October 20, 1947).
36. See Francis J. Murray, The Theory of Mathematical Machines. (1947: King's Crown Press, New York.)
37. Memorandum, J. W. Forrester to R. R. Everett, Subject: "Professor Murray's visit," November 3, 1947; memorandum, H. H. Goode, SDC, to Director, SDC, subject: "Trip to Boston, Mass., 7-9 November 1947; Report of," November 12, 1947.

NOTES TO CHAPTER 5 (CONTINUED)

38. F. J. Murray, "Report on Mathematical Aspects of Whirlwind," submitted to Director, SDC, November 21, 1947.
39. Ltr., John von Neumann to Dr. Mina S. Rees, December 10, 1947.



## Chapter Six

### PROBLEMS OF FEDERAL ASSISTANCE

For Project Whirlwind and the Special Devices Center, 1947 and 1948 were years of increasing difficulties, even while significant progress in the design and fabrication of the physical computer was being accomplished. The joint discharge of the interfingered administrative and fiscal responsibilities which the Institute and the Navy bore was complicated by the organizational and policy changes occurring within the Navy. In consequence, both Project Whirlwind and the Special Devices Center came under intensified administrative and supervisory pressure as the Office of Naval Research consolidated its responsibility and authority for certain aspects of the Navy's research and development program. Misunderstandings between SDC and ONR--particularly the Boston Branch Office--led in October to a division of responsibility for the Project between SDC at Sands Point and the Boston Branch Office: SDC retained technical supervision, but responsibility for "business administration"<sup>1</sup> of the contract was assigned to the Branch Office. Relations between SDC and ONR continued to deteriorate,

nevertheless, until finally technical supervision of<sup>2</sup> the Project also was transferred to ONR. The assumption of direct technical responsibility for Project Whirlwind was effected by ONR between September of 1948 and February of 1949. It marked acceptance of the recommendation of its Mathematics Branch that the Branch "should have the responsibility of promoting those aspects of the program which involve research, the dissemination of information, and advising the Bureaus on novel applications of computers (in systems or otherwise) which involve research effort." The Special Devices Center, on the other hand, should be concerned only with the "application of machines of proved worth to devices within the scope of their responsibility, as the computing elements of training devices."<sup>3</sup>

The transfer of responsibility for Project Whirlwind had been a while in the making, but it was inevitable, for ONR had consistently demonstrated its determination to make itself master in its own house. It could be argued that in formal organizational terms and perhaps in substantial relationships as well, SDC's subordinate position was really not inferior to that which it had held under the Bureau of Aeronautics. However, ONR was created to perform a mission in the realm of research and development that existing naval

bureaus were not to be held responsible for. With the centralization of responsibility and authority for naval research and development under ONR, SDC could not continue to enjoy the wide latitude and flexibility of operation it had possessed when, under the aegis of the Bureau of Aeronautics, it had approved the transition from ASCA to Whirlwind--that is to say, the transition from a flight trainer and analyzer to a general-purpose digital computer. Instead, there occurred a shift in SDC's role that drastically reduced the scope of its activities in support of research and development.

Although there was a general cut in military funds for fiscal year 1948, the striking drop in funds made available to SDC by ONR demonstrates what had happened to SDC's earlier freedom to select and sponsor research and development projects. From approximately eleven million dollars nominally available to SDC in fiscal year 1947, the amount dropped to slightly more than a nominal five million for fiscal year 1948.<sup>4</sup> It was clear by June of 1948 that ONR had lost confidence in SDC's ability to handle the Project, and furthermore, that ONR was unwilling to follow Perry Crawford's "fearless and imaginative jumps into the future" because of limited funds and because of the belief that "the present job should be under control before bigger areas were staked out."<sup>5</sup> Crawford recognized the trend;

in September of 1948 he accepted a temporary assignment with the Research and Development Board of the Department of Defense with the intent, upon completion of the assignment, to return to ONR, but not to SDC.<sup>6</sup>

The delay which ensued between the contractual change of September, 1948 and the implementing directive of February, 1949, transferring technical supervision to ONR, probably mirrored both SDC's reluctance to yield completely the traditional freedom of action it had inherited from its predecessor, the Special Devices Division, and MIT's reluctance to accept technical supervision of Project Whirlwind by the Mathematics Division of ONR. By June of 1948, with the threat of transfer apparently hanging overhead, Nat Sage became sufficiently concerned to presume to discuss the threat with Dr. Alan Waterman, the chief scientist and civilian administrator within ONR. On this occasion Sage expressed the hope that "in making any decisions, the Navy would realize the enormous importance of engineering," including within its "administrative control . . . persons who understood the engineering rather than the scientific attack on a problem."<sup>7</sup>

Waterman, grasping the full import of Sage's comments, assured him that the Mathematics Branch under Mina Rees would not be placed in charge, but would represent the Navy only on "the mathematical

aspects of the project."<sup>8</sup> Subsequently, in September following, the Head of the Mathematics Branch was designated "Scientific Officer" for Project Whirlwind, the title reflecting possibly ONR's efforts to appease both MIT and SDC.

The Special Devices Center continued to exercise some supervision over the Project until the directive of February 8, 1949, which assigned "technical cognizance" to the Mathematics Branch. Thus, between September, 1948 and February, 1949, SDC had been gradually but completely phased out of the picture. The contractual amendment of September which had designated the Head of the Mathematics Branch as "Scientific Officer" of the Project had also contained the last allocation to Project Whirlwind out of SDC funds. The subsequent source of funds became the Physical Sciences Division of ONR, to which the Mathematics Branch was attached.<sup>9</sup>

The lack of harmony which characterized relations between Project Whirlwind and ONR since 1947 had initially stemmed from the apprehensions of the Mathematics Branch over the nature and purpose of the program, the quality of its leadership, and its alleged lack of mathematical competence, on the one hand, and from the inability of MIT personnel to allay these apprehensions, on the other.

An even more profound source of difficulty was involved, however, even though the differing policy views with regard to the proper way to go about developing a computer provided a major source of friction between Project Whirlwind and ONR. This more profound source of difficulty lay on the broadest policy level, from which the federal government through its agencies in the Executive and Legislative Branches appraised the operations of the scientific community and the value of those operations to the nation. The pursuit of scientific research and engineering development to help win the war usually had been sufficient justification in itself for the measures undertaken and the funds spent. But once the security of the nation was no longer in daily jeopardy, the conduct of peacetime research and development (R and D), together with government endorsement especially of such R and D, came once more under renewed, careful, and skeptical scrutiny by program managers, fiscal officers, and administrators in the Executive Branch, and by committees in the Congress. Once more the peacetime policies of the institution of government toward the established institution of science and toward the still relatively uninstitutionalized activities of an emerging scientific technology asserted themselves.

These policies, unfortunately, had been ambiguous and unsettled at best throughout the nation's history.

When one views the situation from this perspective, it is not surprising to learn that the growing gap between Project Whirlwind's views and those of the Mathematics Branch brought about, finally, a confrontation at the top level between one powerful member of the private education establishment, MIT, and its less powerful collaborator-adversary in the federal military establishment, ONR. This confrontation occurred at a time when ONR not only was seeking to assert its responsibility for naval research and development, but also was striving to gain the confidence of the scientific academic community--a confidence which, by and large, it ultimately succeeded in gaining.

The respective positions of MIT and ONR were, in significant measure, the consequences of the operation of historical trends transcending either institution's private history. These trends established the range of limited freedom of action and the apparent alternatives allowed to MIT and to the Navy where Project Whirlwind was involved. To pause and consider these trends is to render more understandable and natural, and less capricious and "political," the attitudes and actions

of the principals. It was not an affair that could be reduced to the appealing, dramatic simplicity of a sparring match carried on between MIT and Naval leaders in order to find out who were the more powerful. Rather, it was one of many smaller events characterizing the dynamic, historical distribution and redistribution of judgments and powers continually taking place and operating to bring about further adjustments in the subtle, ponderous, and leisurely process by which human institutions (in this instance, those of higher education and the national government) achieve a mutual accomodation over the longer ranges of time.

Viewed narrowly, it was Project Whirlwind's relative misfortune to be caught up in this process, but in the wider arena in which national R and D policies and practices were at that time being generated and modified, the stresses to which Project Whirlwind and ONR both were subjected could well be regarded as unexceptional. Indeed, had Forrester, Sage and the others at MIT bowed without a fight to the pressures which ONR mounted, the affair might well have been transformed into a "business-as-usual" situation (although not necessarily a pleasant or heartening one for any of the parties involved).

Viewed in a wider perspective, the difficulties which beset the relationship between Project Whirlwind and the Office of Naval Research emerge as ramifications

of the more fundamental difficulties which accompanied the transition the nation was undergoing as a result of the Second World War, a transition which was made even more urgent by the Cold War that set in shortly after the end of hostilities. It is quite generally recognized that the Second World War and its aftermath had compelled the nation to abandon its foreign policy of isolationism and to commit itself to a role of vigorous, active participation in world affairs. Less widely appreciated, however, is the fact that the War also had compelled the American people and their leaders to reevaluate the role of science and technology in the national life and to revise a national posture which in the pre-War years had been marked by the absence of any popular insistence that the Federal Government should formulate and implement a national policy comprehensively to encourage, coordinate and sustain science and technology as activities of vital concern to the national welfare.<sup>10</sup>

The wartime mobilization and coordination of the nation's scientific and engineering resources was neither new nor unique, for previous wars had seen similar efforts although not as successful or on as large and authoritative a scale. The continuation of this pattern in time of peace, however, by the creation of agencies empowered to direct, coordinate, and fund

R and D as a substantial and vital part of the national life was new and without precedent. When the President on August 1, 1946, signed into law two bills, one creating the Atomic Energy Commission, the other the Office of Naval Research, tangible proof was offered that the government had accepted and was implementing the principle of a continuing and comprehensive responsibility for the advancement of science and technology. The subsequent establishment of similar agencies, such as the Air Research and Development Command of the Air Force, the National Science Foundation, and the National Aeronautics and Space Administration, give further evidence of this continuing acceptance.

The statutory creation of the Office of Naval Research marked a victory within the Navy Department for a group of dedicated and perceptive civilians and military officers who early in the War had seen the need for the creation of a central office to coordinate and direct naval research and development. In the pre-War years naval R and D had been uncoordinated and routine. Conducted to as great an extent as possible within the laboratories of the various Bureaus and the Naval Research Laboratory, it had been concerned primarily with the improvement of existing procedures and equipment. The exception to the routine nature

of naval research and development was that conducted under the aegis of the Naval Research Laboratory where a taste for fundamental work had been developed. The Naval Research Laboratory was, however, "a very small exception to the general lack of research in both Army and Navy."<sup>12</sup>

During the War years as the government's total expenditures for R and D mounted, so did the Navy's. The latter's costs rose from \$13,566,899 in fiscal year 1940 to \$149,887,877 in fiscal year 1944. Total expenditures for this five year period, including monies transferred to other agencies, approximated \$405,000,000, about twenty-two percent of the government's total expenditures for the period. Of the \$405,000,000, the Navy disbursed \$348,626,000 itself: \$97,853,000 in its own laboratories; \$248,834,000 to private industrial laboratories; and \$1,939,000 to education and foundation laboratories.<sup>13</sup> The rising trend established during the war years was, with minor and occasional cutbacks, to be carried over into the post-war period.

Throughout the War, the Navy's bureaus continued to bear primary responsibility for research and development within their respective areas of responsibility. Coordination of the bureaus' respective programs was attempted, however, by the creation in the Office of the Secretary of the Navy of the Office of the

Coordinator of Research and Development. In addition to coordinating internal research and development programs, the Office of the Coordinator represented the Navy on the boards of other research agencies, maintained liaison with the Office of Scientific Research and Development, and kept the Secretary and the various bureaus and offices informed of research and development programs both within and without the  
<sup>14</sup> Navy.

The Coordinator continued to provide whatever central direction there was to the Navy's research and development programs, until the impending dissolution of OSRD, together with the growing complexities of the programs, caused the Secretary of the Navy to establish a central agency, the Office of Research and Inventions. This agency embodied in itself the Office of the Coordinator of Research and Development, the Office of Patents and Inventions, the Naval Research Laboratory, and the Special Devices Division. The aims of the new ORI were to (1) stimulate research and development throughout the materiel bureaus, (2) assume cognizance where a project was of major interest to more than one bureau, and (3) undertake by contract or within its own laboratories "fundamental work not unique to any  
<sup>15</sup> single bureau." The creation of the Office of Research and Inventions indicated that the Navy was

laying plans for the post-war period in recognition of the necessity to coordinate and direct the research and development programs of the respective bureaus and also aggressively to pursue fundamental research in areas pertinent to naval science and technology. Subsequently, these functions of the Office of Research and Inventions were given Congressional sanction when the Congress established the Office of Naval Research.

The Office of Naval Research represented a substantial victory for proponents of the principle of a central departmental authority to coordinate and direct Naval research and development, including basic research. The establishment of the Office of the Coordinator of Research and Development had been a major step in this direction and one accomplished over the protests of the General Board. The subsequent creation of the Office of Research and Inventions had advanced the principle considerably by providing it with Presidential sanction as well as Secretarial. The legislative establishment of the Office of Naval Research added Congressional acceptance of the principle to Presidential and Secretarial. Most importantly, Congressional approval guaranteed appropriation of the funds necessary to fulfillment of the principle--a guarantee not implied in the sanctioning of the

Office of Research and Inventions by Executive Order. Indeed, the Navy had been compelled to request of Congress the passage of legislation establishing the Office of Naval Research, because the House Appropriations Committee had refused to consider monies for the Office of Research and Inventions until the approval of the Congress had been given.<sup>16</sup>

Congressional resistance to the Office of Research and Inventions did not imply objections to the principle of centralization; rather it reflected the resistance of Chairman Carl Vinson of the House Committee on Naval Affairs to continuing Executive "use of war powers in peacetime." Such use, Vinson had warned, "could seriously impair the relations of the Navy Department with the Congress." In fact, the Committee on Naval Affairs had strengthened the Office by writing into the bill changes which gave the Office "control of all naval research," including--subject to Secretarial approval--authority to control the research programs of the bureaus. Thus, if the Secretary approved, the Office of Naval Research presumably would exercise authority over the total spectrum of Naval research and development.<sup>17</sup>

The Vinson Bill creating the Office of Naval Research became law on August 1, 1946, thus antedating by some four years the creation of the altered peacetime successor to the Office of Scientific

Research and Development, the National Science Foundation. Consequently, the Navy was one of the first government agencies to fill, at least partly, the void created by the phasing out at the War's end of the Office of Scientific Research and Development. In fact, the imminent end of the War had hastened the administrative and legislative steps which culminated in the establishment of the Office of Research and Inventions and its successor, the Office of Naval Research.<sup>18</sup>

From the beginning the primary task of the Office was the sponsorship of basic research. Development was to remain with the respective bureaus. The first Chief of Naval Research, Vice Admiral Harold G. Bowen, implemented this policy by supporting research of the most fundamental nature, and by 1947 the Office had planned a research program which would cost about \$20,000,000 annually. Toward the end of 1948 the Office had in its employ some 1,000 scientists distributed among three in-house laboratories and six branch offices. It had contracted for some 1131 projects at 200 institutions, a program accounting for approximately forty percent of the nation's total program in basic research, according to one estimate. The value of contracts in which ONR was involved approximated \$43,000,000, of which \$20,000,000 came from the Office's own funds; \$9,000,000 came from other

federal agencies--principally the Atomic Energy Commission--but distributed by the Office of Naval Research; and \$14,000,000 came from various universities, according to one tally. Thus, during the crucial post-war years while the Congress was debating the kind of organization which should be created at the national level to sponsor basic research, the Office of Naval Research was actively pre-empting the field and continuing a program that many considered vital to the national security.<sup>19</sup>

The founders of the Office of Naval Research had assumed that the Office, once established, would mount a comprehensive and sustained program in basic research, and one not restricted to areas of Naval pertinence only. The proponents of a peacetime Office of Scientific Research and Development, however, had assumed that Navy responsibility for a comprehensive program would be temporary, pending Congressional authorization of a national agency purposed to sponsor basic research in its broadest sense. Vannevar Bush, one of the most forceful advocates of a comprehensive national agency, had supported the Navy undertaking while voicing the reservation that it was "entered into with the full understanding on the part of everyone that it was to a considerable extent a temporary program, and that if the Congress saw fit to establish a Foundation for

the purpose, the principal burden of that work would be transferred to the Foundation."

The Navy would continue to sponsor some projects, Bush opined, but the bulk of basic research would be the responsibility of the foundation, where it could be managed "by a group which can combine the military and the civilian points of view and which can judge the thing from a somewhat broader basis than the services, by their very nature, can hope to judge it." Despite Bush's reference to an initial "full understanding," the Office of Naval Research did not share his opinion. While it was willing to transfer some Navy projects of broad interest to the proposed agency, it intended to continue its own program at the fiscal level already attained, expanding in areas of immediate pertinence.<sup>20</sup>

The expansive powers granted the Office of Naval Research by the Congress implied the eventual centralization of Navy-sponsored research under the new office. As ONR sought to implement the authority inherent within its enabling legislation, it took steps that had profound consequences for the Special Devices Division and for Project Whirlwind. Within a span of two years, as has been noted, the Special Devices Division was subordinated as the Special Devices Center, and Project Whirlwind was transferred to the jurisdiction of the Mathematics Branch of the Office of Naval Research. For Project Whirlwind this meant that the sympathetic,

understanding, fiscal supervision and program encouragement of the engineers of the Special Devices Center was replaced by the skeptical, less-than-enthusiastic supervision of the mathematicians of the Mathematics Branch. It did not help matters that the latter were more interested in the computer as a tool for scientific computation than as the "brain" of a command-and-control center for tactical and logistical operations, such as envisioned by Forrester, Everett, Perry Crawford, and others at Sands Point who became familiar with "L-Notes" L-1 and L-2 which the two MIT engineers had written.<sup>21</sup>

Another complicating factor in the relations between the two groups was the rising cost of carrying forward Project Whirlwind at the very time the nation was undergoing post-war military retrenchment, with its impact upon military budgets. Funding, if a problem, had been a very minor one with no discernible effect until the fall of 1948. From then the question of money was to overshadow all others and continue a chronic source of irritation and difficulty.

The initial amount of money--\$1,194,420--committed by the Navy to the Project under the terms of Task Order No. 1 of Contract N5ori-60<sup>22</sup> had been increased two years later by an additional \$100,000<sup>23</sup> and again in January of 1948 by \$520,000.<sup>24</sup> The first increase was required apparently to meet the extra costs incurred

by extension of the contract's terminal date for one year to June 30, 1949. The second increase was intended to defray the costs of program acceleration requested early in 1947 by SDC.<sup>25</sup> In addition to expanding and expediting the program at MIT, part of the work was subcontracted to Sylvania Electric Products, Incorporated at an estimated cost of \$319,576.75. Sylvania was to "conduct studies and experimental investigations in connection with: 'final packaging design and construction of the Whirlwind I electronic digital computer.'"<sup>26</sup>

The cost of the Sylvania subcontract was included in the request for additional funds for fiscal year 1948 which was forwarded to SDC, in August, 1947, totalling some \$441,520.75. These funds, it was noted, would not cover the costs "for photographic input-output devices" to be purchased from Eastman Kodak or for "aircraft simulation components" which ONR would have to fund separately.<sup>27</sup> The amount finally allocated by the Navy was \$520,000, approximately \$80,000 above the MIT request, but between the time of the request and final Navy action, Project Whirlwind had entered into a subcontract with Eastman Kodak in the amount of \$70,000 for "photographic storage equipment . . . necessary to the completed simulator."<sup>28</sup> Neither MIT's request nor the Navy's final allocation caused any

furor in either organization. Jay Forrester did comment to Nat Sage that the estimated cost contained in Amendment Number 4, which officially allocated the additional funds, was some \$500,000 short, but there the question died.<sup>29</sup>

The "blowup" came in the fall of 1948, following a letter from Nat Sage to SDC in which he requested for the Project funds in the amount of \$1,831,583 for the fifteen-month period between July 1, 1948 and September 30, 1949. This amount, when added to an unexpended amount of \$385,260, produced a total of \$2,216,843 or a monthly expenditure rate of approximately \$150,000. This figure raised havoc with ONR's budget and brought into the problem both the Chief of Naval Research and MIT's top administration.

Earlier, MIT's top management had become concerned about the friction which had developed between Project Whirlwind and ONR, and presumably had become at first a trifle dubious either about its ignorance of the Project in detail or about the Project itself and its management. To determine the quality of the program and its leaders, the Institute leadership through Dr. James R. Killian, Jr., then vice-president, asked Ralph Booth, a member of the MIT Corporation's Electrical Engineering Committee, to review the status of the Project. This was in

the winter and early spring of 1948. Booth, questioning his own competence to "pass on the theoretical and technical merits" of the electrostatic storage tube under development, retained as a consultant Dr. J. Curry Street of the Harvard physics faculty and formerly a member of the MIT Radiation Laboratory.

Booth, and presumably Street, visited the Project in May, July, and August of 1948 to study its operations and obtain the information and impressions which would be necessary for an appraisal of its worth. Booth submitted his report to Killian on August 26, prior to the exchange of views which took place between ONR's and MIT's top administrators in September and December of 1948.

In his report to the vice-president, Booth stated that the purpose of his review had been "to determine whether the accomplishments to date and the organization and procedure of the work currently in hand insured a successful completion of the project approximately in accordance with the present schedule." His report was very favorable. Presumably supported by Street, Booth observed that the Project's "Accomplishments . . . give every promise of providing within the scheduled date a successful computer at speeds hitherto unrealized." With the exception of the storage tube, the program in all its phases

had reached that point, he noted, "where the remaining work can be classified as design engineering or development of refinements." The storage tube, he anticipated, would be successful; however, if not, the other components of the computer could be adapted to other types of memories with no greater penalty than some loss of speed in computation.

Booth and Street were so enthusiastic about the potential of the storage tube that they strongly urged the "Navy be asked to acquaint itself with the high promise of this development, since it is entirely possible that this tube may supplant mechanisms which hold less promise and which are in an earlier stage of development and on which appreciable sums of Navy research money are currently being expended." All in all, Booth, found the Project to be "well-organized, staffed by efficient, capable people, and . . . conducted in proper accord with the timetable . . ."<sup>30</sup>

It is not surprising to learn that when Booth's laudatory comments were added to the support rendered the Project by Nat Sage and Gordon Brown, the MIT top administrators considered themselves sufficiently well informed to become convinced that Project Whirlwind was worthy of their support. So they came to its rescue in the funding crisis of 1948.

Nat Sage's request on August 4 for 1.8 million dollars to cover costs through fiscal year 1949

and the first three months of fiscal year 1950 may not have caught the Navy totally unawares, but it was nevertheless an irritating if not downright disturbing request. Not only did it ask ONR to double the amount ONR had already committed to the Project, but it was submitted some thirty days after the fiscal year to which it was to apply (FY 1949) had begun. To the Navy it must have been a splendid example of the continuing, erratic and unpredictable pattern of behavior followed by Forrester and his colleagues who, between the spring of 1947 and the fall of 1948, had raised their estimates of financial requirements for fiscal year 1949 by some 1.3 million dollars. Here was a pattern particularly disturbing to administrators whose policies were controlled more by financial considerations than by technical.<sup>31</sup>

Looking back, one could see that in the spring of 1947 both MIT and SDC had agreed that fiscal year 1949 costs might equal a half million dollars. But by October, 1947, MIT foresaw fiscal year 1949 costs of \$940,000, and in December it raised the estimate again to 1.2 million dollars. Since the fiscal year would begin July 1, 1948, it was then a bare six months in the offing.

ONR, in the meantime, had been raising its estimates at a different pace. Although ONR's subordinate, SDC, had agreed on the half-million-dollar figure in

the spring of 1947, its reaction to MIT's \$940,000 estimate was to go up only to \$600,000. But during the spring of 1948, SDC once again came into agreement with MIT, now at the new, higher level of 1.2 million. Then in June of 1948, just before fiscal year 1949 was to begin, Admiral Paul F. Lee, Chief of Naval Research, cut ONR's support back to \$900,000.<sup>32</sup> The following month, after the beginning of fiscal year 1949, Forrester proposed to raise the ante to 1.8 million dollars for the fiscal year which had already begun. ONR's response by Admiral Lee's successor, Admiral T. A. Solberg, was a courteous but firm "no"; \$900,000 would remain ONR's commitment.

Admiral Lee's reasons underlying his decision to reduce Project Whirlwind's allocation to \$900,000 for fiscal year 1949 were presumably many and complex, but a lack of funds was not included among them. At the time Lee made his decision it is reasonable to assume that he must have had a fairly good idea of what the amount of unexpended monies to be carried over from fiscal year 1948 would be, for at the time the 1949 budget was under consideration, such monies were estimated to approximate \$32,000,000.

Other considerations had to provide his guidelines, therefore. In view of the pattern of events involving the transfer of technical responsibility for Whirlwind from SDC to the Mathematics Branch,

which was to take place during the winter of 1948 then approaching, one of the considerations very likely was the intent to bring SDC into its proper relationship with the rest of the ONR structure--in this case by diminishing SDC's role in computer research. The decision had been made only after a careful review of SDC's programs for fiscal year 1949<sup>33</sup> and the \$900,000 permitted SDC for allocation to Project Whirlwind represented the last monies SDC was to receive for this purpose. Future funds were to be allocated and controlled first by the Physical Sciences Division, to which the Mathematics Branch was attached, and subsequently, after its formation, by the Mathematical Sciences Division.

Furthermore, the amendment which announced the allocation also announced the appointment of the Head of the Mathematics Branch as "Scientific Officer" of the Project,<sup>34</sup> and within six months Lee's successor was to eliminate SDC from the program completely by giving direct control of the Project to the Mathematics Branch.<sup>35</sup> As has been noted above, Perry Crawford had recognized the trend and left SDC.

Another primary reason, presumably, was Lee's intent to bring Project Whirlwind under firm control. There is no sign that he was discouraged in this by the Mathematics Branch. On the contrary, the Branch had been concerned and disturbed about the Project ever since ONR had become responsible for it. The Project by its own behavior had provided some substance to feed the fears of the Mathematics Branch. Within a period of eighteen months, at most, Forrester's estimates of the additional financial needs for fiscal year 1949 had escalated from \$500,000 to \$940,000 to \$1,200,000 to the final figure \$1,831,583, and this final figure exceeded by a magnitude of three the combined allocations for the two previous fiscal years.

For that matter, it exceeded by some \$700,000 the original monies obligated under the terms of the contract when first negotiated. Moreover, the 1.831 million (1.465 for the 12-month fiscal year) almost matched

the 1.850 million which Lee, in his testimony supporting the proposed Navy budget for fiscal year 1949, had estimated would be obligated in the entire general area of mathematical research. The Whirlwind request, which alone would have used approximately ten percent of ONR's 1949 funds for contract research, thus threatened to consume almost the total amount designated for research in mathematics.<sup>36</sup>

It is to be hoped that Admiral Lee understood his job well enough to have considered both the impact of his reduction upon MIT alone and its reverberatory fiscal effect upon the rest of the academic community with which ONR dealt. And this hope is borne out by further consideration of the fact that the Navy, through its various bureaus as well as ONR, maintained active research and development programs in the nation's universities, spending some \$25,000,000 in fiscal year 1948 alone.<sup>37</sup> In this connection it is doubtful that the 1948 funding crisis of 1.8 million dollars with MIT would have had drastic financial effect upon university relations in general. However, the psychological impact might have been considerable, particularly upon the fragile, newly established relations between ONR and the universities; in this respect the matter had to be handled with the greatest finesse and diplomacy.

Whatever Lee's reasons, Project Whirlwind's

requested allocation for fiscal year 1949 had been cut almost in half, and the monies would be some \$550,000 less than had been planned for. This threatened a reduction in the planned monthly expenditures, from a rate of \$150,000 to an amount slightly in excess of \$105,000, if the group were to stay within the financial limits imposed by ONR's decision.<sup>38</sup> It was a proposed cutback of approximately thirty percent, and its repercussions would be severe. The program's rate of progress as planned by the MIT group and accepted in the main by SDC, although not by ONR, would be seriously curtailed and at a time when Forrester and his colleagues were quite sanguine in their belief they were on the edge of success. The reaction of Forrester and Sage to the threat of inadequate funds to meet the planned schedule provoked a major issue that was then carried to the highest administration levels of both ONR and MIT.

On September 2, 1948, the new Chief of Naval Research, Admiral T. A. Solberg, approached the President of MIT, Dr. Karl T. Compton, with the suggestion that in light of the wide discrepancy between the funds requested by Project Whirlwind and the allocation made by ONR for fiscal year 1949, "future commitments and rate of expenditure be scaled down," pending an evaluation "of both the technical and financial requirements

of the project." Such an evaluation, he implied, might result from a study of all computer programs then being conducted by the Computer Sub-Panel of the Research and Development Board. In the meantime, he proposed a conference be called between MIT and ONR to "reexamine both the technical and financial scope of the project" in order to clarify "future policy and . . . establish a firm basis upon which Project WHIRLWIND should operate in the future."<sup>39</sup>

Solberg's letter to Compton brought the MIT president directly into the matter and precipitated within Project Whirlwind a flurry of activity aimed toward indoctrinating Compton and winning his support to the Project. Within a few days after receipt of the letter, Compton conferred on the matter with James R. Killian, Jr., his vice president (very shortly to succeed him), with Nat Sage, with F. L. Foster, Sage's assistant in the Division of Industrial Cooperation, and with Jay Forrester. At this conference Forrester presented to the group his appraisal of "the size of the total digital computer program . . . the United States was facing," estimating at that time that costs would run some \$100,000,000 per year for ten years "if the apparatus that people counted on getting was to be made available." In his comments, Forrester included the opinion that some estimates of time and cost tended "to be hundreds of times too low."

The MIT president was apparently enough impressed by Forrester's presentation to request that it be suitably prepared to permit him to take it to Washington for distribution within influential official circles. This informal request was followed on the same day by a formal request to Sage from Compton, in his capacity as president of MIT and hence ultimately responsible for Project Whirlwind and also in his capacity as an advisor to the Armed Services, to prepare a report. This report would clearly present the "potentialities for useful applications inherent" in the digital computer and would give some estimate regarding the "time, money and staff" which would be necessary to "carry digital computing equipment to the point of use by the Armed Services."

Such a report, Compton noted, would not only be of immense help to him as he sought to grasp fully the potential use and cost of digital computer programs in general and Whirlwind in particular, but would be also of great benefit to ONR and to any other organizations which might be considering the use of the digital computer. It obviously would provide Forrester with an excellent opportunity to arrange his thoughts and to gain access for them through Compton to higher command levels within the Government, an important factor in the struggle with ONR which

was looming on the horizon.<sup>40</sup>

The decision was also taken at the internal MIT conference to press Ralph Booth for his formal evaluation of the Project. (The conferees could be sure it would be favorable, in light of his complimentary comments in his letter of the previous August 26 to Killian.) It was further decided to ask Booth to serve as a representative of MIT in the forthcoming meeting with the Chief of Naval Research and his staff. Apparently their own background knowledge, Booth's letter of endorsement, Forrester's presentation, and Sage's judgment and confidence in the Project persuaded Compton and Killian that the Project was in competent hands and had a significant contribution to make.

Although the record does not show it, the Institute's leaders may also have recognized that here was a test case made to order upon which they could make a stand suitable to the purposes and need of establishing viable practices and durable relationships favorable to the continuing conduct of military-sponsored research by private universities. It is not unreasonable to suppose that those responsible for Institute policies--who were already involved in an emerging, loose, but effective organization of civilian scientists (known informally among scientists two decades later as the "Eastern Establishment"),

the aim of which was to maintain the intelligent prosecution of private scientific and engineering research funded by federal interests--were astute enough to realize that three years had passed since the War had ended, that the shakedown period into peacetime procedures was drawing to a close, causing these procedures to lose the plastic flexibility they had possessed when new, that the tenor of international affairs was becoming increasingly discordant as a consequence of Stalin's vigorous intransigence, and that the computer technology then dawning offered prospects and applications in war and peace that quite transcended those afforded by the usual military research project. In any event, whether they were moved or not by such explicit long-range considerations in addition to their informed faith in the competence of Project Whirlwind, the MIT leadership made elaborate preparations that beggared those undertaken in ONR.

The confidence and the support engendered at the Institute were displayed not only by Compton's request to Sage for the report he wished to circulate in Washington, but also by Sage's observation that "these reports must be gotten into various people's hands fairly promptly." Even more emphatically, it was demonstrated by the men Compton appointed to represent MIT in the forthcoming meeting

with ONR, for once the Institute's position had been determined and Solberg's proposal accepted, Compton nominated Jay Forrester and Nat Sage, along with Ralph Booth, to argue the Project's cause--three men whose views were known, whose biases and commitments in the matter were shared, and whose policy views were in close accord with those of MIT's top management.<sup>41</sup> The Institute leadership had heard the case, had rendered its judgment, and had not found the Project wanting. Thus prepared, they were ready to meet with ONR.



NOTES TO CHAPTER 6.

1. Memorandum, Perry Crawford to Director, SDC, subj.: "Trip to Boston, Mass., 8 January 1947 to 10 January 1947; report of, "January 28, 1947; memorandum for the file [no author], subj.: "SDC Computer Section Comments on Reference (a)," August 26, 1947 [from SDC's files]; ltr., W. R. Mangis, to Director, SDC, and to CO, Boston Branch Office, ONR, subj.: "Contract N5ori-60 (M. I. T.) - Assignment of Contract Administration Responsibilities," October 2, 1947.
2. Memorandum, Perry Crawford to Director, SDC, subj.: "Report on visit to MIT on 9 January 1948," January 12, 1948; Amendment #6, T. O. #1, Contract N5ori60, September 29, 1948; ltr., T. A. Solberg, Chief of Naval Research, to Director, SDC, subj.: "Contract N5ori60 Task Order I Massachusetts Institute of Technology; change in cognizance of," February 8, 1949.
3. Memorandum, N424 (Mina Rees) to N101, subj.: "Responsibility for Computer Research and Development," January 28, 1949.
4. Annual Report of the Secretary of the Navy for the Fiscal Year 1947, Washington, 1948, p. 82.
5. Ltr., N. McL. Sage to Jay Forrester, June 10, 1948.
6. J. W. Forrester, Computation Book No. 45, November 27, 1946 to December 10, 1948, pp. 134 and 151.
7. Ltr., N. McL. Sage to Jay Forrester, June 10, 1948.
8. Ibid.
9. Amendment #6, T. O. #1, Contract N5ori60, September 29, 1948; Ltr., T. A. Solberg, Chief of Naval Research, to Director, SDC, subj.: "Contract N5Ori-60 Task Order I Massachusetts Institute of Technology; change in cognizance of," February 8, 1949; Amendment #7, T. O. #1, Contract N5ori60, March 31, 1949.
10. See Kent C. Redmond, "World War II, a Watershed in the Role of the National Government in the Advancement of Science and Technology," Charles Angoff (ed.), The Humanities in the Age of Science (Rutherford, N. J., 1968), pp. 166-180. For an excellent history of the federal government and science, see A. Hunter Dupree, Science in the Federal

Government: A History of Policies and Activities to 1940 (Cambridge, Mass., 1957).

11. Annual Report of the Secretary of the Navy - Fiscal Year 1940, pp. 25-7; see also the Report for FY 1939, pp. 24-5.
12. Dupree, Science in the Federal Government, p. 333.
13. The Government's Wartime Research and Development, 1940-44; Report from the Subcommittee on War Mobilization to the Committee on Military Affairs pursuant to S. Res. 107 (78th Congress) and S. Res. 146 (79th Congress) Authorizing a study of War mobilization problems, July, 1945, Part II: "Findings and Recommendations," 79th Cong., 1st Sess., Senate Subcommittee Report No. 5, pp. 5, 56-67, 70-71. Senate Hearings before a Subcommittee of the Committee on Government Operations, House of Representatives, 85th Cong., 2nd Sess., July 14, 15, 17, and 18, 1958, on: Research and Development (Part 2 -- Military Research Representatives).
14. Annual Report of the Secretary of the Navy for the Fiscal Year 1942, Washington, 1942, p. 11; "The Bird Dogs, The Evolution of the Office of Naval Research," Physics Today, XIV (August, 1961) 31-2.
15. Committee on Naval Affairs, House of Representatives, Hearing on H. R. 5911 (ex-4317), to Establish an Office of Naval Research in the Department of the Navy, March 26, 1946, pp. 2821-22. Annual Report, Fiscal Year, 1945, The Secretary of the Navy to the President of the United States, pp. 30-1; see also the Secretary's Annual Report for 1946, pp. 66-73.
16. Hearing ... to Establish an Office of Naval Research, pp. 2834-7.
17. Ibid., pp. 2840-57.
18. "Evolution of ONR," Physics Today, XIV (August, 1961) 33-5.
19. Hearing ... to establish an Office of Naval Research, p. 2847; John E. Pfeiffer, "The Office of Naval Research," Scientific American, 180 (February, 1949) 11-5; Carroll W. Pursell, Jr., "Science and Government Agencies," David D. Van Tassel and Michael G. Hall (eds.), Science and Society in the United States, (Homewood, Ill., 1966), pp. 245-6.

20. Hearings before the Committee on Interstate and Foreign Commerce, House of Representatives, 80th Cong. 1st Sess. on H. R. 942, H. R. 1815, H. R. 1830, H. R. 1834, and H. R. 2027, Bills relating to the National Science Foundation, March 6 and 7, 1947, pp. 208, 231-54.
21. Task Order No. 1 to contract N5ori-60, June 30, 1945, and amendments No. 4, January 21, 1948; No. 6, September 29, 1948; and No. 9, July 1, 1949. Interview, J. W. Forrester and R. R. Everett by the authors, July 24, 1964.
22. Task Order No. 1, Contract N5ori-60, June 6, 1945.
23. Amendment No. 3, T. O. No. 1, Contract N5ori-60, June 26, 1947.
24. Amendment No. 4, T. O. No. 1, Contract N5ori-60, January 21, 1948.
25. J. W. Forrester, Computation Book No. 45, p. 35.
26. Ltr., (illegible signature) to MIT, DIC, subj: "Contract N5ori-60, Task Order 1, Proposed Cost-plus-fixed-fee Sub-contract to Sylvania Electric Products, Inc., in the aggregate amount of \$319,576.75," September 22, 1947; Prime Contract No. N5ori-60, Sub-contract No. 1, DIC Project No. 6345, Revision No. 1, September 24, 1947.
27. Enclosure "A", "Proposed form of letter for budget request to Office of Naval Research," to ltr., J. W. Forrester to F. L. Foster, DIC, MIT, August 14, 1947; ltr., F. L. Foster to CO, Boston Branch Office, ONR, subj.: "Contract N5ori-60, Budget for Fiscal Year 1947-48," August 15, 1947; Memorandum for Files, J. B. Thaler, Procurement Officer, SDC, subj.: "Status of Contract N5ori-60 with Massachusetts Institute of Technology and N6ori-133 with McKiernan Terry Corp. Summarization of facts and events leading thereto," October 23, 1947.
28. Memorandum, J. B. Thaler, SDC, to H. W. Fitzpatrick, Chief Accountant, ONR, subj.: "Contract N5ori-60, Task Order 1, Massachusetts Institute of Technology; Financing thereof," November 3, 1947.
29. Ltr., J. W. Forrester to Nat Sage, subj.: "Amendment No. 4 to Project Whirlwind Contract N5ori60," February 2, 1948.
30. Ltr., Ralph D. Booth to Dr. James R. Killian, Jr., Vice President, MIT, August 26, 1948.

31. Memorandum, Perry Crawford, Jr. to Technical Director, SDC, subj.: "Project Whirlwind, Fiscal Requirements of," July 22, 1948.
32. Memorandum, C. H. Doersam, Jr. to Perry Crawford, subj.: "Project Whirlwind---24-x-3, Budget Estimate of," October 24, 1947; memorandum, Perry Crawford, Jr. to Technical Director, SDC, subj.: "Project Whirlwind, Fiscal Requirements of," July 22, 1948; ltr., J. R. Ruhsenberger, SDC, to Chief of Naval Research, att'n. Code N101, subj.: Project Whirlwind, Contract N5ori-60, Financing of," August 18, 1948.
33. Ltr., J. R. Ruhsenberger to Chief, ONR, August 18, 1948.
34. Amendment No. 6.
35. Ltr., Solberg to D. R., SDC.
36. Hearings before the Subcommittee of the Committee on Appropriations, House of Representative, 80th Congress, 2nd Session, on the Department of the Navy, Appropriations Bill for 1949, p. 968; Amendment #6, T. O. #1, Contract N5ori-60, September 29, 1948; Ltr., J. R. Ruhsenberger, SDC, to Chief of Naval Research, att'n. Code N101, subj.: "Project Whirlwind, Contract N5ori-60, Financing of," August 19, 1948.
37. Hearings on S. B. 1560, p. 28, March 1948.
38. Ltr., J. R. Ruhsenberger, SDC, to Chief of Naval Research, att'n. Code N101, subj.: "Project Whirlwind, Contract N5ori-60, Financing of," August 19, 1948; Procurement Directive, Contract N5ori-60, T. O. #1, Directive #Nr-720-003/7-22-48, August 11, 1948, attachment "C" Clearance Memorandum prepared by J. B. Thaler, Procurement Officer, SDC.
39. Ltr., T. A. Solberg to K. T. Compton, September 2, 1948.
40. N. McL. Sage, "MEMORANDUM of Conference between Dr. Compton, and Messrs. Killian, Forrester, Foster and Sage on Project Whirlwind," September 8, 1948; ltr., K. T. Compton to N. McL. Sage, September 8, 1948.
41. N. McL. Sage, "Memo on Conference . . . on Project Whirlwind," Sept. 8, 1948; ltr., R. D. Booth to Dr. J. R. Killian, Jr., Aug. 26, 1948; ltr., Henry Loomis to T. A. Solberg, Sept. 10, 1948.

## Chapter Seven

### BREAKING NEW TRAILS

Project Whirlwind obtained the support of the Institute leadership in part because of the information and attitudes and judgments that Nat Sage and Jay Forrester conveyed and in part because of the engineering operation that Forrester and his associates had been mounting in the Barta Building.

Within the Project, under Robert Everett's leadership during 1947 the operating requirements of the proposed computer had been incorporated into "block diagrams" stipulating the coordinated and systematic operation of the basic functional components of the proposed machine. Using the block diagrams as master plans specifying the performance of the components singly and together, Everett, Forrester and several of the engineers then proceeded during 1947 and 1948 to lay out and review the design of appropriate electronic circuits. These would carry out the physical operations which would correspond to the mathematical and logical operations associated with binary digital computation and with

the storing, retrieving, and evaluating of such digital information.

Since this is not an engineering history of the Whirlwind machine that was designed, built, and put into operation between 1947 and 1951, specific detailed analyses of the many engineering problems encountered and the solutions worked out have no place here. In the view of the authors, the inside engineering story available only to readers possessing a specialized scientific and engineering technical education does not provide the only means of obtaining an illuminated understanding of the research and development process under case study here -- a process which has become a characteristic social and economic activity of twentieth-century America. Clearly, the technical engineering progress accomplished by the Project Whirlwind engineers continually influenced the course of events, and equally clearly, the engineering story vitally affected the eventual outcome of the enterprise. To give these aspects of the larger story the justice that is their due, a technical digression would be required that is beyond the scope of this case study. Consequently, in selecting an alternative to the specialist's route to understanding, the authors sought to convey the import and the general character of the engineering

activity of the Project by indicating in the language common to us all the more important events that occurred in the uncommon-sense realm of science and engineering.

In general terms, the young MIT graduates in charge of the enterprise faced the task of converting mathematical, logical, abstract concepts into working machinery. The abstract models they conceived and worked up began, for the most part, with theoretical considerations of the arithmetical and logical operations, together with the appropriate and varied sequences of these, that were to be performed by equipment capable of carrying on physical (i.e., electrical) operations corresponding to the abstract arithmetical and logical operations. Until the proper patterns of abstract operations were worked up, no suitable machinery could be devised.

Everett embodied the abstract operations and their patterns in "block diagrams" which set forth the appropriate logical functions. Ideally, once a block diagram had been organized, presenting the sequence of logical steps necessary to accomplish, say, a particular computation, then the engineers could turn to the problem of designing the electronic circuits, including the wiring, the resistors, the condensers, the tubes, and similar elements. These

circuits when properly constructed could accomplish, in physical hardware susceptible to differing, controlled, electrical states, the logical steps and computational results desired.

In essence, the designers' tasks were like those carried out in the following homely illustration: to maintain order at a busy street intersection, colored signal lights are turned on and off in an appropriate sequence. Any driver who has ever encountered malfunctioning stop signals knows how important the orderly sequence is, and any driver who has waited impatiently for a break in heavy traffic in order to make a left turn understands how important it is to install a system of appropriate lights, appropriately colored and appropriately sequenced to give the left-turners their legitimate opportunity to proceed.

The relevance of this illustration to the designing of computers lies in the fact that lights turned on and off correspond to the movement of traffic in different directions, with the result that a pedestrian at such an intersection, even when no cars are in sight, knows the meaning of the pattern and sequence of the colored lights going on and off. In the case of Whirlwind and contemporary early computers the sequence and patterning of

selected radio tubes and circuits turned on or off meant, or corresponded to, analogous logical and mathematical operations being carried out. (In later computers transistors replaced the radio tubes to carry out the same functions in smaller machines employing, more efficiently, less electric power.)

The problem that Everett, Forrester, and their contemporaries faced during the late 1940's was that they had little or no experience working out such sequences; theirs was the predicament of auto traffic planners who have had no practical experience controlling traffic. In lieu of the knowledge of experience, Everett had at his disposal the theoretical insights of the pioneering investigators, among whom were Aiken, Babbage, Bush, Caldwell, Crawford, Eckert, Goldstine, Mauchly, Stibitz, von Neumann, and a handful of others. The practical experience of these pioneers was so limited, in comparison to the challenge the Aircraft Stability and Control Analyzer offered, that Everett was compelled to undertake pioneering and highly complicated system-building of his own which had no precedent, especially in the realms of reliability of performance and rapidity of operation demanded by the simulator.

It is not possible to rank the originality of Everett's and Forrester's contributions-in-detail

with those of their contemporaries and predecessors, other than to point out that Forrester's managerial and inventive talents and Everett's detailed logical designs, together with their resulting embodiment in the assemblage of electronic hardware called "Whirlwind I," produced a working computer of unprecedented speed and reliability and a complement of engineering personnel possessing unequalled (at the time) design sophistication and engineering "know-how." Everett and Forrester, operating as engineering and managerial alter-egos and supplements of each other as the years passed, were primarily responsible for the complexion of the Project and, consequently, for its failures and successes.

Yet the measure of their contribution to the state-of-the-art of the emerging scientific technology of the computer cannot be well assessed for a variety of reasons, of which the most important is the lack of balanced assessment of the contributions of their predecessors and contemporaries after the brief lapse of a quarter of a century. There has emerged instead, in the technical computer literature, a miscellaneous collection of views which reveals that the insights of some of the pioneers were promptly appreciated at the time (e.g., von Neumann's), others were valued at the time and neglected later (e.g., Mauchly's), and others were neglected at the

time and exaggerated later (e.g., the magnificent failure of Babbage). While these do not exhaust the range of instances, they illustrate the confused historical situation existing, a situation that is a consequence of the prevalence of uninformed notions regarding the process by which events of the past give rise to events of the present. The alphabetical sequence of representative names, given above, corresponds neither to the chronological sequence and the overlapping of their contributions nor to the relative value or profundity of importance of their contributions, for investigators have not yet carried out the massive research necessary to clarify the picture and achieve a consensus. As a result, Aiken's contribution, is widely hailed, for example, as it should be, while those of Mauchly or Stibitz or Caldwell -- to cite other examples -- remain obscured. The problem of technical and historical evaluation here is basically epistemological, arising as it does from inadequate understanding of the historical process, and it is typical of a technology in which the inventors of the brick, the wheel, printing, and the telescope, to name but a few, are lost to history, while the identity, significance, and roles of the contributors to the invention of the telegraph, the light bulb, the radio, television, and many other recent items

remain obscure because of the relentless and inappropriate search for heroic figures and the general oblivion to the social character of the inventive process in science and especially technology.

For these reasons the most that is attempted in this history of Project Whirlwind is to lay before the general reader the managerial, fiscal, and technical factors that appear to be both distinctive of the Project and representative of contemporary research and development. Thoroughly representative was the procedure by which the Project engineers proceeded to convert theoretical abstractions to physical operations carried on by pieces of hardware organized into a systematic array. Thus, the electronic circuits had to meet the functional requirements of the block diagrams. But no computer had yet been built to specifications such as Forrester and Everett at MIT and Perry Crawford, Noel Gayler, Harry Goode, Leonard Meade, Peter Gratiot, and (later) Captain O'Rear at SDC contemplated, even though Aiken, Mauchly, Eckert, von Neumann, Goldstine, and others had demonstrated both the practical promise and the theoretical possibility.<sup>1</sup> Furthermore, the operating speeds required by the ASCA problem were so great as to be without design precedent. As an early issue of the Project's Summary Report quietly understated,

"additional detailed knowledge" was needed regarding the "timing and synchronization of operations performed by individual circuits when they are integrated into large-scale systems."<sup>2</sup> Accordingly, "operating times for each type of circuit to be used were determined by measurement, and the block diagrams were redrawn in terms of these specific circuits."<sup>3</sup>

The progress of a single electric pulse through various component parts of the computer could be calculated. Consequently, the engineers could ascertain theoretically whether synchronous operation of the components was being obtained, modify their circuit designs to obtain the synchronization they required, and then test the resulting hardware singly and in system hook-up to make sure it met their design requirements.

They found that essential computing operations could be performed rapidly enough to be acceptable: "Calculations showed that with present circuits the multiplication process could be safely performed no faster than the rate permitted by a time-pulse repetition frequency of two megacycles per second. This speed is considered adequate for Whirlwind I," Forrester reported at the end of 1947. Although faster speeds were attractive and possible, the engineers realized such modifications would also

perpetuate design changes and thereby postpone the operating date. Thus, even though experimental a-c flip-flop circuits appeared to be appreciably faster than the d-c flip-flop circuits the engineers had checked out in detail (fifteen hundredths of a microsecond, as against twenty hundredths), because these circuits would be used over and over again as one of the basic types of building-block throughout the entire computer and because the engineers knew too little about the general performance characteristics of the a-c flip-flops, they would not risk switching, in premature ignorance, to the a-c design. Besides, conversion could be accomplished "with little difficulty if desired" at a later date.<sup>4</sup>

Realistic engineering policy required continuing compromises to be made between the attractive, untried ideal and the practical, in order to achieve actual machinery. The Project was, after all, operating on a schedule, a circumstance that neither the Whirlwind engineers nor the ONR program managers could ignore, in view of the rising costs of the Project. The immediate tasks before the Whirlwind engineers included the formulation of component and subsystem parameters that would stand, the preparation of suitable specifications and drawings, and the delivery of these to Sylvania engineers so that

physical components and subcomponents could be manufactured and delivered to the Barta building, where Whirlwind I would be housed.

At the same time that basic circuit diagrams were being completed, laboratory testing equipment was being designed, purchased and developed so that present and future development of systems could "be facilitated by a line of standardized electronic test equipment for generating, gating, and distributing pulses at desired repetition frequencies." The object was to enable research engineers, "by rapid interconnection of various units, [to] set up and experiment with sections of computer systems."<sup>5</sup>

Professor Murray during his November visit had raised a question with Forrester that pointed up a standing problem confronting all computer designers. Since one defective vacuum tube, one flawed circuit, could nullify an entire calculational sequence and possibly an entire program, how would Whirlwind be protected from tubes or diodes that were about to go bad, that were becoming marginal in their operation? While this was not a completely new question to Forrester, neither was it one to which he had found an answer until, as he recalled afterward, in the throes of trying to formulate a reply at that moment

that would indicate he and his engineers were masters of the situation, "a solution presented itself. I realized that by deliberately varying the voltage and thereby changing the loading on any circuit while requiring it to carry out a simple operation, a tube that was losing its capacity to perform would be forced to reveal its identity under the marginal conditions imposed. This was how 'marginal checking' came to be invented."<sup>6</sup>

The literature of basic and applied science is by custom committed to a policy of outlining for the reader a method of demonstration by which the asserted correspondence of data to interpretations and of facts to conclusions may be established. It is not surprising, in consequence, to find that the circumstances of discovery and invention usually vanish unrecorded from history. Thus, the formal report to ONR indicating that provisions to accomplish marginal checking would be designed into the machine contained no reference to the circumstances in which the technique was invented. Nor was the report couched in terms particularly calculated to reassure those skeptical program managers who were aware that they lacked the "inside" technical view and the visions of a Perry Crawford, as well as the familiarity with engineering detail, balanced against a mathematical

sensitivity that a Murray might be expected to have as a consequence of his professional experience.

Forrester's official summary report of his modest innovation was low-keyed, relatively routine in form, and unexceptionably incorporated as the last half of a six-paragraph description entitled "Trouble-location." It is here re-presented in full from the December monthly report:

Because digital electronic computers contain many thousands of electronic-circuit components, failures must be expected. Such failures almost always cause errors in computation, and temporarily destroy the usefulness of the machine. Rapid trouble-location methods are therefore of great importance.

A scheme which has been proposed for facilitating the location of faults in WWI uses prepared groups of test problems whose answers are known. These problems are of two types:

- (1) Check problems, solved periodically, designed to use as much of the machine as possible. Errors in solutions will indicate that some part of the machine is not functioning correctly.
- (2) Trouble-location problems, designed to use only small portions of the machine. Errors in the solution of one or more of a series of these problems will provide information on the location of a fault after its existence has been demonstrated by an error in the solution of the check problem.

The machine itself may thus be made to locate faults which would require exorbitant time by manual methods. Simultaneous failure of many elements, or failure of certain critical elements, will result in greater difficulty, but such occurrences should be few relative to the total number of failures.

Although primarily intended as a means for finding steady-state faults due to the complete failure of a component, this scheme will be extended to finding of marginal components whose complete failure is imminent, which might be causing random errors. It is expected that such components can be made to give steady-state indications of failure by appropriate variation of circuit supply voltages and of the repetition frequency of applied pulses.

As an example, for certain types of faults, if the voltage of the screen-grid in a marginally operating vacuum tube is lowered slightly, complete failure can be produced, permitting discovery by check problems and subsequent location by trouble-location problems.

Whirlwind I power-supply systems are therefore being designed to permit selective variation of supply voltages in a range above and below normal operating values. The added complexity of cabling and the additional equipment required for this purpose are believed well justified by the expected gain in computing reliability.<sup>7</sup>

By the following spring the basic requirements of a marginal-checking system had been worked out, personnel had been "assigned to design the electrical and mechanical layouts," and preliminary design proposals had been composed.<sup>8</sup> By the end of that year (1948) marginal checking features were being incorporated in the five-digit multiplier and tested. If they worked as expected, they would constitute the basic template, so to speak, of the pattern of marginal-checking facilities planned for the entire computer.<sup>9</sup> The five-digit multiplier was the smallest unit of the arithmetic element that Forrester, Everett and the others felt they could construct as

a representative subcomponent that would early tell them whether they had a sound building-block of the vital computational portion of Whirlwind.<sup>10</sup>

It was typical, too, of their philosophy and mode of engineering procedure: proceed from the level of system-requirements appraisal to the level of a consequent component, establish the detailed design of the latter, build its parts, assemblies, and subassemblies, testing them singly and together as they came into being, in order to establish preliminary operating characteristics, locate deficiencies in design and materiel, remedy these, and test the developing component as thoroughly as possible, taking the time to build whatever special test equipment was necessary. This procedure, they were convinced, would obtain a soundly functioning building block of known operating characteristics that could be depended upon and rendered compatible to the exigencies of systemic (as distinct from isolated) operation.

This philosophy of intimate interplay -- i.e., proceed from logical and mathematical formulations to design, build, test, integrate, redesign, rebuild, retest, reintegrate -- was a major cause of the rising costs associated with Whirlwind which strained relations between ONR and MIT during 1948 and 1949. It was also part of their engineer's dynamic answer

to the continuing problem of quality control, and it was a policy position from which the Project leaders refused to budge, regardless of the larger and larger percentage of ONR's contract-research budget that they kept calling for. From Forrester's and Everett's point of view, it was the only way to maintain the high standards the enterprise required if it was to succeed within a reasonable span of time. Not only were they convinced it cost less to do it right than to do it wrong and then engage in expensive corrections, but they also held a strong personal commitment to a way of doing based upon a philosophy of excellence.

At the beginning of 1948 Forrester had visualized completion of the computer by stages: the arithmetic element of the computer would be ready early, and the five-digit multiplier test portion of this element would be ready even earlier, for the computational speeds were likely to take more time than the information-in, information-out storage speeds or the transfer of information from one part to another of the machine, and it was essential that they fabricate earlier those parts that would require critical and perhaps extensive testing. Electrostatic storage would come last, not because it required little testing -- on the contrary -- but because extensive

engineering research and development were required. These would consume the most time, and Forrester fully realized this.

Time schedules were drawn up for the major parts of the computer, and from February, 1948 onward, progress toward meeting the schedules was reported monthly to the Navy. These reports, comparing actual progress with scheduled progress, began about the time that ONR embarked on another intensive analysis of the Project and its operations in order to establish how it was proceeding. Not only did von Neumann, presumably at Mina Rees' request, spend three days during February in the laboratory, discussing the operations the machine would be called on to perform, as well as examining the block diagrams, potential uses, and arrangement of the projected machine, and in general familiarizing himself with the state of affairs of the project. Mina Rees also brought to the Barta building John Curtiss and H. D. Huskey from the Bureau of Standards, and they "considered in some detail with Project Whirlwind Staff the nature of engineering problems of computer design and the successive stages of development leading to the final product."<sup>11</sup>

By spring Forrester could say that the building of the computer had begun; Sylvania was fabricating

components to the specifications of the Whirlwind engineers. For some units MIT furnished drawings, for others detailed specifications from which the Sylvania engineers could lay out drawings and authorize fabrication. For still others, such as "the prototype of the 28-tube accumulator panel," the Project engineers constructed the first unit to serve as a model for the Sylvania engineers to duplicate, and for still others, such as the storage tubes, the Project maintained its own in-house enterprise throughout.

By early summer in 1948 tests had revealed that a standard type 6AG7 vacuum tube lacked the reliability life span required. Apparently a silicon concentration in the cathode nickel was raising a barrier to current flow, so the decision was made to switch to a tube manufactured under different techniques, the type 7AD7, which appeared provisionally satisfactory. Many tube sockets in the circuits would have to be changed, but such a prospect was not unusual or dispiriting in the engineering view of Forrester and his associates.

Nevertheless, this was one of the factors that accounted for what had become a five-week lag behind schedule, over the first six months of 1948. Forrester announced at the end of that time that

since the regular semi-annual revision in the time schedule was at hand, the schedules would be adjusted and the status of work actually existing in July would become the new basis. By this technique Forrester proposed to put his project on a more realistic schedule and thus compensate, in a programming sense, for "procurement delays, necessary design changes, and heavy demands upon personnel time..."<sup>12</sup>

Although it might appear at first glance that he was trying to make the Project look good by engaging in some sort of scheduling legerdemain, he was in part postponing the completion date and in part recovering some of the time lost by reassessing portions of the program and finding ways to "buy" time by eliminating, shrinking, or clarifying the details of previously scheduled operations that required modification in the light of information acquired. Information gained from the experience of the preceding year or more placed him in a position to specify more sharply the delivery sequence of some items and the physical composition of other units. Generally speaking, "actual progress had been made at about three-quarters of the rate as expected in January. The new schedule extends the work by 30% in recognition of this fact."<sup>13</sup>

Forrester recalled in later years that the detailed manner in which the monthly Summary Reports kept ONR posted regarding technical problems and slippage of schedules had made such a virtue of frankness that one of the unlooked-for effects was the added fuel they provided to stoke the persisting unease the ONR programmers felt.<sup>14</sup> The same tenor of events reflected by these reports did not disturb Nat Sage, Gordon Brown, and the MIT leadership, however, although it should not be supposed that the latter were directly involved and informed as to details until the increasingly apprehensive protests from the Navy reached their ears.

During the second half of 1948 the computer itself began to appear, as racks, subassemblies, and assemblies of various component and subcomponent parts of the computer began to be installed in the Barta building. At the same time, the prospective complexities of setting up and then achieving full operation caused Forrester to postpone the final completion date once again, this time from the end of 1949 to the end of 1950.

One of the complexities was the stubborn way in which an efficient, reliable electrostatic storage tube design continued to elude the researchers' grasp even while encouraging advances continued to

be made. Four activities devoted to the storage tube were included in the schedule charts submitted monthly to the Navy during the first half of 1948; the number of such line items jumped to thirteen after June. Yet this could not be taken as a sure sign of trouble, for six months later, at the start of 1949, Forrester had become optimistic that the storage tubes would be ready sooner than he had earlier expected, as a consequence of gains made by the increased emphasis and effort given during recent months. But earlier, in the summer of 1948, at the very time Admiral Solberg was applying pressure on the Institute to proceed with Whirlwind's development at a more reasonable (i.e., less costly) rate, it appeared that the storage-tube problem was bigger than had been suspected, and the slow rate of progress toward a solution, compared to that which he had expected, caused Forrester to redouble his efforts, and consequently, to up the ante again, to ONR's dismay.

In spite of such vicissitudes, the research, design, development, fabrication, and testing progress being made on all fronts caused Project Whirlwind in mid-1948 to appear, at least to the MIT administrators, as a healthy engineering project indeed, and one that the Institute definitely need not apologize for.

Such judgments, it should be pointed out, did not and could not derive solely or directly from any schedule charts of recitation of weekly and monthly problems encountered and accomplishments effected. There was by this time so much going on in the Project, and so many activities were being coordinated in so many directions, that only an engineer in Everett's or Forrester's positions could be expected to possess the authentic and fully informed "feel" of how Whirlwind was progressing, and it can always be argued that their stake in the success of the Project was too great to allow them the dispassionate acuity of an objective view.

Whirlwind had long since passed the stage at which its likelihood of success or failure could readily be judged, and this circumstance became one of the reasons why the Project was so difficult for the Navy program managers to either bring to a stop or place under tighter rein. The most experienced and sophisticated of administrators and analysts, possessing top-echelon authority and influence -- whether a von Neumann or a Solberg or a Weaver or a Sage or a Compton -- were not at all inclined to take drastic action in either direction. They knew instinctively that such judgments, either to give more rein or pull in the harness, were too

personal, too intuitive, too complex and obscure in their bases to be communicated easily and convincingly to another. To justify or condemn Project Whirlwind on its intrinsic merit, then, was impossible.

From the point of view of basic ONR policy and the responsibility for enforcement of that policy that Mina Rees and subsequently Admiral Solberg shared, the high standards the young MIT engineers were determined to maintain became standards too high and too costly to be long endured. It was inevitable that a difference of opinion should arise between MIT and the Navy as to whether ONR was trying simply to bring Project Whirlwind into line or to kill it off. In this respect the two views were perhaps irreconcilable.

In any event, as the budget disagreement sharpened during 1948 and brought MIT's top management into direct involvement with Forrester's and Mina Rees' policy dispute, the MIT leaders became aware they had a partly finished, well-begun computer of unique design on their hands. They realized also that since Project Whirlwind could not be judged properly, by all the parties concerned, on its own merits alone and since the prospects and perspectives Sage and Forrester had offered opened up singularly powerful channels of persuasion, MIT must undertake

an appropriate educational and thoroughly legitimate propaganda and informational campaign, reserving the "muscle" and power of MIT's position and reputation for any confrontation that might arise.

Accordingly, they requested further detailed information. President Compton's September request to Nat Sage for a report on the uses and costs of digital computers led, as has been said, to a flurry of activity within Project Whirlwind. Between September 14 and October 15, four reports -- defenses of the Project although not labelled as such -- were prepared and published. Two versions of the first report were prepared. One had been prepared and submitted to President Compton on the 14th; this was apparently in response to his request of September 8. The second version was completed and published on the 17th. It discussed the same material, but in more detailed and extensive form, providing more explanatory and illustrative argument and material. The first report set forth the possible military applications of digital computers and included an approximate estimate of the "time and cost to bring such information systems to useful military realization." The authors -- Jay Forrester, Hugh R. Boyd, Robert R. Everett, Harris Fahnestock, and Robert A. Nelson -- estimated the time required would

be about fifteen years and the cost about \$2,000,000,000. The report was not concerned with the digital computer solely, but rather with the complete system of which the computer would be but a part, so that the programs envisioned included "auxiliary equipment, applications studies, field tests, and training of staff required to do research and to produce and operate the equipment." The areas of application which could be foreseen included "air traffic control, integrated fire control and combat information centers, interception networks, scientific and engineering research, guided missile offense and defense, and data processing in logistics." The total report reflected the most advanced thinking of the young MIT engineers and the SDC engineers at Sands Point.<sup>15</sup>

Although the first report was primarily concerned with the application of digital computation systems to military needs in a general sense, it provided a defense for Project Whirlwind without referring to any specific computer development program, if only by pointing up the advantages to be gained once successful development of digital computation systems made possible "the better integration and more effective use of other military equipment." In this manner it justified "the diversion of men and resources to digital information-system development."<sup>16</sup>

Forrester and his co-authors were reiterating their thesis that the potential of the digital computer was so great and the benefits to be derived from its use so immense that the costs involved, no matter how great, were warranted. To them a national computer development program was as important to the well-being of the nation as had been those programs which had led to the development and use of radar and the harnessing of nuclear energy.

The analysis of the research and development program essential to the achievement of digital computing systems reflected the experience Forrester and his colleagues had gained since they had embarked upon their own program in 1945. They noted in their report, perhaps ruefully, that the costs of making equipment for military application appeared sometimes to have been "underestimated because of linear extrapolation of past laboratory programs." Instead, they argued, similar development programs had "grown exponentially," and they cited the development of radar as the "nearest parallel."<sup>17</sup> This observation although reflecting their own errors as to time and costs, countered the charges made by the Project's opponents that it was too expensive in money, time, and manpower for the benefits it would provide. The cumulative costs of such programs, the authors

argued, would be more than repaid by the benefits the nation would derive from the application of digital computers to national needs.

The second report, also quite detailed and extensive, was directly pertinent to Project Whirlwind, outlining three possible "levels" of operation for the period 1949 to 1953. This report was completed on September 21, 1948, one day prior to the conference with the Chief of Naval Research, and was designed to serve as a basis of the presentation which the MIT group intended to make to Admiral Solberg.

Plan 1 indicated the extent of "the research, development and limited experimental computer operation" possible under the proposed budget of 1.8 million dollars per year. Plan 2 was based upon an annual operating budget of 3.8 million which would allow the addition of "a substantial operating force for the efficient solution of engineering and scientific problems." Plan 3 which proposed an annual budget of 5.8 million would further permit the inclusion of a research program into "the application of digital computers to the field of control and military uses."<sup>18</sup> To some extent, the second report complemented the first by noting how Project Whirlwind's program could be organized to permit the realization of the military applications outlined in the report of September 17.

It is interesting to note that the second report contained no discussion of any program which could be conducted under the minimal figure of 1.8 million dollars. The minimal rate considered was that proposed by Sage in his letter of August 4 concerning the allocation for Fiscal Year 1949 which had been based upon an anticipated average monthly expenditure of \$150,000. There seems to be no doubt but that the directors of Project Whirlwind were determined not to strike their flag, if strike it they must, without a battle. Forrester recalled in after years that the Whirlwind group had by this time become so deeply committed to the idea of doing the technical job right or not at all ("Do it on our terms, or let it be shut off!"), that the arbitrary, unilateral nature of the view they took was not readily apparent to them. Instead, they were aware that there were other worthwhile projects to which they might apply their talents, should the Navy find itself unable to supply the proper funds.<sup>19</sup> Their high spirited mixture of determination and bravado was not put to the test on this occasion, however.

September 22, 1948 had been agreed upon as the day for the Navy and the MIT representatives to meet in Washington to discuss the financial and technical ramifications of Project Whirlwind. ONR was sufficiently

impressed with the importance of the conference to hold a "rehearsal conference" on the 21st, a rehearsal that lasted all day but which, when compared to MIT's preparation, was barely minimal. The purpose was to acquaint the Chief of Naval Research with the program, but more importantly, perhaps, "to establish a common understanding within the Office of Naval Research and to solidify the thinking of all individuals within the Office of Naval Research on the Navy's position relative to Project Whirlwind."<sup>20</sup> Thus, ONR was establishing its "party line" even as MIT, through the conference called by President Compton on September 8, had established its "party line." Each organization had taken a tentative position for the first round of discussions, but each, as events were to prove, had also remained sufficiently flexible to permit compromise.

The general conference of the 22nd served in many ways as a forum for the reiteration of previous questions and explanations. Forrester explained the reasons underlying the transition of emphasis from an aircraft simulator to the digital computer, covering the same ground he had covered many times before. Mina Rees once again related the questions of comparative costs between Whirlwind and the computer von Neumann was developing at the Institute

for Advanced Studies. Captain J. R. Ruhsenberger, Director of SDC, "presented an emotional plea for the aircraft analyzer and the benefits that would accrue from it." Perry Crawford, to Forrester's dismay and irritation, was strangely quiet about the many conversations he and others from SDC had held with Forrester and his associates concerning the course Project Whirlwind was following and the varied and diverse applications open to it, leaving the "erroneous impression" Forrester later noted, that SDC "had been steadily interested in the aircraft analyzer problem to the exclusion of other applications."<sup>21</sup>

What Forrester perhaps did not know or recognize at the time was how greatly the visionary spirit with which SDC had infected Project Whirlwind had waned within the Navy. SDC had lost its fight against ONR, and Crawford's silence was in part the silence of dejection and in part the continuation of a policy that de Florez had adopted at the start. For although de Florez's experience with aircraft simulators and the insights he had picked up from Hunsaker, whom he had known since college fraternity days, all led him to place greater emphasis in his own mind upon the design aid that a simulator run by a computer could render and less emphasis on its undoubtedly virtues as a training aid, he had nevertheless always

led from strength when appealing for support from within the Navy by stressing the trainer application. Crawford in later years reflected that probably by early 1948 de Florez had given up seeking a broader mission for SDC.<sup>22</sup> The aggressive, visionary spirit infecting Special Devices personnel in 1947 and responsible for encouraging Forrester and Everett, in frequent meetings at Sands Point, to see more ambitious prospects of the sort that had stimulated the forward-looking systems-control views represented by their L-1 and L-2 Reports, had all but disappeared from SDC by the time of the ONR--MIT meeting in September of 1948.

The important point made by ONR in the course of the September discussions was its inability to meet the financial requirements of the Project as set forth by its directors. Indeed, the question was raised if the Project as envisioned by the Institute was not too large for ONR to handle and, possibly, too large even for the Navy. The ceiling of \$900,000 already established was to the maximum for ONR, and the MIT representatives were asked to determine "how the program could be continued for the fiscal 1949 period for that amount." The reply, made by Nat Sage, was that "no immediate or tentative solution was foreseeable." Nevertheless, the expenditure rate of

\$150,000 per month would be reviewed in the hope a reduction could be effected. Then Sage left the door open by volunteering the observation that an allocation of \$1,200,000 rather than \$900,000 "would probably be sufficient to finance Project Whirlwind to the end of Fiscal 1949 (30 June 1949)."23

The larger amount, when added to the \$385,260 carried over from Fiscal Year 1948, would permit an average monthly expenditure of approximately \$132,000, some \$18,000 under the anticipated rate of \$150,000. It was an amount which would undoubtedly permit the program to continue without any drastic cutbacks, although the rate of acceleration would be less than Forrester could have preferred. If the additional monies were allowed by ONR -- and eventually they were -- then the total funds available to Project Whirlwind for Fiscal Year 1949 would be approximately a quarter of a million less than the original request.

Despite the adamant stand the ONR representatives took regarding the \$900,000 ceiling, the conference concluded with the formulation of an agreement signed by the representatives of ONR and MIT, which strongly implied that additional monies would be forthcoming, provided MIT would strive to hold costs to the minimum by a "reasonable diminution of effort." The \$900,000

would be formally allocated. The Project's estimated date of completion would be extended to April 1, 1949; however, every effort would be made by Forrester to stretch the allocation to cover as much as possible of the three months between April 1 and the end of the Fiscal Year, June 30, 1949.

MIT had been granted the \$900,000 for a nine month period upon the understanding frugality would be practised. Meanwhile, evaluation of the Project would be continued, to determine what would be the "best reasonable rate of effort on a scaled-down basis for future operation."<sup>24</sup> A week later to the day, Amendment No. 6 to Task Order 1 of Contract N5ori-60 was issued, extending the date of completion and confirming the allocation of \$900,000.

The conference ended in a compromise, leaving for future discussion and solution the final resolution of the rate at which the Project would be conducted. MIT had received assurances of continued support, even if not to the extent desired. ONR had received assurances that measures would be taken to limit the Project's rate of acceleration and had succeeded in reducing its allocation without seriously offending the Institute. This latter was without doubt a very serious and sensitive consideration for ONR as it sought to establish and retain the growing confidence

of the academic community in ONR's ability to mount sustained, consistently managed and funded research programs.

The agreement which ended the conference was temporary and expedient, pending final evaluation and decision. Subsequently, Solberg emphasized this view in a communication to Compton, expressing his conclusion that the Project was a "long-range one," to be evaluated within the context of the total national computer development effort as well as within the context of ONR's total research program. Solberg was striving to be fair. He was willing to accept temporary continuation of the program at a rate which approached ten percent of ONR's University Research Program funds and even to consider the allocation of more funds if absolutely necessary. He was not willing, however, to grant unlimited funds and freedom of direction, at least not until a thorough investigation of the Project had provided a sound appraisal of Whirlwind's genuine importance and position within the total national computer effort.<sup>25</sup>

Compton in an "off-the-record" reply to Solberg explained that future discussions on Project Whirlwind would be conducted for the Institute by James R. Killian who, upon Compton's resignation to succeed Vannevar Bush as Chairman of the Research and Development

Board, would become president of MIT. Observing that in light of his new appointment, he could not properly be "an intermediary in these discussions," Compton nevertheless did informally convey to the Chief of Naval Research some thoughts which he believed Killian would later express concerning the Institute's stand in the matter. In general he concurred with Solberg's opinion that the government computer development program was so important and costly that it deserved "the most expert possible evaluation," pledging the Institute in the meantime to respect the agreement reached at the Washington conference.

Turning to Project Whirlwind, Compton explained that "our group" -- the term was his -- was preparing a memorandum for the Chief of Naval Research which would "add considerable clarification of the issues" for Solberg as it had for Compton. The memorandum would explain the "philosophy" of approach taken by Project Whirlwind. As Compton saw it, this approach appeared to differ in three respects from other computer development programs, "especially the one at Princeton." He was convinced, he wrote Solberg, that the IAS and MIT programs were "essentially non-competitive in the sense that one may prove to be a useful research tool and the other a useful operational tool." Through his informal reply, Compton permitted Solberg

to infer not only the Institute's position, but also and perhaps more importantly, the position which the Chairman-designate of the Research and Development Board would probably take. In a sense, Solberg was being forewarned by Compton.<sup>26</sup>

NOTES TO CHAPTER 7.

1. Interview, P. O. Crawford by the authors, October 25, 1967.
2. Project Whirlwind (Device 24-x-3), Summary Report No. 3, Dec. 1947, p. 2. Hereafter cited: Summary Report No. -. These reports began to be submitted on a monthly basis, beginning in Nov., 1947, to "the Special Devices Center, Office of Naval Research, under Contract N5ori60" by the Servomechanisms Laboratory.
3. Ibid., p. 3.
4. Ibid., p. 5.
5. Ibid..
6. Interview, J. W. Forrester by the authors, July 24, 1964.
7. Summary Report No. 3, Dec. 1947, p. 8.
8. Summary Report No. 6, Mar. 1948, p. 19.
9. Summary Report No. 15, Dec. 1948, p. 19.
10. Summary Report No. 3, Dec. 1947, p. 7.
11. Summary Report No. 5, Feb. 1948, p. 3.
12. Summary Report No. 9, June, 1948, p. 8.
13. Summary Report No. 10, July 1948, p. 2.
14. Interview, J. W. Forrester by the authors, July 31, 1963.
15. J. W. Forrester, H. R. Boyd, R. R. Everett, H. Fahnstock, and R. A. Nelson, "Forecast for Military Systems using Electronic Digital Computers," Report L-3, Servomechanisms Laboratory, MIT, September 17, 1948; the first version prepared by the same authors was entitled, "A Plan for Digital Information Handling Equipment in the Military Establishment," Report L-3, Servomechanism Laboratory, MIT, September 14, 1948.
16. Report L-3, September 17, 1948.

17. Ibid.
18. J. Forrester, H. R. Boyd, R. R. Everett, H. Fahnestock, and R. A. Nelson, "Alternative Project Whirlwind Proposals," Report L-4, Servomechanisms Laboratory, MIT, September 21, 1948.
19. Interview, J. W. Forrester by the authors, October 26, 1967.
20. Memorandum from J. B. Thaler to Director, Contract Division, SDC, Subj.: "Trip to Office of Naval Research, Washington, D. C. on 20 September 1948 concerning Contract N5ori-60; Report on," September 24, 1948.
21. Ibid.; J. W. Forrester, Computation Book No. 45, pp. 134-5.
22. Interview, P. O. Crawford by the authors, October 25, 1967.
23. J. W. Forrester, Computation Book No. 45, pp. 134-5.
24. Ibid.; Ltr., T. A. Solberg to K. T. Compton, September 29, 1948.
25. Ltr., T. A. Solberg to K. T. Compton, September 29, 1948.
26. Ltr., K. T. Compton to T. A. Solberg, October 7, 1948.

## Chapter Eight

### R & D POLICIES AND PRACTICES

The memorandum to which President Compton referred in his letter to Admiral Solberg emerged in definitive form as two reports -- the third and fourth in the series of four generated by Compton's desire that the nature and purpose of Project Whirlwind be clearly articulated. Bearing the dateline October 11, 1948, both reports sought to emphasize the importance of Project Whirlwind by explaining the unique characteristics of the Project and the contribution it had to make to contemporary computer technology. The third report, "Memorandum L-5," set forth the general philosophy and plan of attack which the Project sought to follow.<sup>1</sup> The fourth report, "Memorandum L-6," offered a comparison between MIT's Project Whirlwind and the computer program at the Institute for Advanced Study in Princeton, New Jersey.<sup>2</sup>

The third report, which was prepared by Jay Forrester himself, set the Project within the context of the policies and procedures of the Servomechanisms Laboratory in an effort to show how the program that Project Whirlwind was following reflected the purposes and procedures of the Laboratory and, by implication, the very principles which

MIT itself followed. Noting the preference of the Laboratory for projects which combined "engineering research and development with systems consideration," Forrester sought to point up the design, development, and construction of Whirlwind as but one element of a system which was, in this instance, by contractual agreement an aircraft analyzer. At the same time, he reiterated a favorite argument: "the scope of the project might not be justified by this application alone, were it not for the benefits which will accrue to all other digital computer applications." These were applications which Forrester felt could be so important that the Navy might even decide to "redirect future work." To Forrester this meant the natural development of sophisticated "control systems" for practical military use in the future.

Time was of the essence, he argued. It could not be wasted by following the usual sequential procedures of "research, development, and design." These three steps had to overlap, even run concurrently when possible, in order to obtain a "reliable operating" computer at the earliest possible moment. Herein lay the singular strength (and costliness, it should be admitted) of Project Whirlwind, for even as the Project conducted research, it built and tested. In addition, through its use of graduate students in the tradition of the Servomechanisms Laboratory, it produced trained and experienced personnel for "a development of national value."<sup>3</sup>

Forrester and Everett jointly prepared the fourth report as a rebuttal to the charges of those who had persistently implied that the digital computer project at MIT was inferior to the von Neumann project at the Institute for Advanced Study. The two authors argued that the two programs had been established for different purposes and consequently followed different procedures. About the only thing they possessed in common was the intent to design and construct a "parallel-type digital computer;" otherwise, the two groups held "very few common views on the methods for specifically achieving working equipment."

As von Neumann wisely had done before them, in his remarks to Mina Rees regarding Professor Murray's report, Forrester and Everett made no attempt to demonstrate the superiority of their program by denigrating the IAS program. Rather, they recognized the differences between the projects to be quite valid, for their origins lay in different purposes and projected uses. Von Neumann and his associates at the Institute for Advanced Study were "engaged in scientific research . . . the study of high-speed computing techniques;" Forrester, Everett, and their associates at MIT were "engaged in engineering development . . . to produce and use computers." Moreover, von Neumann was seeking to design and construct a digital computer; the young MIT engineers were seeking to design and construct a system employing a digital computer as an integral element.

Speed and reliability were of greater importance to the MIT program, since the digital computer within the system had to operate "in real time" and with minimal error. The IAS program, on the other hand, since it was to be used primarily for mathematical computation did not need to meet the same standards of speed and reliability. These differences in purposes and goals made necessary a difference in procedures that in turn led to a difference in costs. Project Whirlwind was building a "prototype," and although the approach followed was "less efficient and more expensive," it was faster, and this was a consideration of primary importance under contemporary conditions.<sup>4</sup>

While Solberg and Compton were exchanging views and Forrester and his associates were preparing reports, the press for additional funds from ONR continued. The \$900,000 which ONR had allocated for the fiscal year 1949 was some \$300,000 short of the amount Nat Sage had proposed during the fall as an acceptable compromise. Forrester had accepted, albeit reluctantly, a \$1,200,000 ceiling and with his staff had planned a program conforming to an allocation in that amount. He aggressively sought to prevent any further reduction in what he already considered an inadequate budget.

In addition to forestalling further cuts, he had to convince ONR that additional funds were mandatory if a minimal rate of progress was to be maintained. To this end, Forrester and members of his staff prepared in massive detail

position papers which explained, on the one hand, the financial needs of the Project for the balance of fiscal year 1949 and, on the other hand, the disastrous impact upon program goals which would follow if the Navy failed to meet those needs.<sup>5</sup>

In addition to preparing position papers that would convince both MIT's top management and official government circles of the immediate and vital importance of computer development programs in general and of Project Whirlwind in particular, Forrester became more active and more personally involved in "selling" his Project to his more influential and recognized colleagues within the Institute. Early in November he spent a morning with Dean T. K. Sherwood of Engineering, Nat Sage of the Division of Industrial Cooperation, and Professors Harold L. Hazen, Jerome B. Wiesner, Samuel H. Caldwell, and Gordon S. Brown, "discussing applications of Whirlwind I, particularly to scientific problems and to control applications."

With the exception of Caldwell, those present at this meeting were to participate in a subsequent conference in December with representatives from ONR. Meetings of this kind not only reflected Forrester's desire to win the support of the more influential members of MIT's academic community, but also Compton's intent to have the Institute's top scholars in areas directly pertinent to the work of Project Whirlwind become more familiar with its nature and purpose and with its director.

Three days before the meeting with ONR, Forrester had lunch with Professor Hazen and used the occasion as an opportunity to explain that the "lack of mutual understanding" present between ONR and Project Whirlwind found its origin in the different approaches each took to computer research and development, approaches based upon their respective views concerning the ultimate use of the computer. Mina Rees and her associates approached the matter from the view of the mathematician, whereas Forrester and his associates approached it from the view of the engineer. Since Hazen was planning to have lunch with Mina Rees and E. R. Piore, Deputy for Natural Sciences, ONR, Forrester apparently hoped Hazen would become an intermediary and try to make clear the validity of the engineering approach and possibly clear up some of the misunderstanding. Other causes for the misunderstanding were probably discussed also, for Forrester entered in his record of the lunch, the cryptic comment that the two men has also "covered . . . the current political situation."<sup>6</sup>

The persistent pressure for more funds, Solberg's desire to gain a greater insight into the nature and purpose of Project Whirlwind, and the mutual intent to resolve the differences between ONR and MIT stemming from the conduct of the Project led to another conference between representatives of the two organizations at Cambridge on December 9, 1948. Both organizations sent their top staff to the meeting. ONR was represented by the Chief of Naval Research, Rear

Admiral T. A. Solberg, accompanied by Dr. Alan T. Waterman, Deputy Chief of Naval Research and Chief Scientist; Dr. T. J. Killian, Science Director; Dr. Mina Rees, Head of the Mathematics Branch; and several others, among whom was Perry Crawford, then on temporary duty with the Research and Development Board.

MIT was represented by its President-designate, Dr. James R. Killian, Jr., accompanied by Dr. T. K. Sherwood, Dean of Engineering; Nat Sage, Director of the Division of Industrial Cooperation; Professor Gordon Brown, Director of the Servomechanisms Laboratory; Professor H. L. Hazen, Head of the Department of Electrical Engineering; Professor J. B. Wiesner, Assistant Director of the Research Laboratory of Electronics; and Forrester and other members of Project Whirlwind's staff. The official positions and quality of the representatives alone gave proof of the importance both ONR and MIT attached to the meeting and to the matter under discussion.

The meeting was chaired by Dean Sherwood, who also acted as the chief spokesman for the Institute. After a few remarks by Forrester on the program and its rate of progress, the group from Washington toured the laboratory. Then the two groups settled down to a serious discussion of the matters at issue. The exchange of views was quite blunt. Sherwood, in order to counter rumors to the contrary, "stressed the united MIT support of the Project," and noted the "desirability

of having good communication among all groups concerned to prevent the spread of rumor, and especially of the need of having some technically competent person in Washington designated to follow the Project." The very presence of MIT's scholars and administrators gave substance to Sherwood's point.

The conferees acknowledged that "confusion" had been created and "some appreciation of background lost" with the transfer of supervisory authority from SDC to the Mathematics Branch of ONR. Mina Rees acknowledged that her ignorance in engineering made her incapable of comparing the Whirlwind program and its emphasis upon an engineering approach with other computer projects which sought different goals and followed different procedures. Consequently, she expressed her intention to have the Project "evaluated by independent experts."

When the discussion turned to financial requirements, Nat Sage estimated the additional funds necessary from fiscal year 1949 funds to approximate \$275,000. Sherwood, in his comments on funding, left the ONR representatives with a thinly veiled warning by explaining that it was MIT's policy to give three months notice when releasing staff members. Since there remained only sufficient funds to continue operations for four months, he suggested a "prompt decision" concerning the allocation of additional monies be forthcoming from ONR. Bending to this pressure, the ONR group intimated

that more funds would be made available and requested a statement on the amount necessary to get the Project through fiscal year 1949. In light of the blunt exchange of views and ONR's acceptance of the need for more funds, it is small wonder that Forrester expressed the conclusion that the "general result of the meeting seemed to be quite satisfactory."<sup>7</sup> Perhaps in some ways he was a bit optimistic, for although the December conference brought out the full strength of MIT, it also marked the apogee of the support which the Institute's directors were to render Project Whirlwind.

The following day Nat Sage submitted to ONR a request for additional funds in the amount of \$378,186 to carry the Project to June 30, 1949. The Navy ultimately made available \$300,000,<sup>8</sup> providing thereby a total of \$1,200,000, the amount which Sage upon several occasions had suggested would be acceptable. In a letter informally notifying Sage of the additional funds and the extension of the contract to June 30, 1949, Mina Rees added that budgetary considerations indicated ONR would be unable to allocate to Project Whirlwind more than \$750,000 for fiscal year 1950. Consequently, she advised, it was essential the Project "eliminate . . . any long range activity, supported by ONR, which does not contribute in a direct way to the completion of Whirlwind I." That this could be done, she explained, had already been determined in conversations with Forrester in Washington in early January, 1949.

In his conversations with Mina Rees, Forrester had acknowledged that research could be terminated if necessary to "conserve funds for the completion" of the computer, but his point of practical reference was the continuation of the \$100,000 per month rate of expenditure which he and his colleagues had accepted as the minimum amount for the maintenance of a dynamic and active program. This was the amount required to underwrite the anticipated program outlined in the draft of Memorandum L-10, which Forrester and Mina Rees had before them. He predicted that if this rate were continued, the computer could be completed, using a test storage component by October 1, 1949 and an electrostatic storage component by December or January. Mina Rees asked if it were not possible to continue this rate of expenditure for six or eight months and then taper off; this Forrester acknowledged as a possibility. Mina Rees and C. V. L. Smith of ONR, who joined the conversations, were both of the opinion that \$1,000,000 was the maximum which could be anticipated for fiscal year 1950. They recommended that the program be planned on that basis. Forrester was left with the impression that the Chief of Naval Research was unwilling to approve a larger amount; for that matter, the figure mentioned did not yet have his approval.<sup>9</sup>

It seems clear, from the discussion between Forrester and Mina Rees and the subsequent allocation of \$750,000 for Project Whirlwind, that the directors of ONR had determined

to reduce the costs of the Project to a level that would place the allocations more in line with the monies available to ONR for research and development in computers. Whether this was a general goal or Solberg's alone cannot be determined, but certainly Forrester's comments following his discussion with Mina Rees convey the impression that the Chief of Naval Research was exercising a strong influence in this direction.

Apparently, Solberg had become convinced that the time had arrived to terminate research and push the Project to completion at minimal cost. Political as well as technical considerations made necessary a more gradual reduction than perhaps he preferred, but the evidence suggests he had no intention of underwriting the Project at the level proposed by Forrester. Subsequent actions by the leaders of MIT suggest that, fully recognizing the problems besetting ONR, they were more amenable to a compromise than was Forrester, with the result that he found himself being placed under greater pressure within the Institute itself. The creation of a computer center at MIT appeared to be an instance of this pressure, but it was an operational development that Forrester himself suggested in order to facilitate the efficient operation of the Whirlwind project.

Neither position papers nor oral argument were successful in moving the Navy, however. ONR allocated to Project Whirlwind for fiscal year 1950 not the one million dollars that Mina Rees and Forrester had talked about early in 1949,

but three-quarters of a million, and Forrester had to tailor his program accordingly. Fortunately, his luck, or that of the Project, was still running strong. Since July of 1949, conversations had been taking place between Forrester and his associates and representatives of the Air Force over the possibility of applying the digital computer they were developing at MIT to air traffic control. These conversations led eventually to the negotiation of a contract in the amount of \$122,400 for research in this area during the period from March 1, 1949 to April 30, 1950.<sup>10</sup> Although the amount of money involved was not relatively large, it did help ameliorate the situation, if only by providing a means whereby key professionals could be kept under salary. More importantly, however, it was to lead to the creation of a pool of experience which proved of immeasurable value when the Air Force later turned to MIT and the rest of the national educational establishment in its desperate search for air defense techniques and equipment.

With the funds available to him for fiscal years 1949 and 1950 Forrester was able to push the Project steadily on toward completion. Most of the people on the Project became aware of the pressure that the Mathematics Branch of ONR was applying, but few outside of Forrester, Everett, Boyd, Fahnestock, and others who had been there since 1946 realized how far the Navy had shifted from its once enthusiastic support of ASCA to its grudging funding of Whirlwind. To the

old hands, the crisis of the fall of 1948, the recurring inspection-visits by different outside experts, and the attitude of ONR and its Boston Branch Office personnel made it clear that the old happy days were gone. The Project was being put on its mettle.

There was so much to be done, inside the laboratory, the problems were so new and so challenging, the materials needed were sufficiently available, and the signs of design progress were so encouraging that philosophy, attitudes, and morale remained optimistic, confident and constructive, for the most part, among the working personnel. In one respect, the engineers, the graduate assistants, and the technicians, were surrounded by nothing but technical problems, but they saw these, variously, as easy to solve, or fascinatingly challenging and ultimately soluble, or as sufficiently stubborn and unyielding to require alternative appraisal and a shifting of the line of attack in order to alter the problem to soluble form.

Forrester was sufficiently optimistic about the progress they were making to draft a memorandum to Sage in January of 1949, declaring that the research phase of the project was virtually complete and that "only a small amount of design, a fair amount of construction, and installation remain to be finished."<sup>11</sup> The subassemblies of the computational heart of the computer, the arithmetic element, had been completed and tested separately and were being linked together to form

the element itself.<sup>12</sup> Marginal checking techniques were being tested on circuits of the five-digit multiplier and yielding promising preliminary results. While only three of the four electrostatic storage tubes fabricated in December functioned well enough to submit to circuit and life tests,<sup>13</sup> such incidents in engineering development operations were to be expected, especially since the fabrication techniques for these special, giant cathode-ray tubes were as experimental as the very tube designs themselves.

Multiplication and shifting operations were attempted first in the arithmetic element, and as summer approached, further testing indicated that the speed, versatility, and reliability sought in the design phase were being approached in preliminary operational phases.<sup>14</sup> In the meantime, type 7AD7 vacuum tubes were found to be deteriorating sufficiently rapidly (in the five-digit multiplier, for example) to warrant investigation of the causes by Sylvania and MIT engineers. Since the 7AD7 pentodes, together with 7AK7 gate tubes, comprised about two-thirds of the 4,000 tubes Whirlwind I was expected to require, project engineers began to ride the problem closely. They had already shifted from type 6AG7 tubes earlier, in an attempt to eliminate this problem. Impurities in the coating materials applied to the cathodes were again the target of their studies, and circumstances of the fabrication of various batches of these tubes by the manufacturers came under close scrutiny.<sup>15</sup>

By the end of 1949 they were able to point to certain alloys used in tube manufacture that continued to support the theory that silicon was a cause of deterioration, but the sources of the silicon traces were not always easy to determine, nor was there evidence enough yet to demonstrate how tubes should be fashioned to guarantee a tube life running into the thousands of hours desired if tube replacements were to be kept from occurring at too high and impractical a rate. Thus, although knowledge of the causes of tube failure seemed to be sufficient, engineering fabrication and the "reduction to practice" of that knowledge provided challenging problems to which only continuing analysis and controlled life-testing would provide acceptable, long-run, practical answers. Such problems, with their preliminary solutions and long-term resolutions, were characteristic of the tenor of technical events in the Project during 1948, 1949, and 1950, and they remained unaffected by the funding adventures generated by changes in national (as well as Navy) fiscal and program policies.

The momentum that Project Whirlwind had generated by 1949 largely determined the character of its operations during the remainder of the time that the Navy remained the principal, federal program manager and during the first years after the Air Force stepped into the picture. It was not until the mid-1950's that the larger momentum of continental-defense policy needs, which caused MIT to create Project Lincoln and

the Lincoln Laboratory, presented a superior force. To this force the Project stubbornly bent, then yielded, modified its operational character in the transformation, and lost first control over its own destiny and then its original leader.

But in 1949 the individuality and the dynamic character of the Project as a group of young men organized to carry on specialized electronics research and development were in full strength, and the sources of this vitality were to be found not only in the personal qualities of the leadership Forrester, Everett, and their group leaders provided, essential though these were, but also in the technical procedures and policies that Project personnel followed in carrying out the technical tasks and resolving the technical problems that arose.

It would be presumptuous to state which conditions, which procedures were essential and which were not, but it is possible to describe the conditions that prevailed. From among these, as well as others not mentioned, one might imagine, select, and combine the elements of a research and development enterprise of commanding efficiency and excellence of performance. Further, one might operate it under enlightened philosophies of costs, of resources, of management, and of goals. In these respects, the virtues as well as the deficiencies of Project Whirlwind and its mode of operation may prove useful to examine here.

As has been indicated, there was a persistent search for talented and intelligent personnel. Continuing efforts to

keep standards high were matched by continuing efforts to supply whatever materials were needed. Quality personnel required quality supplies if they were to be kept busy and if the policy were not to degenerate into a mere slogan. The Project was allowed no choice but to follow the wills of its masters and operate in an atmosphere calculatedly kept as free as was humanly possible from the exasperations arising from delays caused by lack of suitable materials or by inadequate performance of inferior substitutes. Preliminary design and testing obviously could not avoid every exigency that might unexpectedly appear, and some unplanned-for delays were to be expected while an elaborate piece of test equipment, for example, was being ordered or built to cope with a new technical requirement. Shortages of standard supplies, however, were considered inexcusable because of the very fact that they were avoidable. The man who lacked the care or the ability or the pride to avoid the avoidable found the Laboratory too strenuous and soon left, either by choice or invitation. The Project placed a premium on foresight and careful anticipation of needs. To encourage these, it provided a more expensive working climate of planned yet prudent plenty, in which efficiency, morale, and productivity prospered, than (one could argue) would have been necessary in a "business-as-usual" operation.

Along with this philosophy of plenty ("hot and cold running secretaries," as one critic sarcastically put it),

went a philosophy of prudence and accountability. Office walls were to be kept clean and bare of cartoons and frivolous pictures. Verbal and written reports in quantity were insisted upon, and since the immediate availability of these was considered imperative because of the need to circulate the technical information they contained, the Project soon had its own print shop and photo lab. Friday afternoon work-bench clean-up resulted in incidental inventory and thus kept work space from degenerating into storage space. One never knew when Everett or Forrester would stop by a work bench or a test rig to see what was going on; there was no question they were keeping in touch, nor was there any reason to doubt their ability to grasp the essentials of a problem and see promising avenues of attack.

"Their approaches were very different," reminisced one engineer. "Bob Everett was relaxed, friendly, understanding--and I have never seen anyone who could go right to the heart of a problem so fast! Jay was as fast, maybe faster, but he was always more formal, more remote somehow, and you weren't always sure how dumb he thought you were, or how smart. That kept us on our toes, I suppose. It was difficult to know what he was going to do next, but he was so terribly capable, it didn't matter if you couldn't follow his reasoning. He was always thinking with seven-league-boots on. It made him a pretty formidable guy to work for--partly because he and Bob always made sure you understood the problem you

were working on, by finding out what you didn't know as well as what you did know, if you get what I mean. I never represented Jay's obvious ability, but he wasn't the sort I'd call easy to work for. He definitely never was 'one of the boys.' He was the Chief, cool, distant, and personally remote in a way that kept him in control without ever diminishing our loyalty and pride in the Project, somehow."<sup>16</sup>

"Forrester would come into the lab and tear everything apart," recalled another with a smile, "and Bob would come along and put it back together again."

"Tear you apart, you mean," said a third.

"Well, maybe so.... There was absolutely nothing personal about it, though. He was not an easy guy to know. No small talk, or if there was, it was such an obvious preamble to getting down to business! The chances were, your problem was one he'd run into before somewhere and found the answer to, and I never could see how he could be so patient. There was no question who was boss. You took it for granted he could design anything you could faster and better, but then, I was a graduate student privileged to work in a hush-hush classified project--that was before Korea changed everything--and to my eyes at that time he had had an awfully impressive amount of experience, from World War II days on. Bob had, too, but somehow I was never in awe of him the way I was with Jay."

"There were Jay's Friday afternoon teas," recalled the third. "I remember feeling I'd really arrived when I was asked to attend. That was later on, and he kept the group manageable small, so that whoever was reporting was talking about something of use or relevance to your work. Like everything Jay did, it was run very efficiently--very high signal-to-noise ratio!"

Nearly a third of the technical workers (as distinct from supporting and clerical personnel) were graduate students, seeking or working on thesis topics. Their participation produced highly motivated, rather than perfunctory activity, and as they were brought up to a sufficient level of familiarity and competence to handle the short-wave, video-pulse phenomena around which the brand-new type of machine (Whirlwind I) was being built, they were set to work on particular problems in solo fashion.

The lines of investigation were many, relative to the number of staff, and Forrester and Everett keenly realized that the cessation or interruption of any individual's work could bring to an abrupt end one of many concurrent courses of inquiry that were essential to the continuing progress of the project. Moreover, if the investigator were to leave, because of a cutback in funds, his work could not readily be picked up by another because it was not routine.

Since the fundamental business of the Project was probing the engineering unknown, research to obtain engineering

specifications and parameters was regarded as an essential preliminary to design, and wherever possible, design was to incorporate advance provisions for testing. It was an idealized principle in Professor Gordon Brown's Servomechanisms Laboratory that the gamut should range from research and creative design through a practical, working prototype, and Project Whirlwind held tenaciously to this principle; "experimental equipment, merely for demonstration of principle and without the inherent possibility of transformation to designs of value to others, does not meet the principle of systems engineering."<sup>17</sup>

Systems engineering required the "reduction of equipment to accurate drawings, and results to well-written reports...."<sup>18</sup> Its goal was dual, and sounded simpler than it was: "to produce and use computers."<sup>19</sup> Systems engineering, Forrester explained in his report prepared to meet Compton's requests, involved "the knitting together of important and valuable new systems from old and new components" in order to demonstrate "the useful application of the research results."<sup>20</sup> In this assertion he was a trifle wide of Whirlwind's mark, as a consequence of the spectacular lack of old components and the hazy prospect of nothing but relatively formless and untried mechanisms. It could be argued that vacuum tubes and crystal diodes and circuits of all sorts were really just "old components," but to those interested in the prospects of electronic computers these were not the interesting or the

vital components, except to the engineer. The impressive components were the computational "heart" of the machine and its internal "memory," for with a sufficiency of these, appropriately controlled and tied to information input and output devices, a computer really became a computer worth thinking about.

While Project Whirlwind sought a "systems approach" to the building of computers, major interest elsewhere in the nation continued to center upon questions of what performance one might expect from a finished machine. From performance, prowess could be estimated; the kind of performance in view was calculational, and the kind of prowess esteemed was logical and mathematical. Project Whirlwind, on the other hand, was spending all its energies--and all those ONR dollars--on prior questions of physical structure and electronic performance, rather than on calculational performance. This was a consequence of the fact that attention at MIT focused on empirical considerations which the young engineers in the Barta building considered inescapable. To them it was at once a truism and a serious fact of engineering life that "in many systems the greatest difficulties lie in achieving the required reliability."<sup>21</sup>

The Project leaders sharply appreciated and shared the view that "producing a satisfactory working system often requires greater technical contribution than producing the basic components of that system."<sup>22</sup> Engineering research

and development must be combined with system considerations; this was a policy commitment ingrained in the very name of the Servomechanisms Laboratory, and it was a policy commitment the abrogation of which the Project leaders found unthinkable when trying to design and build the Whirlwind computer.

As the authority of Special Devices Center personnel faded and that of Mathematics Branch personnel grew, so the visible respect that ONR felt for the IAS computer project at Princeton became more significant. Aware of this trend, Forrester and Everett sought to show how different was MIT's systems-engineering approach from that pursued by von Neumann, Goldstine, and their associates. They realized, as has been remarked at the beginning of this chapter, that the two philosophies of research and development in question started from different postulates and followed different routes in reaching their common goal, the manufacture of a working computer.

Any comparison based on adoption of either of these philosophies as a standard could only judge one project at the expense of the other and produce invidious comparisons while hopelessly confusing and intermixing the differing means and ends of the two projects. If the MIT project were selected as the norm, then the IAS project must be considered inadequate and unacceptable. If the IAS approach were to provide the criteria, then the MIT procedures must be rejected as wasteful

and inappropriate.

Since von Neumann and Goldstine had made it abundantly clear in 1946 how profound was their understanding of the potential value of the automatic-sequence-controlled-calculator mode of attacking hitherto prodigious and unassailable problems by mechanical (including electronic) means, the two unheralded young MIT engineers were at a disadvantage from the start. Nevertheless, they hammered away at the differences between the IAS and the MIT research and development procedures. "IAS," they pointed out in their analysis to Compton in October, 1948, "is presently engaged in constructing what is essentially a breadboard model of a computer." MIT, on the other hand, "is building what can more correctly be called a prototype and not an experiment or a breadboard."<sup>23</sup> This analysis of their differences in 1948 was equally descriptive and to the point in 1949 and 1950.

Fellow engineers, as well as basic-research scientists and mathematicians pure and applied, could be expected to perceive the distinction: IAS was committed to making one of a kind, while MIT was fabricating the parent of a subsequent line of computers. Obviously, the latter effort was the more ambitious, since not a half-dozen computers had yet been put into successful operation.

The experience to date was so limited and the field of development was so wide and so full of unknown pitfalls of

all sorts that there was no way in the world of guaranteeing in advance that the MIT venture would not come a cropper. If what Everett and Forrester were saying was true, then the enormity of the risks they ran was obvious to anyone who had any sort of acquaintance with the problems of developing new machines that must work when built. In this respect, the apprehensions of ONR personnel that they might well be pouring money down a bottomless though chromium-plated and gleaming drain appeared well-grounded indeed; only after the fact could they reliably take the measure of the MIT enterprise. There was no way of knowing whether the Whirlwind approach was catastrophically premature or a dramatic leap forward. If the lessons of all past research and development experience were worth anything, they suggested that Project Whirlwind would most likely turn out to be neither of these alternatives. It would become instead an attenuated fizzle, discreetly squelched, from which useful gleanings might be garnered in such salvage operations as would prove practical before the whole business was quietly swept under the rug of the obligingly silent past.

Obviously, the Whirlwind group stoutly rejected such a dismal prospect. After all, they knew what they were doing, and in the intimate fullness of this knowledge, they explained that "on the basis of considerable study, MIT has reached a fairly firm conclusion as to the nature of the computer needed." What was "fairly firm" supposed to mean? Was it

to become another funnel down which MIT would ask the Navy to pour another million dollars when even a quarter of a million would be risky and hard to come by?

Certainly there was no disagreement regarding the aptness of Forrester's and Everett's admission that "much is still to be learned that can only be learned from this machine itself."<sup>24</sup> At least, later investigators might prosper from their mistakes made possible by de Florez's original enthusiasm for the aircraft analyzer and ONR's subsequent reluctant expenditures. So might one reflect gloomily.

The MIT engineers went into amplifying detail, to indicate how they hoped to avoid large mistakes (including the production of a machine that would be obsolescent before it was finished). But these amplifications, designed to support MIT's case, could as easily be read and as reasonably be interpreted to raise new spectres for ONR, because the fundamental issue, undemonstrated by a thousand or a million words, lay in the question of whether the talent to reduce to practice in the manner they were proceeding was a talent the young engineers--men not even of Ph.D. rank--really possessed or not. Consequently, when they declared that their prototype was being built "as near to the presently foreseen needed characteristics as possible, with the following differences," the effects of these remarks could only be to reassure those, such as Nat Sage, who remained confident of their abilities and to redouble the misgivings of those such as Mina Rees,

who were uneasy yet knew they were responsible and who expected to be held to account.

The curious feature of these statements by Everett and Forrester is that they became apt descriptions and accurate forecasts of the procedures by which Project Whirlwind actually carried forward its research and development investigations. As descriptions and forecasts, they were idealistic in tone, as is customary, and to the degree that they represented the smooth, untroubled tenor of events in the world of the ideal, they of course failed to recognize those rough edges of reality that give the world of experience its relatively scratchy character. The MIT engineers happened to possess sufficient sense of proportion, however, to employ usable ideals convertible to practical expression in the forging of events that constitutes the research and development process. Historically speaking, this judgment becomes easy to render after the fact: had the young men failed, then ipso facto they would have lacked the talent required; since they succeeded beyond even their own first dreams (although not later ones that came with greater knowledge and experience), then equally obviously they possessed the needed talent, and the estimates and judgments they employed in gauging future general needs were indeed appropriate.

"Great flexibility is being built in," they pointed out. "Every facility for easy study, maintenance, and modification is being provided. Wherever compromise on specifications has

been necessary it has been made only with provision for later improvement and without relaxing the specifications for other elements. Where necessary to meet specifications, special component research has been undertaken. Elaborate and sometimes redundant trouble-location and prevention equipment has been designed. The intention is that the prototype should embody as many as possible of the desired features and characteristics, and to insure this the prototype will probably include many which are not needed."<sup>25</sup>

This was how they expected to carry on the research and development program they had long since begun. Furthermore, they had adopted a strategy of research, design, build, and test sharply different from that which von Neumann, Goldstine, and their associates employed: the IAS approach to the problem of building a machine unlike any yet built was experimental. To call it trial-and-error was to distort its true character, for there was no blind casting about, no "let's see what happens if--." It was a plan of attack that shifted back and forth from the realm of the ideal to that of the practical, in order to see how close an approximation could be obtained between the performance of physical equipment and the execution of logical procedures.

The IAS builders would be willing to go back to the drawing boards more times than would the MIT builders in order to achieve a given degree of technical improvement. The IAS approach, said Forrester and Everett, was "to attain

the desired goal by an iterative procedure, the first step of which is a single attack aimed in the estimated direction."<sup>26</sup> This attack would produce a test-bench, or "breadboard," device, the subsequent performance of which would tell them whether it needed refinement or whether it would suffice until such time as connecting it up with other elements in a system would reveal deficiencies--a frankly and honestly linear and experimental procedure.

Project Whirlwind's approach was more ambitious: "to estimate the goal more exactly and then to flood or saturate the area surrounding that estimate in a complex attack."<sup>27</sup> Although subsequent fiscal, administrative, and programming events were to show that these words fell on deaf ears and that even ears at MIT seemed to grow slightly hard of hearing at times, this description of the research and development approach the Project was following was quite honest and accurate. Indeed, it was a strategy the engineers had deliberately adopted and adhered to, not fallen into, as a consequence of their World War II engineering experience in Professor Brown's Servomechanisms Laboratory.

Unfortunately, its virtues were not apparent, even though it was a technique of procedure superbly fitted to cope with certain problems inherent in the analyses of systems of machinery. It was expensive. It was elaborate. It was not widely used, partly because it was so expensive, partly because it placed such unremitting emphasis upon premium-quality

performance, and partly because it required a rare measure of engineering sophistication, experience, and insight. In addition, those most interested in the new computers and most influential were not versed in such engineering modes of procedure; they were interested in scientific problems, in the tantalizing, potential applicability of the computer, in mathematical problems, or in information-retrieval problems, and because these interests were non-engineering in their direction and did not join issue on the policy level with the problems of design, fabrication, and performance, they failed to appreciate the power, the virtue, and the relative, long-run cheapness of such a formidable and, in a sense, daring research and development procedure.

Thus, the Project and its way of doing things were vulnerable not only because of the relatively small funds made available by ONR for fiscal year 1950--three-quarters of a million dollars--but also because of the peculiar, if not unique, and costly nature of the research and development procedures that the MIT engineers insisted on adhering to, so different from traditional and prevailing modes accepted by Navy administrators during the late Forties.

## NOTES TO CHAPTER 8

1. Jay W. Forrester, Memorandum L-5, "Project Whirlwind, Principles Governing Servomechanisms Laboratory Research and Development," Oct. 11, 1948, cc to: Dr. K. T. Compton, Dr. J. R. Killian, Mr. Henry Loomis, Mr. N. McL. Sage, Dr. G. S. Brown, Mr. Ralph Booth. The memo, one of the "L"-series of Whirlwind reports, covered three typewritten pages, single-spaced.
2. Jay W. Forrester and Robert R. Everett, Memorandum L-6, "Comparison between the Computer Programs at the Institute for Advanced Study and the MIT Servomechanisms Laboratory," Oct. 11, 1948, cc to: President's Office (2), Mr. N. McL. Sage, Prof. G. S. Brown, Mr. Ralph Booth. This "L"-series report covered six typewritten pages, single-spaced.
3. Memorandum L-5, p. 1.
4. Memorandum L-6, pp. 2, 5.
5. Memorandum, Hugh R. Boyd and Robert A. Nelson to Jay W. Forrester, subj.: "Detailed Estimates of Costs Project Whirlwind, Applicable to Period from November 1948 through June 1950," November 4, 1948; Memorandum L-8, J. W. Forrester to N. McL. Sage, subj.: "Cost Trends, Contract N5ori-60," November 19, 1948; Memorandum, J. W. Forrester to N. McL. Sage, subj.: "Budget Adjustments," December, 1948.
6. J. W. Forrester, Computation Book No. 45, entries for November 2, 1948, p. 141; December 6, 1948, p. 146.
7. J. W. Forrester, Computation Book No. 49, entries for December 9, 1948.
8. Amendment No. 7, T. O. No. 1, Contract N5ori-60, March 31, 1949.
9. Ltr., N. McL. Sage to ONR, December 10, 1948; ltr., Mina Rees to N. McL. Sage, February 23, 1949; Memorandum, Harris Fahnestock to J. W. Forrester, subj.: "Comments on Letter Dated February 23, 1949 from Dr. Rees to Mr. Sage," March 15, 1949. J. W. Forrester, Computation Book No. 49, entries for 1/12/49 and 1/13/49, pp. 15-17. See also Memorandum L-10 (Draft), J. W. Forrester to N. McL. Sage, subj.: "Analysis of Whirlwind Program," January 13, 1949.

10. Ltr., J. W. Forrester to C. V. L. Smith, May 20, 1949; ltr., N. McL. Sage to Head, Computer Branch, Mathematical Science Division, ONR, June 14, 1949; ltr., C. V. L. Smith to N. McL. Sage, July 18, 1949; Amendment No. 9, T. O. No. 1, Contract N5ori-60, July 1, 1949; ltr. M. S. Stevens to Head, Computer Branch, subj.: "Supplementary information on request for funds for Contract N5ori-60 (NR 048 097)," July 27, 1949; File Memorandum, J. W. Forrester, subj.: "Air Traffic Control Project," March 10, 1949; J. W. Forrester, Computation Book No. 45, entry for July 28, 1948, p. 121; Computation Book No. 49, entry for March 10, 1949, p. 36.
11. J. W. Forrester, Memo L-10 (Draft), subj.: "Analysis of Whirlwind Program," Jan. 13, 1949, p. 1.
12. Summary Report No. 15, Dec. 1948, p. 2.
13. Ibid.
14. Summary Report No. 16 (Jan.), No. 17 (Feb.), No. 18 (March 1949).
15. Summary Report No. 18, Mar. 1949, pp. 14-15.
16. The engineers interviewed here by the authors requested their identities remain anonymous.
17. Memorandum L-5, subj.: "Project Whirlwind, Principles governing Servomech. Lab. Research and Development," October 11, 1948, p. 2.
18. Ibid.
19. Memorandum L-6, subj.: "Comparison between the Computer Programs at the Institute for Advanced Study and the MIT Servomechanisms Laboratory," October 11, 1948, p. 2.
20. Memorandum L-5, p. 1.
21. Memorandum L-5, p. 2.
22. Ibid., p. 1.
23. Memorandum L-6, pp. 1, 2.
24. Memorandum L-6, p. 2.
25. Ibid.
26. Ibid.
27. Ibid.

## CHAPTER NINE

### THE COLLISION COURSE OF ONR AND WW

When the Office of Naval Research in the spring of 1949 made its conservative allocation of fiscal year 1950 Whirlwind funds, a skirmish or even a battle may have been lost, but Forrester had not yielded the field. In December, 1949, responding to a request from the deputy Director of ONR, Captain J.B. Pearson, Forrester projected into fiscal years 1951 and 1952 a program for digital computer work at MIT which would have cost \$1,150,000 and \$943,000, respectively. The program he envisaged was quite expensive, including a "normal continuation" of the existing program and an expanded program for research in the area of application. Again Forrester warned against "the over-optimism and unfounded promises which have been so apparent in much of the digital computer planning and publicity." The programs would be long, and sponsors could not expect "immediately hardware for the complete solutions of their own problems."<sup>1</sup>

Forrester's response to Captain Pearson brought forth some rather strong opposition from various

members of ONR. R.J. Bergemann, Physical Scientist for the Boston Branch Office, severely attacked Forrester for not containing his program within the limits established by ONR. Instead of planning to complete the computer at minimum cost, he charged, Forrester's thinking was directed "towards the great possibilities that lie in computer application." Herein lay Forrester's sin, for he had clearly been instructed, according to Bergemann, to eliminate "long range planning." Bergemann directly attacked the Project for producing "less for the money than might be obtained elsewhere," and he unfavorably compared it to the "Hurricane" computer under development at Raytheon.

The Raytheon project, Bergemann argued, was technically superior and cost less, primarily because the men engaged in it possessed greater experience and competence. Whirlwind personnel on the other hand, he noted, had had no "previous digital computer experience," and few of the Project's engineers had had "any engineering experience other than under OSRD-NDRC contracts where cost was no object."

Bergemann's recommendation was that "ONR reemphasize the necessity for lower expenditures in Project Whirlwind by concentration of effort on completion of the computer

in its simplest useful form." Forrester, he explained, must be made aware of the difficulty of justifying the spending of "one twentieth of the ONR budget on his project, when Raytheon has done so much more on a smaller expenditure."<sup>2</sup> Unfortunately for Bergemann, the project to which he so unfavorably compared Whirlwind did not measure up to expectations. Within the year the recommendation was made that the Raytheon contract be terminated, upon the grounds the company could not meet its "contractual obligations ... with their existing organization, on their presently estimated schedule and at the estimated cost."<sup>3</sup>

C.V.L. Smith, Head of the Computer Branch, was another who seriously attacked Forrester and his Project, referring to the latter's estimates in his letter to Pearson as "fantastic." He found "appalling" Forrester's refusal to recognize "that funds simply are not available to support such an extensive program." Smith also found the program projected for Whirlwind "excessive" and the staff not sufficiently qualified "to justify this expenditure." He did not, however, repeat Bergemann's unfortunate mistake of comparing it unfavorably to the project under development at Raytheon. He summed up his argument by recommending that Whirlwind be made operational during 1951 and that Forrester be convinced of the necessity to reduce drastically expenditures and to stop thinking "in terms of a million or so per year."

In response to a proposal, apparently advanced by Pearson, that a conference be called to discuss the financing of Project Whirlwind, Smith's attitude was negative. He opined that Whirlwind had been "oversold," that "a very considerable skepticism" had arisen. It would be "a great mistake" to call a meeting before it was possible to demonstrate a fully operable machine." He suggested the machine be tested by running several diverse problems on it to permit "a really convincing demonstration" of its potential. "Anything short of this would not only be futile, but probably harmful in its total effect."<sup>4</sup>

It is interesting to note that during the course of a visit to Project Whirlwind on January 12 and 13, neither Bergmann nor Smith was, understandably, as caustic in his comments to Forrester and his colleagues as each permitted himself to be in memoranda intended for internal Navy eyes. At least, the trip reports prepared by the two men give no evidence of such blunt and candid exchange. Smith did, however, upon this occasion review with Forrester the latter's proposed budget for fiscal year 1951, explaining that it was impossible for ONR to raise the 1.15 million dollars proposed and that at best the office was planning to allocate \$250,000 to \$300,000.<sup>5</sup>

Once again Forrester's proposals on program and budget

for Project Whirlwind raised the matter to the highest levels within both MIT and ONR; once again the decision was taken to discuss the matter in a general conference to be hosted this time by the Institute; and once again Forrester, in preparation for the exchange of views, sought to win the Institute's administration to his side. In a letter to the Provost, Dr. J. A. Stratton, Forrester explained in great detail the capabilities of the computer which, he wrote, would be assembled by the fall of 1950 and ready "to start research into 'real-time' applications." He predicted the computer would be capable of "preliminary work" in at least eighty per cent of the applications listed in his letter to Pearson, including fire-control studies, logistics studies, centralized digital computer service, weapon evaluation, engineering and scientific applications, antisubmarine-control studies, air-intercept combat-information center research, simulation, and air-traffic control research.<sup>6</sup>

This time, however, Forrester was less successful in persuading his superiors to give him full support. Viewed from one direction, the Navy, or more particularly, ONR, finally was able to execute an end-run around Forrester and Sage and reach MIT's top administration without effective interference. Viewed from another direction, Forrester had been unable longer to convince his superiors that he re-

cognized the funding realities of life that prevail in the conduct of research and development during that uneventful spring before the Korean War suddenly broke out.

From still another direction, one might speculate that Whirlwind had become a computer without a practical, specified mission as a consequence of Forrester and Everett's commitment to an avowedly general-purpose instrument and as a consequence of their single-minded concern to bend all their efforts to bringing such a computer into being. The research and development process itself had for years demonstrated the practical worth of Benjamin Franklin's shrewd rhetorical query (uttered in reply to a critic of the first balloon flights) -- "Of what use is a new-born baby?" -- but this general wisdom did not automatically justify, in the particular instance of the Whirlwind project, the torrential outpouring of ONR dollars that Forrester sought. Everything in the traditional philosophy of government funding of research and development indicated that the Navy could not afford to gestate so costly a baby of so uncertain pedigree when more promising purebreds, such as the IAS machine, were costing so much less. Nor did it simplify the problem to have some of the Navy program managers feel that Forrester's reiteration of his demands was bordering on the arrogant.

Forrester recalled in later years that he had shared the apprehensions of Special Devices Division personnel regarding confidential projections which called for a Russian atomic strike capability by 1953.<sup>7</sup> These concerns were less central in the minds of the mathematics and science oriented programmers in ONR who were responsible for maintaining liaison and surveillance relations with Project Whirlwind by early 1950.

Finally, it could be argued from still another direction that the series of investigations and inspection visits instituted by MIT and by the Navy over the past three years were by this time exerting an appreciable cumulative effect. In any event, these investigations and their findings came to constitute a factor that did not improve -- if it did not actually harm -- Project Whirlwind's chances of gaining and maintaining the degree of financial support Forrester so unremittingly and unrepentantly sought.

The determined efforts made by ONR to reduce the costs of Project Whirlwind and to restrain Forrester indicated that the early apprehensions of those directly responsible for the administration of the Project had not been allayed. Instead, their concern eventually had reached even the highest levels of ONR and MIT. Both Warren Weaver and Francis J. Murray in 1947 had been relatively mild in their respective evaluations, neither one finding any major flaws in the

Whirlwind program. Yet both had confirmed, if only mildly, the fears of the Mathematics Branch of ONR that the Project was weak in mathematical competence and direction. The only evaluation which had been outspoken in its praise was that prepared by Ralph Booth, assisted by J. Curry Street. To some extent this investigation could be discounted, on the grounds that it had been conducted by a sympathetic and prejudiced investigator.

Other evaluations, however, were more sharply critical if not outright condemnatory of the Project, its cost and the absence of a definite purpose. One of these evaluations was prepared sometime during 1948 for Mina Rees and without doubt by someone in whom she had implicit confidence. His criticisms were so strong that even sixteen years later she declined to reveal his identity. This anonymous critic sharply challenged the Project, finding it completely "unsound on the mathematical side" and "grossly over-complicated technically." It was a program without purpose, one which had become "one of the most ambitious in the country ... notable for the lavishness of its staff and building." Apparently, there was little about the program and its directors which the critic could praise, although he did, perhaps grudgingly, approve Forrester's "ideas about great reliability and the necessity of convenient and complete provisions for checking and for locating trouble..."<sup>8</sup>

Acutely aware of the controversy revolving around Project Whirlwind, of her own lack of understanding of the engineer's approach, and of the necessity for a valid, competent, and objective evaluation if her recommendations concerning the program were to possess substance and merit, Mina Rees undertook in late 1948 and early 1949 to organize an inquiry which would at one and the same time familiarize her with the program and provide the critical analysis she needed. The organization of a team for this purpose was not easy, for although she could appoint members from the ONR staff, it was exceedingly difficult for her to find an impartial expert acceptable both to her and to the administrators of MIT and the Project.<sup>9</sup>

Eventually, Dr. Harry Nyquist of the Bell Telephone Laboratories was settled upon. The committee, comprised of Dr. Nyquist as the impartial expert, Mina Rees, C.V.L. Smith, and Dr. Karl Spangenburg, head of ONR's Electronics Branch, visited the Project in the spring of 1949. The group reviewed and analyzed the program, finding apparently no major weaknesses. Some technical questions were raised regarding "the means of communicating with the machine," the "means of auxiliary storage," the computer's word-length and the storage tube development program, but all in all the group, according to C.V.L. Smith, "was favorably impressed by the thoroughness of the engineering effort displayed by the

Whirlwind staff, and by the energy, enthusiasm and directness of approach with which the numerous difficult problems encountered have been attacked."<sup>10</sup>

In response, Forrester expressed his appreciation. At the same time, he noted that in an earlier communication he had anticipated the committee's recommendations by suggesting new task orders to cover the proposed work.<sup>11</sup> This was not exactly what the committee had really recommended, for new task orders meant additional funds.

The evaluation which hurt most came at the end of 1949, and it brought a sharp and irritated rejoinder from Forrester. This was the investigation which the Chief of Naval Research had been anticipating, conducted by the "Ad Hoc Panel on Electronic Digital Computers" of the "Committee on Basic Physical Sciences" of the Research and Development Board. The Ad Hoc Panel had been created on July 29, 1949. It was composed of Dr. Lyman R. Fink, chairman, Dr. Gervais W. Trichel, and Dr. Harry Nyquist, and it proposed "to look critically at the several projects comprising the program on digital computing devices in the Department of Defense, with emphasis on the objectives, management, engineering planning, current status, and probability of successful completion of each project."

After visiting various contemporary digital computer

projects, holding hearings, attending the Second Symposium on Large Scale Digital Calculating Machinery at Harvard University and studying contemporary progress and engineering reports, the committee prepared and issued a tentative report of its findings and recommendations on December 1, 1949. Jay Forrester, it is interesting to note, was not included in the distribution list; von Neumann was.

The panel concerned itself with the total computer program supported by agencies within the Department of Defense. Forrester, in his rebuttal, observed, however, that the panel had overlooked the United States Naval Computing Laboratory operated in St. Paul, Minnesota by Engineering Research Associates on a budget and staff level three times that of Project Whirlwind. This was, perhaps, his retort to the panel's conclusion that the "scale of effort" on Project Whirlwind was "out of all proportion to the effort being expended on other projects having better specified objectives." If the panel's figures were even approximately accurate, Project Whirlwind's estimated completion costs, made at about the same time as the panel was conducting its inquiry, were about twenty-seven per cent of the total amount the panel estimated would be the cost of the overall Department of Defense program. This overall program, comprising some thirteen machines under development by eight suppliers,

would cost, the panel estimated, some ten millions of dollars.

Its tentative report was circulated for "information and comment." In it the panel discussed broadly the need for high speed digital computers, the requirements which should be met for a rational, over-all program, and the status of the contemporary program. In critically evaluating the contemporary overall program and the individual projects-in-being composing it, the panel found it to lack coordination, organization, and centralization, and noted that it failed, therefore, to realize "optimum" return from the effort and money expended. The panel did not ascribe these basic flaws to any particular agency or cause; indeed, it conceded that the projects appraised antedated the establishment of the Department of Defense. The services should in fact be "commended rather than criticised for the degree of voluntary cooperation" that had taken place. These words were probably the kindest the panel wrote into the report.

Critically, the panel observed that "no specific procedure" had been established "for the review, coordination and control of high speed digital computer development." The over-all program was marked by the absence of a central agency which could collect and distribute information or one

which could evaluate performance in order to provide new users with "reliable sources for advice and assistance in technical procurement in an unfamiliar field."

The panel further concluded that the technical guidance and supervision provided the individual projects by their respective sponsoring agencies were insufficient. In several instances technical reporting was poor and not kept current; technical directors were not always aware of the contemporary state-of-the-art; the exchange of information was often poor; contractors were not always given proper direction; in some instances contractors had made "important changes in the operating characteristics of systems" without approval; estimated dates of completion were not realistic; and some devices were being "built as part of contracts for other devices or incident to service contracts."

To reiterate: the over-all program lacked coordination, organization, supervision, and centralized control. To cope with and eliminate these fundamental weaknesses, the Ad Hoc Panel recommended the creation by the Research and Development Board of a panel subsumed not under a mission-oriented engineering committee but under a committee that would be expected to regard the computer for the scientific engine the panel knew it to be. Such a panel should properly be placed under the Committee on Basic Physical Sciences, to coordinate the Department of Defense digital computer

programs. Any project not approved by the proposed panel would be denied budgetary support. The existing projects, however, the Ad Hoc Panel recommended should be left "substantially intact, since this would serve the "best interests" of the Department.

In its treatment of specific projects, the panel was no more charitable than it had been in its treatment of the over-all Department of Defense program; even the program at the Institute for Advanced Study received its fair share of critical comment. Project Whirlwind, although commended for its "excellent job training" of graduate students, for the excellence of its engineering and scientific staff, and for the quality and quantity of its engineering reports, was held to be lacking a "suitable end use." Consequently the recommendation was made that if the Navy could not find one, "further expenditure for the completion of the machine should be stopped." However, the panel did suggest that consideration be given to using MIT's excellent staff "on system studies and on the development of specific computing components," especially the storage tube.<sup>12</sup>

The reaction of Forrester and his colleagues to the panel's findings and recommendations was one of distress, concern, and anger. Acknowledging many of the panel's general recommendations to be excellent, they opined that the

portions of the report which dealt with specific projects were incomplete, based upon inaccurate information, and superficial. The panel had stressed the flaws and weaknesses disclosed by the investigation to such an extent, Forrester charged, that it had raised the "real danger" of "shaking confidence in the field" and destroying thereby the efficacy of the general recommendations offered "for strengthening the digital computer program."<sup>13</sup>

Without doubt, a good portion of Forrester's irritation and that of his associates found its genesis in the panel's comments on Project Whirlwind itself. This is understandable, for the Whirlwind staff had prepared long and detailed explanatory memoranda, describing the purpose and nature of the program, its historical background, and the contribution the Project was making and would continue to make to computer technology -- all apparently for nought.<sup>14</sup>

In addition, many of the criticisms and recommendations expressed within the report either reflected or were modified versions of the very points Forrester had been advancing over the course of the preceding years. Without doubt, the engineers and scientists of Project Whirlwind felt both let down over what to them must have been the panel's failure to recognize their contribution to the state-of-the-art, and angered by the panel's recommendation that the program to which they had dedicated themselves be eliminated unless a

specific and positive use for Whirlwind was found.<sup>15</sup>

The cries of anguish and anger were not Project Whirlwind's alone. The charges made by the panel were sufficiently penetrating to compel the Acting Head of the Computer Branch of ONR, A. E. Smith, to prepare a rebuttal which in essence defended not only ONR, but Project Whirlwind as well. Replying to the specific charge that ONR had no purpose in view for Whirlwind, Smith noted that when completed, the computer would be "useful ... to point the way to the solutions of the numerous control and real time simulation problems of importance to the Department of Defense." Listing the various areas of application which had been considered or which were under study at the time of writing, Smith argued that each would require "voluminous arithmetic experimentation ... before goals can be set with any precision or efficiency." This, coupled with the "interest of the many different activities in Whirlwind," provided justification enough to proceed with the program. As far as Smith was concerned, Whirlwind's completion was mandatory, in order to realize "the original goal of the project" and also the proposals made by the panel itself concerning the use of the MIT group for system studies and the development of components.<sup>16</sup>

It was in such an atmosphere of investigation, criticism, complaint, and counter-complaint, or in the climate influenced

in part by such developments, that Forrester late in February and early in March, 1950, was unable to persuade his superiors at MIT to give him the full support he desired. One might argue that the common cause allying ONR and MIT against a common, hostile critic, the Research and Development Board and its Ad Hoc Panel, caused the two organizations under attack to submerge their smaller differences, such as how much funding support to give Project Whirlwind, and expediently to close ranks to deal with the issue at hand. Whatever the combination of causes, Forrester found himself corralled as never before. Three days after his letter to Stratton, ONR representatives came to Cambridge to discuss the Project and its place in the overall ONR computer program. Two conferences took place on March 6, 1950, one in the morning and one in the afternoon. At the morning conference were Provost Stratton, Dean Harrison, and Dean Sherwood of MIT, and Dr. A. T. Waterman, Dr. Mina Rees, and Dr. C. F. Muckenhaupt of ONR. This was a policy meeting which Forrester did not attend, and this fact suggests not only did the MIT administration recognize that some of the Navy criticism of Project Whirlwind was taking strong colors of personal criticism of Forrester's way of conducting his affairs, such a thorn had he become, but also the MIT authorities recognized the wisdom of maintaining ONR support in common cause against such Defense Department criticisms as the

Ad Hoc Panel had levelled. It was to the advantage of both the parties to resolve existing differences as unemotionally as possible.

In any event, Forrester did not attend the meeting. It appeared that the MIT leadership had decided to formulate, with ONR representatives, broad guidance principles to which Forrester and his associates would have to conform. But the MIT leaders could afford to pursue their course tactfully and magnanimously, for they had as ace up their sleeve, and of this circumstance Forrester was reassuringly aware, especially since the Project had assisted in placing it there.

The morning conferees discussed MIT's thoughts concerning the "advisability" of combining the Institute's computer programs under a single head while permitting the individual programs to continue within the departmental structure. The representatives of ONR welcomed the proposed reorganization, provided a "suitable head of the program ... be chosen." Forrester was not among those considered for the appointment. The conference ended with MIT's agreeing to see that Project Whirlwind lived within a \$250,000 budget for the following year, the maximum amount ONR could allocate to the program.<sup>17</sup> Thus Forrester's hopes appeared to be frustrated without any possibility of an appeal to a sympathetic MIT administration.

Mention has been made that MIT had an ace up its sleeve. Events suggest the Navy knew it was there and was equally good-humored about it. The afternoon meeting was attended by all the morning conferees except Dean Harrison; present also were Jay Forrester, C. V. L. Smith, and a new figure, Dr. George Valley of the MIT Physics Department. Valley was chairman of an Air Force committee that had been created to investigate the state of contemporary air defense with the purpose of recommending improvements and changes. It was Valley's presence that altered the whole financial picture for Project Whirlwind, for he proposed at the afternoon conference that Whirlwind be applied to experiments in air defense. To this purpose, he believed, the Air Force would be willing to allocate some \$500,000. All agreed that this would be an excellent solution to the situation, for it would assist the Air Force in a problem of great national importance, yet leave the computer available for scientific use and for use on Navy problems also.<sup>18</sup>

The following day, March 7th, another meeting was held to discuss in greater detail the financing of Project Whirlwind and the program suggested in Forrester's letter to Captain Pearson. The financial basis for the discussion was \$300,000 from the Computer Branch of ONR plus another \$30,000 from the Armament Branch for a fire-control study. Monies over and above this total of \$330,000 would have to

come from sources other than ONR, in this instance from the Air Force for its air-defense study.

The two-day conference did demonstrate that neither MIT nor ONR was inclined to permit Project Whirlwind to become an operation resembling in size or cost such efforts as the wartime radar program or the Manhattan Project, as Forrester had occasionally suggested.<sup>19</sup> If it had not been for the Air Force and its search for an adequate air defense system, Project Whirlwind might well have been limited to scientific calculations and such modest Navy projects as might have arisen. The program underwritten by ONR alone would never have met Forrester's desires or expectations. In the imposition of limitations upon the program, ONR had finally won the cooperation of MIT, aided by the fortuitous cooperation of the Air Force.<sup>20</sup>

Accepting \$780,000 as the maximum allocation for fiscal year 1951, Forrester planned the Project's program accordingly. He submitted a memorandum to this effect to Nat Sage, who in turn forwarded it to ONR in May of 1950 as an enclosure to the Institute's official request for funds for fiscal year 1951. In addition to the \$780,000 --- \$280,000 from ONR and \$500,000 from the Air Force --- Forrester anticipated an additional \$120,000 from the Air Force for the Air Traffic Control study and approximately \$32,000 for an additional Navy study in the application of digital computers to fire control: a total of \$932,000.<sup>21</sup>

Late the following month C. V. L. Smith, Head of the Computer Branch, replied that scientific approval of the proposed budget had been granted and that MIT would shortly hear from ONR's Contract Division. On June 29, 1950 the Amendment officially confirming the allocation and extending the time of the contract to June 30, 1951 was issued. In his letter, Smith remarked that it was planned by ONR that the \$280,000 would carry the Project for about four and one half months; meanwhile, the Air Force, he anticipated, would transfer to ONR \$500,000 of Air Force Funds for fiscal year 1951, to carry the Project to June 30, 1951.<sup>22</sup>

In a comment to Nat Sage, Forrester observed that the ONR allocation was for a four and one-half month period, and it was his intention to implement his program on the assumption an additional \$500,000 would be forthcoming from the Air Force to finance the program for the balance of the fiscal year. Sage's brief reply that Smith's letter of June 26 provided the answer to Forrester's implied question suggests that Sage had either accepted the impossibility of obtaining more funds from ONR or had become convinced that the Air Force would accept the recommendations of George Valley and his committee and underwrite the proposed experimental program in air defense.<sup>23</sup>

The Air Force was slow in making its funds available, however, perhaps in part because of the indecision and confusion which resulted from the intensification of concern over the state and adequacy of the nation's air defenses. This concern was a product of the Cold War, which had become more ominous with Russia's detonation of an atomic bomb in August, 1949, and also a product of the outbreak of active fighting which occurred when Communist North Korea invaded the United States-supported Republic of Korea to the south in June of 1950. Finally, in mid-November, 1950, the Air Force transferred to ONR \$480,000, to which the Navy added \$20,000 to provide the anticipated \$500,000 for the air-defense study.<sup>24</sup>

The entry of the Air Force into Project Whirlwind, together with reorganizations carried out by MIT to centralize and coordinate computer-development projects in which various of its faculty were engaged, resulted in a broadening of the Institute's involvement with digital computers and furnished a not uncommon instance of the growing importance of the computer on the American technical scene at that time. In an action that left the Whirlwind project freer to pursue its air defense investigations and that was carried out in part at Forrester's suggestion, MIT ultimately established a Center for Machine Computation under the direction of Professor Philip M. Morse of the Physics Department. His became

the responsibility to "combine and coordinate the use of existing computing machines at the Institute," both Institute and government owned, including Whirlwind, but Morse's Center was not concerned with air defense problems.<sup>25</sup> Formally, the Whirlwind staff that originated in the old Servomechanisms Laboratory of the World War became the Digital Computer Laboratory already housed in the Barta Building, under the direction of Jay W. Forrester, with Professor Gordon Brown as Faculty Adviser, Robert Everett as Associate Director, Harris Fahnstock as Executive Officer, and J. C. Proctor as Personnel and Security Officer.<sup>26</sup>

The Whirlwind group eventually was to join other MIT groups and become incorporated, as "Division Six," into the Lincoln Laboratory, which was established by MIT to research, design, and develop a centralized air defense network utilizing a high-speed digital computer at its center. The Whirlwind computer was to play a dramatically important role in demonstrating the feasibility of such a system. It was also to be used by the Navy, which continued to allocate rather substantial sums of money to MIT for research and development in the mathematics of computer design and use.

Beginning with fiscal year 1951, although still carried under the original contract, allocations were designated

by ONR for use not by Project Whirlwind, but rather by the Center for Machine Computation for research in applied mathematics and for research which would lead to improvements in computers, and which would advance the state-of-the-art. To this end, the Center was allocated \$600,000 in June of 1951, and an additional and final allocation of \$50,000 was granted for studies in the application of the digital computer to air defense.<sup>27</sup> In March of 1952 \$250,000 was made available; a year later \$285,000. Although subsequent allocations were made, they were much less; these apparently were the final allocations of substance to be made by ONR under Contract N5ori-60.<sup>28</sup>

The success by March of 1950 of ONR's policy to reduce the dollar cost of Project Whirlwind to the Navy does not appear to have been contingent upon Air Force willingness to follow Professor Valley's judgment and "pick up the tab," nor was it made possible by the fact that Whirlwind I finally appeared to have found the practical reason-for-being (after long ago giving up the aircraft simulator) that the Ad Hoc Panel had criticized it for lacking. The principal reason may have been the Navy's willingness to write off to experience the unacceptably high cost of maintaining Project Whirlwind's lavish standards of operation that temporarily had prevailed, for ONR never did endorse Forrester's mode of operation, and

neither did the Air Force or the Department of Defense. But the situation was probably not that simple.

It could be argued that, in a sense, even MIT found it necessary to repudiate Forrester's way of doing things, as Nat Sage's letter of July 11, 1950 implied. It could be argued further that Forrester's style could not be MIT's style, under the circumstances of limited funding that prevailed on the federal government level before the Korean War broke out in June. Even then, there was a delay while government echelons convulsively executed about-faces. So, one might conclude, it was not the essential merit of the Project's research and development record and performance that moved the Air Force to follow Valley's lead, but rather it was the emergency, "crash-program" nature of the need imposed by the nation's vulnerability to aerial attack from over the Pole that furnished Forrester his reprieve.

There remains still to be taken into account the behavior of the Navy and its ONR program managers. As Forrester recalled, in spite of their repeated protests the Navy managers did continue to support the program financially during the crucial development years of 1947, 1948, and 1949.<sup>29</sup> When the manager's misgivings grew sufficiently intense, an investigating committee made a dubious or negative report, this disturbing news was sufficient to generate the appointment of another investigating committee. In short, if the

Navy did not dare--or did not care--to support wholeheartedly the Whirlwind project, with its insatiable funding demands, neither could it shut it off, apparently.

ONR's growing reputation with the scientific community was not likely to be shattered or even seriously sullied by a simple decision to stop funding an unorthodox engineering venture preoccupied with a scientific machine and directed by young men who were still scarcely more than graduate students and considerably less than professional faculty. What about pressures within the Navy, then? They were not all one-sided. Administrators in Mina Rees's position, for example, could be expected to respond to the multiple pressures that existed within their own organization, yet these pressures did not appear to generate an obvious and unvarying resultant whose measure can be quickly taken in retrospect. The military establishment was looking closely into computers when these were still largely disembodied and tantalizing promises . It became a continuing military commitment, nevertheless, even when an agreed-upon military mission was not discernible, and the cross-currents of technical promise and practical doubt could as well have made it awkward as have made it easy for ONR to drop Whirlwind. Yet, on balance it appears to have been awkward rather than easy.

As the months passed into years, the R & D momentum

which the Project built up became a factor contributing to its survival. While the technical, visionary, command-and-control projections that Forrester and Everett had committed to paper in their early L-1 and L-2 reports on antisubmarine warfare techniques failed to impress the Navy with the obviousness of their anticipation of things to come, neither did the mounting dollar costs of the Project cause Admiral Solberg, Alan Waterman, Manny Piore, Mina Rees, C.V.L. Smith, and their associates to agree to "pull the plug".

The reticent character of the recollections of some of the Navy programmers in after years may prompt the disinterested observer to infer that they had the well-known bear by the tail, were unable to let go, and would rather not remember it, but the fact remains that in those pioneering days in the history of computers they were extending continuing financial support with the palm of one hand while extending continuing technical skepticism with the back of the other. Perhaps the soundest lesson to be offered here is the typical character of the R & D situation in which the MIT engineers and the ONR programmers found themselves. Although the particular events were of course unique, the pattern of relations between buyer and seller was customary. Those familiar with the conduct of R & D will recognize that Project Whirlwind was not staggering from precipice brink, as in some melodrama, but was following, with respect to the events set forth here, a conventional course in its pursuit of R & D.

The ambivalence of the Department of Defense with regard to Project Whirlwind's situation has already been indicated in the account of the preliminary report of the Ad Hoc Panel to the Research and Development Board. Re-affirmation of this dissatisfaction, slightly muted, is to be seen in the final report of June 15, 1950, even though by this time the status of Whirlwind had been considerably altered by the Air Force's decision to accept the recommendations of the committee headed by Professor George Valley and to employ Whirlwind to test the feasibility of establishing a centrally controlled air defense system with a high speed digital computer as its nucleus.<sup>30</sup> Jay Forrester did not neglect to bring this decision to the attention of the Research and Development Board, which had authorized further hearings and discussions to be held in order to air the various criticisms of the tentative report of December 1, 1949, so that proper corrections and modifications could be made.<sup>31</sup>

The Ad Hoc Panel's parent Committee on Basic Physical Sciences had met on February 9, 1950, to consider the panel's first report, submitted the preceding December, and to hear objections from "interested persons ... invited to be present and be heard ... "<sup>32</sup> The criticisms heard fell into three categories, according to the panel: (1) inaccuracies and omissions, (2) defects in suggested means of implementing

certain panel recommendations, and (3) errors in findings and recommendations related to particular computer projects. The Committee took due note of all evidence, accepted the panel's report, and discharged the panel, thereby putting it out of existence.<sup>33</sup>

In institutional affairs, the power to destroy implies the power to create, and the following May the chairman of the Research and Development Board "invited the members of the ... Panel to act as consultants" in order to modify the report they had given the Basic Physical Sciences committee and produce thereby a final report which the Research and Development Board might consider. Responsively, Fink, Nyquist, and Trichel spent May 11th and 12th together in Washington. "Acting in their capacity as consultants" to the RDB Chariman, they made "minor revisions ... to meet pertinent objection," eliminated certain criticisms for which corrective action in the intervening nine months had been taken, corrected various errors that had been brought to light, and heard verbal reports from project representatives in order to bring their information up to date.

The May sessions convinced the former panel members that "no serious criticisms" of their earlier report had been found, that original objectives had been sound, and that the "broad purpose" to which their study was devoted remained unchanged.<sup>34</sup> The broad purpose had been to survey

existing computer projects being sponsored by agencies in the Department of Defense and see what sort of integrated and coordinated program should be developed to meet "the over-all needs and objectives of all three Services ..." As the consultants saw it, limitations must be placed on the "laissez-faire" practices of the recent past and the present exercised by different federal agencies. The appropriate organization to determine the limitations required should be, the consultants felt, the Research and Development Board.

One significant cause of the difficulties and counter-criticisms they encountered in shaping their report is to be found in the research and development philosophy the panel embraced, as is revealed by changes made in the later form of the report as a consequence of the passage of time. Their philosophy was as different from Forrester's and Everett's as was that of the Mathematics Branch and ONR.

A second cause of the problems they dealt with was the sharpness of the counterattacks the first report received from industry, government, and university representatives. Events showed that while these RDB representatives of the scientific and industrial establishment were seeking to curb and coordinate computer funding and programming practices, they were unable to keep their own houses in order.

Thus, a specific recommendation in the preliminary report against building and leaving with the builder a computer over which the Department of Defense retained no rights of priority, title, or recapture was excised from the final report,<sup>35</sup> although the information that the IAS machine would "remain at the Institute and be its property with the Department of Defense holding no title to the machine or to its use," appeared elsewhere in both versions of the report.<sup>36</sup>

Again, the panel had recommended that "a study project be initiated on the subject of standardization of input and output language of future machines." Apparently this suggestion was premature and ill-advised, perhaps considered impractical at the time by many experts, for it, too, was deleted from the final report.<sup>37</sup>

Similarly, the panel's objections to "a program for copying a non-existent machine or a machine with an indefinite completion date" and to a program "without a well defined objective of value to the Department of Defense" were stricken from the final report.<sup>38</sup>

Hindsight permits perhaps too facile criticism to be made of the Ad Hoc Panel's efforts to provide the Research and Development Board with information and insights that it might use to bring the progress of computer research and development under more efficient, orderly, and prudent

control. History shows that these efforts failed, and it shows also how difficult it was, even for accredited experts, to appraise the operations then going forward in the newborn computer technology.

Dr. Lyman R. Fink had received his Ph.D. in electrical engineering at the University of California (Berkeley) in 1937 and was in the employ of the General Electric Company at the time. Dr. Gervaise W. Trichel had obtained an M.S. degree in engineering at MIT, a Ph.D. in electrical engineering at the University of California in 1938, and was then serving as a staff assistant to the General Manager, Chrysler Corporation. Dr. Harry Nyquist had taken his Ph.D. in physics at Yale in 1917, had worked in Vannevar Bush's Office of Scientific Research and Development during World War II, and had been associated with the Bell Telephone Laboratories since 1934.

Yet, neither these men nor the Research and Development Board nor the Secretary of Defense himself possessed the leverage to effect the sort of coordination and control the panel and presumably its parent Board sought. The cause of this ineffectiveness lay neither in the newness of the computer nor in the novel forms the emerging computer art was assuming nor in any still unappreciated, still undreamt developments in its future. It lay instead in the deeply ingrained habits of carrying on highly technical

industrial and engineering affairs in the American economy. According to these habits, the freedom of industrial and engineering enterprises to enter into research and development projects with interested federal agencies was a traditional and respected practice the overturn of which could only damage the dynamic mechanism that enabled established interests to share in and contribute to the wealth being distributed. If the coordinative management practices the panel recommended were to be followed, that wealth would be distributed in novel, arbitrary, and capricious ways, restricting the freedom of action of business, of universities and of individual government agencies. Such a consequence was as unthinkable as it was insufferable in its implications of the extension of government control; the major recommendations of the panel were not followed.

Aside from its lack of political leverage in attempting to streamline the conduct of research and development where computers were concerned, the panel also ran into trouble when it attempted to evaluate the technical competence of individual projects and ascertain whether they were carrying on research and development operations in an appropriate manner. Instead of consistently taking the true measure of their worth -- a task more approachable in the light of hindsight than of foresight, -- the panel entangled itself in inconsistencies of judgment of which it was not always aware. To conclude therefore that the panel

members, as well as their superior, the Committee on Basic Physical Sciences, which accepted their report, were (to put it bluntly) stupid is to fail utterly to recognize the fundamental problem confronting all those engaged in computer research and development. The problem was--and is--that of entertaining policy judgments about the conduct of research and development affairs that are sufficiently pertinent to the practices being carried on as to provide effective control of those practices.

On the one hand were the policy ideals; on the other hand were the daily, monthly, annual operations. The comparability and correspondence of the one to the other constituted the truly formidable problems to which Forrester and Everett were so sensitive, to which Perry Crawford was so sensitive, to which Luis de Florez and Nat Sage and Gordon Brown were so sensitive, to which Mina Rees and Warren Weaver and John von Neuman were so sensitive, to which Compton of MIT and Solberg of ONR were so sensitive. Such problems, of course, are not restricted to computers but permeate all scientific technology. In the case study here at hand, Project Whirlwind, there was, as has been shown, conspicuous divergence of opinion regarding the proper correspondence that should be maintained between policy and practice, and Forrester's opinions were

sufficiently extreme to cause ONR continuing trouble because of the more orthodox views its programmers held.

The Ad Hoc Panel's struggles to co-estimate policy and practice are significant because they were so typical of the resistances that Project Whirlwind encountered when contemporary experts, handicapped by the fact that the expert is always also the measure of contemporary ignorance, sought to appraise its unorthodox procedures. For example, the panel criticised Project Whirlwind in its earlier report for being without a specific, practical end use. It was frank enough to admit it was unable to find such a use, and said so with such blunt candor that a reader might well infer there really was none, and that the fault here lay principally with the machine and with the conduct of the project.<sup>39</sup>

At the same time, the panel in both versions of the report criticised the general conduct of computer research and development because it did "not include sufficient emphasis on real-time computation."<sup>40</sup> Presumably Whirlwind did not qualify; "we may remark, in passing," said the panel in its first report and in its final report, "that there are at present no real-time electronic digital computers in operation."<sup>41</sup> And they were right; there were no such machines in full operation. Was the panel supposed to be prescient enough to realize that Whirlwind

soon would qualify? In 1949 they found Whirlwind I, "as presently envisioned, ... a very large machine with but five decimal digits capacity, extremely limited memory capacity, and with as yet indefinite plans for input and output equipment." <sup>42</sup> Assuming these views to be the ones the panel developed from its visit to the Laboratory on May 26, 1949, they were sufficiently in error and out of date a year later, in part as a consequence of the progress of the Project, so that they were replaced by more accurate and more optimistic information in the final report: "...somewhat limited in its application due to its short word length (16 binary digits) and limited internal storage (256 words). Progress is being made toward 2048 word storage, however, and certain applications for the machine have been set forth in which the short word length is not a severe handicap." <sup>43</sup>

From the panel's point of view, the fact still remained in 1950 that there were no real-time computers in operation. True, "a large portion of the [MIT] computer has been operating as a system since the fall of 1949," admitted the consultants, and "it is planned that the machine will be available for useful computation in the fall of 1950 ..." <sup>44</sup> Yet following its survey, the

panel could conclude reasonably that these prospects still did not invalidate their judgment that too little effort was being devoted nationally to providing real-time computers. The machine farthest along was the unorthodox, untried Whirlwind, but the panel did not appear to find this very reassuring.

The panel knew that Whirlwind had indeed come far, in one respect: it had found a mission since the Research and Development Board had learned of its predicament in 1949. "Equipment currently is being connected," the consultants were happy to point out in their confidential final report, "for using the machine in real-time air defense research for the Air Force, and plans for the first year of machine use have been crystallized recently."<sup>45</sup>

The striking feature of the Ad Hoc Panel's predicament in assessing the state of the art stands clearly forth when one asks why the panel could not earlier find a practical use for Whirlwind at the same time it was bemoaning the lack of work in the real-time area of computer applications. The answer would seem to be that the panel could not free itself from the conventional attitudes that prevailed. Instead, it pointed out sharply in 1949, as has been remarked, that "the scale of effort on this project is out of all proportion to the effort being expended on other projects having better specified objectives."<sup>46</sup>

Considered years later, such conspicuous lack of vision about the conduct of research and development is readily seen, but the bases of this lack are often times obscured. In this historical instance, the Ad Hoc Panel's honest statements remove enough of the obscurity to make it plain that their policy views were not well adapted to describe or to cope with research and development practices, yet appeared to reflect and appraise the existing situation. In the final report the panel softened its earlier criticism of the way in which Project Whirlwind had operated (and was continuing to operate). Said the panel in June of 1950, after the Air Force and Professor Valley's committee had come to Forrester's timely rescue, "the technical direction of WHIRLWIND seems to have suffered seriously by frequent changes in objective and transfer of the project from one division of the Naval Establishment to another. Although the machine now has a definite objective, which seems to us suitable and of enough importance to justify the scale of expenditure contemplated, the current objectives have only recently been assigned. During much of the time this project has been in existence, the scale of effort has been out of proportion to that being expended on other projects having more definite objectives."<sup>47</sup>

In accommodating its policy judgments to the changing

state of affairs, the panel took inconsistent positions: there was not enough emphasis on real-time applications, there had not been enough emphasis in the past. Whirlwind was, after all, only one such machine, and furthermore it had been a machine without a task. As a machine without a task, it could not merit the lavish research and development support it had received, although now that it had a task, it somehow did merit the support it was receiving.

Was the panel being realistic? Practical? Consider the evidence in the realm of costs. The Raytheon "Hurricane" prototype computer would cost an estimated \$460,000, the IAS computer \$650,000, the Eckert-Mauchly UNIVAC \$400,000 to \$500,000, the National Bureau of Standards Interim Computer \$188,000, the U.C.L.A. Institute for Numerical Analysis "Zephyr" Computer \$170,000, the ONR-supported CALDIC computer at the University of California \$95,000, the famous ENIAC \$600,000 (three years old), its successor EDVAC, \$470,000, ORDVAC at the University of Illinois \$250,000, and the Harvard Mark III (just finished) \$695,000.

The maximum order-of-magnitude of cost appeared to be a half-million to two-thirds of a million dollars. Whirlwind, however, would cost three million dollars, according to current estimates and if all research costs were thrown in since the beginning of the project, another three-quarters of a million dollars should be added on top of that. To say that Project Whirlwind was out of step is to put it mildly.

The fact that Whirlwind was costing as much as Forrester had estimated two or three years earlier he had thought it should, rounding off at about five million dollars over a five or six year span, -- whatever satisfaction this correspondence gave Forrester was irrelevant in the light of the criticisms of "excessive cost."

The policy judgment called for was clear: no computer should cost so much, and all other research and development experience with computers supported this policy view. Whirlwind was way, way out of line. It appeared that Forrester somehow had sucked MIT, and along with MIT, ONR into carrying on research and development at a fantastically inflated scale and cost. Again one is reminded of the heated remark uttered years later by a veteran engineer and project manager, "we are not going to have another Whirlwind!" Seen from this perspective, the criticisms of the Research and Development Board's panel were temperate indeed. No wonder they had urged in 1949 that if no practical end-use could be found shortly, then the Project should be closed out!

Reflecting philosophically on these events a decade and a half later, Mina Rees had remarked of the Project's success in acquiring Air Force funding, "They were lucky." They were lucky in the sense that for one set of reasons Forrester and his associates had designed, developed, and

built a high speed, parallel type digital computer which happened to be becoming operational, even if to a limited extent, at a time when, for a different set of reasons, a serious national need had become evident. Without doubt, the need of the Air Force had converged with the need of Project Whirlwind at a time that was most opportune to both. It is possible that other major uses could have been found for Whirlwind, Forrester's search for applications had been persistent and extensive and had become more intensive as Whirlwind approached operational status. The curious historical feature, nevertheless, is that dedication, persistence, and industry -- political as well as technical -- had produced a device at a time when there became apparent a crisis in the national defense which it might help meet. Project Whirlwind thus became a splendid example in support of the novel and not-well-tested argument that research and development, throughout its whole spectrum, should be supported for its own sake, because the use will always be found. If this was a sound moral to be learned from the Whirlwind experience, the lesson was not subsequently applied. Instead, the story of Project Whirlwind became submerged in the larger drama of Lincoln Laboratory's crash programs, and basic policy judgments about the conduct of research and development on a national scale remained as before.



<sup>1</sup>Ltr., J. W. Forrester to J. B. Pearson, Deputy Director, ONR, Devember 2, 1949; Memorandum, H. R. Boyd to J. W. Forrester, subj.: "Budget Basis," December 15, 1949.

<sup>2</sup>Ltr., R. J. Bergemann, to Head, computer Branch, ONR, subj.: "Project Whirlwind Progress: Comments On," December 22, 1949.

<sup>3</sup>Memorandum, C. H. Doersam, Jr., Synthetic Warfare Section, SDS subj.: "Contract N7 ONR-38902, Project Hurricane, recommendation of termination of," November 1, 1950: Report, C.H. Doersam, Jr., to code 920, subj.: "Trip Report to Raytheon Manufacturing Company on 8 November 1950, "Project Hurricane, C. H. Doersam, Jr." November 20, 1950: Internal Note, Samuel Nooger, Head, Engineering Branch, SDC, N.D. [circa November 21, 1950]

<sup>4</sup>Memorandum, C. V. L. Smith to Code 434, subj.: "Letter of J. W. Forrester, dated 2 December 1949, on the future financing of digit 1 computer work at M.I.T.," January 16, 1950.

<sup>5</sup>R. J. Bergmann, "Record of Visits January 1950-June 1950," January 12, 1950: C. V. L. Smith "Summary of conference on Trip," January 12 & 13, 1950.

<sup>6</sup>Ltr., E. R. Piroe, Deputy for Natural Sciences, ONR, to J.A. Stratton, Provost, Mit, 2 Feb. 1950; ltr., C. V. L. Smith to J. W. Forrester, 2 March 1950: ltr., J. W. Forrester to J. A. Stratton, 3 March 1950.

<sup>7</sup>Interview, J. W. Forrester by the authors, October 26, 1967.

<sup>8</sup>Ltr., Mina Rees to Kent C. Redmond, July 15, 1964.

<sup>9</sup>C. V. L. Smith to George R. Stibitz, December 13, 194 : ltr., Mina Rees to Professor Harold Hazen, MIT, December 28, 1948: ltr., H. L. Hazen to Mina Rees, January 10, 1949: ltr., H. L. Hazen to Mina Rees, January 12, 1949: J. W. Forrester to Mina Rees, January 17, 1949: Mina Rees to Joseph E. Desch, January 31, 1949: "Memorandum of Conversation with Dr. Alan Waterman," March 7, 1949.

<sup>10</sup>Ltr., C. V. L. Smith to J. W. Forrester, August 19, 1949.

<sup>11</sup>J. W. Forrester to C. V. L. Smith, August 26, 1949.

<sup>12</sup>Research and Development Board, Committee on Basic Physical Sciences, "Report of the Ad Hoc Panel on Electronic Digital Computers," PS 13/5, December 1, 1949. (Hereafter referred to as the preliminary report.)

<sup>13</sup>J. W. Forrester, "Comments on the Report of the Ad Hoc Panel on Electronic Digital Computers of the RDB Committee on Basic Physical Sciences," L-17, Servomechanisms Laboratory, MIT, January 13, 1950.

<sup>14</sup>J. W. Forrester, "Information on Whirlwind I as requested by Lt. Cmdr. Rubel, Research and Development Board, in letter dated 7 February 1949, "Memorandum L-11, Servomechanisms Laboratory, MIT, February 15, 1949; J. W. Forrester "Notes for RDB Committee meeting," August 29, 1949; in addition, Memoranda L-3 and L-12, and Air Traffic Summary Report No. 1 were made available to the panel.

<sup>15</sup>J. W. Forrester, "Discussion of the Comments on Project Whirlwind made by the Ad Hoc Panel on Electronic Digital Computers of the Basic Physical Science Committee of the Research and Development Board," Memoranda L-16, Servomechisms Laboratory, MIT, January 13, 1950.

<sup>16</sup>Memoranda, Code 434 to Code 102, signed by A. E. Smith, Acting Head, Computer Branch, ONR, subj.: "Confidential Report of the Ad Hoc Panel on Electronic Digital Computers, RDB, Comments on," 20 December 1949.

<sup>17</sup>Diary Note, A. T. Waterman, subj.: "Whirlwind Conference held at Massachusetts Institute of Technology," March 6, 1950: C. M. [Carl Muckenhoupt], "Record of Visits January-June, 1950," March 6, 1950.

<sup>18</sup>Diary Note, A. T. Waterman, subj.: "Whirlwind Conference held at Massachusetts Institute of Technology," March 6, 1950: Memorandum, C. V. L. Smith, "Summary of Conference on Trip," March 6-7, 1950.

<sup>19</sup>Memorandum, C. V. L. Smith, "Summary on Conference on Trip," March 6-7, 1950.

<sup>20</sup>Memorandum, N. McL. Sage, April 7, 1950.

<sup>21</sup>Ltr., N. McL. Sage to Chief of Naval Research, May 5, 1950; Memorandum D-23, Jay W. Forrester to Head, Computer Branch, Mathematical Science Division, ONR, subj.: "Request for Funds for Contract N5ori-60 for the Period July 1, 1950 through June 30, 1951," May 3, 1950.

<sup>22</sup>Ltr., C. V. L. Smith to N. McL. Sage, June 26, 1950.

<sup>23</sup>Ltr., J. W. Forrester to N. McL. Sage, July 6, 1950; N. McL. Sage to J.W. Forrester, July 11, 1950.

<sup>24</sup>Memorandum for File 6345 by Paul V. Cusick, DIC, MIT, October 6, 1950; Amendment #12, T. O. #1, Contract N5ori-60, November 14, 1950.

<sup>25</sup>Ltr., N. McL. Sage to C. O., ONR, February 23, 1951; interview, J. W. Forrester and R.R. Everett by the authors, October 26, 1967.

<sup>26</sup>Ltr., J. W. Forrester to C. O., ONR, September 25, 1951.

<sup>27</sup>Amendment #13, T. O. #1, Contract N5ori-60, June 28, 1951; Amendment #14, T. O. #1, Contract N5ori-60, June 28, 1951.

<sup>28</sup>Amendment #16, T. O. #1, Contract N5ori-60, 26 March 1952; Amendment #19, T. O. #1, Contract N5ori-60, 18 April 1953.

<sup>29</sup>Interview, J. W. Forrester by the authors, October 26, 1967.

<sup>30</sup>Research and Development Board, "Report on Electronic Digital Computers by the Consultants to the Chairman of the Research and Development Board, PS 13/8, June 15, 1950.

<sup>31</sup>J. W. Forrester, "Statement of Status of Project Whirlwind Prepared for the Research and Development Board," L-24, Servomechanisms Laboratory, MIT, May 10, 1950.

<sup>32</sup>"Report on Electronic Digital Computers by the Consultants to the Chairman of the Research and Development Board," June 15, 1950, p. vi. (Hereafter referred to as the final report.)

<sup>33</sup>Ibid., pp. vi-vii.

<sup>34</sup>Ibid., p. viii.

<sup>35</sup>Cf. pp. 9 and 9 respt., of the preliminary (December 1, 1949) and final (June 15, 1950) versions of the report.

<sup>36</sup>Cf. pp. 44 and 45, respt., of the preliminary and final versions of the report.

<sup>37</sup>Cf. pp. 8 and 23 of the preliminary report with pp. 9 and 22, respt., of the final report.

<sup>38</sup>Cf. pp. 9 and 9 respt., of the preliminary report and the final report.

<sup>39</sup>Cf. pp. 11, 39 in the preliminary report, December 1, 1949.

<sup>40</sup>Cf. pp. 34 and 33 respt., in the preliminary report and the final report.

<sup>41</sup>Cf. pp. 15 and 13, respt., in the preliminary report and the final report.

<sup>42</sup>Preliminary report, p. 53.

<sup>43</sup>Final report, p. 54.

<sup>44</sup>Ibid.

<sup>45</sup>Ibid.

<sup>46</sup>Preliminary report, p. 39.

<sup>47</sup>Final report, p. 40.

## Chapter Ten ADSEC AND WHIRLWIND

The Air Force phase--the "Valley Committee," Project Lincoln, and SAGE--was the beginning of the triumphant end for Project Whirlwind, for its greatest successes and the vindication of Forrester's and Everett's research and development policies occurred during this period. None of the principals was in a position to perceive in 1950 that the end was coming into sight, of course, even though the computer had found a specific mission, nor did they realize that it would take the form of a subtle transformation of the Project as a consequence of gradual alteration of the R & D climate in which the Project originally had achieved its identity and begun to flourish. This transformation of the Project which occurred as it adapted to the changing R & D climate and began to live with its own successes produced at the same time an assurance and a sophisticated maturity of operation, and intimations that the frontier in which the Project had come to life was passing on and that a more settled way of life was approaching, even while the Project continued to busy itself with the technical innovations which were its prime concern. Several years were to pass before Forrester himself became convinced that the settled way of life which loomed was not for him, and it was not until 1956 that he set out again, to find the frontier.

That the Air Force should have become involved in 1950 in determining Project Whirlwind's ultimate

fate, rather than the Navy, was the circumstance of a current of events generated quite independently from those that had brought Whirlwind into existence. These events are worth looking at because they created the conditions in which Whirlwind was to succeed, because they were not anticipated by the Navy and the MIT managers who were bringing Whirlwind along, and because they were central in setting in motion the changes in the R & D climate which were subsequently to transform the Project.

In one sense, although not a particularly profound one, the antecedent events began to take form when war in the air became a practical, prospective possibility after the Wright brothers ushered in the age of heavier-than-air flight at Kitty Hawk, North Carolina, on December 17, 1903. But it was not until the Second World War that the technology of aerial warfare became sufficiently advanced to pose the threat of aerial attack upon the United States by foreign, land-based bombers. Consequently, the American people, their government and military leaders, sustained by a confidence derived from geographical isolation and continental dominance, had not during the years preceding the War seen any vital need to plan, research, and build an integrated continental air defense system.

In the immediate post-war days, the American people, basking in the warmth and fellowship of the Allied victory and the birth of the United Nations and residing in a homeland unscathed by the War, did not foresee the dangers inherent within the changed international balance of power. Increasing difficulties with Russia, however, coupled with the advances made in military technology during World War II, aroused Americans from the euphoria of victory and compelled them to give serious consideration to the nation's vulnerability to attack from the air in the new age of long-range bombers, missiles, and atomic warheads.

This growing awareness was reflected by the United States Air Force in December of 1947 when General Hoyt S. Vandenberg, Vice Chief of Staff, in a letter to Dr. Vannevar Bush, Chairman of the Research and Development Board, expressed anxiety over the lack of an adequate national air defense system. The Vice Chief of Staff discussed a projected plan which, it was estimated, by using obsolescent radar of World War II design would give the nation by the end of 1952 some degree of increased protection, pending modernization with post-war developed and manufactured equipment. It was this last, however, which seriously worried General Vandenberg, for he was of the belief that the existing state of electronics research and development in air defense and guided missiles would not produce advanced designs available for production until after 1953.<sup>1</sup> This was particularly critical, since the mid-fifties were estimated to be a period of especial danger for the nation.<sup>2</sup> With these fears in mind, Vandenberg put three questions to Dr. Bush: Were new developments coming along at the best rate possible? Was the Army-Navy-Air Force program properly balanced? Were there serious technical deficiencies in the military programs which required immediate correction?<sup>3</sup>

Reacting to the seriousness of the problems raised by the General, Dr. Bush asked the "Subpanel on Early Warning" of the "Radar Panel" of the "Committee on Electronics" of the "Research and Development Board" to investigate the existing state of national air defense and to prepare a careful analysis of the system or systems-in-being and contemporary programs seeking improvement in the national air defense posture. The Subpanel concluded that existing research and development programs were ample, but insufficient

funding and difficulties caused by the two-year fiscal restrictions imposed by the Constitution on military appropriations made impossible maximum progress in the implementation of long range plans and programs. An improved air defense system could be made available even earlier than the Vice Chief of Staff had predicted, the Subpanel opined, provided existing research and development programs were given immediate emphasis and accelerated by increased funding.<sup>4</sup> Otherwise, a new system would still be years off.

The existing system was inadequate and decentralized. It comprised separate defense areas, each with its own radar and each responsible for locating, identifying, and intercepting hostile aircraft that had penetrated the area. The radar was automatic, but accumulation, analysis, and interpretation of the data received, whether from radar or other sources, were performed by men; hence the defense of any single area was dependent upon the speed, competence, and efficiency of the forces responsible for it, and these were not unlimited.<sup>5</sup>

Weaknesses within the system were numerous. The radar could easily be saturated. There were communication difficulties between machines and operators. Serious gaps existed in low-altitude coverage, and the employment of smaller radars to fill such gaps was not considered feasible, since additional data would impose a heavier burden upon already overtaxed control centers. Voice-radio communications between stations and control centers were not reliable. Along the Eastern and Western sea frontiers, arrangements for early warning of approaching aircraft were inadequate. The primary weakness, however, was the limited ability of the system to process, organize, and use the information it gathered.<sup>6</sup>

Air Force interest in the condition of the nation's air defense system was boosted in March of 1948 when the Joint Chiefs of Staff gave it primary responsibility for defense against air attack. This assignment of primary responsibility, coupled with the Air Force's definition of air defense as "all measures designed to nullify or reduce the effectiveness of the attack of hostile aircraft or guided missiles" after they had become airborne, left the Air Force with almost exclusive responsibility for continental air defense. The only exception, anti-aircraft artillery, continued a functional assignment of the United States Army.<sup>7</sup>

In September of 1949, Americans heard the alarming news that Soviet Russia had successfully detonated an atomic bomb the preceding month. This broke the American monopoly several years earlier than had been anticipated. Shortly thereafter, intelligence sources suggested that Russia possessed sufficient air carriers to penetrate existing American air defenses, and might possibly within the immediate future match the United States in both the number and size of nuclear weapons and the quality and quantity of jet bombers. For informed Americans this increased Russian military power in the midst of Cold War tensions transformed a threat into a clear and present danger and compelled the Air Force to reappraise the state of the nation's air defenses. The reappraisal was carried out by several study groups, each of which analyzed the existing systems and equipment with a view to determining their weaknesses and recommending corrective measures. Nor was the Air Force alone in its fears, the Department of Defense, reflecting similar apprehensions, also established a study group under the aegis of the Weapon Systems Evaluation

Group. A new sense of urgency regarding air defense was spreading throughout the federal military establishment.

Two ad hoc programs were mounted by the Air Force. One, conducted under contract by the Bell Telephone Laboratories, was concerned with possible immediate improvements of the existing system and its equipment.<sup>8</sup> The other studied possible new approaches and new systems. It came into being as the result of actions generated simultaneously in the military and in the scientific communities. General Vandenberg was the military proponent and Professor George E. Valley of the Massachusetts Institute of Technology was the scientific proponent.

As Air Force Chief of Staff, General Vandenberg became increasingly apprehensive after the Russian atomic success of August, and he urged upon his colleagues the desperate need to act immediately to develop and construct a continental air defense system that could cope with the offensive potential of jet bombers and missiles. The Vice Chief of Staff, General Muir S. Fairchild, relayed these fears to a civilian consultative body, the Air Force Scientific Advisory Board (SAB), and added his superior's request that the Board undertake "a continuous study of the technical aspects of the air defense of the Continental United States."<sup>9</sup>

Concurrently, Professor Valley, a physicist and a member of the SAB, was in the latter capacity involved as both observer and participant in testing and discussing the existing national defense system. Disturbed by the confusion and poor understanding of the problem that appeared to him to exist, Valley proposed in November, 1948, that the Board create an "Air Defense Committee" to conduct a tech-

nical investigation of the existing system and to determine "the best solution of the problem of Air Defense."<sup>10</sup>

In response to Vandenberg's and Valley's urging, the Air Defense System Engineering Committee (ADSEC) came into existence in order to provide a mechanism for examining the apprehensions of military and scientific planners, assessing the vulnerability of the nation to aerial attack and proposing appropriate solutions. The chairman of ADSEC was Professor Valley, whence the frequent informal references to ADSEC as the "Valley Committee." Its members included George Comstock, Allen F. Donovan, Charles S. Draper, Henry G. Houghton, John Marchetti, and H. Guyford Stever.

All of the Committee's members save two--George Comstock and John Marchetti--were members of the SAB and all came from the northeast section of the nation. The geographical concentration was deliberate, intended to permit easy consultation and recourse to the facilities of the Air Materiel Command and the Continental Air Command at the Air Force Cambridge Research Laboratories in Cambridge, Massachusetts. Valley had recommended this arrangement on the assumptions that the members of the committee would be able to serve only on a part-time basis, that solution of the problem would be difficult and long, and that experiments which would require the use of radar and aircraft would be conducted. Moreover, he thought, conditions on the West Coast might be sufficiently different to require a special investigatory group for that region.<sup>11</sup>

Events following the creation of ADSEC proceeded on at least two levels significant to the story of

Project Whirlwind: one was the level of long-range, institutional policy development; the other was the level of unfolding scientific, technical, and military insights into the practices that might be pursued to achieve a working defense system which would constitute a defensive force-in-being. Against aerial attack over the North Pole no standing army, no navy at battle stations could provide the needed force-in-being. Aerial attackers would have to be detected in the air and defeated in the air before they released the bombs and rockets they carried. The first problem thus became one of scientific technology, even before appropriate military and naval operations could be instituted. ADSEC indeed had its work cut out for it, nor was there sure warrant in advance that ADSEC or any other responsible agency could effect a practical solution before time ran out.

Before further developments on this scientific and technological level of events are pursued, a glance at major developments on the institutional policy level during the early 1950's, following the creation of ADSEC, may provide a longer-range perspective and a useful framework in which to examine the circumstances that shortly swept up Project Whirlwind. Whirlwind became involved in these affairs early in 1950 and subsequently became an operation so wholly transformed that even to its leader it changed its original identity as a research-and development enterprise before the job was done. Both the transformation and the consequences were profound, and the policy-level events responsible, following the formation of ADSEC, occurred in one-two order. First, they took the form of the independent researches conducted by ADSEC and by the committee operating under the aegis of the Weapon Systems Evaluation Group. Second, they took the form of the conclusions these two bodies reached.

Their conclusions reinforced each other and confirmed Air Force fears about the inadequacy of the nation's air defenses. In extremely strong language, the SAB subcommittee compared the existing system "to an animal that was at once 'lame, purblind, and idiot-like,'" insisting that "of these comparatives, idiotic is the strongest. It makes little sense for us to strengthen the muscles if there is no brain; and given a brain, it needs good eyesight."<sup>12</sup> Translated into technical language, this meant that an adequate air defense system needed not just improved interceptors, ground-to-air missiles, and antiaircraft artillery, but improved radar and advanced command-and control centers.

The cumulative impact of the two studies and their separate but reinforcing conclusions produced a request from the Air Force in December of 1950 to the Massachusetts Institute of Technology, asking the Institute to create a laboratory which would dedicate itself exclusively to research and development pointed to the design and construction of an adequate national air defense system. Early in the next month, the SAB added its voice to the Air Force request and in addition asked the Institute to undertake a more intensive study of the technical problems connected with continental air defense. From this latter request evolved Project Charles, an investigation conducted between February and August of 1951<sup>13</sup>

Upon the approval of its board of governors, the Institute acceded to the request of the Air Force and established Project Lincoln, subsequently better known as Lincoln Laboratory. Physical facilities to house the Laboratory were shortly constructed at Hanscom Air Force Base in the nearby Bedford-Lexington region. The Laboratory's immediate task was to imple-

ment the defense concepts formulated by the Air Defense Systems Engineering Committee and Project Charles. This immediate task plus its own researches led, in the months and years that followed, to the Lincoln Transition System and then to the Semiautomatic Ground Environment (SAGE) system, a total continental air defense system which was at once "a real-time control system, a real-time communication system, and a real-time management system," using "digital computing systems to process nation-wide air-defense data."<sup>14</sup>

Project Whirlwind--to return to the technological level of events--had become involved with ADSEC immediately after the formation of the committee. Ferry Crawford, visiting Project Whirlwind on January 20, 1950, had informed Jay Forrester of the creation of the committee.<sup>15</sup> Within a week there occurred a coincidental chance meeting on the MIT campus of two of its faculty members, Dr. Valley and Dr. Jerome B. Wiesner.<sup>16</sup> As Valley recalled it years later, Wiesner asked him conversationally how things were going, one remark led to another, and soon he was indicating to Wiesner his need for an information-gathering and information-correlating center that could organize great numbers of diverse pieces of information with extreme rapidity. Wiesner suggested he take a look at Jay Forrester's operation to see if it would have anything of value to offer. Valley followed up this lead and found a machine so promising that he and his fellow ADSEC members seriously investigated the prospects and decided to support a test harnessing of Whirlwind in order to determine whether geographical information received by radar scouting stations could be transformed into tactical information and directions that would enable a fighter to inter-

cept a bomber long before the latter reached its target.

For Forrester, the involvement of Whirlwind in ADSEC affairs came to pass within a few days after Crawford told him the committee had been formed. On Friday, January 27, he was having lunch with Professor Wiesner. At the lunch table, as Forrester wrote in his notebook later, with customary attention to recording developing events, "we were joined by George Valley to discuss his committee work on Air Defense System Engineering."<sup>17</sup> As yet, Forrester had no special reason to believe that decisions of particular consequence would follow. This was but another of several "leads" he was pursuing as part of his probing operation to find a use for Whirlwind and incidentally either relieve or justify (or both) his heavy dependence on ONR for funds.

Wiesner's role appears to have been that of the unobtrusive broker who, having been deliberately instrumental in setting events in operation, fades gracefully into the background and equally deliberately passes on to the principals involved the responsibility of carrying affairs forward. During the course of their lunch, Valley explained the purpose of his committee, outlined the weaknesses of the existing air defense and early warning system, and discussed "his plans for a research project involving the Cambridge Field Station, Draper's group, and possibly the Digital Computer Laboratory." After lunch, he accompanied Forrester to the laboratory. There, after observing Whirlwind I operating with test storage, the two men discussed the possibility of the computer project's participation in the work of the Valley Committee. Valley upon this occasion expressed his intention to pursue the matter

further. Forrester gave him copies of the L-Reports, L-1 and L-2, that he and Everett had written more than two years earlier, in the early autumn of 1947, detailing how computers might be employed to handle interception problems in antisubmarine warfare.

They arranged to get together the following week with certain of Valley's defense-work associates, and Valley spoke of using this situation to help him prepare the inevitable proposal to Washington. "He seemed quite interested in possible use of Whirlwind I for analyzing data from a chain of doppler radar stations which would give range rate only," Forrester noted at the time.<sup>18</sup>

Three days later, on Monday, January 30, Valley returned to the laboratory, bringing with him three other members of the committee, John Marchetti of the Air Force Cambridge Research Laboratory, H. Guyford Stever, and Charles S. Draper, both of the Institute's aeronautical engineering faculty.<sup>19</sup> Accompanying the group was Eugene Grant, a colleague of Marchetti but not a member of the committee. The visitors reviewed some of the material Forrester and Valley had discussed at their earlier meeting, observed the computer and the "storage tube television display," and discussed the use of Whirlwind I in some of the "trial systems" the committee was considering.

The trend of the discussion revealed that Valley's initial interest, which had been heightened by his reading of the L-Reports and had brought him back to the laboratory, was shared by his colleagues on this occasion. To Forrester, his visitors seemed "very enthusiastic about the prospect," and this opinion was given substance by the group's discussion of the question of funding. "Valley and the other men," wrote Forrester that day, "seemed well aware of the fact

that they would be called upon to share some of the basic \$600,000 a year budget for the laboratory, plus additional charges for special work on their own project. . . . The Committee is apparently meeting in Washington in the next few days to crystallize the matter further."<sup>20</sup>

Forrester's observation was quite accurate. ADSEC became committed almost immediately to the use of Whirlwind I, as was evidenced by its meeting of February 17, which was attended by Forrester, Fahnestock, and Everett. The discussion on this occasion focused on the possibility of attempting some trial interceptions, using the Bedford MEW radar to track the aircraft involved and feed the data to Whirlwind I to process. The committee's enthusiasm was such as to induce Forrester to enter in his record of the meeting, the cautionary observation that "we shall have to be careful not to be driven into a situation demanding more than we can deliver."<sup>21</sup>

Of greater significance to Forrester and his colleagues was the information Valley had given Forrester just a few days before the meeting. When informing Forrester that formal approval for ADSEC to go ahead with its work was anticipated by March 1, Valley had confidentially disclosed that the Air Force, if required, might assume the entire cost of keeping the project going.<sup>22</sup>

As a consequence of this groundwork laid early in 1950, Valley was in a position formally to offer the dollar support that neither ONR's nor MIT's top management considered feasible to continue longer to provide under the long-standing relationship that had begun with de Florez' office and since passed through many Navy hands. The apparent withdrawal of

a measure of MIT support from Forrester on this occasion, as it may have appeared from ONR's vantage point, was tempered by the awareness in Stratton's office of what Valley was doing in response to his own feeling of concern, as well as that of various offices in the Pentagon and other Executive Branch agencies, regarding the unsolved problems of continental defense, including temporary "quick fix" proposals. Project Whirlwind now would have an opportunity to prove itself and relieve itself of the sort of onus cast upon it by the KDB Ad Hoc Panel's preliminary report.

Forrester was able to go into the March meetings with ONR with reasonable assurance of Air Force financial support and, moreover, with the confidence that at long last, he was to be given the opportunity to demonstrate the concept he and Perry Crawford had consistently advanced: the usefulness of the high speed digital computer to a command-and-control center.

Actually, the ease with which Project Whirlwind became incorporated into the program of the Valley Committee was understandable and logical. In several different ways, the Project was uniquely qualified to serve the committee's needs. It was conveniently located geographically. It had brought to the edge of operational status a high speed digital computer which was not only appropriate but even essential to some of the tests the committee envisaged. It possessed a pool of scientists and engineers trained and experienced in digital-computer research and development. Lastly, its long-standing commitment to practical problems, which had caused Warren Weaver in February, 1947, to ask probingly whether it was trying to produce "biscuits" or "cake," which had impelled Forrester and Everett several months later to write those first L-Reports, on naval warfare, and

which later had involved the Project in investigations for the Air Force into air traffic control had given it a unique expertise. In naval warfare problems and in the "application of digital computers to the long-term Common System of military and civil air traffic control," lay applications "in many ways similar to air defense."<sup>23</sup> The problem common to all was the processing and organizing of information to provide "the ability to see a complex situation as a whole." Possessing computation speeds immeasurably greater than man's, the computer could "scan the component pieces of information which make up the system so rapidly that it is able to approximate a continuous grasp of the whole situation."<sup>24</sup> Therein lay its value to naval and air defense as well as to air traffic control, for these represented the obverse and reverse of the same problem.

Of greater significance and importance to the air-defense phase of Project Whirlwind was the recognition gained from the air traffic control study that the approach to such problems had to be systems-oriented rather than component-oriented. When in March of 1949, Project Whirlwind had contracted with the Air Force to undertake the air traffic control study, "there was little or no background in the use of a digital computer as the control element of a physical system and very few people . . . experienced in this sort of work." The investigators were working in virgin territory, and as they gained experience and understanding, it became more and more evident that the application of the digital computer to air traffic control was "as much a systems problem as a computational problem." Totally "new concepts of the whole system" were required if maximum results were to be derived.

from using the computer.<sup>25</sup> The insights into systems engineering gained from the air traffic control study were of great benefit to the engineers of Project Whirlwind who later, as a part of Lincoln Laboratory were to contribute vitally to the development of the SAGE air defense system.

<sup>1</sup> Ltr., Gen. Hoyt S. Vandenberg, VCS, USAF, to Dr. Vannevar Bush, Chairman, RDB, December 9, 1947.

<sup>2</sup> Memorandum K-1810, A. P. Kromer, subj.: "Minutes of Joint MIT - IBM Conference, Held at Hartford, Connecticut, January 20, 1953," January 26, 1953.

<sup>3</sup> Ltr., Gen. Hoyt S. Vandenberg, VCS, USAF, to Dr. Vannevar Bush, Chairman, RDB, December 9, 1947.

<sup>4</sup> Ltr., W. L. Barrow, Chairman, Panel on Radar, Committee on Electronics, RDB, to Norman L. Winter, Executive Director, Committee on Electronics, RDB, January 5, 1948.

<sup>5</sup> History of the Air Force Cambridge Research Center, 1 July - 31 December, 1953, Vol. XIX, part 1, pp. 247-252.

<sup>6</sup> La Verne E. Woods, "The Lincoln System," ADC Communications and Electronics Digest, IV (January, 1954) pp. 4-11, cited ibid., pp. 249-250.

<sup>7</sup> C. L. Grant, The Development of Continental Air Defense to 1 September 1954, USAF Historical Studies: No. 126, pp. 14-18.

<sup>8</sup> Operational Plan, Semiautomatic Ground Environment System for Air Defense, HEDADC, March 7, 1955, p. v.

<sup>9</sup> Memo for DCS/C, DCS/P, DCS/O, and DCS/M, HEDUSA from General Muir S. Fairchild, VCS, USAF, subj.: "Air Defense Technical Committee of the Scientific Advisory Board," December 15, 1949, cited in History of Air Force Cambridge Research Center, Vol. XV, Part 1, Appendix 11.

<sup>10</sup> Ltr., G. E. Valley to Dr. Theodore Von Karman, November 8, 1949.

<sup>11</sup> Ibid.; ltr., General Muir S. Fairchild to Commanding General, Air Materiel Command, subj.: "Air Defense System Engineering Committee, Scientific Advisory Board to the Chief of Staff, U.S. Air Force," January 27, 1950.

<sup>12</sup> ADSEC Report, "Air Defense System," October 24, 1950

<sup>13</sup> Ltr., General Hoyt S. Vandenberg, COFS HEDUSA to Dr. James R. Killian, President, MIT, January 19, 1951.

14 R. R. Everett, C. A. Zraket, & H. D. Benington, "Sage - A Data Processing System for Air Defense," reprint from Proceedings of the Eastern Joint Computer Conference, Washington, D. C., December, 1957, p. 148.

15 J. W. Forrester, Computation Book No. 49, p. 77 entry for January 20, 1950.

16 Interview, Professor George E. Valley by the authors.

17 J. W. Forrester, Computation Book No. 49, p. 83, entry for January 27, 1950.

18 Ibid.

19 Dr. Stever was at that time an associate professor of aeronautical engineering and a member of the USAF Scientific Advisory Board. Dr. Draper was director of the MIT Instrumentation Laboratory and in 1951 became head of the Department of Aeronautics.

20 J. W. Forrester, Computation Book No. 49, p. 84, entry for January 30, 1950.

21 J. W. Forrester, Computation Book No. 49, p. 88, entry for February 21, 1950.

22 J. W. Forrester, Computation Book No. 49, p. 86, entry for February 15, 1950.

23 C. R. Wieser, in Lincoln Laboratory Quarterly Progress Report, Division 6--Digital Computer, June 1, 1952, pp. 6-9.

24 C. R. Wieser, "Digital Computers in Control System," Report R-181, Servomechanisms Laboratory, MIT, April 27, 1950.

25 C. R. Wieser, in Lincoln Laboratory Quarterly Progress Report, Division 6--Digital Computer, June 1, 1952, pp. 6-9.

## Chapter Eleven

### INTERNAL STORAGE PROBLEMS

When ADSEC had come on the scene during the winter of 1949-50, the engineers and technicians in Project Whirlwind were involved in fabrication, assembly, and testing operations that kept them too busy to draw any conclusions other than the pragmatic day-to-day and week-to-week conclusions that genuine progress (as well as apparent physical progress) was being made in all areas--with the possible exception of the electrostatic storage tubes, and even the latest designs of these were performing impressively. Forrester found it entirely reasonable to assert a decade later that World War II electronic-circuit development experience had left a fairly straightforward prospect for computer design, but that satisfactory internal-storage elements had posed an entirely different problem.<sup>1</sup> The mercury delay line, as well as the rotating magnetic drum developed by Electronics Research Associates in St. Paul, were both too slow for Whirlwind's real-time response needs dictated originally by the aircraft simulator and then held to by Forrester and his associates in the knowledge that it was an essential feature of the sort of general-purpose machine they visualized. From 1947 to 1950, one could argue, they and Perry Crawford appeared to be about the only ones who could see actual, attainable applications for such a computer; yet this gave them no cause to depart from their conviction. To others in the growing computer business, their

stubborn conviction was part of the irregularity their project exhibited, part of the youthful and immature enthusiasm that "tainted" them.

Their early analysis of "moving-target indicator" tube designs generated out of the British invention of radar led them to conclude that this type of storage system, which initially looked promising, suffered from limitations that became prohibitive when examined in greater detail. This was the Williams storage tube, in its basic concept, and when Forrester considered its prospects for further development and refinement as a digital-computer element, he concluded, rightly or wrongly, that it "inherently lacked the high signal levels, the high signal-to-noise ratio, the ability to give good signals from the noise, that we would require for our high-reliability application . . ." In consequence, recalled Forrester, "we did not stay with the Williams tube idea for very long . . ."<sup>2</sup>

In this state of the art of the key storage elements of the computer, Forrester and his associates had early come to recognize the "serious disadvantages" of any device, including the MIT Radiation Laboratory tube, similar in some respects to the Williams tube, that the Project finally settled upon for further development work. Forrester's chariness from the start to state when Whirlwind would incorporate reliable storage tubes indicates how serious he felt the contemporary lack of a high-speed, reliable, internal-storage element to be, with respect to its effect upon the reliability and speed of operation of the entire computer.

In 1947 Forrester even had briefly considered using interconnected "gas glow discharge cells" in three-dimensional array, for these offered such advantages, at least theoretically, as "high initial breakdown voltage," "... low holding voltage, and low forward impedance after breakdown . . . ." But investigations were "soon dropped because of the variability of the individual cells with time and from sample to sample."<sup>3</sup>

Once Forrester had committed his project to electrostatic storage tube research and development, he publicly buried his earlier misgivings in order to get on with the specific course of action that to him appeared to be least impractical so far as an internal-storage device was concerned. His experience and instincts in design continued to keep him apprehensive and sensitive, apparently, to the complexion of research events that followed during 1947, 1948, and 1949. During this period, as the electrostatic tubes began to be developed, new problems continually were emerging, were being overcome, and were being replaced by other problems. Forrester, and later Everett, closely watched the design progress from the earliest research-demonstration tube forms, through versions in 1948 that Forrester later called "experimental research tubes," to a more compact form of late 1948-early 1949, which possessed a shorter beam-throw from the writing and reading electron "gun" and from the holding "gun" to the storage surface. The first gun was "an ordinary type cathode-ray gun." The second operated at a low voltage to furnish "a uniform flood of electrons

over the entire surface," thereby keeping the different charges on the mosaic points, or "spots," of the storage surface from fading away. These charges, depending on their value, were the "bits" of alternative, binary-code "information" the tube was expected to store, relinquish, or alter on demand.<sup>4</sup> Access to a tube's information (including obtaining a "read-out" of a single charge, or bit,) should have been six microseconds, but ten to 25 seconds was the performance of the tubes that were available early in 1949.<sup>5</sup>

The laboratory work was proceeding forward perhaps about as well as might be expected, but within Forrester's own thoughts, out of sight and knowledge of his Project Staff, except for perhaps Everett, was disquiet and a continuing watchfulness. The electrostatic storage tube was analogous to a patient making reasonable--or perhaps not quite reasonable--progress, yet by no means out of danger of a relapse.

Forrester had early recognized that rapid access time must accompany a vast storage capacity, hence he saw the advantages of the geometry of arrangement suggested by three-dimensional coincident excitation storage, such as the gas glow discharge cells had seemed at one time to offer. When, in the spring of 1949, he saw an advertisement announcing the industrial availability of a magnetic material "Deltamax," there recurred to him the possibility of reviving his three-dimensional, coincident-current system, employing reversibly magnetizable intersections.<sup>6</sup> In June he began to make entries in his computation book that indicate he was at work in the lab again.

"Shortly before the Air Force came into the picture, from where we sat," recalled a former graduate student in the Laboratory, "Jay took a bunch of stuff and went off in a corner of the lab by himself. Nobody knew what he was doing, and he showed no inclination to tell us. All we knew was that for about five or six months or so, he was spending a lot of time off by himself working on something. The first inkling I got was when he 'came out of retirement' and put Bill Papian to work on his little metallic and ceramic doughnuts. That was in the fall and winter of 1949-1950."

Forrester at the end of June, 1949, had begun to test rings, or cores, of the Deltamax magnetic material he had seen advertised by a subsidiary of the Allegheny Ludlum Company, the Arnold Engineering Company.

He could see advantages in certain theoretical prospects. The question was, would the magnetic materials behave every time the way they should in principle, or, like the electrostatic tube, would they prove delicate, operable at a peak (and essential) level of performance for only a relatively brief time, and difficult to produce and maintain at the quality and reliability levels required? In principle, a magnetic core should be capable of holding either of two electromagnetized states and should require sharp differences of energy to change from one state to the other. This property, describable in more technical terms as a rectangular hysteresis-loop effect, would provide the binary "bits"--the "yes" and "no" information--required. Such information could be tapped ("read")

or altered ("written"), depending on the kinds and strengths of pulses fed to the magnetic core.

Everett recollects it was early in the summer of 1949 that Forrester had brought the possibilities to his attention in the form of a plan for a planar array of cores. Shortly, he showed Everett a three-dimensional plan, and by early August he had satisfied himself that tiny cores an inch or less in diameter ought to perform well if composed of suitable magnetic materials; the hunt for proper materials and sizes had begun.<sup>7</sup>

On August 1, Forrester talked over the phone with Dr. Eberhard Both, a specialist in the subject working at Fort Monmouth, New Jersey, about the composition of Deltamax and other materials, and during August and September he was carrying on conversations regarding his need with Allegheny Ludlum and its subsidiary, Arnold Engineering. In the fall, Forrester selected William N. Papian, a graduate student looking for a thesis topic in the Laboratory, to go to work testing individual cores by the dozen in order to ascertain ranges of performance and to pick out cores exhibiting exceptionally good properties.

To someone visiting the Laboratory it might have appeared that young Papian was engaged in a routine chore of sorting and testing the tiny rings, but both Papian and Forrester regarded it as a development project committed to converting an attractive theoretical principle to reliable practice. "He set me to work

developing magnetic-core storage," recalled Papian, "and he treated me in that way anybody in the lab would recognize, supporting and guiding the effort to success with very little detailed nagging."<sup>8</sup> During October new cores received from Allegheny Ludlum proved to be some three hundred times faster in their switching interval from one state to the other: down from ten thousand microseconds to about 30 microseconds. Electrostatic tubes were still faster, however.

Following his practice of keeping in touch with the work the Laboratory engineers were carrying on and carrying out his specified responsibility for the graduate assistants as they pursued their thesis investigations, Everett made it a policy to get around the Laboratory every couple of weeks, stopping to go over and discuss the work in progress. Thus, he was aware both of the details of Papian's work and of the status of the electrostatic storage tube program. In response to a request from Forrester, he prepared a cost estimate early in January 1950 on the storage tubes, and Hugh Boyd followed this with another in mid-February.<sup>9</sup>

In the summer of 1949 electrostatic storage tube research and development had not yet reached the stage at which assemblies were being built to be incorporated into Whirlwind I. There was no question that an array of tubes could be built and installed into the computer as its automatic, internal "memory," but reliability problems and opportunities for improvement continued to present themselves.

Meanwhile, the other elements of the giant computer, rack on rack and row after row, were being steadily built up, tested, interconnected, tested, further interconnected and tested again, and it would not be long until Whirlwind I would be in existence as a computer without any storage facilities more ample than the small, relatively static, hand-test storage designed to check the operation of portions of the machine and the steps in preliminary operations, the success of which would render the machine ready for larger and faster internal storage.

Since Forrester had no intention of allowing Whirlwind to become a computer with a "white-elephant" reputation, lacking fast and ample storage, he took care to assess and reassess his situation. But his own and Papian's investigations showed that the attractive promise of the cores could not be realized in time to meet the needs of an otherwise operational Whirlwind. Even before this information was well in hand he had told ONR in his fall Summary Report that "the next quarter will be used to construct the first set of tubes for WWI."<sup>10</sup> His sense of engineering prudence caused him to qualify this bold assertion, however: "the electrostatic storage system will not be connected to the rest of the computer until it has been demonstrated that trouble-free operation can be expected.... Reliability runs of the complete storage row," he concluded, "are not expected until February 1950."<sup>11</sup>

Boyd's February cost analysis of proposed "standard, 100-series" electrostatic tubes, each storing 256 bits of information in a

two-dimensional 16 x 16 array, indicated a high cost of approximately \$1500 per tube and a low rate of production of "one and one-half satisfactory tubes per week."<sup>12</sup> The cost was indeed high, but it was the best that could be done, and under the circumstances must be accepted, even though the cost (including all tubes) per tube per week to the Laboratory approached \$2250.

Necessity alone is never the mother of invention, yet the relatively marginal viability of the electrostatic tube as a research and development product was clear to Forrester. He sought to put the best face on the situation he could in reports he sent to MIT Provost Julius A. Stratton's office in March of 1950, in September, and in the following January, 1951. In none of these formal letters did he inject a word about the magnetic-core research then being carried forward. He was not making a secret of this research; on the contrary, he was keeping Valley and others on the MIT staff informed in an informal manner as 1950 wore on. He simply was unwilling to speak of his core-storage concept in the same breath with descriptions of the nearly-operable, newly-operable Whirlwind I computer.

Three days before the fateful meeting with ONR representatives at which Valley stepped in formally to offer new federal funds, Forrester wrote Stratton's office that "we expect to have the first bank of storage tubes operating before the first of July...."<sup>13</sup> He offered this estimate in the context of remarks opining that "we are probably being much too modest in our claims for the machine."

Certain minimum terminal equipment, together with the first bank of tubes, would give by the end of summer "a computer even more complete and flexible (with respect to the entire range of possible computer applications) than any other computer either now in existence or with prospect of materializing within the next two or three years."

He was indeed putting the best face on the situation, perhaps too enthusiastically, for he went on to say, "It will be better for some applications than others. It will be the only computer which can be applied to many important problems (because of its speed). In a few categories of applications it may not be (in its initial form) as useful as some other machines. The machine at that time should no longer be the limitation on advancement of digital computer utilization; the shortage of enough trained personnel will become the bottleneck."<sup>14</sup>

These were brave (and prophetic) words, drafted in full awareness of the impending meeting with ONR over funding problems--the meeting at which Valley stepped forward--, but they were no solution to the internal-storage problem. Forrester himself admitted that one bank of 256-digit storage tubes would provide only one-eighth of the ultimate storage capacity he then had in mind. Unwilling to leave his terms "minimum" and "ultimate" undefined, he pointed up his meaning in typical personal style. Were a stranger to ask, "Can you do my computing job?" wrote Forrester, the answer appropriate to a minimum computer's capacities must be, "Probably, but we

must analyze it to find out." But if one possessed the ultimate system Forrester had in mind, then the answer to that question "can fairly safely be, 'Yes, what is it?'"<sup>15</sup>

But the storage tubes were not part of the computer yet, nor were they operating in July, as he had hoped, nor in September. "The final testing and alignment of the first storage bank is moving along steadily," he wrote, injecting first a diplomatic note of tempered optimism, "but more slowly than I had expected," he added, perhaps ruefully, perhaps philosophically, but nonetheless matter-of-factly.<sup>16</sup> He was finding it a touch-and-go-business, and a careful reading between the lines, augmented by the power of hindsight, indicates again how heavy were the pressures that weekly were becoming heavier as the Air Force phase of proposed application began to take more explicit form. Careful testing of storage tubes and associated circuits in each digit column was producing "numerous small difficulties;" this was to be expected with new equipment, Forrester argued. But "our biggest problem," he confessed, "is that of trying to do in two months what we have had six months to do on other comparable parts of the computer."<sup>17</sup>

They were running out of time, including the planning time Forrester had allotted himself two years earlier. In the summer of 1948 he had published a "Long Term Whirlwind Schedule." It has appeared in the August Summary Report to the Navy. As of that date, Forrester had spent thirteen months, by his own reckoning, on the analog-computer phase of the Aircraft Stability and Control Analyzer

before abandoning that line of inquiry at the end of 1945. From about October 1945 to February 1946 he was in transition, and from January 1946 he and the engineers he gathered around him were committed to the digital computer as the most practical alternative.

Digital design studies involved the Project for the following ten months of 1946, and during the eighth and ninth months specifications of the speed and storage requirements of the ASCA had been settled upon. Speed and efficiency considerations during the ninth, tenth, and eleventh months led to the decision to undertake parallel, or simultaneous, digit transmission, and by the end of 1946 it was clear to the Project Whirlwind engineers that they were talking about a machine that could cope with far more than aircraft analyzer problems; they were undertaking to develop a practical, general-purpose machine, or at least the prototype of one.

During the first half of 1947, the block diagrams detailing the basic logical (though not electronic) functions for a parallel arithmetic element and central control had been worked through by Everett, and in the spring of that year the design of the Whirlwind prototype had begun to be laid down. Since the proper operation of the arithmetic element was utterly essential to the success of the machine, the five-digit multiplier was conceived as a preliminary test component and committed to construction during the middle of 1947.

Sixteen months after they began the design of Whirlwind itself, they began to receive electronic panels from Sylvania under subcontract, and by the end of 1948 the basic racks were up, the arithmetic

element of the computer was going in, and power sources were being installed. So in 1948 the physical computer began to emerge, acquiring the physical equipment of the central control element during the first half of 1949 and beginning to receive minimum input and output equipment after mid-1949.

During the third quarter of 1949, "after nearly two years of research, design, construction, and tests," Forrester was happy to report, "the computing section of WWI, has just passed a most significant milestone: solving an equation and displaying its solution" on an oscilloscope, showing values for  $x$ ,  $x^2$ , and  $x^3$ . Previous test problems had called for only "single-point solutions," whereas the progressive display required by this problem, "no matter how simple, can result only when all the basic parts of the computer act in harmony."<sup>18</sup>

Only the storage element was left, and the state of completion of the rest of the computer, as 1949 passed into 1950, made it mandatory that it begin to be incorporated in order that testing, maintenance, checking, and trouble-location procedures might continue. These had begun to be applied to the emerging machine at the start of 1949 and were essential prerequisites to a fully tested and operable machine--operable, that is, for the purposes of putting it to work and thereby exploring its nascent capabilities.

But the incorporation of the storage element depended upon the state of progress of the storage-tube research and development which had been carried forward since 1946, especially after parallel transmission of digits had been decided upon late in 1946.

By September 1950 Forrester was still able to point out, as he did in his letter to Stratton, copies of which went to Valley and to C. V. L. Smith of ONR, among others, "Thus far at least, the computer status is not holding up progress of the Air Force intercept program, although a working storage-tube bank will soon be necessary."<sup>19</sup>

In the meantime, the test storage had proved capable of meeting the demands placed on it in "testing out the terminal equipment and telephone line transmission of radar data from Bedford.... We have made two trials of transmitting radar data into and through the computer with promising results."<sup>20</sup>

While work on the first bank of storage tubes continued in the Barta building, Papian reported in September on his work of the preceding nine months. "The problem is bracketed on the one hand," he wrote, "by a metallic core . . . which has excellent signal ratios and a 20 micro-seconds response time, and on the other hand by a ferritic core . . . which has only fair signal ratios and a 1/2 micro-second response time."<sup>21</sup>

Prospects were sufficiently attractive so that Papian proceeded to a 2 x 2 planar array of cores. In October he obtained "successful operation . . . with 30 micro-second switching time," demonstrating experimentally one of the conceptions Forrester had set forth in a laboratory report he duplicated in May and submitted in June to the Journal of Applied Physics as an article.<sup>22</sup>

While this work was progressing, MIT prepared a proposal in October 1950 to present to ONR regarding research problems considered worth pursuing not by Project Whirlwind alone but by another laboratory. The title of the proposal was "Program of Applied Research at the Laboratory for Insulation Research," and the first project proposed was one urging ONR to support the work of Dr. von Hippel in conducting "an investigation of the ways and means by which the hysteresis loops of ferroelectrics and of ferromagnetic semiconductors can be shaped to order."<sup>23</sup> The pertinence of the work to progress in computer research and development was stressed: "The present-day ferrite materials show that this goal can be achieved by dielectrics, if rectangular hysteresis loops can be produced. Project Whirlwind is therefore extremely interested in the outcome...."<sup>24</sup>

They had reason to be. For while the magnetic cores were becoming increasingly attractive prospects for further research, the electrostatic tubes were posing new problems. By the middle of 1950 the smaller tubes, equipped with guns providing a shorter "throw," clearly were not living up to expectations or specifications: ". . . a reliable operating tube with 32 x 32 density has not been achieved." Thirteen of thirty-three tubes produced in one three-month period were research tubes.<sup>25</sup> When the first bank of standard tubes was connected into the computer in July and short programs were run, successful operations could be obtained for several minutes at a time, or even for an hour, but the tubes were

not reliable. "... the behavior of the storage," said the third quarterly report for 1950, "depended on the programs used and their frequencies, and it varied when different areas of the storage surfaces were used. There was evidently much that we did not understand about the operation of the storage as an integrated part of the computer."<sup>26</sup>

It was in this same fall report that the possibilities of magnetic-core storage were first discussed in some detail in a periodic, formal, progress report for the record to the Navy. The results of Papian's work were presented and the proposed direction of further investigations was indicated. Two lines of inquiry appeared desirable, one devoted to "improving materials to reduce eddy-currents and increase hysteresis-loop rectangularity," and the other aimed at "uncovering and solving the problems associated with operating large numbers of these cores in a high-speed memory system.<sup>27</sup>

The core-storage article Forrester had sent to the Journal of Applied Physics in June appeared in the issue for January 1951. Also during that month Forrester made another formal report to Stratton's office.<sup>28</sup> In it he reported that since the first bank of storage tubes had been connected (in July) the computer had been "operating satisfactorily much of the time." In consequence, they had been able to make "steady progress" on their Air Force task and at the same time get "organized for engineering applications...."<sup>29</sup>

The storage tube problem was not one to be solved in a hurry, because the complex structure and operations of the tubes posed so many sub-problems. Forrester felt compelled to admit at the beginning of 1951 that "performance of the storage tube bank is not yet good enough to permit predicting future scheduled performance of the machine with complete confidence." Again he made no reference to the possibilities of magnetic-core storage.

He turned instead to the matter of scientific applications of the machine, a goal which Mina Rees had undeviatingly held in view while ONR was bringing Whirlwind expenses into line with those of similar funding operations which ONR sponsored. Comments that Dr. Rees had made to Professor Morse caused Forrester to describe the resources of the four-man mathematics and applications group on the Project staff, headed by Charles Adams. While most of the work of Adams' group had been devoted to working out "basic machine techniques and procedures,...a few specific problems" were appropriate to call to the attention of Stratton's office in order to show that the computer was indeed moving steadily toward being a useful tool.

The proper integration of output equipment and solving the electrostatic storage problems, including increasing the storage capacity beyond the present, unreliable, single bank, remained as tasks that would not be concluded by the end of the 1951 fiscal year, as Forrester had hoped. Money problems were by no means over, just because ADSEC had come to the rescue a year ago.

In the preliminary funding discussions during that January twelve months earlier, when Valley and his colleagues had for the first time visited Project Whirlwind, John Marchetti of the Air Force Cambridge Research Laboratories had tentatively suggested an annual expenditure rate of \$200,000 from Air Force funds to underwrite Whirlwind's participation in the committee's program.<sup>30</sup> By March 1950, this amount had more than doubled, implying the urgency of the program and the importance the committee had come to attach to the role of Whirlwind I. In addition to the figure of \$500,000 which Valley mentioned at the March discussions with ONR, the Air Force the following month redirected the balance in the air traffic control study contract to "the experimental application of digital computers to air defense in accordance with the needs of ADSEC."<sup>31</sup>

This action apparently caused some confusion concerning the amount of money the Air Force would make available. The MIT Provost understood the amount would be some \$600,000, including \$120,000 left in the Air Traffic Control study account, whereas John Marchetti believed the \$600,000 to be an additional sum. Marchetti's projected budgets for the following two fiscal years also included an annual allocation of \$600,000, again effectively demonstrating the committee's reliance upon Whirlwind I.<sup>32</sup> The actual amount transferred by the Air Force in November was \$480,000; this, plus the \$20,000 given by ONR and the \$120,000 from the Air Traffic Control study account, made initially available for the ADSEC phase of Project Whirlwind a sum of about \$620,000.

The urgency which the Air Force attached to the work of the Valley Committee was illustrated not only by the redirection of the Air Traffic Control study, but also by the manpower and financial requirements imposed upon the Cambridge Research Laboratories to support the committee's efforts. Despite the reluctance of his laboratory chiefs, John Marchetti, without doubt under pressure from higher echelons, established within the Laboratories an "interim Air Defense Group," possessing a "priority in excess of any other job in the house." In his directive, Marchetti noted that "the task must be done, and no further delay can be tolerated."<sup>33</sup> In addition to drawing upon the best scientific and engineering talent in the laboratories, the Air Force, in finding funds for the Valley Committee, also tapped the Laboratories' appropriation, again to the dismay, if not the irritation, of many CRL personnel.<sup>34</sup>

In April of 1950 in accordance with its instructions from the Air Force, Project Whirlwind discontinued its work on the air traffic control problem and turned its attention to "the application of high speed computers to tracking while scanning in accordance with the needs of the Valley Committee."<sup>35</sup> This "TWS" phase of Project Whirlwind's activities was continued under the air traffic control study contract until the latter's termination on June 30, 1951, at which time Project Whirlwind's participation was financed through a new contract, AF 19(122)-458, administered by the Institute as account number DIC-6889. The new contract reflected, of course, official confirmation of the agreements reached by MIT and the Air

Force the previous January when the Institute agreed to implement the ADSEC recommendations, conduct further investigations into the air defense problem under the code name "Project Charles," and establish Project Lincoln. All three programs were initially coordinated and supervised by the first director of Project Lincoln, Dr. F. Wheeler Loomis, on leave to MIT from the University of Illinois.<sup>36</sup>

Since Whirlwind I was still under construction during 1950, the initial work carried out for ADSEC "consisted of (1) studying the TWS problem in order to program (or 'instruct') the computer and (3) devising a means of inserting radar data into the machine." The radar set which the group anticipated using was a NEW set located at Hanscom Air Base in Bedford. This set had previously been used by the Cambridge Research Center to test one of its developments, a Digital Radar Relay, a system which "permitted transmission of range and azimuth data over an ordinary telephone line."<sup>37</sup> Once the components were ready and the system was joined, experiments were conducted which produced flight data. But the data were of poor quality because of erratic behavior on the part of the MEW radar. So between February 12 and March 12, 1951, the system was shut down for overhaul and repair.<sup>38</sup>

Once back in operation, the system received further minor technical corrections until it successfully "tracked a single aircraft and computed the proper magnetic heading instructions to guide the aircraft to an arbitrarily chosen geographical point."

Subsequent similar successes led to an attempt to perform "a computer-controlled collision-course interception." On April 20, 1951, three such interceptions were successfully completed. The pilot of the intercepting aircraft reported that from a distance of about forty miles, he was brought to within 1,000 yards of his target on each occasion.<sup>39</sup>

The interceptions were hailed as a success. They demonstrated the feasibility of the concept, they vindicated, in a preliminary way, the predictive arguments Crawford, Forrester, and Everett had held to for years, that such practical, real-time operations lay within the power of electronic information systems, and they provided the basis for an expanded, experimental, multiple-radar system to be established later in the Cape Cod area. At the same time, such tests offered "valuable experimental experience in preparation for operating" Whirlwind I in the expanded system.<sup>40</sup>

The successes achieved by ADSEC and Project Whirlwind won the commendation of Air Force Chief of Staff Vandenberg. In a letter to George Valley, he observed that the "successfully accomplished digital computation of interception courses with the Whirlwind Computer" gave "real hope of being able to eliminate some of the delays and inaccuracies inherent in conventional manual Air Defense control systems."<sup>41</sup>

There are as many ways of determining when a computer is "in operating condition" as human ingenuity can devise, because the digital computer in an integrated system of electronic circuits

which evolves through many stages of testing and preliminary electronic operation of, first, its elements, then groups of its elements, then eventually all of the elements together. Further, there is the repetition of this pattern, once the entire machine is considered operable, through many levels of logical complexity as determined by the programs and information-processing paces through which the machine is put. However, long before the electronic capacities of the components of the machine have finished being checked out, the logical calculational capacities have begun to be explored, inasmuch as these logical capacities take expression first in the electronic operations of the machine.

If one is most concerned about whether a computer will perform the basic arithmetical operations, then these may become his principal criteria of its operability. If a given storage capacity must be demonstrated first, then that is the consideration which may determine whether a particular computer is "really" in operating condition or not. If a computer is expected to carry out a certain type of program, then that may become the standard of its "true operating condition." Nor are these the only examples.

Forrester, Everett, and their engineering staff recognized full well that the usefulness of this computer which they had been touting as a "general-purpose" machine depended on the size of its internal, automatic storage. The manually controlled test storage could be set to put the other control and calculational elements of the machine through enough paces to demonstrate their capacities,

their speed, their reliability, and in this respect, determine whether they would "really" operate or not. But an operating standard which could only be demonstrated with the passage of time was that of how dependably the machine would perform its multiple operations and how often it would have to shut down, or even pause, for repairs.

The Project engineers and many of the technicians had sufficient evidence, and from that evidence, knowledge that the computer would calculate as they had expected it to long before the intercept tests were run. They did not know how it would operate with full storage capacity because such capacity had not yet been achieved. Meanwhile, the success of the first air-intercept tests was another milestone, and a dramatic one, demonstrating the potentiality of Whirlwind as a truly useful machine.

In this perspective, the chronic troubles the Project had been having with the electrostatic storage tubes were not crippling, for those troubles had not prevented the tubes from contributing to the success of the intercept tests of the spring of 1951. Since June 1950 the first bank of 16 tubes, comprising a storage capacity of 256 registers, had been connected to the rest of the computer.<sup>42</sup> Preliminary results were at once gratifying and irritating. The tubes worked, but not all of their storage capacity was dependable. Parts of the storage surfaces would operate without error for over an hour, while other parts would not hold up beyond five minutes. It was the over-all delicacy and instability of the tube-bank's

performance that caused Forrester, Everett, and their staff to remain profoundly dissatisfied with the reliability prospects. These were quite intolerable when measured by the standards originally laid down. Improved quality control in tube design and tube manufacture was imperative.

However promising magnetic-core storage appeared to be, the research and development problems involved in magnetic core applications were altogether too formidable and time-consuming to meet the current needs of Whirlwind, for it was now a computer going through shake-down operations. So there was nothing for it but to continue testing and adjusting present tube and circuit designs while continuing also to construct and test special research modifications on tubes not wired into the rest of the computer. Obviously, the key lay in controlling the characteristics of the individual tubes that were to serve as the building blocks of the total internal storage element.

During 1950 and 1951, then, the Project was engaged in exploring and improving the operating capacities of the entire computer with one hand, so to speak, while carrying on development work on component elements with the other hand. The principal elements needing improvement were the internal storage and the input-output equipment. Aware that the possible modes of input and output were many and varied, and estimating at the start that the design and development problems were far less formidable with regard to this terminal equipment than with regard to over-all machine control,

arithmetic computation, and internal storage, Forrester had been willing to let the Special Devices Center contract for part of the terminal equipment separately. After he had participated in establishing the requirements this equipment must meet in order to integrate compatibly with the rest of the computer, he had been satisfied to have the Eastman Kodak Company take on the job of designing and developing the reading and recording unit. Eastman would work with Project Whirlwind at the engineering level but would be responsible contractually, fiscally, and administratively to the Special Devices Center. The input-output control element would be developed at MIT, in order to ensure that numbers would be transferred satisfactorily between the input-output register of Whirlwind and whatever external memory and recording devices were used--in this instance, the Eastman reader-recorder.

The Eastman Company had delivered the first reader-recorder unit on September 13, 1949, after testing it during the preceding three months.<sup>43</sup> Reliability tests were next in order, to find out how well and consistently the unit would "record binary numbers on photographic film by...light from...a cathode ray tube," and how well and reliably it would "read such recorded data by scanning of the processed film with a cathode ray tube."<sup>44</sup>

By the beginning of summer, 1950, the reader-recorder unit was proving sufficiently unreliable when connected to the computer to indicate the need for more extensive testing than had been planned. Characteristically, the Summary Report issued that spring began

discussing other forms of input-output equipment: "The input-output requirements have been studied," said the report, "and a proposal has been made for the minimum amount of terminal equipment needed to make the computer useful in handling general problems."<sup>45</sup> Input equipment called for included a typewriter, a coded-paper-tape punching device, and film units. Output equipment would include a typewriter, a paper-tape punch, film units, and "a versatile oscilloscope display." Acquisition and integration of such units would occupy the coming year.<sup>46</sup>

By the time of the successful air intercepts over the Atlantic Ocean in the spring of 1951, the Eastman units had been quietly discarded, a Flexowriter typewriter and punched-tape units were in service, and plans were laid for "a flexible system that will accommodate various kinds of terminal equipment," including paper tape, magnetic tape, oscilloscopes, a scope camera, and a control to introduce preprogrammed marginal checking whenever desired.<sup>47</sup>

More important than the incorporation of improved input-output equipment was the progress achieved by the beginning of 1951 in electrostatic storage tube design. As a result of this advance, Forrester felt free to proclaim to ONR in his official report of the spring of 1951 that Whirlwind had become "a reliable operating system." By the end of March, 1951, applications were being programmed on the machine a scheduled thirty-five hours a week. "Of this time, 90 percent is useful." The computer had achieved as many as seven consecutive hours of error-free operation more than once.<sup>48</sup>

Alterations in the "charge on the glass windows" of the electrostatic tubes were identified as a major source of trouble and led to a redesign, the 300-series tube, in which coating "the entire inside of the envelope...with aquadag" removed the fluctuations of the high-velocity gun's beam that had earlier caught the investigator's attention. Once all sixteen tubes had been replaced, Forrester was willing to call the computer "a reliable working system."<sup>49</sup> Five years had passed since Forrester had made the decision to abandon the analog-computer approach to the Aircraft Stability and Control Analyzer problem and had committed himself to the alluring but virtually untried electronic digital computer.

A final significant footnote: the regular vacuum tubes employed in the pulsed circuits had become so reliable after the interface problems had been solved that wholesale replacement of tubes within less than 20,000 hours was considered to be not only unnecessary but "unwise."<sup>50</sup> A new high in reliability and longevity had been achieved in the course of Whirlwind's development, potentially increasing the life span of many standard-design vacuum tubes from a few hundreds of hours to several thousands of hours.



NOTES TO CHAPTER 11

1. Testimony of J. W. Forrester in records of Patent Interference No. 88,269, pp. 27-28.
2. Ibid, p. 31.
3. J. W. Forrester, Coincident-Current Magnetic Computer Memory Developments at MIT, p. 2. This paper was given at the Argonne National Laboratory Computer Symposium of August 4, 1953. Cf. J. W. Forrester Memo. No. M-70, subj: "Data Storage in Three Dimensions," April 29, 1947.
4. Project Whirlwind Summary Report No. 17, February, 1949, p. 7.
5. Ibid, p. 8.
6. Interview, J. W. Forrester by the authors, July 24, 1964.
7. Interview, R. R. Everett by the authors, July 31, 1963.
8. Letter, W. N. Papian to T. M. Smith, February 12, 1968.
9. Memorandum, R. R. Everett to J. W. Forrester, January 9, 1950; Memorandum L-18, H. R. Boyd to J. W. Forrester, February 15, 1950.
10. Summary Report No. 20, Third Quarter, 1949, p. 17.
11. Ibid, p. 27.
12. Memorandum L-18, February 15, 1950, p. 1.
13. Letter, J. W. Forrester to J. A. Stratton, March 3, 1950, p. 1.
14. Ibid.
15. Ibid, pp. 3-4.
16. Letter, J. W. Forrester to J. A. Stratton, September 28, 1950, p. 1.
17. Ibid.
18. Summary Report No. 20, Third Quarter, 1949, p. 9.

NOTES TO CHAPTER 11 (CONTINUED)

19. Letter, J. W. Forrester to J. A. Stratton, September 28, 1950, p. 2. The upper right-hand corner of page 2 bears the date "Sept. 23, 1950."
20. Ibid.
21. Report R-192 by W. N. Papian, subj.: "A Coincident-Current Magnetic Memory Unit," September 8, 1950, abstract.
22. Report R-187 by J. W. Forrester, subj.: "Digital Information Storage in Three Dimensions Using Magnetic Cores," May 16, 1950.
23. W. N. Papian patent interference testimony. The proposal pointed out that this problem was "at present in the center of interest for highspeed digital computers like Whirlwind. Here simple yes-no devices are required that can switch in fractions of a microsecond and lend themselves to a three-dimensional storage of information."
24. Ibid.
25. Summary Report No. 23, Second Quarter, 1950, p. 15.
26. Summary Report No. 24, Third Quarter, 1950, p. 6.
27. Ibid, p. 24.
28. Letter, J. W. Forrester to Prof. J. A. Stratton, January 18, 1951.
29. Ibid, p. 1.
30. J. W. Forrester, Computation Book No. 49, p. 84, entry for February 1, 1950.
31. C. R. Wieser, Lincoln Laboratory Quarterly Progress Report, Division 6--Digital Computer, June 1, 1952, pp. 6-9.
32. J. W. Forrester, Computation Book No. 49, p. 102, entry for April 12, 1950.
33. Directive, John H. Marchetti, Director, Radio Physics Research, to Col. Mitchell, R. E. Rader, Dr. Hollingsworth, Dr. Spencer, Dr. Samson, Dr. Foster, and Mr. Davis, June 29, 1950.

NOTES TO CHAPTER 11 (CONTINUED)

34. Memorandum, Robert E. Rader, Chief, Air Defense Group, Base Directorate, Radio Physics Research, subj.: "Transfer of Personnel to Air Defense Group," July 5, 1950; Memorandum, R. E. Rader to Dr. George E. Valley, October 31, 1950.
35. Servomechanisms Laboratory, MIT, Summary Report No. 6, April 25, 1950--July 25, 1950, "Submitted to Watson Laboratories, Air Material Command, Under Contract AF 28 (099)-45," p. 2.
36. Conclusions--Scientific Advisory Committee Meeting, January 19, 1951; Contract AF 19 (122)-458, January 20, 1951. Memorandum A-117, H. Fahnestock, Subj.: "New Funds under Contract AF 19 (122)-458, DIC 6889," May 7, 1951.
37. C. R. Wieser, Lincoln Laboratory Quarterly Progress Report, Division 6--Digital Computer, June 1, 1952, pp. 6-9.
38. J. W. Forrester, Computation Book No. 49, pp. 135 and 136, entries for February 1, 1951; Servomechanisms Laboratory, MIT, Summary Report No. 9, January 25, 1951- April 25, 1951, "Submitted to Air Force Cambridge Research Laboratory under Contract AF 28 (099)-45," p. 1.
39. Ibid, p. 23; C. R. Wieser, Lincoln Laboratory Quarterly Progress Report, Division 6--Digital Computer, June 1, 1952, pp. 6-10; Memorandum M-1515, D. R. Israel, subj.: "Interception Experiments with Bedford Mews," June 11, 1952.
40. Memorandum M-2092, C. R. Wieser to J. W. Forrester, subj.: Experimental Interceptions with Bedford Mew Radar," April 23, 1951.
41. Letter, Hoyt S. Vandenberg, Chief of Staff, USAF, to George E. Valley, Jr., Chairman, ADSEC, May 28, 1951.
42. Summary Report No. 23, Second Quarter, 1950, p. 5.
43. Summary Report No. 20, Third Quarter, 1949, p. 28.
44. Ibid.
45. Summary Report No. 22, First Quarter, 1950, p. 21.
46. Ibid.

NOTES TO CHAPTER 11 (CONTINUED)

47. Summary Report No. 25, Fourth Quarter, 1950 and First Quarter, 1950 and First Quarter, 1951, p. 5.
48. Ibid, p. 6.
49. Ibid, p. 5.
50. Ibid.

## Chapter Twelve

### MAGNETIC CORES AND R & D PROGRESS

One of the follow-on test projects that the investigators working in Project Charles had recommended was the more elaborate, Cape Cod multiple-radar system. Whirlwind I would form the machine element at the information-processing, command-and-control center of this system. The final report of Project Charles, although more comprehensive and detailed, recommended a solution of the air defense problem which was essentially similar to that proposed by the Valley Committee, the feasibility of which was being investigated and demonstrated by Whirlwind I. Project Charles' investigators were familiar, of course, with the successful experiments conducted by Project Whirlwind and ADSEC. They had been unable to find any "other solution to air defense data processing . . . equal in long term value to the digital transmission and automatic analysis of data." Hence they recommended the construction of a model system in the Cape Cod region of Massachusetts. This system could consist of a series of radar stations tied to Whirlwind I at MIT. The data obtained from this experimental network, it was anticipated, would provide guidelines for the design, development, and construction of a more sophisticated digital computer. In this next-generation computer there would be incorporated the unique functional characteristics required by the information and control center of the proposed centralized air defense system.<sup>1</sup>

The model system built in the Cape Cod region became the joint responsibility of Divisions II and VI of the Lincoln Laboratory, the air-defense laboratory built and managed by MIT for the purpose of coordinating and implementing the recommendations of ADSEC and Project Charles and for performing within its own facilities research, development, and tests in the general area of air defense.<sup>2</sup> The primary task which thus confronted Lincoln Laboratory at this time was the "development of a system using a high-speed digital computer to receive, process, and transmit air-surveillance, identification, and weapon-guidance information."<sup>3</sup>

Concurrently with the establishment and organization of Project Lincoln in 1951, MIT reorganized its computer program. At Jay Forrester's request, Project Whirlwind was detached from the Servomechanisms Laboratory and reconstituted as the Digital Computer Laboratory under Forrester's direction in the autumn of 1951.<sup>4</sup> This administrative reorganization within the Institute at the same time served to placate ONR and facilitate administrative relations between Project Whirlwind and Project Lincoln.

In the fall of 1951 Jay Forrester discussed with Professor Wheeler Loomis, Lincoln's director, the nature of the relationship between the two groups. On this occasion Loomis mentioned two possible administrative relationships: "one a sort of sub-contract arrangement . . . the other a closer administrative tie. . . ." Forrester, as could have been anticipated, noted his preference for an independent status and suggested that the Digital Computer

Laboratory's part of Lincoln's program remain at Cambridge until the computer his group was to design especially for air defense had been assembled or put "into initial operation." This, he estimated, would not be until 1954.<sup>5</sup>

When Project Lincoln was first organized by MIT, responsibility for digital computer research and development had not been assigned to it. Second thoughts, however, added it to Lincoln's program, and six months later those operations and staff members of the Digital Computer Laboratory concerned with Whirlwind I and its application to air defense were incorporated as "Division VI" into Lincoln Laboratory.<sup>6</sup> Consequently, Jay Forrester came to wear two hats: one as director of MIT's Digital Computer Laboratory, the other as head of Lincoln Laboratory's Division VI. In the latter capacity, he was responsible for Whirlwind's participation in the Cape Cod experimental system and for the design and construction of a digital computer possessing the characteristics "desired for a future operational air defense system."<sup>7</sup>

Forrester and his colleagues within Division VI had four major tasks facing them from the outset: (1) the organization of the Division and the formulation of guidelines for its relations with its parent organization, Lincoln Laboratory; (2) the design, construction, operation, and expansion of the Cape Cod system in cooperation with Division II; (3) the design and construction of a prototype air defense computer and its ancillary equipment, with the necessary concomitant research and development; and (4) the

selection of an industrial source for production of the air defense computer. All four tasks were pursued concurrently during the closing months of 1951 and throughout 1952-53.

Administratively, Division VI was divided into six groups, each charged with a specific responsibility within the overall program. Group 60, under the direction of Harris Fahnestock, was responsible for administrative services. Group 61, under C. R. Wieser, was responsible for the Cape Cod system. Group 62, under Norman Taylor, bore the responsibility for the design and construction of the projected air defense computer. Group 63, under D. R. Brown, was responsible for magnetic materials. Group 64, under S. H. Dodd, was responsible for the maintenance and improvement of Whirlwind I. And Group 65, under Pat Youtz, conducted the electrostatic storage tube development program.<sup>8</sup>

Later, the Sage System Production Coordination Office and the FSQ-7 Systems Office were organized within the Division. The Production Coordination Office maintained "liaison with industrial and military organizations outside Lincoln" and acted "as the coordination agency for Division VI portions of SAGE system planning and implementation, thus providing suitable direction and control of the program with respect to Lincoln's over-all responsibility for the system." The Systems Office maintained coordination with the International Business Machines Corporation, which subsequently became the manufacturer of the production model of the air-defense computer, the FSQ-7.<sup>9</sup>

To a very great extent Forrester and his engineers enjoyed the same independence and freedom of action within Lincoln that they had enjoyed when attached to the Servomechanisms Laboratory. Although nominally attached to the Laboratory, Project Whirlwind had been virtually self-sufficient and independent. This was especially true after the Project had so expanded in size that it became necessary for it to be physically dissociated from the Servomechanisms Laboratory and to move to separate quarters in the Barta Building. Here the Project established its own shops and maintained its own service crews, an autonomy acceptable to Nat Sage's Division of Industrial Cooperation, which administered the contract. Thus, the freedom of action permitted by Nat Sage in technical matters had also been permitted by Nat Sage in administrative matters. The cumulative effect confirmed Forrester's desire and belief that Project Whirlwind was actually, if not contractually, a free agent.<sup>10</sup>

During 1951 and 1952, prior to the physical integration of Project Whirlwind into Lincoln Laboratory, Forrester and Everett worked with the parent organization as Forrester had previously worked with ONR. No members of the Whirlwind staff, for example, attended Lincoln's meetings. So from the start, Division VI established a pattern of autonomous behavior within Lincoln Laboratory, and it was a pattern which persisted throughout the Division's association with Lincoln. Even after Forrester's departure in 1956, Everett as the new director continued the semi-autonomous and

highly individualistic role until the Division was separated from the Laboratory in 1959 to form the MITRE Corporation.

When the Division finally moved physically to the Laboratory's new quarters at Hanscom Air Base in the Bedford-Lexington area of Massachusetts, the giant Whirlwind I computer was left behind in the Barta building on the MIT campus. The Division took its service organizations with it, however: the print shop, machine shop, purchasing office, etc. These services duplicated those already provided by the parent Lincoln organization, but Forrester and his associates refused to disassemble the smooth-functioning organization they had created at MIT as Project Whirlwind. There were those within Lincoln who were irritated by this display of independence and saw it simply as a determination to continue going "first class" regardless of the added cost. But there were also those who took advantage of the efficiency which the additional facilities and services offered. Among the latter was George Valley, head of Division II, who when in need of immediate assistance would resort to the Division's supporting facilities.

At the same time that Division VI remained as highly individualistic as Project Whirlwind had become, continuing to do business its own way, it worked effectively with the radar engineers in Division II. The latter, under the direction of Valley, were responsible for aircraft control and warning and possessed a special competence and experience, which Whirlwind's personnel lacked, to set up the preliminary, experimental, Cape Cod system employing several radar stations to feed data into Whirlwind I.

This particular kind of practical application of the general-purpose computer, of which Whirlwind I was the first example, required dimensions and directions of technical electronic knowledge and experience that Project Whirlwind personnel had never acquired and very likely could not have mastered in the brief time that development schedules allowed. The engineers took this situation for granted, and there was from the start the view that such pooling of resources and talents as Division II and Division VI possessed was the natural course to pursue. It was the efficient way to proceed, and it became the way they all successfully proceeded.

In this respect then, Whirlwind was not ruggedly independent. Rather, its independence manifested itself in the group's self-resourcefulness and daily conduct of its affairs. Although later, by the move from the MIT campus to Lexington, it lost the injection of the rich new blood that had been contributed by the graduate students, who had contributed so much to the élan and the resourceful operations of Project Whirlwind, the Project's way of doing things had already become firmly established, and it persisted for some time, even without a continuing supply of graduate students, in maintaining its special character and vitality against the pressures imposed by the more conventional organization of Lincoln.<sup>11</sup>

Two of the four major tasks facing the administrators of Division VI were of vital importance to the over-all program of Lincoln Laboratory, for unless they were successfully completed, the program could become a total failure. These two tasks were the

construction, operation, and evaluation in cooperation with Division II of the Cape Cod experimental system, and the design and construction of a next-generation high-speed digital computer that would possess the characteristics "required for an operational air defense system."<sup>12</sup> The Cape Cod experimental and evaluation system recommended by Project Charles was a natural extension of the earlier system established for the tests run by ADSEC. The Cape Cod system was designed to test a multiple-radar network linked to Whirlwind I; the total system would collect and process data and would guide and control defensive countermeasures taken.

As an experimental system, Cape Cod served several purposes: it developed "system concepts for a high-track-capacity system;" it permitted new components to be tested in an operating prototype of the air defense system envisaged by ADSEC and Project Charles; it furnished data and other information necessary to the preparation of "specifications for digital computers designed specifically for air defense;" and it permitted verification of the "soundness of the whole concept by experiments using live radar data and controlling live aircraft."<sup>13</sup>

Division VI's responsibilities for the Cape Cod system included "air defense center planning, automatic information processing (including data screening and automatic tracking), the computation of control orders for weapons, and the provision of the digital equipment necessary in the air defense center." These responsibilities fell mainly upon Groups 61, 64, and 65.<sup>14</sup> When work upon

the system was commenced by the groups concerned, it was anticipated that the program would consist of three parts: "(1) Construction and operation of a three-radar network; (2) construction of a 14-radar network; and (3) planning for a future operational system." The first part, which was scheduled for completion during fiscal year 1952, called for expansion of the single-radar system with which the Valley Committee had conducted its successful intercept experiments in the spring of 1951.

Throughout fiscal year 1952 the primary objective was "aimed at learning how to track an aircraft through a network of radars having overlapping coverage," in order to gain the knowledge and experience necessary to implement the second part, expansion into a 14-radar network. The hope was that by July of 1952 the larger network would be under construction and that in the course of the year, Whirlwind I would be sufficiently improved technically, by the installation of magnetic drums, to expand the computer's capacity to handle the requirements of the full Cape Cod radar net. If this schedule were met, then it was anticipated that prior to June of 1953 the Division would be able "to commence operation with the 14-radar network with automatic data-handling capacity for data screening, automatic track-while-scan, and the control of a large number of aircraft."<sup>15</sup> Realization of these plans was of extraordinary importance to the total air defense project, for in addition to laying the groundwork for an expanded experimental system, the initial efforts would permit the military to evaluate the concept even as it was in process of implementation.<sup>16</sup>

Military evaluation of the project was more important than it appeared to be on the surface, for even while Project Lincoln had been in the process of organization by MIT and the national military establishment, another air defense program had been under way at the Willow Run Research Center of the University of Michigan. This was a program similar in goal, but different as to the proposed method of attainment. The competition between the two programs could be "sticky," not solely because of the impact upon the educational institutions involved, but also because the situation reflected the competition between the Rome Air Development Center at Rome, New York, and the Air Force Cambridge Research Center at Cambridge, Massachusetts. Both were Air Force agencies, but both were striving to become preeminent if not dominant in air-defense research and development and in the concomitant area of military electronics. Furthermore, the competition was not divorced from politics. Political representatives of the two regions concerned sought to keep the program, since it held great potential for economic growth and stability. Industry also did not remain unininvolved, although the concern there was less narrowly regional. The International Business Machines Corporation became involved eventually in Lincoln's program. General Electric had expressed interest in the program conducted at Willow Run.<sup>17</sup>

The problems raised by the competing programs were finally resolved in 1953 by the decision to terminate the University of Michigan's program and concentrate the whole effort upon the

centralized concept then under development by Lincoln Laboratory and the Cambridge Research Center.<sup>18</sup> There were the unavoidable mutterings that the decision had been politically motivated, and it would be naive to assume that political and economic pressures played no role. Nevertheless, the feasibility of the concept Lincoln was implementing had been demonstrated by the experiments first conducted for ADSEC and then expanded under Divisions II and VI of Lincoln Laboratory in the Cape Cod system. In comparison, the Michigan system had "not been demonstrated as successful."<sup>19</sup>

By March of 1953, the Cape Cod system, though incomplete, was supplying "valuable experimental data from existing equipment." The nucleus of the system was the network which had been put together for the ADSEC experiments, but two smaller radars located at Rockport and Scituate, Massachusetts, using slowed-down-video data transmission links, had also been used in some tracking tests. The data generated by the radars were fed into Whirlwind I. The computer processed the information and provided "vectoring instructions for mid-course guidance of manned interceptors and . . . special displays for people who monitor and direct the operation of the system."

The information and experience gained from such tests, using "live aircraft, live radar, and an operating computer" proved immensely "valuable in planning an advanced air defense system."<sup>20</sup> By December of 1953, the system was operating with a large radar set (FPS-3) located at Truro and two gap-filler radars. The data

provided were processed automatically by Whirlwind I, which was able to "carry the tracks of as many as 48 aircraft," and guidance instructions were transmitted to intercepting aircraft.<sup>21</sup> Although by December, the "electronic systems and the programs" were functioning smoothly, good radar data were still lacking.<sup>22</sup>

The role Whirlwind was able to play in these exercises by the end of 1953 hinged crucially upon the progress that had been made in magnetic-core development work since Papian had built his first  $2 \times 2$  planar array of cores in the fall of 1950. During 1951, efforts were continued to obtain core material that would hold its polarization in spite of electronic system "noise" and that would switch rapidly from one state to the other when required. A  $16 \times 16$  array of cores in one plane was constructed in order to test and observe the effects of "noise," of the switching of nearby cores, and the running of program patterns through the array. By the end of 1951 "a fair demonstration of the practicability" of the arrangement had been achieved: "error-free operation for periods of considerable differences in the characteristics of the 256 cores used in the array."<sup>23</sup> The cores continued to appear promising, indeed, yet they were still far from achieving the operating standards required.

The addition of a second bank of electrostatic storage tubes to Whirlwind at this time increased its storage capacity by 1024 registers without adding to storage access time.<sup>24</sup> But experience during 1952 with the new tubes revealed that contradictory to

earlier judgments the internal-storage problem was not yet out of the woods. The new bank of tubes possessed a larger storage density of 32 x 32 registers, compared to 16 x 16 in the first bank. Unfortunately, they were by no means trouble-free. New "400-series" tubes replaced the 300-series tubes to no avail. By April 1952 the decision was reluctantly made to hold up adding any more banks of tubes of the new design, and immediate plans to replace the 16 x 16 tubes in the original bank were suspended. New 500-series tubes were being manufactured as fast as possible, but it would take time to increase production sufficiently to maintain an adequate stock of reserves, even if these did prove reliable. "A large fraction of Whirlwind operating time" was being devoted to maintenance and special checking of the installed storage tubes. Furthermore, the limited supply of replacement tubes available indicated it would be risky to put Whirlwind on a 3-shifts-a-day schedule, even though applications programs were stacking up as the demands grew for more machine time.<sup>25</sup>

In the meantime, Papian and his assistants were constructing and testing another 16 x 16 planar array of cores composed this time of non-metallic ferritic material instead of rings of thin metal ribbon wound on itself to form a doughnut. In May of that same year (1952) it became clear that the switching speeds were approximately twenty times faster than with the metallic cores-- down to one microsecond or less. So promising was the performance of the non-metallic ferrites by July that Forrester, Everett, and

their engineers made the decision to build 32 x 32 arrays and stack them sixteen-high, in a true three-dimensional arrangement.

Since Whirlwind was by then in heavy demand for preliminary Cape Cod and other applications, it would not be possible to use it to test the 16,384-bit core memory, comprising 1,024 registers, which was then being built. The design of Whirlwind's operating units had long since become standardized, however, so the solution most practical to the engineers was to turn out a semi-copy of Whirlwind--another computer to test the magnetic storage. So the "Memory Test Computer" came into being as a concept during the summer of 1952, and construction of this machine--more modest in its size and capacities than Whirlwind--proceeded into the fall. By November Forrester and Everett committed themselves fully to the use of non-metallic ferrites for the Memory Test Computer's storage bank. The following May 1953, the Memory Test Computer was operating sufficiently well to demonstrate the "highly reliable operation of a 32 x 32 x 16 magnetic ferrite storage."

To Forrester that summer the performance of the new core storage meant the end of his search for a practical internal storage element. Electrostatic storage tube manufacture and development were abruptly halted as soon as it was clear that Whirlwind would receive a core-storage unit-in-being, and on August 8, 1953, the first bank of core storage was wired into Whirlwind. A second bank of cores went in on September 5. Access time had dropped from 25 microseconds for tube storage to 9 microseconds for the magnetic

cores.<sup>26</sup> But best of all, the chronic, incurable maintenance troubles and the high costs of tube manufacture were at an end.

A brief valedictory in behalf of electrostatic storage was nevertheless in order; the Summary Report pointed out that Whirlwind "could not have reached its present state of development without ES. No other form of high-speed storage was available when Whirlwind I was put in operation."<sup>27</sup> Four years had passed since Forrester had set Papian to work on the magnetic core development project, sorting, testing and analyzing the electromagnetic properties of the tiny rings.

Also during those four years Whirlwind had found a mission and was about to spawn a successor that would take advantage of advances in the electronics state of the art since Whirlwind had been conceived. Long before the end of 1953 Forrester's engineers had begun to consider the parameters the new defense computer should have. The Cape Cod tests added valuable data and experience. All of these would be of assistance in designing and constructing the computer to be used in the projected continent-wide air defense system.

Existing computers, including Whirlwind I, were "suitable for studying the digital control of air defense," but they did not possess the unique characteristics necessary to an air-defense system. Moreover, the design and construction of an air-defense computer was urgent; national security required the "availability of an improved Air Defense System" as promptly as possible.<sup>28</sup>

The question, before the end of 1951, was not "whether to build a machine or not, but rather to build the best machine possible, considering speed, cost, capacity, and complexity."<sup>29</sup> In consequence, concurrently with the construction and operation of the Cape Cod system, Division VI, primarily through Groups 62 and 63, embarked immediately upon a research and development program pointed to the construction of an air defense computer, utilizing primarily its personnel in Group 62 and Group 63.<sup>30</sup> This computer was first thought of as "Whirlwind II," then it became the "XD-1" and finally the "FSQ-7."

In its approach to the research and development program which resulted eventually in the FSQ-7, Division VI operated upon three premises. First, Project Charles' recommendation for an "improved air defense system using a digital computer information center" had to be implemented and realized "as soon as practical." Second, no contemporary digital computer could be more than a "laboratory model" for the proposed system. An advanced design was needed which would "improve reliability, reduce maintenance, be tailored to the air defense application, and incorporate the necessary facilities for the required terminal equipment." Third, the Digital Computer Laboratory would furnish the "key personnel and background experience for the estimated design program."<sup>31</sup>

In a series of meetings held in the fall of 1951 to plan and schedule the research and development program for the air defense computer, those members of Division VI participating decided that

the computer should be fast, flexible, and reliable. It should be as fast as, if not faster than, Whirlwind I. It should possess a register length of 24 bits. The use of marginal checking, magnetic cores, and transistors was considered in order to achieve maximum reliability, even though sufficient perfection in the manufacture, reliability, and techniques of using transistors and magnetic cores still lay ahead at the time of the discussions. Reliability was so important, the conferees believed, that they even considered the installation of additional machines in the command and control center, to be available as an instant reserve. A general-purpose computer was essential, since flexibility was another major requirement. The time estimated to complete the program was three years.<sup>32</sup>

By January of 1952 the design staff for Whirlwind II was in process of organization. It was anticipated that the first half of Fiscal Year 1953 would be spent in determining and establishing the air-defense computer's characteristics and selecting its components; the second half of the year would be devoted to its design. Four major areas of concentrated effort were scheduled: "(1) a study of new components and circuits, (2) the determination of optimum machine logic to utilize these new techniques, (3) the development of new magnetic materials for reliable high-speed storage and for switching purposes, (4) close liaison with the Cape Cod System to formulate the computer characteristics peculiar to air defense data processing."<sup>33</sup>

A problem of primary importance which beset the leaders of the Digital Computer Laboratory and of Division VI from the outset and which possessed special significance for Groups 61, 62, and 63 responsible for the Cape Cod system and Whirlwind II, was the shortage of personnel trained and experienced in computer technology. The problem was acute enough in itself, but it was further complicated by the rapid physical and organizational expansion of the Digital Computer Laboratory as it sought to meet its responsibilities not only to the Air Force, but to the Navy and MIT as well, for as Whirlwind I had become operational, programs other than those in air defense were also assigned to it.

To cope with these complications as they bore upon Division VI, Everett and Fahnestock recommended that regularly scheduled formal meetings of group leaders and laboratory chiefs be instituted. Such meetings, they reasoned, would keep the leaders aware of the activities going on within other groups, permit critical analysis of the program, and assist in the assignment of personnel and job priorities. The Friday afternoon teas which in the days of Project Whirlwind had provided those attending a pleasant and informal means for keeping abreast of the program were no longer sufficient. The responsibilities of the Digital Computer Laboratory were just too complex, too great.<sup>34</sup>

The first meeting held on March 26, 1952, considered the seriousness of the shortage of experienced personnel and the impact of the shortage upon the Whirlwind II and Cape Cod programs in

particular. Norman Taylor, responsible for Whirlwind II, predicted that the schedule established for the design and construction of the production model of the air defense computer would prove "unrealistic," for of the twenty-three members of his group, only four, excluding D. R. Brown and Taylor himself, had any previous experience with Whirlwind I. Requesting the transfer to his group of one or two more men possessing Whirlwind I training, Taylor estimated that even if all effort were taken off Whirlwind I, completion of Whirlwind II would not be accomplished until January 1, 1956, two years later than scheduled. If Whirlwind I personnel were not used, then completion of a production model would be extended another two years.

In response to Taylor's predictions and pleas, Steve Dodd, head of Group 64, argued that a similar lack of experienced personnel would interfere with and delay the planned program for the improvement and enlargement of Whirlwind I. Whirlwind I had been considered a "training ground for Whirlwind II," but already the requirements for the former's program had increased "faster than the training;" consequently, no surplus of personnel existed for transfer. Furthermore, he warned, dilution of "the effort on Whirlwind I and Cape Cod might easily push Whirlwind I out to the original time estimates for Whirlwind II." Forrester concluded the discussion by recommending that the problem be taken under consideration and investigated further by Dodd and Wieser.<sup>35</sup>

The conclusions reached by Dodd and Wieser were presented at the group leaders' meeting of June 9. Dodd, acting as spokesman, told his colleagues that "no experienced staff could be transferred without seriously cutting into the Cape Cod program."<sup>36</sup> This could have been particularly damaging to the over-all program, especially if Wieser's earlier warning--that the Cape Cod system would be "running too late to be of any use in affecting decisions in the design" of Whirlwind II--proved accurate.<sup>37</sup> Forrester's decision, given at the end of the meeting, was that a transfer of experienced personnel from Group 64 to Group 62 would not be in "the best interests of the laboratory." Consequently, Taylor was required to train his own men and to make his assignment accordingly.<sup>38</sup>

The fourth major task which faced the directors of Division VI was the selection of a manufacturing source for the air defense computer. From the middle of 1951 on, analyses were undertaken and applications from qualified manufacturers were solicited, informally at first. Ultimately, the Air Force placed the International Business Machines Corporation under contract. From the outset of contractual negotiations, IBM had preferred a prime contract. This view was shared by MIT because the Institute's administration was loath to expand its budget through "additional funds for large sub-contracts."<sup>39</sup> On the other hand, the Cambridge Research Center, representing the Air Force in the day-to-day negotiations, believed that a prime contract was impossible at the start because revised Air Force regulations made difficult if not impossible the

justification of IBM as a "sole source." Consequently, John Marchetti, speaking for the Center, recommended a temporary sub-contract with Lincoln Laboratory.<sup>40</sup>

Although MIT and IBM pressed the matter as a matter of principle, using it as a means of inducing the Air Force to ease its contractual procedures,<sup>41</sup> both ultimately accepted a sub-contractual relationship which, it was anticipated, would within a matter of months be replaced by a prime contract between the Air Force and the Corporation. Marchetti, however, made very clear the Air Force's intent to have MIT bear primary responsibility for the program, for it would "continue to monitor the prime contract" even after the Air Force had taken over with "production money."<sup>42</sup>

On October 27, 1952, the Institute issued to IBM a Letter of Intent under the terms of which the Corporation commenced a cooperative project with Lincoln Laboratory. The contract was ultimately to involve the Western Electric Corporation, the Bell Telephone Laboratories, the Burroughs Corporation, and the System Development Corporation, and it culminated in the development and construction of the SAGE air defense system. The story of the success of the Cape Cod system and the subsequent building of SAGE, however, is not a part of the Whirlwind story. It is Whirlwind's sequel and is a story the research and development aspects of which remained still wrapped, a decade and a half later (at this writing), in the cloak of classified, national security information. For these reasons, the curtain must drop here. Project Whirlwind had run its creative,

independent course as an on-campus research and development organization and was being shunted by ever stronger external forces, such as ONR in different ways had once sought to generate, along a course that would bring it into closer alignment with the larger organization of Project Lincoln and into an increasingly subordinate, assisting role, supporting and implementing the specific technological needs of the growing military defense establishment.

It was one thing for a young graduate-student engineer to attack an aircraft simulator problem in the closing years of a great war, another thing to open a door onto undreamt-of, challenging, untrodden vistas of pioneering computer research and development waiting to be traversed, and still another to become a training and development organization ten years later, trapped in the detailed implementation work of devising third and fourth generation computers for increasingly routine, even though more complex, military assignments. When Forrester perceived what appeared to lie ahead of the organization he had built up, he found he was not particularly challenged by the prospect. Firmly convinced he could not restore the past and possessing extensive organizational experience acquired over a decade, he left the rapidly growing "computer business" in 1956 to immerse himself in the serious, academic study of principles and techniques of industrial and engineering organization.

Subsequently, Everett left Project Lincoln also. Convinced the past could not be restored but still keenly attracted to other untrodden vistas in the realm of computer research and development, he became instrumental in forming a new organization to probe the engineering unknown, The MITRE Corporation. But these are other stories, still unfolding, better told elsewhere, and not a part of this account.



NOTES TO CHAPTER 12

1. MIT, Problems of Air Defense, Final Report, Project Charles, August 1, 1951, vol. I, pp. 96-122; C. R. Wieser in Lincoln Laboratory, Quarterly Progress Report, Division 6 - Digital Computer, June 1, 1953, pp. 6-10--6-11; Brief Summary of Activity Planned by Project Lincoln During F/53, PLA-126, Jan. 9, 1951.
2. Schedule to Contract No. Af 19 (122)-458.
3. HEADC, Operational Plan, Semiautomatic Ground Environment System for Air Defense, March 7, 1955, p. vi.
4. Memorandum A-124, J. W. Forrester, subj.: "Change in Laboratory Name," September 20, 1951.
5. J. W. Forrester, Computation Book No. 49, p. 139, entry for October 1, 1951.
6. Interview, J. W. Forrester and R. R. Everett by the authors, July 31, 1963.
7. Memorandum L-32, J. W. Forrester, subj.: "Project Lincoln, Division VI Program, July 1952-June 1953," January 7, 1952.
8. Administrative Memorandum A-131, H. Fahnestock, subj.: "Accounting Procedures, DIC 6886," March 26, 1952; J. W. Forrester, Lincoln Laboratory Quarterly Progress Report, Division 6--Digital Computer, June 1, 1952, pp. 6-7.
9. Memorandum 6L-173, J. W. Forrester, subj.: "Organization and Tasks of Division 6," November 16, 1954.
10. Interview, John C. Proctor by the authors, July 13, 1964.
11. Interviews, Harris Fahnestock and John C. Proctor by the authors, July 15 and 13, 1964, respectively.
12. Memorandum L-32, J. W. Forrester, subj.: "Project Lincoln, Division VI Program, July 1952-June 1953," January 5, 1952; Memorandum L-45, J. W. Forrester and R. R. Everett, subj.: "Condensed Summary of FY 54," June 10, 1952.
13. Memorandum L-86, C. R. Wieser, subj.: "Cape Cod System and Demonstration," March 13, 1953.

NOTES TO CHAPTER 12 (CONTINUED)

14. "Brief Summary of Activity Planned by Project Lincoln during FY 53," PLA-126, January 9, 1951; Memorandum L-45, J. W. Forrester and R. R. Everett, subj.: "Condensed Summary of FY 54," June 10, 1952.
15. Memorandum L-32, J. W. Forrester, subj.: Project Lincoln, Division VI Program, July 1952 - June 1953."
16. Memorandum M-1810, A. P. Kromer, subj.: "Minutes of Joint-MIT-IBM Conference, Held at Hartford, Connecticut January 20, 1953," January 26, 1953, p. 3.
17. Memorandum L-64, C. R. Wieser, subj.: "ADEE Steering Committee Meeting at Colorado Springs, October 7, 8, 1952;" Memorandum L-66, Arthur Kromer, subj.: "Discussion of contract status with IBM," October 17, 1952; J. W. Forrester, Computation Book No. 53, p. 9, entry for November 3, 1952; Memorandum L-71, D. R. Brown, subj.: "Group Leaders' Meeting, November 24, 1952," November 24, 1952.
18. Letter, Major General D. L. Putt, Vice Commander, HEDARDC to COMRADC, subj.: "Revision of Command Policy Pertaining to ADIS," May 6, 1953; letter, Lt. General Earle E. Partridge, Commander, ARDC, to Dr. Harlan Hatcher, President, University of Michigan, May 6, 1953.
19. Memorandum L-64, C. R. Wieser, subj.: "Meeting at Colorado Springs, October 7, 8, 1952," October 10, 1952; Limited Memorandum L-65, J. W. Forrester, October 13, 1952; Memorandum L-71, D. R. Brown, subj.: "Group Leaders' Meeting, November 24, 1952," November 24, 1952.
20. Memorandum L-86, C. R. Wieser, subj.: "Cape Cod System and Demonstration," March 13, 1953.
21. Lincoln Laboratory, Quarterly Progress Report, Division AF-24 6-- Digital Computer, December 15, 1953, p. iii.
22. Memorandum L-129, D. R. Brown, subj.: "Group Leaders' Meeting, December 7, 1953," December 7, 1953.
23. Project Whirlwind Summary Report No. 28, Fourth Quarter 1951, p. 11.
24. Ibid., p. 11.

NOTES TO CHAPTER 12 (CONTINUED)

25. Memorandum L-36, R. R. Everett, subj.: "Second Bank of 1024-Digit Storage Tubes for WWI," April 3, 1952.
26. Summary Report No. 35, Third Quarter, 1953, pp. 5, 32-33.
27. Ibid., p. 33.
28. Memorandum M-1810, A. P. Kromer, subj.: "Minutes of Joint MIT-IBM Conference, Held at Hartford, Connecticut January 20, 1953," January 26, 1953.
29. Memorandum M-1321, B. E. Morris, subj.: "Fourth Meeting on Air Defense Computer," November 8, 1951, p. 4.
30. Memorandum L-45, J. W. Forrester and R. R. Everett, subj.: "Condensed Summary of FY 54," June 10, 1952.
31. Memorandum L-30, J. W. Forrester and R. R. Everett, subj.: "Digital Computers for Air Defense System," October 5, 1951.
32. Memorandum M-1327, D. R. Brown and B. E. Morris, subj.: "Summary of First Four Meetings on Air-Defense Computer," November 9, 1951.
33. Memorandum L-32, J. W. Forrester, subj.: "Project Lincoln, Division VI Program, July 1952 - June 1953," January 7, 1952.
34. Memorandum, R. R. Everett and H. Fahnestock to J. W. Forrester, subj.: "Senior Staff Meeting," March 5, 1952.
35. Memorandum L-34, H. Fahnestock, subj.: "Group Leaders' Meeting, March 26, 1952," March 27, 1952.
36. Memorandum L-46, D. R. Brown, subj.: "Group Leaders' Meeting, June 9, 1952," June 11, 1952.
37. Memorandum L-37, D. R. Brown, subj.: "Group Leaders' Meeting, April 7, 1952," April 9, 1952.
38. Memorandum L-46.
39. Memorandum L-66, J. W. Forrester, subj.: "Discussion of contract status with IBM," October 17, 1952; J. W. Forrester, Computation Book No. 53, pp. 1, 2, & 4, entries for October 17, 1952.

NOTES TO CHAPTER 12 (CONTINUED)

40. J. W. Forrester, Computation Book No. 53, p. 7, entry for October 27, 1952.
41. J. W. Forrester, Computation Book No. 53, pp. 1, 2, & 4, entries for October 17, 1952.
42. J. W. Forrester, Computation Book No. 53, pp. 6-7, entries for October 22 and 27, 1952, respectively; Memorandum M-1739, A. P. Kromer, subj.: "Summary of IBM-MIT Collaboration, October 27, 1952 to November 30, 1952 inclusive," December 3, 1952; Digital Computer Laboratory, MIT, Purchase Requisition, DIC-L 33210, December 4, 1952.

## Chapter Thirteen

### IN RETROSPECT

For all the adventures that befell Project Whirlwind and for all the changes that occurred in the aims and procedures of the Project, from the days of the aircraft simulator to the days of continental air defense, it enjoyed a remarkable constancy of identity as a team of investigators dedicated to the prosecution of the research and development enterprise. In the continuity of its style of carrying on its inquiries and in the depths of its commitment to producing practical machinery, it maintained a unity of character and a philosophy of investigation which marked it as unique among R & D projects.

In a sense, every R & D project is unique, of course, just as in another sense every R & D project is representative. Certainly, Whirlwind was both, and in these respects it offers provocative clues to the nature of the historical process in general and to the nature of the R & D process in particular. Someone has said it is the business of the past to produce a present that is different, and the lessons of twenty-five centuries of science and two centuries of scientific technology suggest that furthermore it is the business of science and R & D to annihilate their pasts to produce the novel present.

All the signs indicate this process is an evolutionary one (marked here and there, it is true, by changes so rapid as to seem revolutionary in their impact), and Project Whirlwind was no exception. Superficially the story of Whirlwind divides quite naturally into two parts which although interdependent and interrelated stand distinct. This distinction is the more evident from the fact that the divisions are chronological, covering the periods 1944 to 1950 and 1949 to 1956. Moreover, each period correlates with a distinct, minor era in the nation's transition from the Second World War, through the retrenchment of peace to the Cold War and the Korean War, and each period can be further defined by the dependence of the Project upon either Navy or Air Force financing and managerial assistance, interference, and supervision.

Looking back, one sees the years between 1944 and 1950 comprising for Project Whirlwind a period of gestation, for it was during these years that emphasis shifted from the restricted-purpose simulator to the general-purpose computer, and it was during these years that the computer was brought to birth as an operating machine. Also, these years broadly delimit the period in which the Navy played the primary role in the program, and--perhaps of greatest importance for the climate in which R & D operated--they approximate the interlude between the end of the Second World War and the beginning of the international police action called the Korean War. This was an interlude marked by a

national policy of military retrenchment which significantly affected the magnitude and momentum of the Project, and this has been the period of primary concern to this study.

The years between 1949 and 1956, reflecting the anxieties of a world divided, were marked by a re-emergence of concern over the inadequacies of the nation's air defenses and by programs mounted to study and correct those inadequacies. One of these programs led to the successful demonstration by Whirlwind of the feasibility of an air-defense command and control center equipped with the digital computer as the principal information-coordinating element. The end result was Air Force participation and the displacement of the Navy as the major source of funds and purpose. Also there resulted the assimilation of the Whirlwind team into the Air Force program and the development of the semi-automatic ground environment (SAGE) air defense system which, prior to the missile age, was intended to provide maximum security against attack from the air.

This same concept--a centralized computer of large capacity fed by geographically scattered radar sensors--was subsequently modified and applied to continental defense against missile attack. Thus the conceptions of "Command and Control" which Whirlwind had demonstrated as feasible, and in the development of which Whirlwind had played a vital role, was incorporated into the national defense structure as an essential element. Further, the conceptions

of command and control were to expand well beyond military use through application to other governmental needs and to the needs of industry and society in general, as the computer moved in the direction of becoming one day a true public utility which, so proponents argued, would rank with the telephone and the water faucet.

Such in brief was Whirlwind's national historical significance. In addition, it was significant as an instance of the R & D process in operation, for it was at once continuous and broadly predictable in its research procedures, at once discontinuous and unpredictable in its potential applications and in the inventive originality of its engineers. The Project was at the same time highly personal and individual with respect to the inventive and developmental talents displayed by its members in the administrative realm, the fiscal realm, and the technical realm. It was at the same time social and anonymous in its exploitation and coordination of the engineering talents of its team.

While the technical story could be understood only by the specialist, the general shape and color of this R & D enterprise can be appreciated by the citizen, and in addressing this case history to him the authors hope that more questions have been raised than answered. For the rise of R & D is a recent historical phenomenon which first began to emerge in its modern form in

eighteenth-century Europe, and it is still too new to be well understood. Nevertheless, with it man is already remolding the world and moving out into Space, and although Project Whirlwind becomes in this perspective an obscure enterprise that most readers will never have heard of, it presents many characteristic features and several unusual ones the perception of which sharpens our citizen's understanding of the R & D process and improves our prospects of more intelligently directing it in the best interests of our republic.

To begin, there are the accomplishments of Project Whirlwind, and of these there are several technical accomplishments that should be singled out. Forrester, looking back from the perspective of more than a decade, could find over a dozen devices, processes, and applications which Whirlwind contributed or brought to a practical working level. The details of their operation belong in a technical engineering history of the origins, development, and perfection of the Semi-Automatic Ground Environment (SAGE) air defense system, a technical story which would begin with Whirlwind (not with the Aircraft Stability and Control Analyzer) and would include the next-generation ANAFS-7 production computer that demonstrated the worth of the Whirlwind technical concepts in the military operation of SAGE. Although this is not the place to describe these accomplishments in detail, they may be enumerated briefly here to provide one essential measure

of the practical success of Project Whirlwind as an innovating engineering enterprise.

The most famous contribution was the random-access, magnetic core storage feature, which was to be widely employed in succeeding generations of faster and more compact digital computers. Marginal checking, to detect deteriorating components, was another novel and highly practical feature. The Whirlwind computer was also first and far ahead in its visual display facilities. One form of information output was a cathode ray tube display "capable of plotting computed results on airspace maps."<sup>1</sup> Associated with it was the "light gun," or light pen, with which an operator could "write" on the face of the cathode-ray tube display and provide new information which the computer could store and use. As a consequence of these two features, simultaneous man-machine interaction at will became feasible, adding to the versatility and usefulness of the digital computer.

Simulation techniques were perfected by which hypothetical aircraft flights could be programmed into the computer for study and training purposes. As a consequence the practical prospects for digital simulation were greatly enhanced, and digital simulation subsequently was richly exploited in a wide variety of fields. The crystal matrix switch designed by David Brown, the magnetic matrix switch developed by Kenneth Olsen, and the cryotron invented by Dudley Buck were all Project Whirlwind products that were to see

continuing use and development later as digital computer electronics design progressed.

The Whirlwind machine was extensively programmed to carry out the novel procedure of self-checking, including the tasks of identifying defective components and typing out appropriate instructions to the operator. Early random tube failures posed another hurdle that the Whirlwind project negotiated successfully, for scrutiny and modification of tube-fabrication techniques led to dramatic increases in length of tube life through procedures applicable to the manufacture of hundreds of standard radio tube types.

The need to send radar-gathered data long distances to a computer control center caused successful techniques to be developed by the Lincoln Laboratory for sending digital data over telephone lines and opened the way for later commercial applications. The incorporation of a computer in a control network involved the important development of a practical "feedback loop," in which the computer changed its control instructions as it received new information and thus maintained pertinent control, as when directing interceptor aircraft toward their targets in the early Whirlwind air defense tests. Here lay the prophetic significance of the L-1 and L-2 Reports of 1947, and here was another practical application of the computer--the feedback control loop--that would see continuing military and commercial use in the years to come.

A related pioneering development is to be seen in the doctoral dissertation of a Project Whirlwind engineer, William Linville, on "sampled data control theory." Linville investigated the effects of sampling operations upon the stability of feedback control loops in those situations in which different users would be sharing the services of a large computer to solve their separate problems.<sup>2</sup>

Last, and of profound influence upon subsequent computer design, was the working out for Whirlwind of the intricate systemic details of "synchronous parallel logic" -- i.e., the transmitting of electronic pulses simultaneously within the computer rather than sequentially, while maintaining logical coherence and control and accelerating enormously (compared to other computers of that day) the speeds with which the computer could process its information. As Forrester has noted, "The parallel synchronous logic worked out for the Whirlwind computer and first appearing in the block diagram reports done by Robert Everett and Francis Swain set the trend for many later computer developments."<sup>3</sup>

In addition to these technical accomplishments, Project Whirlwind also demonstrated several fundamental features of the research and development process. Two of these features of the R & D process are the twin historical phenomena of "convergence" and "divergence." There was the convergence of concerns and enterprises involving the design of new airplanes and the design

of flight trainers that lured the Massachusetts Institute of Technology into preliminary involvement with the Navy, and there was the consequent divergence represented by the findings of the aerodynamicists, which brought Gordon Brown and the Servomechanisms Laboratory into the next phase of the inquiry. As an instance of even greater strategic import, there was the multiple convergence of the various intellectual, scientific, technical, and mechanical traditions that brought the incipient computer state of the art to the position it occupied when Crawford, Forrester, Everett, and their associates in the Navy and at MIT began to explore it, and there was the divergence characterized by the writing of the L-1 and L-2 Reports and by the abandonment of the aircraft simulator for the computer. Still another example of convergence was the combination of events that produced first Valley's air defense committee and subsequently Valley's discovery of a Whirlwind already far along in construction. The divergence that followed appeared for a while as though it would cause Project Whirlwind to be swallowed up by Lincoln Laboratory, but the vitality of the former and the actions of its leaders produced another course of action.

It would be an oversimplification and a distortion of history to assert that simple, direct cause-and-effect relationships of a one-to-one nature exist between every convergence-divergence sequence or between every divergence-convergence sequence of events.

But such a schema, when loosely yet carefully applied, helps to explain the weaving of the fabric of events that typically constitutes the R & D process in particular and the larger historical process in general.

Along with the phenomena of convergence and divergence are to be found the essential evolutionary continuities and the equally essential revolutionary discontinuities which characterize the purpose and direction, as well as changes in direction, of human affairs. Fiscal, technical, and administrative obstacles (restriction of funds, erratic storage tubes, inspection visits to the Project) tested continuity of purpose and validity of policy judgments at the same time that they provoked inquiry into alternative directions and courses of action.

None of these remarks is intended to suggest that the R & D process, or for that matter history itself, is fundamentally predictable or determinant in character, but they do indicate the availability of analytical tools which render the conduct of research and development more understandable than national policy, for example, hitherto has recognized it to be. It is possible to follow the history of Project Whirlwind while laboring under the traditional misunderstanding of how science and technology are thought to interact, but more insightful avenues lie open before us. Among these is the perception that the measure of R & D -- its proper operation -- can not be taken by applying either the

traditional, impractical standards of "Pure Science" or the traditional, practical standards of technology.

Some of the difficulties the mathematicians and the physics-oriented scientists of the late Forties encountered when trying to evaluate Project Whirlwind as an R & D enterprise arose from their commitment to the historic values of pure science. While these were appropriate enough for science, they were not appropriate for R & D. Thus, when the question was asked whether Whirlwind might not be poor biscuits because it was trying to be cake, the possibility was not seriously considered that engineering, instead of science, might be the cake. The curious notion has long prevailed that products of the mind alone are somehow loftier and mightier than the products of mind and hand combined. While the "biscuit-cake" analogy (perhaps 'metaphor' would serve better) was not pushed so far as to fault Forrester for not being committed to a Newton-like or Einstein-like intellectual enterprise, the conception that math-and-physics standards might not be applicable at all apparently did not occur to the investigators.

Similarly, the distinctions drawn between the Project Whirlwind engineers' ways of proceeding and the Institute for Advanced Study scientists' ways of proceeding apparently fell on deaf ears, for all the difference it made in the way ONR or the Lincoln Laboratory conducted their affairs. (One is

tempted here to speculate on how the Lincoln Laboratory would have conducted its affairs had Forrester ever taken the helm.) It is appropriate to ask how much more attention might well have been paid to those distinctions had policy-makers felt more keenly the importance of distinguishing more perceptively between the mix of goals and procedures subscribed to by the Whirlwind leadership and the mix of goals and procedures embraced by the builders of the IAS computer. At issue here is the difference between basic research of the pure science tradition and that which is sometimes called "developmental research" in the R & D tradition.

The following archetypal metaphor between the basic researcher and the developmental researcher may make it more clear why the Whirlwind engineers' expensive way of proceeding was so difficult for ONR and its consultants to appraise. (In all fairness, it should be added that Air Force endorsement was born not out of any depth of understanding, for that of the Air Force was no deeper than the Navy's, but out of a desperate practical need.)

The Basic Researcher may work with pencil and paper and theoretical equations describing the idealized phenomena. He may also work with laboratory equipment which produces a desired, artificial, controlled environment in which certain phenomena are isolated and manipulated for closer study. The basic researcher is interested primarily in understanding and explaining the

phenomena he is examining in that environment, and he may achieve this understanding by proceeding from his paperwork to his laboratory equipment, from his laboratory equipment to his experimental results, and from his experimental results to further paperwork describing the test results, analyzing them, and relating them to existing and emerging understanding.

The Developmental Researcher, on the other hand, is more interested in devising hardware or perfecting a process and testing how well it performs. He is interested in applying the research understanding already gleaned, together with engineering know-how, to the problem of making hardware that will work. To this end, he may subject the hardware to the artificial, controlled environment that isolates and identifies the roles the phenomena play. Interested in what happens to his equipment when it is subjected to the environmental stresses imposed by "real" working conditions, he develops hardware that will exploit and profit from his scientific understanding of the phenomena and from his engineering knowledge of the materials and the working conditions. Often, in the course of subjecting his hardware to a working environment, he uncovers new phenomena or new roles that known phenomena are seen to play, thereby creating new paperwork problems and opening up new lines of inquiry for the basic researcher to pursue. Likewise, the basic researcher while concentrating on understanding the phenomena may turn up

new information and vital questions of immediate or delayed use to the developmental researcher.

In this paradigm of the basic researcher and the developmental researcher it has been assumed, to make the exposition easier, that these are two different individuals. However, it is possible for the same individual to play both roles, and the more sophisticated the R & D problem (such as that of developing the magnetic cores, for example), the more likely the same individual or the same group of investigators will play both roles before they are finished. The extraordinary insistence with which Forrester and Everett maintained their policy of circulating information heavily and rapidly and soon, in written form, among the members of the Project tended to encourage the interplay of roles, just as did the availability of quality materials and services. Although theirs was nominally an engineering enterprise, Everett when working out his historically influential block diagrams or Forrester, and subsequently Papian, when working with the phenomena of magnetic remanence, were deeply involved in both basic and developmental research because they were unwilling to stop after the conception had been set down.

The ratio of supporting-services costs to technical costs was high in Project Whirlwind, and it exposed the Project to accusations of "gold plating" practices by those who held to a philosophy dominated by conceptions centering around an

unexamined commitment to what might be called an "economy of scarcity." To such critics, the only alternative was an "economy of plenty," which could really be nothing more than an irresponsible fool's paradise inasmuch as all costs had to be recovered, and directly, even if it meant robbing Peter to pay Paul. Indeed, that was precisely what Project Whirlwind was flagrantly doing, with its insatiable demands for more money and more money and more money!

But from Forrester's and Everett's point of view, one need not be trapped into choosing only between these two polar opposites. "There is a need to subordinate these problems of balance," Everett remarked, "to a philosophy of creative force and inherent growth which tells you how to proportion your services to your technical effort."<sup>4</sup> The Whirlwind project demonstrated this, as far as Forrester and Everett were concerned in after years, yet the lesson remained unappreciated in the thronging halls of R & D, and this, too, the two men felt keenly. Although a senior executive from the International Business Machines Corporation had asked, after a first visit to the Whirlwind installation, "How do you achieve so much with so little?" it was as clear in the early Fifties when he visited the Barta building as it was a decade and a half later that the essential intangibles of the Whirlwind philosophy of conducting research and development had gone unappreciated. That philosophy involved far more than a services costs to technical costs ratio, as this case history has shown.

In a world without pain, where value judgments did not compete for selection and where the proper course of action was always discernible, there would be no need for the harassment of inspection visits by third parties, but in the world of R & D where Project Whirlwind dwelt such inspections play a healthy, tonic role, provided there is basic policy agreement about how research and development affairs should be conducted. To cite an obvious failure to agree on basic policy, an inspection team which criticized a fundamental research laboratory for its failure to maintain engineering development projects on a massive scale would only be throwing sand in the gears, however well intentioned and sincere its motives. Similarly, a legislator who criticized the National Science Foundation, for example, for supporting impractical, "out in the wild blue yonder" basic research with hard-earned public tax dollars either would be making what he judged to be the right "political sounds" for the home folks in an election year or else would be demonstrating that he really did not share the basic policy attitudes which technically versed administrators have found to be viable over the many centuries that science has flourished.

No such gross mistakes in selecting inspection visitors appear to have occurred in Whirlwind's case, for qualified personnel were selected, for the most part. Unfortunately, the qualified person is apt to be a specialist, and the specialist is as much a custodian of essential knowledge in his field as he is a creative entrepreneur. At stake here is the philosophical

commitment which science and engineering long ago made when they developed the habit of seeking the judgment of professional peers when assessing the worth of an enterprise. Whirlwind's experience with inspection visitors does not contradict the historical generalization that the peer system has proved inherently cautious and conservative, preferring only modest departures and limited leaps into the future.

Project Whirlwind appears to have been sufficiently the victim of this professional timidity syndrome to run into trouble, but the true heart of the trouble lay neither with the Project's unorthodox modes of conducting its R & D affairs nor with the premium placed upon conservative judgment by the inspection process itself. Rather, it lay in the fact that, for all their sophisticated abilities to innovate--indeed, innovation is their reason for being--the basic and the applied sciences have been unable to develop reliable techniques for distinguishing the seer from the fool. Consequently, they play it safe and endorse the modest change which does not break sharply with tradition. It was just this sort of crippling inability which caused the Ad Hoc Panel to criticize Whirlwind for its lack of a mission on the one hand while complaining that not enough attention was being given by computer projects to "real time" applications on the other.

Nevertheless, appraisal by peers is likely to remain in force, and in the R & D area, where the prospect of practical

accomplishments is a guiding consideration, it is probably a helpful technique and provides a useful mode of healthy external criticism.

Because Project Whirlwind succeeded does not mean that it was inevitably destined to succeed. It was, like all challenges, a creature of human endeavor. It did achieve its goal, however, and it did accelerate computer progress both by the concepts it demonstrated and by the talented engineers it developed. As a consequence, hindsight permits one to hail it as a model of R & D. But had its funds been cut off, it would likely have joined that ever growing number of military R & D enterprises which come to an end on the scrap heap. Whatever the Project's vitality (and it was considerable), and whatever the resourcefulness of its leadership, Whirlwind was in part master of its fate and in part a creature of larger circumstance. The words of another observer of American science come to mind. Although uttered in another context, they are relevant to Whirlwind. "We must realize," remarked W. Carey of the Bureau of the Budget in 1957, "that when science and education become instruments of public policy," -- and, we should add, of public funding -- "pledging their fortunes to it, an unstable equilibrium is established. Public policy is, almost by definition, the most transient of phenomena, subject from beginning to end to the vagaries of political dynamism. The budget of a government, under the democratic process, is an expression of the objectives, aspirations, and social values of a people in a given web of circumstances. To claim stability

for such a product is to claim too much. In such a setting, science and education become soldiers of fortune."<sup>5</sup> The story of Project Whirlwind, as well as the story of what became of this R & D enterprise in the years after 1956, is that of a soldier of fortune.



NOTES TO CHAPTER 13

1. Ltr., J. W. Forrester to C. W. Farr, December 18, 1967.
2. For an introduction into the possibilities of using the time-shared computer, the reader is directed to: D. F. Parkhill, The Challenge of the Computer Utility, (Addison-Wesley, 1966).
3. Ltr., J. W. Forrester to C. W. Farr, December 18, 1967.
4. Interview, R. R. Everett by the authors, October 26, 1967.
5. Scientific Manpower -- 1957, National Science Foundation Document No. NSF-58-21, p. 25.



THE  
MITRE  
CORPORATION  
BOX 208  
BEDFORD  
MASSACHUSETTS 01730