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IDA PAPER P-2538

DARPA TECHNICAL ACCOMPLISHMENTS
VOLUME III

AN OVERALL PERSPECTIVE AND ASSESSMENT OF THE TECHNICAL ACCOMPLISHMENTS OF THE DEFENSE ADVANCED RESEARCH PROJECTS AGENCY: 1958-1990

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July 1991

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13. ABSTRACT ***(Maximum 200 words)***

IDA has been documenting and assessing the major technical accomplishments and contributions to technological advance made by the Defense Advanced Research Projects Agency (DARPA). Phase I (published as Volume I, P-2192, and Volume II, P-2429) reviewed the origins, evolution, and impact of 49 projects or program areas. This paper (Phase II) focuses on the broader context of DARPA's contributions, including influences external to the agency. Its purpose is to describe the motivation for programs undertaken by DARPA, the intrinsic nature of the research, and the technological advances that emerged. Some programs, such as ballistic missile defense and large acoustic arrays for antisubmarine warfare, were highly successful in their own context, while others, such as computing and computer networks, had wider ramifications for national technological developments. The paper also derives lessons learned for future DARPA strategic planning and project management.

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PREFACE

It was a daunting task to capture the significant aspects of DARPA's evolution over its thirty-three year history, arrive at insights and conclusions regarding the Agency's impact and effect, and determine implications for DARPA's future. While much of the information and analysis used to prepare this volume derived from our research conducted for Volumes I and II of this study, we also had to capture broader perspectives regarding DARPA's overall program thrusts and motivations, concepts that went beyond individual programs and projects. We found documentation of these broader aspects relatively sparse, with some being captured in Congressional testimony made by the Directors of DARPA, the DDR&Es, and others on their staffs. A book prepared in 1975 by Richard A. Barber and Associates is the only detailed effort to capture the early history of ARPA. We used this manuscript as one source, but we also referenced literature dealing with particular points of history, for example, Dr. Herbert York's *Making Weapons, Talking Peace*. We interviewed several former DARPA Directors and DDR&E's and had them review our assessment and analysis. For DARPA's history after 1975 we found no existing study or research that covered the period or any significant aspects of it. Therefore, we relied on primary sources of testimony and interviews to augment our own research for Volumes I and II into the individual program thrusts of this latter period.

We particularly wish to thank the following individuals for providing us their insights and perspectives on DARPA and for reviewing earlier drafts of this manuscript: Former Directors, Defense Research and Engineering, Dr. John S. Foster, Jr., Dr. Malcolm R. Currie, and Dr. William J. Perry; and former Directors, DARPA, Dr. Eberhardt Rechtin, Dr. Stephen Lukasik, Dr. George H. Heilmeier, Dr. Robert Cooper, and Dr. Craig I. Fields. We have attempted to capture their views and perspectives accurately and fairly. From these knowledgeable and interested sources we were gratified to receive a variety of very useful, thought-provoking ideas for either elaboration, modification, or additional development. Many of these we were able to accommodate in the scope of this paper, but others would have extended our endeavor beyond the scope and resources available. These ideas and provocations encourage us to consider ways to continue the intellectual interchange this study has opened up.

From these comments and suggestions it is clear that DARPA is seen by these former Directors as a vitally important organization that provides a unique capability to the Department of Defense and the country. Yet, efforts to attain perspective on and engage in retrospective and introspective assessment about DARPA and its role have been very few and limited in scope. As DARPA and the Department of Defense in general confront the changing world environment, we hope this volume provides some useful insight, and that DARPA and the DoD continue to sustain DARPA's vital role of supporting advanced research and development into the technologies that the United States will need for its vitality and strength in the future.

CONTENTS

FIGURES

GLOSSARY

ABM	anti-ballistic missile
AFTI	Advanced Flight Technology Integration
ALL	Airborne Laser Laboratory
ARPA	Advanced Research Projects Agency
ATA	Advanced Test Accelerator
ATR	automatic target recognition
BMD	ballistic missile defense
DARPA	Defense Advanced Research Projects Agency
DDN	Defense Data Network
DDR&E	Director of Defense Research and Engineering
DRB	Defense Resources Board
DSB	Defense Science Board
DSO	Defense Science Office
EEMIT	Experimental Evaluation of Major Innovative Technologies
FOFA	Follow-on Forces Attack
GDL	gas dynamics laser
ICBM	Intercontinental Ballistic Missile
IDL	Interdisciplinary Research Laboratory
IPTO	Information Processing Techniques Office
IR	infrared
ISTO	Information Science & Technology Office
JSTARS	Joint Surveillance and Target Acquisition Radar System
MAC	Machine Aided Cognition
MIRV	Multiple Independently Targeted Reentry Vehicle
MOU	memorandum of understanding
NASA	National Aeronautics and Space Administration
NASP	National Aerospace Plane
OMB	Office of Management and Budget
OTH	over-the-horizon

PGM	Precision Guided Munition
PM	Program Manager
PRESS	Pacific Range Electromagnetic System Studies
PSAC	President's Scientific Advisory Committee
R&D	research and development
RPV	Remotely Piloted Vehicle
SCP	Strategic Computing Program
SDI	Strategic Defense Initiative
TTO	Tactical Technology Office

EXECUTIVE SUMMARY

This is the third volume of an extensive review of selected DARPA projects and programs from the inception of the Agency until the present time. The purpose of the review has been to describe how ARPA and then DARPA influenced the world of defense technology and technology more broadly. The histories of some 49 projects and programs are described in detail in the first two volumes of this work. The purpose of this third volume is to present an integrated overview of the Agency's evolution, its work, the influence of that work on the technology of operational systems, and the conditions making for successful transfer of the Agency's work into applications. This perspective is intended to help DARPA in planning future program strategies and in effecting successful transfers to using agencies and activities.

The project and program histories of the first two volumes show that DARPA has made many highly significant contributions to defense technology and to technology of interest to the world in general. DARPA efforts have been initiated by many means, from assignments by the Secretary of Defense (sometimes with the President's urging or at least explicit approval) to DARPA initiation of major efforts with the Secretary's and Congress' agreement. Measures of success are highly variable, ranging from direct transfer of a successful development (such as the ARECIBO radio telescope), through transfer of successful technology applied in Service systems, such as the FPS-85 phased array radar or the SURTASS long, towed acoustic array for ASW, to partial or indirect transfer of knowledge and capability that was applied in Service and civilian programs. In most of the areas of success, while there was a history of ongoing work or existing concepts, DARPA acted as a catalyst to achievement that might otherwise have been long delayed or ever lost.

Three technology areas stand out as ones that have endured with significant contributions through many projects and phases during most of DARPA's history. They are: sensing and surveillance; information processing; and directed energy weapon technology. DARPA made seminal contributions in several key aspects of these technologies. It is fair to say that in the information-processing area DARPA-supported work was responsible for the revolution in information processing and transfer technology that has seen networked computers and desktop computing sweep the world, and is now having a similar impact on parallel processing.

DARPA success in developing technology and transferring results has depended on management techniques and environments that can be characterized by:

- Its ability to attract extremely good people to develop and manage its programs;
- Extensive autonomy for the project or program managers, and a short command chain with few if any intermediaries from the program manager to the Director;
- Its fostering and drawing upon the whole U.S. technical community;
- Rotation of knowledgeable people with energy and ideas as the program has changed;
- A civilian/military staff mix that has helped to bring an operational and Service flavor into much of the output; and
- Contracting through the Services and involvement of potential using communities in the work.

Most successful projects have had some form of Service participation, even in cases where the Services may have had reservations, initially or throughout a project, about accepting or participating in the work directly.

DARPA has gone through three major periods in its lifetime: (1) a period, into the mid-1960s, dominated by the "presidential" issues that brought the Agency into being in 1958 after the launch of Sputnik--space, ballistic missile defense, and nuclear test monitoring; (2) a period of exploration and probing from the mid-1960s to mid-1970s, characterized by relatively diverse research programs, varying from R&D support for the Vietnam war effort to the beginnings of DARPA's long history of involvement in advanced computing; and (3) a period of major technological thrusts that began in 1975 and has continued until the present. The DARPA budget was high during the first period, but declined when early major programs were transferred to the Services. The budget was low, and declined more than the DoD's technology base R&D program as a whole, during the second period, and reached high levels again during the third period, commensurate with the high costs of the system work the Agency undertook [Figs. S-1, S-2].

Figure S-1 overlays on the constant dollar budget a breakout of many of the most important DARPA program areas from a macroscopic perspective. The transition from a

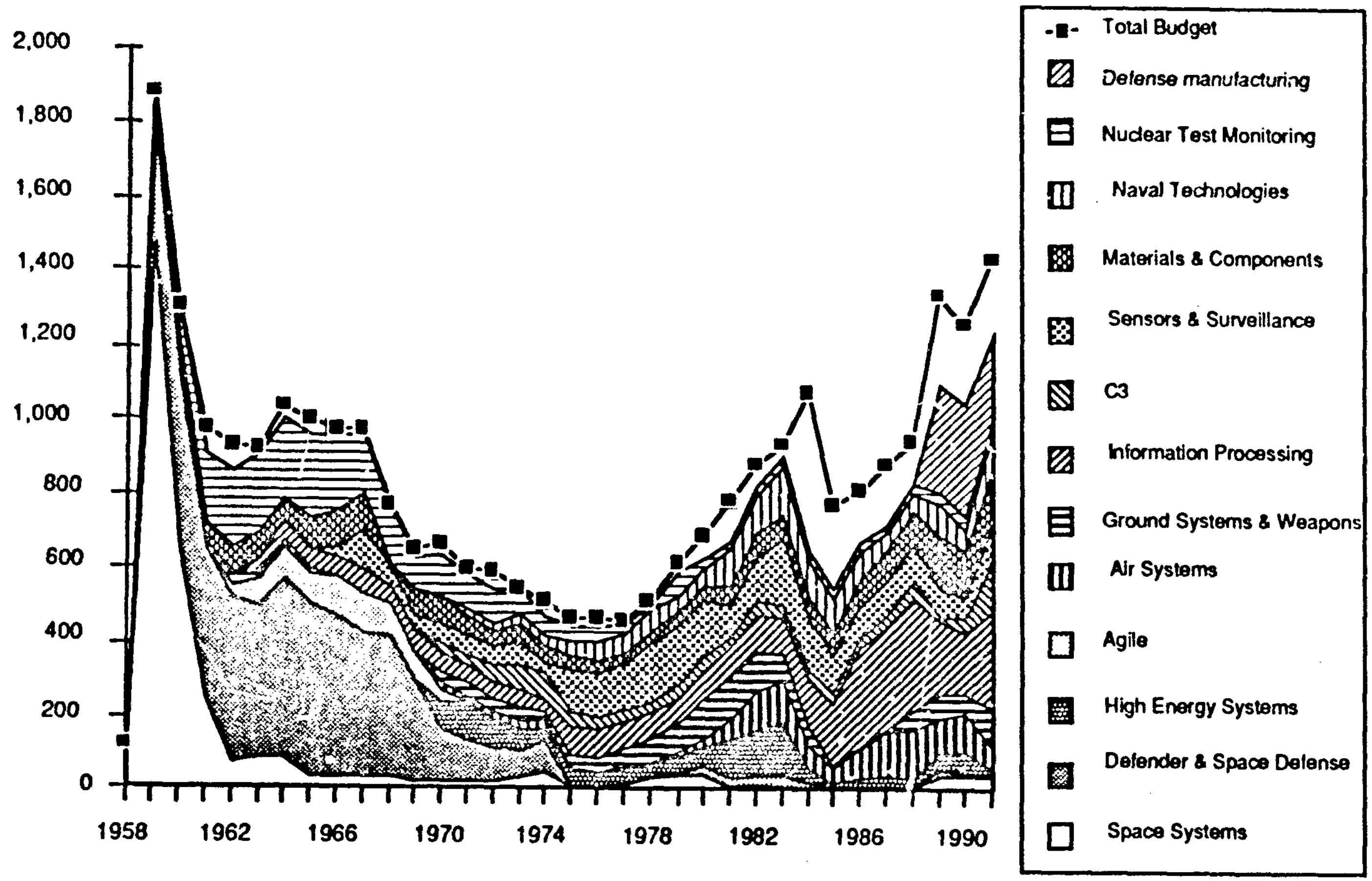
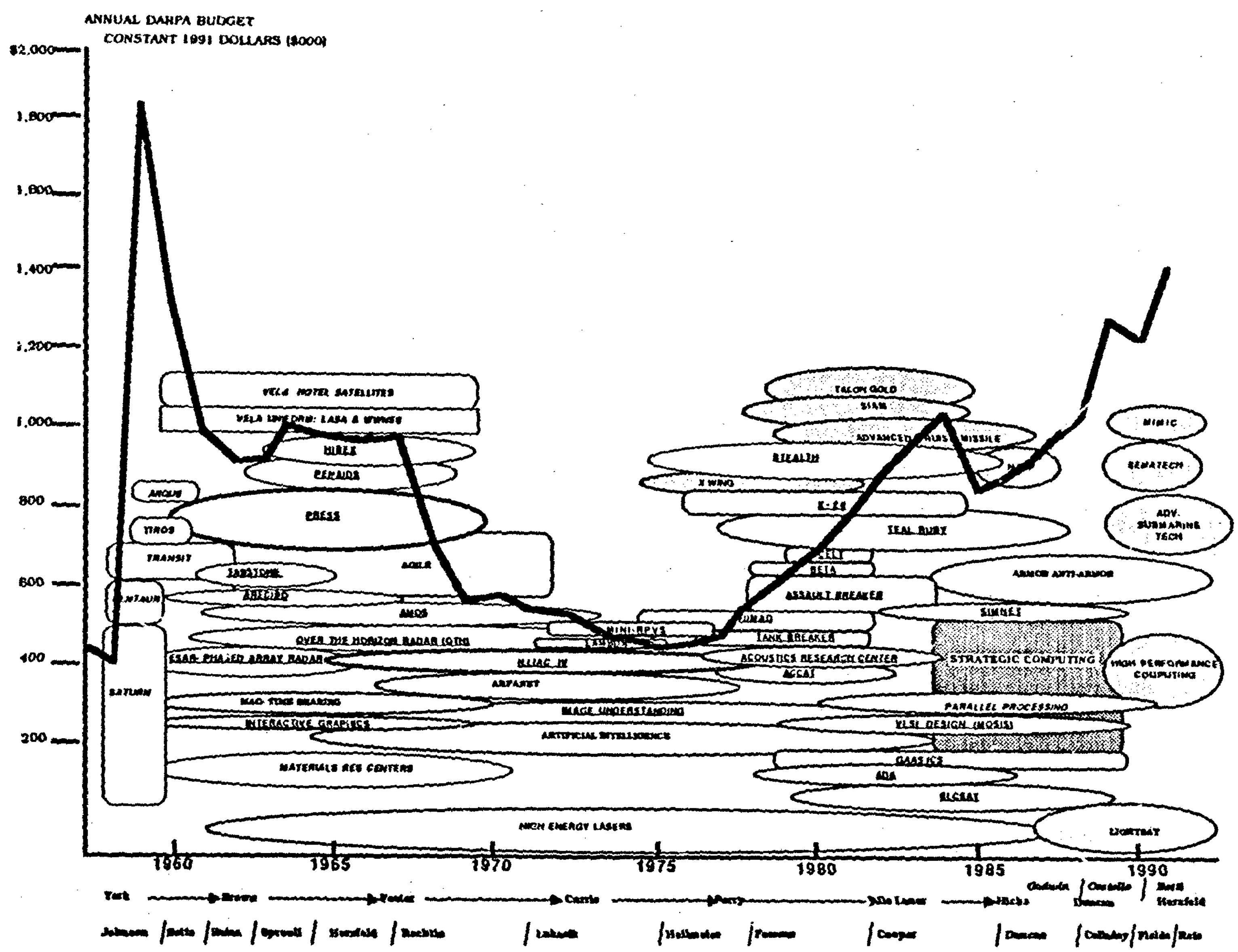


Figure S-1. DARPA Funding Thrust (Constant 1990 Dollars)



*All programs shown except those shaded are reviewed in Volumes I and II.

Figure S-2. Timing and Relative Size of DARPA Programs in Accomplishments Study space-oriented agency in the early years, to one focused on nuclear weapons concerns (DEFENDER and VELA) in the mid-sixties is clearly evident. These two programs accounted for 60 to 70 percent of ARPA's funding at this time. Counter-insurgency R&D (AGILE) was another important program during this time, but its directly attributable funding peaked at around 10 percent of the budget. With the termination of AGILE, the reduced VELA program, and the transfer of DEFENDER, the program profile in the late sixties and through the seventies takes on a much more diverse character. It is important to note that several programs derivative of the AGILE and DEFENDER work were reorganized, combined with other programs, which continued at relatively low levels during this period, notably sensing and surveillance, and directed energy.

As the funding levels reached their lowest levels in 1975, the newly defined program initiatives of Director George Heilmeier become evident as major expansions of funding in sensors and surveillance, strategic technologies (particularly space-based high-energy systems), naval technologies, STEALTH, and tactical systems for meeting Soviet armor superiority relative to NATO--particularly the ASSAULT BREAKER program. By the mid-1980s the very large increase in attention to information-processing technology, as concentrated in the Strategic Computing Program, is visible; air systems funding also increased relative to proportionate decreases in strategic technologies (transferred to SDI) and to tactical systems (with transfer of ASSAULT BREAKER, TANK BREAKER, and other related technologies). The late 1980s are characterized by an added major new thrust into defense manufacturing on top of existing program areas.

Historically, the first period was driven by the need to regain what at the time appeared to be lost U.S. technological leadership in space and strategic systems. The second was dominated by the stresses generated by the Vietnam war and the search for new areas of potentially great impact following the transfer or de-emphasis of its initiating programs. The third period was devoted to helping the country recoup the loss of the U.S. military position in the NATO arena (following years of de-emphasis and relative Soviet build-up) by leveraging our technological superiority.

With the subsidence of the Soviet threat, DARPA and the country now face a further period of strategic uncertainty, in which DARPA management must decide how the agency can best continue its significant contributions to the technology of national security. We identify what we feel are the most significant issues for DARPA's strategic planning, noting that DARPA faces a difficult task of finding a sure path for itself when the country and DoD are groping. *This is a new situation compared to DARPA's earlier history--when* *it got started, or when it changed direction, the country knew where it wanted to go. Today this condition does not hold; the country still has not found its preferred new military R&D directors.* To deal with this situation DARPA faces decisions regarding the following:

- Toward what threats and military needs should its research programs be oriented?
- What technologies have the potential of fundamental impact for the future and should be pursued?
- What should be DARPA's links with the civilian world?
- How should investment risk be balanced against potential for payoff?
- How should programs that are currently the focus of interest and accomplishment evolve in the new environment?

In our view, DARPA's endeavors in the future must be driven by the same precepts that were the basis for its successful work in the past: (1) their potential for making a difference for a perceived national security need, and (2) the need for a special place to pursue the work. Sustained efforts by DARPA should focus on enduring problems that are of great significance but are very difficult, and technology areas that are just emerging where additional research creates its own opportunities as the area evolves. Within these precepts a range of possible emphases for DARPA's future posture can be defined including:

- **A focus mainly on technology exploration or search aimed primarily at generic application**

  Given the ambiguities pervading the security arena, DARPA's most effective role might be in fostering generic technologies and the overall technology base, rather than concentrating resources on specific technology applications. It seems to be the case that many of the thrusts of 15 years ago have essentially played out and reached fruition, the threat environment is uncertain, and the prospect of new acquisitions to employ major technological breakthroughs is low; thus, it would be reasonable for DARPA to concentrate on a technological search mode, similar in some ways to the early- to mid-1970s. In essence this perspective suggests that on-going thrusts should be carefully assessed to determine whether they are reaching diminishing returns and carrying on mainly through their own momentum and whether they still meet the primary criteria for DARPA investment: high potential for making a difference in an area of defense importance and requiring the special offices of DARPA.

- **A focus on shaping the advanced defense technology base**

  DARPA could become the focus for advancing the DoD technology base and bringing it to the point where DoD could capture new directions of development of military systems from the DARPA output. In this strategy, DARPA would concentrate in the 6.1 and 6.2 areas; it would establish links to the DoD laboratories and any mechanism newly established to manage them, and it would, by judicious support of projects and programs at the labs and in industry, pave the way for advances in new areas of military technology. This could include building "fieldable" prototypes and small systems or subsystems for new technology insertion in existing systems, following the DoD technology strategy. This might also include extensive programs in such areas as software engineering, manufacturing technology to reduce costs of highly variable, few-at-a-time procurements, and other approaches to productivity increase under the new conditions in which DoD must operate.

- **A special-mission-oriented DARPA**

  In keeping with its early history, DARPA could be re-focused as a special-mission organization, designed to meet fundamental challenges facing the DoD and possibly the country, to which the DoD has been asked to contribute. Such initiatives could include:

  - great expansion of simulation networks for training military forces and evaluating new equipment in combat-like situations, while saving much of the cost of doing so in the field;
  - "foraging" for civilian technology applications to military systems, as recommended by the DSB, and demonstrating those applications in prototype military equipment of suitable size and character;
  - a program to advance our national "technological security" in the manner of High Performance Computing, SEMATECH, High Definition Systems, and other high-technology "seed corn" related to defense technology development and applications. Underpinning this thrust is an understanding that the technology posture of the United States, relative to that at the time of DARPA's inception, has changed fundamentally, and requires new ways of DARPA and DoD overall to interact and cooperate with other Federal departments to meet the challenge. In fulfilling such a role programs would have to be structured and executed in such a manner as to ensure that in the process DARPA does not become excessively distracted or disabled from pursuing its main mission--developing advanced technology for the nation's defense.

Clearly, all these modes of operation need not be considered as mutually exclusive. DARPA could, subject only to budget limitations, undertake to follow elements of several or even all of these options in a program tailored to suit conditions and the desires of its higher level sponsors and the national need. But it would have to be careful not to make the mix so random that no clear management and program pattern shines through. The history examined here shows that DARPA efforts had their greatest success when there was a clearly defined sense of mission and direction in the agency and DoD.

I. INTRODUCTION

A. PURPOSE

The DARPA Accomplishments project began in late 1987 as a study of how DARPA over its history had influenced defense technology and technology developments more broadly and, to the extent possible, to derive lessons useful for DARPA's future ability to respond to national defense and security needs.[1] At the time, with DARPA approaching its 30th anniversary, there was a feeling that DARPA programs had had substantial impact and influence, and that it might be useful to document that impact and the key factors underlying it. However, there was only sporadic, piecemeal information available for such a retrospective. For example, a book had been compiled on the VELA program,[2] and a somewhat controversial ARPA history had been produced in 1975, under ARPA sponsorship, by Barber & Associates, that focused mainly on management aspects, rather than on technical content, and covered only the years up to 1975.[3] Therefore, DARPA management tasked IDA to provide a systematic documentation of selected research programs that could bring out aspects of the Agency's influence over technology development and application that would be helpful for DARPA's planning and management needs.

The work conducted in this study has focused on the technical, substantive content and impact of selected ARPA/DARPA programs. The focus of the retrospectives documented in Volumes I and II was on the following questions: (1) what were the origins of each project or program, (2) what did DARPA itself do, and (3) what was the result, impact, and effect of the work that DARPA supported? Technology transfer, transition to application or use, clearly has been a key element of this latter concern--but it is not the only "success" criterion. The impacts of DARPA's programs have been extremely varied. Indeed, programs that some have tagged as "failures," others have seen as major accomplishments. Some of the "failures" may have had significant influence elsewhere,

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1 DARPA Project Assignment to the Institute for Defense Analyses, A-119, October 1987.
2 *The VELA Program*, DARPA, 1985.
3 Richard J. Barber Associates, *The Advanced Research Projects Agency, 1958-1974*, December, 1975.

while some "successes" may have had little downstream effect. In some cases this may be due to different views of what the project itself did or led to, in others it may be related to different views or criteria of how DARPA should assess and even manage its programs. These disagreements and debates often are revealing about specific DARPA programs, but are perhaps even more of interest in assessing the Agency's approach to stimulating research and development into new concepts, technologies, and applications for the Department of Defense.

The focus of the first two volumes on specific research programs was useful for eliciting the details of what DARPA actually did in the selected projects, and what their explicit substantive results and impacts were. We were aware, however, that this project-by-project perspective did not capture some of the broader, more general aspects of the Agency's overall impact, orientation, and activity. This became even clearer from the review comments on Volume I by certain individuals, particularly some former Directors of the Agency, who felt that some of the overall motivations and intent of the Agency's research program were not adequately conveyed by the project-specific write-ups. In this paper we attempt to stand back from the individual projects and present a more synoptic view, by which we endeavor to capture the overall scope and purpose of ARPA's programs and their evolution as a whole, and to provide a perspective that the individual program discussions may not have conveyed. The purpose of the integrated overview provided in this volume is to draw broader lessons from the work that will be useful to DARPA in current strategic planning.

B. ORGANIZATION OF VOLUME III

Thus, this Volume provides an overall assessment and perspective regarding the evolution and impact of DARPA's research, based upon the work on assessing individual programs reported on in Volumes I and II, and the perspective we have gained from additional discussions and interviews and research conducted on broader aspects of ARPA and DARPA research. We have organized this overall review into the following sections:

Section 2 - Overview of DARPA Program Evolution

Gives an overall picture of ARPA/DARPA funding and programs from its inception in 1958 through 1990, providing a synopsis covering the initial establishment of the Agency and how it evolved.

Section 3 - DARPA in Historical Perspective

Looks at how DARPA was influenced and affected by the larger environment of U.S. national security concerns, historical events, and other aspects of the world environment.

Section 4 - Enduring DARPA Technology Program Areas

Describes a set of technology areas in which DARPA has had an enduring, persistent involvement; provides a view of the relative emphases of these program areas, how they evolved, and their effect.

Section 5 - DARPA Program Initiation and Management

From what was learned about individual DARPA programs, we synthesize and draw inferences about Agency program initiation and management, the degree to which programs resulted in transfer or had other impacts, and other characteristics that led programs to have successful results.

Section 6 - Implications for Planning DARPA's Future

Based on the research accomplished and the inferences and perspective drawn from it, we derive some implications for DARPA's future strategic planning needs.

II. OVERVIEW OF DARPA PROGRAM EVOLUTION

A. OVERVIEW OF ARPA PROGRAMS--1958-1990

Initially established as the Advanced Research Projects Agency (ARPA) in 1958, and re-named the Defense Advanced Research Projects Agency (DARPA) in 1972, DARPA has experienced a history characterized by several periods of very rapid growth and periods of regrouping and redirection. These ebbs and flows can be attributed to a number of factors including changes in the national security environment, the changing world of technological opportunities and directions--some the result of DARPA's own successes--and differing emphases and budgetary exigencies within the Government.

DARPA's history can be depicted as three major periods, each having decidedly different characteristics: (1) the period of its establishment to the mid-1960s, during which it was dominated by a few, very large "presidential issues"--space, missile defense, and nuclear test detection; (2) a period of exploration and probing, from the mid-sixties to mid-seventies, characterized by relatively diverse research programs, and assigned special programs (e.g., AGILE); and (3) a period, starting in 1975 and extending to the present, of large-scale thrusts in several "high-risk" areas building in large part on the earlier exploratory research. Within this broad pattern, the following more detailed evolution can be identified:

1958-60 Establishment in response to Sputnik with primary focus being the "Presidential Issues" of space; Ballistic Missile Defense (DEFENDER) and nuclear test detection (VELA); avoiding future "Sputniks" as its broader overall charter.

1960-65 With civilian space programs transferred out to NASA and most military space programs back to the Services, concentration on ballistic missile defense (DEFENDER) and nuclear test detection (VELA); beginning of AGILE program supporting counter-insurgency R&D related to burgeoning war in Vietnam. Basic research programs in computer processing, behavioral science, and materials begun.

1966-75 With DEFENDER transferred to Army, a period of reformulation and re-organization. ARPA under strong pressure, both within DoD and from Congress, regarding relevance and appropriateness of its research, and from Services about its duplicating their efforts, or encroaching on their interests. Emphasis on joint Service-ARPA programs and transitioning programs to Services. The 1972 Mansfield Amendment further emphasized defense-specific research. With transfer of mature projects, an exploratory period during which projects tended to be less focused around major thrusts or themes.

1976-80 Major new thrusts begun in several "high risk, high potential payoff" areas with concept of major technology demonstrations. These included STEALTH, Space-based Infrared Surveillance (TEAL RUBY), stand-off follow-on forces attack (ASSAULT BREAKER), and several aircraft (X-29, X-Wing). Research programs (information processing, materials, etc.) were challenged to demonstrate defense-relevant applications. Strong high-level interaction among DARPA, Services, and JCS, with DDR&E and Secretary of Defense support to establish thrusts.

1980-85 Review of major thrusts and transfer to Services of several: STEALTH, ASSAULT BREAKER, X-29, TEAL RUBY. Strategic technologies transferred to SDI; new effort to leverage computing advances into defense applications through Strategic Computing Program.

1986-90 Culmination and transfer of several key thrusts, assignment from OSD of Armor-Antiarmor and "productivity" oriented programs: SEMATECH, MIMIC, STARS software initiative. Undersea Warfare thrust and LIGHTSAT, small-satellites for tactical C^3, begun.

One indicator of the perturbation in DARPA programs is funding, another is the composition of the research program itself.[1] The funding history for ARPA/DARPA, in *then year* dollars [Fig. II-1] shows its remarkable start up with the Space Program with funding in 1959 of $485 million, its precipitous decline following the transfer of these programs to between $250 and $280 million during the period 1962-1965. The funding declined from 1966 to 1969, reaching under $200 million, and stayed nearly constant,

1 As will be discussed in greater length below, the *amount* of funding per year is not as significant as the content of the program and what underlay the changes in funding levels. Some periods of relatively lower funding were the result of the transfer of mature programs to the Services, during which time new programs were initiated that later became major thrusts. Thus, funding levels are indicative of the overall program foci and thrusts in DARPA, and basic changes in funding are indicative of major changes in DARPA--changes that are explained by a variety of factors on which we endeavor to provide some insights in this paper.

hovering around $200 million from 1970 until 1977. From this time funding increased sharply every year through 1984, when it reached $865 million. In 1985, the transfer of SDI-related programs brought a dip down to just under $640 million. From 1986 to 1991 funding levels again increased precipitously, reaching $1,451 million.

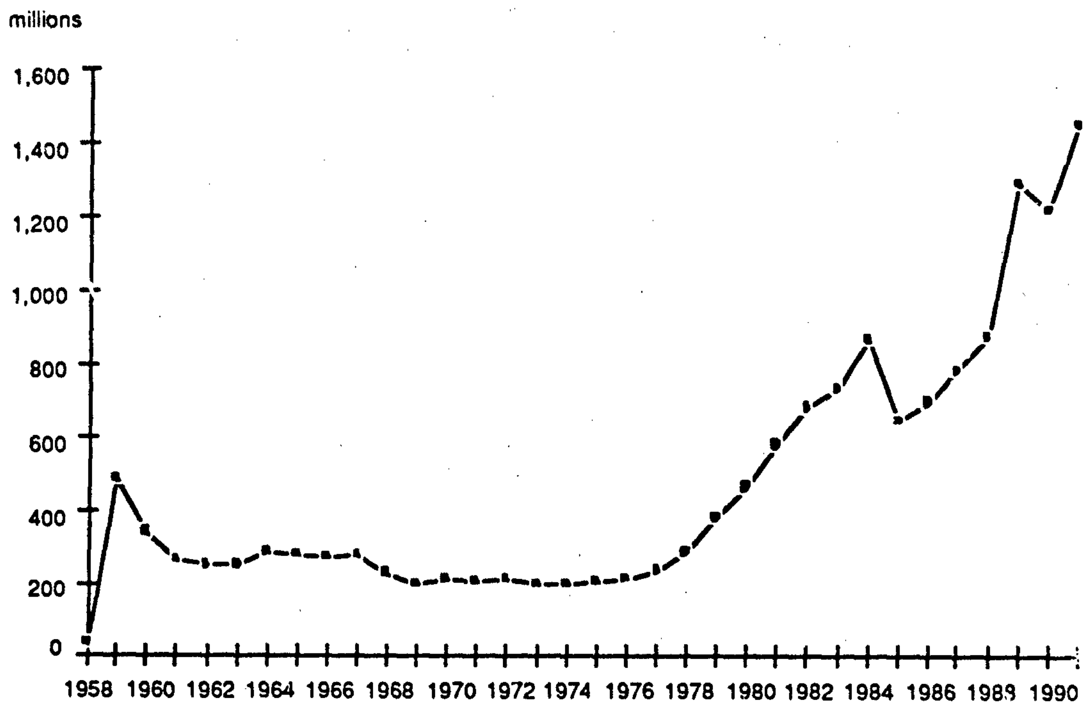

Figure II-1. DARPA Budget 1958-90 (Then-Year Dollars)

Figure II-2, showing the ARPA/DARPA budgets in *constant* 1990 dollars, evidences a more significant variation in the Agency's funding. In particular, the relatively high inflation in the seventies meant that the constant funding based around $200 million bought decreasing amounts of research and development. From this perspective, the original funding spike of 1959 is the equivalent to a $1.8 billion program in today's dollars, and the post-space program ARPA of the sixties was the equivalent of a $1 billion program--a level which DARPA did not achieve again until 1989.

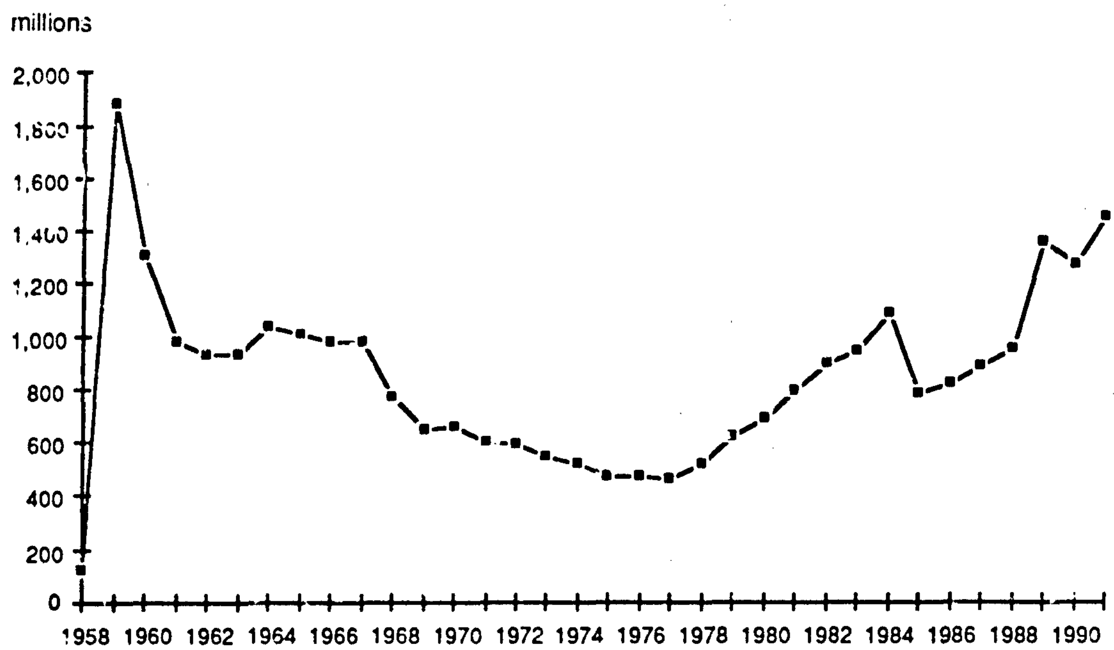

Figure II-2. DARPA Budget 1958-1990 (Constant Dollars)

Looking at the funding profile of DARPA in retrospect, with the benefit of hindsight, we are struck by what it shows about the Agency's adaptiveness and flexibility, as well as some of the constraints that it operated under, particularly during the tumultuous period at the end of and immediately following the Vietnam War. We can see that by the mid-1960s the Agency had executed successfully the main research and development responsibilities for the three key areas that were well defined at its origination--space, BMD, and nuclear test detection. As these were carried out decisions were made in the first two areas to transfer these programs to others for implementation, and in the third, with the signing of the Test Ban Treaty, to reduce effort accordingly. Having reached a point of maturity in these areas, they were ready for routine exploitation--in essence ARPA had done its job up to that point.

The issue facing the ARPA Director and the DDR&E was: what should the Agency pursue next? What were the high potential areas of research that should be explored? Even at its inception ARPA had probed into new areas where such prospects might evolve, particularly in its early information-processing and behavioral science research. But with the transfer of the major "presidential initiatives," the Agency went from having a set of large-scale, overarching research thrusts to a period of carrying out smaller assigned projects and exploring potential new directions and ideas that might be the basis of major thrusts in the future. Such an exploratory enterprise, by its very nature differed in character and scale from that at the beginning. In retrospect this re-orientation of ARPA is itself noteworthy and is given specific consideration later.

Program content is in our view more important than funding levels per se. Figure II-3 overlays on the constant dollar budget a breakout of many of the most important DARPA program areas from a macroscopic perspective.[2] The transition from a space-oriented agency in the early years, to one focused on nuclear weapons concerns (DEFENDER and VELA) in the mid-sixties is clearly evident. These two programs accounted for 60 to 70 percent of ARPA's funding at this time. Counter-insurgency R&D (AGILE) was another important program during this time, but its directly attributable funding peaked at around 10 percent of the budget.[3] With the termination of AGILE, the reduced VELA program, and the transfer of DEFENDER, the program profile in the late sixties and through the seventies takes on a much more diverse character. However, it is important to note that several of the new programs were derived from the AGILE and DEFENDER work--reorganized, combined with other programs, and continued at relatively lower levels during this period, notably sensing and surveillance, and directed energy.

During the late 1960s and into the 1970s a diverse set of relatively small, essentially exploratory research programs were pursued. Many of these were organized into newly defined project offices, such as the Strategic Technology and Advanced Engineering

2 This funding breakout is derived from the best judgments of the authors based on DARPA documentation and Congressional records. It is important to note that over its 30-plus years, successive ARPA/DARPA management teams accumulated their research funding into programs and categories differently, and that the degree of detail varied significantly at different times. It is also necessary to point out that a significant amount of DARPA's programs, particularly in the post-1975 time period, was highly classified and that it is likely that some of the funding we have categorized in these programs is distributed in other classified areas. Moreover, some of the programs could easily be classified across many different categories; for example, sensing *and* command and control, *and* information processing, *and* space systems. Where possible and discernible, we have distributed funding across these categories, but the information available for doing this was not always sufficient.

3 However, because of the three overseas offices in this program (Saigon, Bangkok, and Beirut) nearly half of the people in ARPA were associated with AGILE.

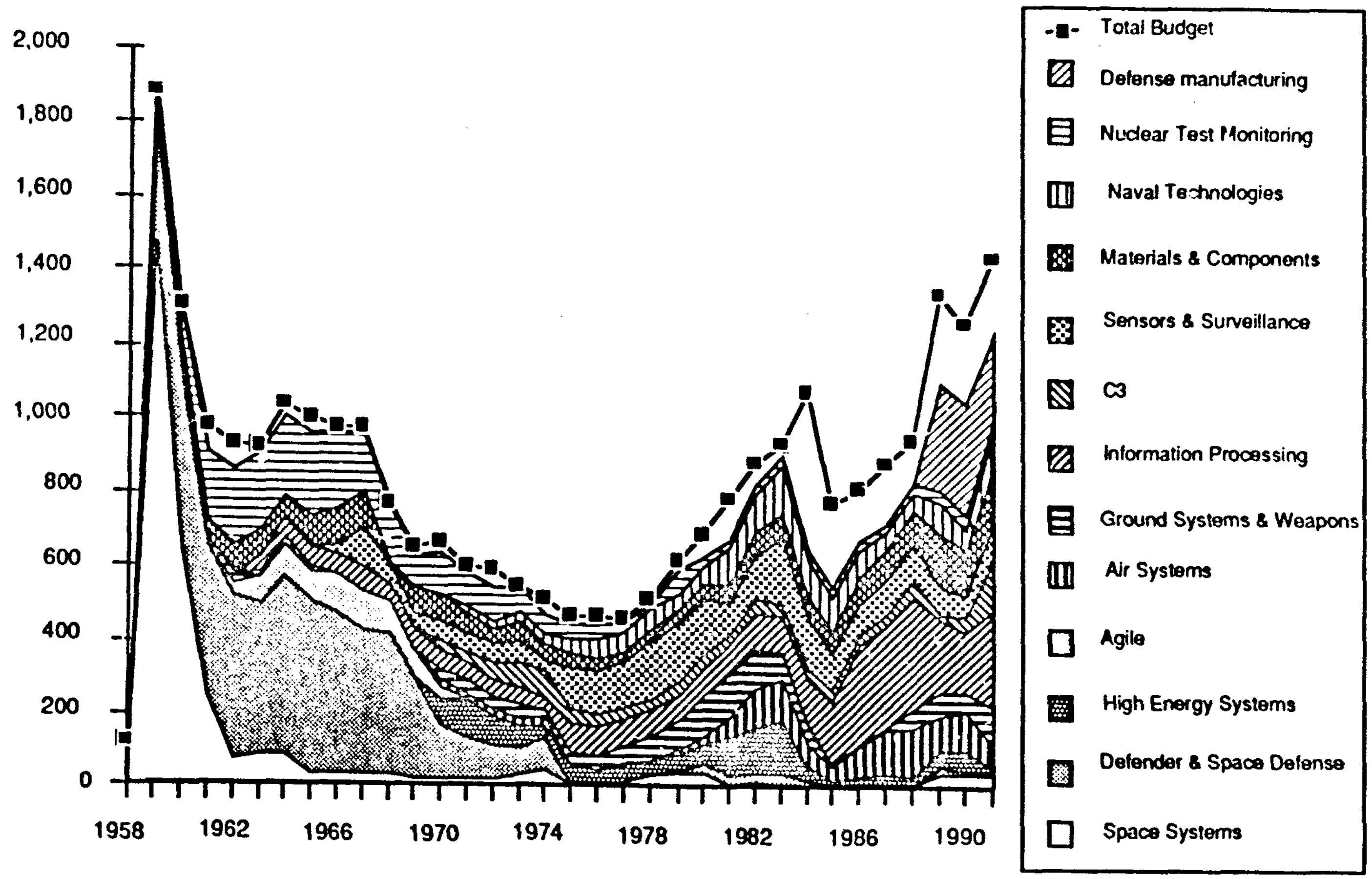

Figure II-3. DARPA Funding Thrust (Constant 1990 Dollars)

Offices, and subsequently the Tactical Technology Office, while the Information Processing Techniques Office became relatively more prominent and visible.[4]

In the linchpin year of 1975, decisions were made that resulted in a fundamental redefinition and refocusing of DARPA. The Agency defined and got funding for a set of large-scale, ambitious, defense-related technology demonstration programs. Importantly, many of these were based on the results of ARPA-supported exploratory research in the preceding years, but some were essentially new departures. These thrusts became a major focal point of DARPA's effort and accounted for much of its large funding increase over the next decade. By the end of this period, these thrusts had largely demonstrated their potential (some showing tremendous prospects and others their lack thereof) and DARPA faced decisions on (1) transferring or terminating several of the programs that had been started and (2) defining what directions to take next. By 1990, with the radically changed geopolitical and military environment, and some important changes in the economic situation as well, DARPA again faces the issue of how to define its programs for the future in a more constrained and uncertain atmosphere. We discuss this issue further in Section VI, below.

The newly defined program initiatives become evident as major expansion of funding in sensors and surveillance, strategic technologies (particularly space-based high-energy systems), naval technologies, air systems, and ground systems. By the mid-1980s the very large increase in attention to information-processing technology, as concentrated in the Strategic Computing Program, is visible; air systems funding also increased relative to proportionate decreases in strategic technologies (transferred to SDI) and to ground systems (with transfer of ASSAULT BREAKER, TANK BREAKER, and other related technologies). The late 1980s are characterized by an added major new thrust into defense manufacturing on top of existing program areas.

Amid the perturbations and refocusing of programs, there are elements of stability or continuity as well. In particular, three areas are of note: information processing, sensing and surveillance, and directed energy. Moreover, it is important to note that certain aspects of these programs are inter-related and that DARPA itself, over time, has been increasingly conscious of and taken the lead in fostering these interdependencies. These

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4 Richard A. Barber Associates, Inc., *The Advanced Research Projects Agency, 1958-1974*, Washington, D.C., December 1975, Chapters VIII and IX, details the changes in the Agency's organizational structure during this period. The Information Processing Techniques Offices subsequently was renamed Information Processing Technology Office and then the Information Science & Technology Office.

areas and their inter-linkage are discussed below under the topic of DARPA's enduring research areas (Section IV).

B. ARPA'S BEGINNING

ARPA was initially chartered in response to a specific event, the orbiting of the Sputnik satellite, that raised the specter of the Soviet Union as a technological as well as political threat to the United States.[5]

Figure II-4 shows DoD funding for R&D overall from 1955 to 1965, and compares this funding to that for ARPA. The increase in funding for ARPA (once the funding for the space program transfer is taken into account) clearly paralleled the large increase in overall DoD R&D funding. ARPA started in 1958 primarily funded by the transfer of programs from other organizations, rather than by new funds.[6] Even though the re-orientation of ARPA after the creation of NASA and the re-assignment of other space programs back to the Services was a difficult adjustment, by 1959 ARPA had already received additional assignments on ballistic missile defense, solid propellant chemistry, materials sciences, and nuclear test detection, all linked to Presidential-level concerns.[7] These became major elements of ARPA's program over the next decade.

By 1960, ARPA focused on long-term, basic research emphasizing two specific Presidential issues, ballistic missile defense (DEFENDER) and nuclear test detection (VELA), that were in the forefront of national security concerns.[8] The counter-insurgency

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5 The Sputnik launch can be characterized as a "surprise" and a "failure of judgment" rather than a threat itself. This provided evidence of fundamental lack of attention to Soviet capabilities and priorities in space and missiles, and their implications for national security. Moreover, it raised specifically the issue of scientific and technological expertise at high levels within DoD, providing impetus for both ARPA and the creation of the position of DDR&E in the Pentagon. See, H. York, *Making Weapons, Talking Peace*, Basic Books, New York, 1987, pp. 134-171.

6 Ibid., pp. 144-148.

7 York states that it was well understood in ARPA that its broad role in space programs was temporary, with the creation of NASA already in the works both in the White House and in Congress. Ibid., p. 143. A history of ARPA's early years by Richard J. Barber goes into ARPA's role in the beginnings of the space program and the issue of the non-military space program's transfer to NASA. See Richard J. Barber Associates, op. cit., particularly Chapter III.

8 See *DARPA Technical Accomplishments: An Historical Review of Selected DARPA Projects*, Volume I and II for discussions of DEFENDER and VELA programs: specifically on DEFENDER see Chapters VI through X of Volume I and Chapters I through IV of Volume II; and for VELA see Chapters XI through XIII of Volume I.

program (AGILE) was started during this period, as the Vietnam War heated up.[9] Dr. Jack Ruina, as ARPA Director from 1961 to 1963, was followed by Dr. Robert Sproull as Director from 1963 to 1965; he continued these basic thrusts. From 1961 to 1966 ARPA's budget grew steadily, peaking at just under $300 million.

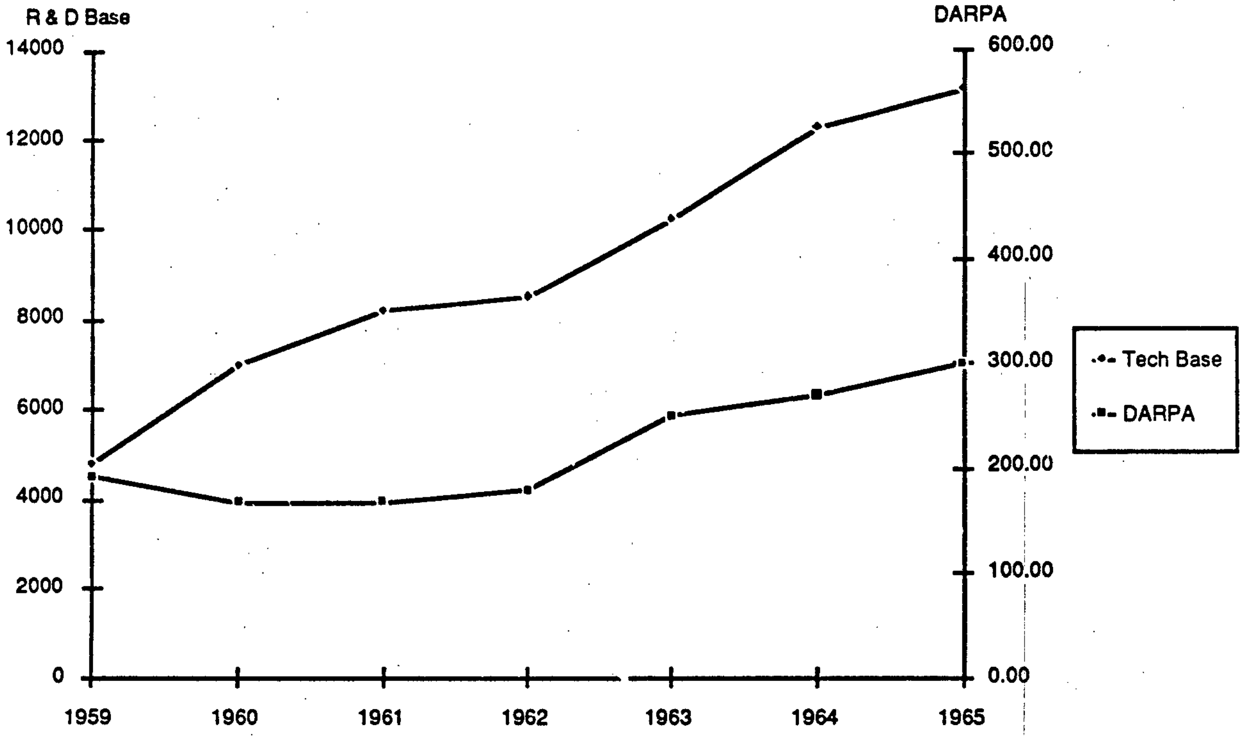

Figure II-4. DARPA and R&D 1959-1965 (Then-Year Dollars) (In millions)

From 1965 to 1967, when Dr. Charles Herzfeld was the Agency's Director, the research agenda was solidly established, and its research was maturing into visible results. DEFENDER accounted for approximately 40-50 percent of ARPA's total budget during these years. VELA funding peaked during this period, at about 20 percent of ARPA's funding, and subsequently decreased gradually, but continuously, after the Limited Test Ban Treaty. The counter-insurgency program (AGILE) also peaked during this period to $30 million, approximately 10 percent of the budget. Basic research in several technology areas grew during this time. The most notable of these, the Interdisciplinary Material

9 Ibid., Volume I, Chapters XIV through XVII for details of several AGILE projects.

Science Laboratories, reached funding of nearly $30 million, or about 10 percent of ARPA's budget.

ARPA encountered increasing concerns regarding several aspects of its research program toward the end of this period. Key elements were the transfer of DEFENDER to the Army, the political controversies surrounding the AGILE program, and the broader pressures on DoD's funding and support of basic research created by the Vietnam war. By the late 1960s, questions regarding ARPA's proper role in both basic and military-specific research became more prevalent both in DoD and in Congress. Some were even voicing the view that ARPA had "outlived its usefulness" and "was simply carrying forward activities." Deputy Secretary Vance at one point apparently advocated abolishing ARPA.[10]

C. ARPA REFOCUSING, 1967-74

After several years in which ARPA's budget grew to and stayed relatively stable at about $270 million (then year), in 1967 ARPA faced a cutback in its programs and budget. Key to this was the decision to transfer the core of the DEFENDER program to the Army, after high-level decisions were made to deploy a "thin" ABM system in 1967. While within ARPA there was strong interest in maintaining the BMD role, the DDR&E, Dr. Foster, felt it was necessary to transfer DEFENDER out of ARPA. From his perspective, the Army needed better talent to execute its advanced BMD program, and also the nature of much of the research itself had become more incremental in nature.[11]

In addition, the AGILE program as well as ARPA's behavioral sciences program came under severe scrutiny. An Army project to study revolutionary wars, CAMELOT, had created a highly contentious atmosphere in Congress, and this affected ARPA's behavioral science research programs as well.[12] But a key element underlying all of this

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10 Barber, op. cit., p. VII-3. In particular 1967 was a difficult year for ARPA with Dr. Herzfeld leaving as Director, and a 7-month hiatus until Dr. Rechtin came in as his replacement. Dr. John S. Foster, Jr., who succeeded Harold Brown as the DDR&E, while strongly supporting ARPA, also became more directly involved during this transition period in defining its program and in its management. Barber, op. cit., pp. VII-3-VII-8.

11 Dr. Foster states that ARPA was needed to *develop* the BMD technologies, but the Army did not have the talent for *implementing* these technologies into an operating system. This, in his view, necessitated the transfer of people with the program to the Army. This transfer of people with the transitioning of a mature program has occurred several times in DARPA's history and is a hallmark of some of its most successful transfers into practice. Note from Dr. J.S. Foster, Jr., 5/2/91 and discussions on 5/29/91. See Barber, op. cit., pp. VIII-29 to VIII-37, for details on DEFENDER's transfer.

12 See Seymour J. Deitchman, *The Best Laid Schemes: A Tale of Social Research and Bureaucracy*, MIT Press, 1976.

was growing frustration over ARPA's involvement in Vietnam.[13] There were two centers of opposition to AGILE: (1) the Services and some members of Congress complained that ARPA was not doing advanced research but "mission-oriented engineering," which was seen as encroaching on the Services' responsibilities; and (2) the Senate Foreign Relations Committee, which claimed that much of AGILE's work encroached on the responsibilities of the State Department. There was also strong support for AGILE, including the DDR&E, and other parts of the Services.[14]

Moreover, the scope of military funding for university research in such areas as behavioral and materials sciences was increasingly criticized by Congress (as well as within the DoD), eventuating in the Mansfield amendment, which severely restricted the scope of military funding for basic research.[15] Thus, ARPA moved into a much more constrained environment in which funding was declining, and, perhaps more importantly, the role of the Agency in promulgating R&D was narrowed to being much more Defense-specific. In this environment the question on the table was much more explicitly the degree to which research was relevant to and being transferred into military use or application.

Foster, as DDR&E, had a strong sense of ARPA taking on "aggressive, accelerated, ambitious programs."[16] In essence he thought it was necessary for ARPA to go back to the fundamental purpose that made it unique and be a zealous supporter of new concepts and ideas.[17] Apparently Foster's concept of ARPA taking a more active posture in new areas created some dissonance between some on ARPA's staff, who saw Foster as targeting their programs, and Foster, who was searching for new roles and programs for ARPA.[18]

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13 Ibid.; also Barber, op. cit., p I-11.

14 Seymour J. Deitchman, op. cit.

15 See Deitchman, op. cit., pp. 420-434, for discussion of the origins and the impact of the Mansfield amendment.

16 Barber, op. cit., p. VIII-4.

17 Ibid.

18 This apparent discord is reported by Barber, ibid. Apparently Foster's more direct attention to ARPA's programs and his involvement in its management, particularly during the interim between Herzfeld and Rechtin, created some dissonance. Some of this tension apparently continued at the staff level into the period when Dr. Rechtin took over as the new Director. One aspect of these tensions revolved around the issues of "defense-relevance" on the one hand, and the pursuit of "good science" on the other. Clearly, some of these tensions arose from those who desired to continue to pursue research at ARPA that the DDR&E (and his new Director) wanted to transfer or terminate, and also from differing views about ARPA's proper role in supporting the Vietnam conflict.

Vietnam had a major impact. Much of ARPA's basic research program was centered in universities, which had become the focal point for agitation against the Vietnam war. ARPA in general became a target of this hostility du:· · Project AGILE. In addition, ARPA's Vietnam role, particularly through its Advanced Sensor program, created conflict with the Services, particularly the Army. Dr. Eberhardt Rechtin became ARPA Director in 1967, after a period of 6 months when Dr. Franken, then Deputy Director, served as acting Director. Rechtin was brought in by Foster to emphasize programs explicitly of military relevance and to promote their transfer into application by the Services. Moreover, Rechtin emphasized that he wanted to more explicitly direct the program toward "the original ARPA" role which he stated was to "preclude sputnik-like surprises" by conducting "high-risk R&D of a revolutionary nature in areas where defense technology in the United States appears to be falling behind or in areas where ' . :annot afford the risk of falling behind."[19] However, while ARPA under Rechtin tried to stimulate new programs in such directions, apparently, despite a declining budget, there was "more money than ideas" to pursue.[20]

When in 1971, Dr. Stephen Lukasik became Director, ARPA had in large part completed work on the "presidential issues" which it had received at its outset--space, ballistic missile defense, and nuclear test detection. It was withdrawn from a major initiative of its own, counter-insurgency support, which had initially been backed at high levels within DoD. No comparable issues of national urgency replaced them and ARPA continued to focus on the exploration of new areas of potential impact and more fundamental research. The ARPA projects (termed "assignments" in ARPA nomenclature) during this period emphasized more joint-Service-ARPA projects. Following on the emphasis of Foster and Rechtin, Lukasik worked to establish strong linkage to the operating commands and the JCS, and saw these links as necessary to properly define programs and to be able to transfer them to the military.[21]

The programs during this period were organized around Strategic Technology, which picked up the remnants of the DEFENDER project (primarily high energy laser and directed energy particle beam), Information Processing (building on MAC), and the emerging Tactical Technologies (picking up some aspects of AGILE, Advanced Sensors,

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19 E. Rechtin, testimony to Congress, 1971, quoted in Barber, op. cit., p. VIII-13.

20 E. Rechtin, quoted in Barber, op. cit., p. VIII-15.

21 Ibid., p. IX-5.

and adding related projects).[22] The Information Processing Techniques Office (IPTO) under Lukasik became "vectored" more toward military command and control applications, particularly with the evolution of ARPANET into demonstration and application.[23] IPTO research into artificial intelligence was given greater focus around speech understanding (Project SUR) with specifically defined quantitative goals and the HASP program to use artificial intelligence (AI) techniques for signal processing for both nuclear test detection and ASW.[24] A re-oriented Materials Office, after transfer of the IDL program in 1972, directed materials work more toward military applications, but included a wide range of basic materials research into such areas as high-temperature superconductivity and polymerized liquids. Also begun was research into developing a ceramic gas turbine engine.

The Agency's budget "stabilized" at ~$200 million, which in real terms meant a continued decline in funding. Moreover, the budget situation, combined with inflation, was seen as having a profound impact on ARPA. Rechtin, in a retrospective discussion, felt that by 1975 the budget had declined to a point where not only were there "more good ideas than funding to pursue them," but that with such a severe budget crunch, ARPA itself might be eliminated.[25] In reflecting on the budgetary situation, Lukasik offered a more incremental perspective that "ARPA needed a little bit more money, not a lot more money" to pursue the work that he felt needed to be done.[26] This position differed from the view of the new DDR&E, Dr. Malcolm S. Currie, who replaced Foster in 1973.

D. DARPA'S NEW THRUSTS, 1975-1985

DARPA came out of the mid-1970s a very different agency than it was for the preceding years. This re-definition of the type of programs, how they were to be conducted, and what the mix would be, stemmed from decisions made in the mid-1970s by

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22 Ibid., p. IX-7.

23 See Volume I, Chapter XX, "ARPANET."

24 See Volume I, Chapter XXI, "Artificial Intelligence."

25 Ibid., p. IX-19. During this period some saw ARPA as an organization which went through some trauma as it was moved out of the Pentagon, faced frustrations in attaining the budgets that it had requested, and was affected by larger problems surrounding the aftermath of the Vietnam War and the political turmoil in the country at this time. Ibid., Chapter IX. Also correspondence from Dr. Lukasik, March 12, 1991, and Dr. Rechtin, February 6, 1991.

26 Ibid., p. IX-20.

Dr. Currie, the DDR&E, and Dr. George Heilmeier, the new DARPA Director appointed by Currie, and the Secretary of Defense, Dr. James Schlesinger.

This reorientation of DARPA can be seen in Figure II-2 (above), its funding history shown in constant dollars. The qualitative nature of this reorientation is perhaps of greater importance than the funding change, as shown in Figure II-3, which depicts selected DARPA program initiatives. A fundamental emphasis on using technology through an aggressive R&D program was inaugurated as the means of leveraging U.S. capabilities against what was seen at that time as a major imbalance emerging vis-a-vis the Soviet Union. The Soviet military seriously outnumbered that of the United States and its allies, and at the same time it was catching up technically.

DARPA was given a major role in creating technological leap-ahead options to overcome the Soviet Union's numerical superiority. Dr. Currie recalls that the re-vitalization of DARPA was not "a response to alarm about specific Soviet threats. Rather, it was a need to do something to get DoD's R&D back on track."[27] The way he puts it, "DARPA was spread too thinly and doing things it shouldn't be doing--getting into international conferences, extraneous things. Instead, I wanted DARPA to hit hard on basic research and big projects that could make a difference."[28]

Currie recalls he was afraid that DARPA was becoming "just another well functioning bureaucracy suffering from encroaching middle age."[29] In his view it needed a more "proactive program that took some risks." Moreover, Currie saw DARPA as a counterpoint to the DoD labs, many of which he thought had become moribund. A key element of this rejuvenation was personnel and management. From his perspective in DARPA "the same people were funding the same people to do the same things."[30] He determined there was a need for new people and new management to reinvigorate the DARPA program.

Heilmeier, as the incoming DARPA Director, had laid out an explicit investment strategy for the Agency's program--looking out over 10 years. Apparently such long-term strategic planning, particularly at the level of overall thrusts, was not a common feature of ARPA management, at least since the early days of ARPA's planning for the space

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27 Discussion with Dr. Malcolm S. Currie, October 30, 1990.

28 Ibid.

29 Ibid.

30 Ibid.

program.[31] This strategy was designed around a well-defined set of thrusts, which was determined through direct interaction with the Services, and particularly Gen. George Brown, the Chairman of the JCS.[32] Heilmeier recounts that he also made sure he had the backing of not only Currie and subsequently Dr. Perry, as the next DDR&E, but also the Secretary of Defense, first Schlesinger and then Brown.[33]

The thrusts Heilmeier defined dominated the history of DARPA from 1976 onward. The basic thrusts included: follow-on forces attack with stand-off weapons and associated C^3; tactical armor and anti-armor programs; infrared sensing for space-based surveillance; high-energy laser technology for space-based missile defense; antisubmarine warfare; advanced cruise missiles; advanced aircraft; defense applications of advanced computing, particularly AI and interactive computing; and STEALTH.[34] Many of these thrusts built upon, but substantially accelerated and congealed into ambitious programs, work already under way in DARPA. TEAL RUBY, for example, built upon IR focal plane technology that traces its origins through Vietnam sensing programs and DEFENDER research. But the concentrated, purposely ambitious demonstration of this technology as a space-based surveillance system clearly was a much bigger and more risky endeavor than any programs DARPA had pursued over the preceding decade.

The large thrusts themselves were nothing new to DARPA--its early history was defined around a very few high-level thrusts [Fig. II-5]. However, these thrusts were introduced into an organization that at the time was not accustomed to such large-scale endeavors, and in some cases not well-disposed to them. Heilmeier himself was concerned from the beginning that the large thrusts, if allowed to, could consume or seriously affect DARPA. The large-scale demonstration programs, therefore, were separated into a new set of program elements designated Experimental Evaluation of Major Innovative Technologies (EEMIT) so as to provide a buffer between them and the "core"

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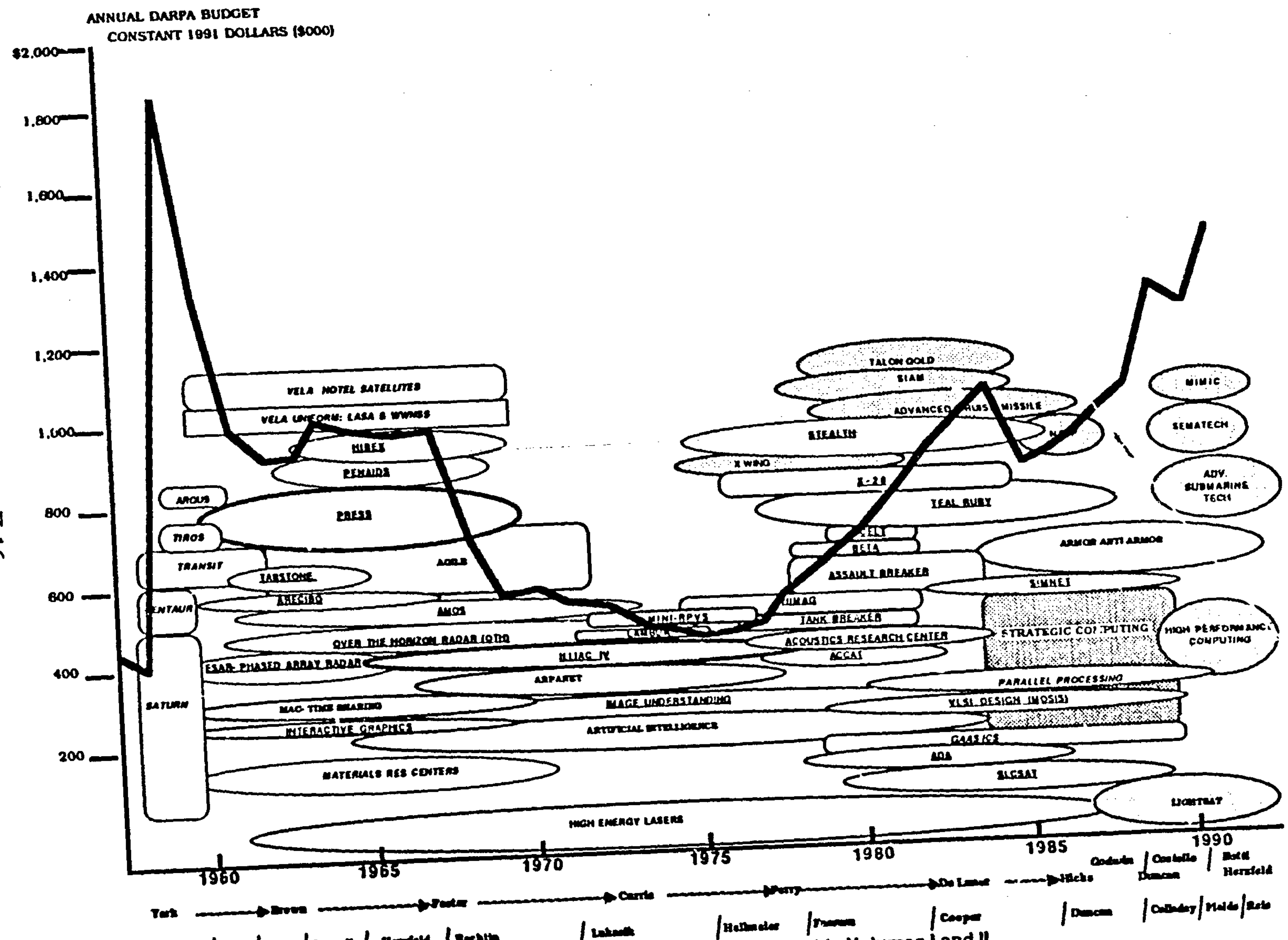
31 See Barber, op. cit., pp. X-25-27 on the informal, and relatively short-term nature of ARPA planning. Heilmeier laid out the basic concepts for his investment strategy in his statement on DARPA's FY 1977 program before the Subcommittee on Research and Development of the Senate Armed Services Committee, March 9, 1976.

32 Discussion with Dr. George Heilmeier, October 29, 1990.

33 Ibid.

34 Testimony by Dr. George Heilmeier to Congress, 1976, op. cit.



*All programs shown except those shaded are reviewed in Volumes I and II.

Figure II-5. Timing and Relative Size of DARPA Programs In Accomplishments Study

DARPA research programs.[35] Another concern was that such thrusts could be self-perpetuating and too autonomous. The Forward Swept Wing aircraft program is cited by some as such an example.[36] Yet, how much to reign programs in and how to keep from stifling them was and remains a major balancing problem.

Subsequent to Heilmeier, Dr. Robert Fossum became Director in 1977, and had to contend with the thrusts that were underway. In essence Fossum inherited many existing "mortgages" and he had relatively little freedom of maneuver.[37] Funding was the least of his problems as DARPA's budget went from $235 million to $455 million in 1980. Despite the existing dominant thrusts, he did encourage advances in integrated circuit research that had substantial subsequent payoff. The purpose of what Fossum called the "sub-micron electronic technology and electron devices" program was to "skip a generation in feature size on chips."[38] This program was to evolve into the VLSI program, which led to a fundamental revolution in integrated circuit design and had major impact on computer technology.[39] Fossum also supported the shift in the research focus of the then struggling behavioral science program to more applied training-oriented research, which became the progenitor of SIMNET, a program that has had significant impact on military training and operations.[40]

However, relative to the large-scale EEMIT programs, these were small endeavors. An added complication to the already heavily taxed DARPA organization was the assignment by Congress, in 1978, of the Charged Particle Beam program. This amounted to the addition of another major "EEMIT"-like program. The motivation for pursuing this program was concern that the Soviet Union had a "technology base program" for directed energy weapons.[41]

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35 Heilmeier, 1990, op. cit. The motivation behind this separation stemmed largely from concerns about how Congress might cut the basic research portion of the ARPA budget if these large, and potentially controversial demonstrations were not explicitly placed into a separate budget category. From Heilmeier, op. cit., October 1990.

36 See Volume II, Chapter XI.

37 The fact that Heilmeier's "thrusts" left a large legacy of committed programs and funds to be dealt with by Fossum was something of a departure from earlier ARPA practice, where a departing ARPA director left relatively few new initiatives to "hamstring" his successor. See Barber, op. cit., p. VI-61.

38 Dr. Robert Fossum, Testimony to the Committee on Armed Services, Research and Development Subcommittee, House of Representatives, March 6, 1979.

39 See Volume II, Chapters XVII and XVIII, on the VLSI program.

40 See Volume II, Chapter XVI, on SIMNET.

41 Mr. Battista of the Armed Services Committee staff stated during Dr. Fossum's 1979 testimony, "As far as the technology goes, they the USSR are probably a few years ahead of us here."

Fossum emphasized his concern over the potential effect of the large-scale technology demonstration programs in his testimony to Congress in 1979, when he stated that "one of my management objectives [is] to insure the technology base is always kept at a constant level and doesn't drop at the expense of the technology demonstration programs."[42]

By 1981, when Dr. Robert Cooper became Director, the thrusts begun by Heilmeier had reached a point where it was time to take stock and make some decisions about which to continue, which to transfer, and which to terminate. In addition, the creation of the Strategic Defense Initiative (SDI) led to the transferring out of major aspects of the Strategic Technology Office's research program. Most of the major thrusts started in the 1976 time frame were sufficiently mature and creating such funding demands that decisions were required.

Cooper states he was concerned that "DARPA was ill-equipped to deal with the types of programs . . . [Dr. Heilmeier] . . . had put into the EEMIT program."[43] These entailed larger scale systems development activities that needed to be run through some sort of intermediate management team to handle their scope of operation. The small size of DARPA made it difficult for it to run these new programs. In essence a large amount of the program management and direction had to be entrusted to others. At a minimum this required a great deal of negotiation and coordination, but also it intermeshed DARPA's goals, objectives, and perspectives with those of others, who often had very different interests and priorities. Thus, Cooper found himself taking stock of not just the programs themselves, but also assessing from the preceding 5 years' experience the scope and type of activity that he felt DARPA could best handle, and how to manage such activities.

Thus, when Cooper came in, he not only took stock of the EEMIT programs, he

> . . . went even further--went back to basics. I emphasized technology and limited the EEMIT programs [that were focused on *systems* demonstration]. I squeezed the EEMIT --deleted or transferred many of them. For the good ones, I got Service MOUs to get Service dollars in them so they [the Services] would have a sense of ownership.[44]

Of the EEMIT programs, Cooper felt the one that was most important and needed was ASSAULT BREAKER. He says he pushed hard with the Secretary of Defense to get

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42 Fossum, op. cit.

43 Discussions with Dr. Robert Cooper, December 5 and 7, 1990.

44 Ibid.

this program transferred into Advanced Development as a joint Service program.[45] Cooper recalls that the TEAL RUBY program was by far his biggest concern in terms of the resources it consumed and the lack of progress that had been made. The original supporters of the program began with a plan to launch a satellite in 3 years, and "the program stayed three years to launch from then on."[46] He brought in the Aerospace Corporation to review TEAL RUBY and "rebaseline" the program. Given the sunk costs, he decided to restructure the program around a tightly bound set of resources and progress conditions and forged an agreement with the Air Force to drive the program to completion.[47]

Cooper was interested in transferring the large-scale but what he considered "far out" strategic defense development and demonstration efforts--particularly such EEMIT programs as TALON GOLD, LODE, and ALPHA. Together such programs were a substantial proportion of DARPA's budget (on the order of 25 percent) and Cooper thought they were "of marginal utility to DoD."[48] He wanted to scale back these efforts with a longer term focus on technology research, rather than technology demonstration. With the creation of SDI Cooper states he was "ready and more than able" to transfer these programs to SDI.

The funds that were thus "squeezed" from the EEMIT programs Cooper "pumped into DSO (Defense Science Office) research and into TTO and STO technology development efforts."[49] Major technology thrusts were for the Strategic Computing Program in information-processing development and applications, and aviation technology. Also important at this time was a major thrust in "naval technologies" that remains highly classified.[50]

The Strategic Computing Initiative defined an omnibus program within which was combined applications of advanced computing with efforts to stimulate the technologies ranging from gallium arsenide microelectronics to new parallel computer architectures to

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45 See Volume II, Chapter V, for details of ASSAULT BREAKER.

46 Cooper, op. cit.

47 Ibid. See Volume II, Chapter IX, "TEAL RUBY," on this "rebaselining" and program restructuring.

48 Cooper, op. cit.

49 Ibid.

50 DARPA had supported research in this area during the early 1970s, but nothing of the scale of this new thrust. See Barber, op. cit., pp. IX-25-IX-26.

artificial intelligence software.[51] Underlying this thrust was the view that significant advances had been made across several aspects of information-processing technology, ranging from massively parallel computer technologies to image understanding algorithms, that warranted their being combined together and integrated into experimental applications. A key motivator behind this thrust was the desire by DARPA to take DARPA-stimulated advances in parallel processing computers beyond research and into development and applications with specific focus on such artificial intelligence applications as autonomous vehicle guidance and C^3I processing. The resources needed to realize these advances in an integrated manner were seen to exceed what could be accomplished from the comparatively small, disparate, but coordinated technology research budgets in the Defense Science and Information Science and Technology offices.

In developing this thrust, Cooper recalls early discussions with Dr. Robert Kahn, then the Director of the Information Science and Technology Office (ISTO), regarding the high leverage that had been achieved from ISTO programs, but how this was limited by the fact that it was achieved from small groups at a few universities that DARPA had been funding "forever."[52] The question was how to "scale-up" these developments out of the universities into technology applications that would more directly influence defense. The approach to contracting projects within Strategic Computing Program (SCP) was designed explicitly to create larger, applications-oriented teams, that would link university researchers and industrial partners. In contrast to the ISTO academically focused work, these teams would bid in competition for SCP projects. Cooper felt that this was a needed approach to transfer the technologies developed in the universities under DARPA sponsorship into the industrial firms that produced systems for DoD.[53]

Aircraft technology was a major area of concern with several of DARPA's EEMIT programs--such as the X-29, X-Wing, STEALTH, as well as large RPVs and Mini-RPVs--subjects of attention as Cooper became Director. At the time, DARPA was also involved in a Special Operations aircraft for low-intensity conflict injection and retrieval. Cooper recalls that when he first came into the organization, he was mostly concerned with terminating or transferring the aircraft programs already in the organization. In the end, he

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51 See Volume II, Chapter XVIII, "VLSI: Enabling Technologies for Advanced Computing"; see also *Strategic Computing*, First Annual Report, DARPA, February 1985.

52 Cooper, op. cit.

53 Cooper, ibid.

created more aircraft-related programs than he ended.[54] The focal point for exploring such ideas was the National Aerospace Plane (NASP--or Hypersonic research program). The NASP program had three areas of attention: engine technologies, aerodynamic simulation, and materials. Cooper explicitly emphasizes that this was a technology program, no experimental aircraft was part of the program.[55]

Cooper saw two other "technology thrusts," derived from the earlier STEALTH and ASSAULT BREAKER programs, as "particularly important": (1) "next generation STEALTH" and (2) stand-off weapons with terminal homing. The former interest derived from concerns that the initial STEALTH approaches had shown "drawbacks that could limit their application and DARPA had to explore ways to do it better."[56] For the latter, Cooper states the problem was that "there were millimeter wave seeker programs going on everywhere in the Services," but that the focus was generally narrow and was not leading to cumulative results. DARPA put together a program, first, to gather data on sensor performance more systematically, and then "to develop a high quality sensor for target recognition."[57]

E. RECENT HISTORY, 1985-1990

Cooper was followed as Director by Dr. Robert Duncan in 1985. The major concerns for DARPA in 1985 still entailed completing and transferring some of the large EEMIT programs, but the focus was by then much more on the completion of the Strategic

54 Discussion with R. Cooper, August 1989. A major problem was that NASA was not having success in getting new aeronautics programs funded. In fact, in 1981, the Office of Management and Budget (OMB) had "zeroed out" this program, and DoD had explicitly requested that it reconsider and allow DoD to determine what effect this would have on defense concerns. A major "national" study under the President's Science Advisor, Dr. Keyworth, ensued resulting in recommendations for more aircraft R&D and increased aircraft programs. Experimental aircraft (X-planes) were specifically discussed, but NASA was unable to get any funding for these approved through OMB. In the meantime, the Air Force did not have a major experimental aircraft program. It had the Advanced Flight Technology Integration (AFTI) program, which modified an F-16 with various experimental apparatus, but no major X-plane type of program. At the same time there were according to Cooper "an enormous number of ideas (for aircraft concepts)."

55 Ibid. Underpinning DARPA's involvement was the concern of the DDR&E, Dr. Richard DeLauer, that "the ramjet industry was dying." He specifically requested Cooper to determine what could be done to see that efforts were made to stimulate developments of this technology from a defense standpoint. Apparently the Navy and Air Force were not planning to pursue further ramjet R&D. Cooper met with representatives of these Services and got them to put funding back in this technology and developed a DARPA program for supersonic ramjet R&D.

56 Ibid.

57 Ibid.

Computing Program, the new TTO and STO technology development programs, and the definition of "follow-on" programs to these. DARPA funding, like the overall DoD RDT&E budget, continued to increase markedly during this period, reaching a level commensurate in real terms to the early ARPA DEFENDER era.

The 1986-1990 period brings this review up to the present. Cooper's "reshaping" of the major thrusts undertaken by Heilmeier, with an emphasis on "technology" thrusts, as opposed to "system demonstration" appears to have carried forward during this period. Strategic Computing clearly became the dominant technology thrust and results of this program became evident in new "massively parallel" computers becoming commercialized,[58] and applied to explicit defense concerns, such as image understanding.[59] Armor-Antiarmor, under the strong external support from OSD, and later with great Congressional interest, continued as a major focus. Target acquisition and target detection remained a major thrust, as did naval and submarine warfare.

During this period DARPA began to pursue a new concept for satellites, LIGHTSAT, which seeks to demonstrate the capability to disperse rapidly an array of small, special-purpose satellites for defense communications and other uses that provide a distributed ARPANET-like system. In 1988, DARPA established the Defense Manufacturing Office to accommodate a new set of "directed" programs dealing with defense productivity concerns, specifically SEMATECH, which was mandated by Congress, and the MIMIC, Focal Plane Array, and STARS programs, which were transfered out of the Office of the Deputy Under Secretary for Research and Advanced Technology.

An issue with which DARPA had to contend during this time was how to deal with a recommendation of the President's Blue Ribbon Commission on Defense Management (the Packard Commission) of 1987, regarding DARPA's taking on a major new role in prototyping. After review, DARPA concluded and the Secretary of Defense agreed that DARPA should not redefine its prototyping role in a way substantially different from those technology demonstration prototypes it already had been developing.

This period was one of rather high turnover in the Director's position, as Dr. Duncan became the acting-DDR&E, and then assumed that position in 1988, with Dr. Raymond Colladay, formerly of NASA, serving as DARPA Director for 18 months.

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58 See Volume II, Chapter XVIII, "VLSI: Advanced Computer Architectures."

59 See Volume II, Chapter XIV, "Image Understanding."

In 1989, with the beginning of the Bush administration, Dr. Craig Fields, the Deputy Director for Technology, became Director, but remained only 1 year, before he left to assume other duties in DoD, then moving to private industry. Dr. Victor Reis then became the new Director.

III. DARPA IN HISTORICAL PERSPECTIVE

DARPA has been directly affected by policy and events. Indeed, it was founded to be such an agency. Figure III-1 shows the major world events that impinged on DARPA's evolution over its 32 years.

The immediate post-war era built upon advances in technologies that were fostered during the huge infusion of research that was part of the World War II effort. Nuclear weapons, ballistic missiles, jet aircraft, advanced radar and sonar, even the basic electronic computer applied to defense, all sprang from these intensive R&D activities. After the defeat of the Axis powers the United States was left with a vast array of newly emerging and only partially realized military technologies, and an adversary who had access to and apparent intentions of acquiring results of these technologies as well. The political confidence of the United States entering the post-war era was shaken by the capacity of the Soviet Union to quickly develop military technologies that made them a threat to the continental United States in a way never before experienced. The Soviet atomic bomb in 1949 was less of a shock than their ability in 1952 to detonate a thermonuclear explosion only months after the United States did. With the onset of the Korean War, the United States had to devote resources to combat which further de-emphasized the priority for advanced R&D and supported the need for near-term development and procurement of the systems and technologies based on the legacy of World War II.[1]

The Sputnik launch produced a second generation of investment in defense-related technology and ARPA was created to play a major role. The emphasis of this thrust was strategic systems and space. Initially, given the imperative to counter Soviet achievements, the program emphasized bringing to fruition work underway in the Services stemming from technologies first developed during World War II. However, there followed from the large infusion of effort and resources during the early 1960s a tremendous advance of technological capability that began to enter into systems applications later in the decade.

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1 The development of nuclear powered submarines and carriers, while building on nuclear expertise gained in World War II, is to some degree an exception to this.

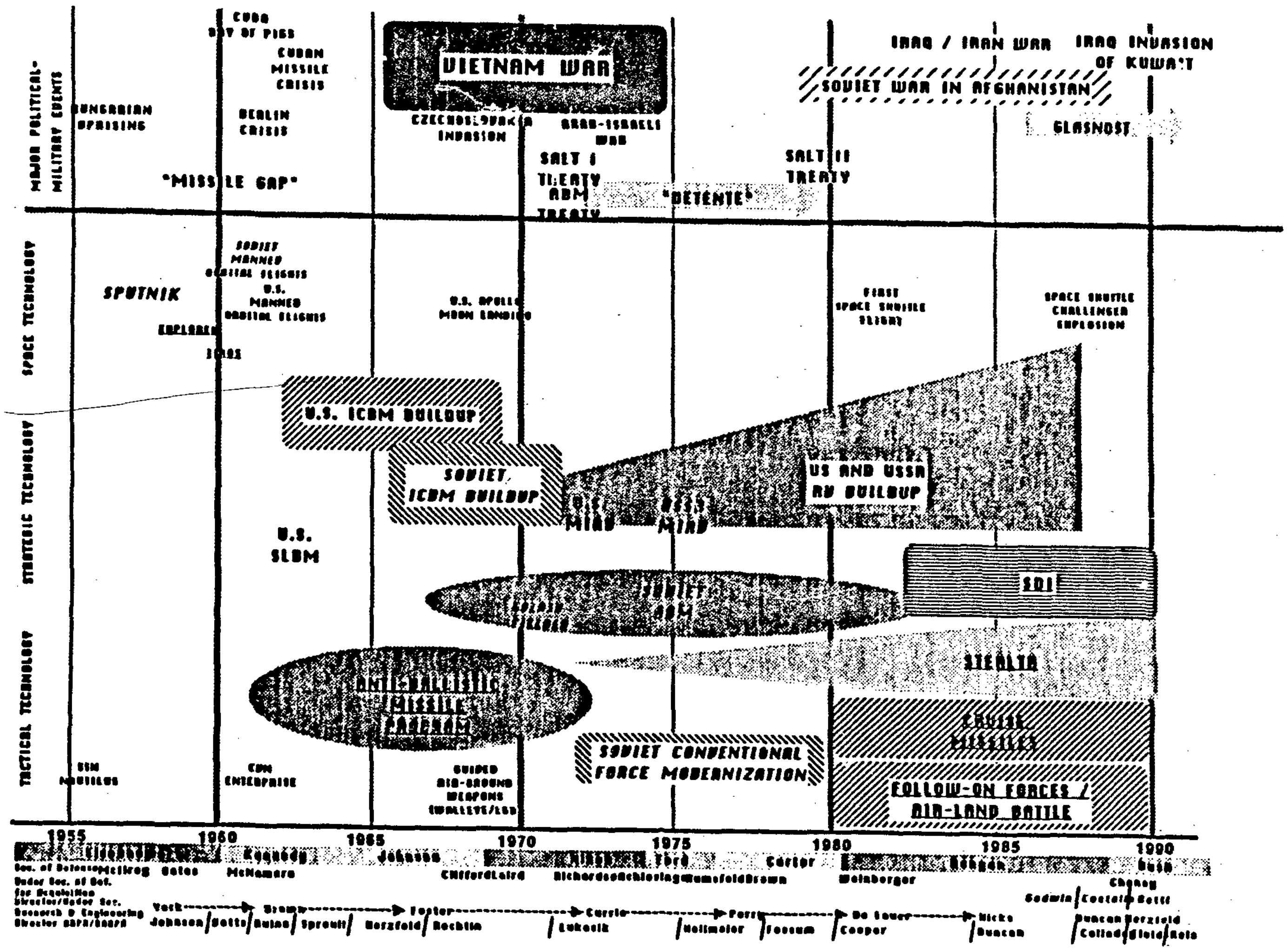

Figure III-1. Timeline of Major Political/Military and Technological Developments During DARPA's History

One such critical technological advance was putting into practice the capabilities of, first, the transistor and later, integrated circuit technology. For example, this was a crucial advance allowing the United States to develop multiple independently targetable re-entry vehicles (MIRVs) in the 1970s, which, in turn, played a major role in the decision to deploy only a limited ABM defense. Advances in electronics also played a major role in advancing phased-array radars, an area in which ARPA made important contributions.

ARPA itself was specifically a creation of the Sputnik challenge. Its first years were heavily focused on developing an integrated space program and with coming to grips with the highly contentious ballistic missile defense program. These areas contained similar characteristics: they stemmed from demonstrated Soviet technological advances that put U.S. technological prowess and management of R&D into question; they were heavily charged with inter-Service rivalry enmeshing ARPA in issues of Service roles and prerogatives; there were very wide-ranging differences on the nature of the work that should be done; and they entailed very large, risky programs.

The space role was complicated further by the fact that President Eisenhower and others held strong convictions that space should not be dominated by the military. This led to the creation of NASA in 1958, and the subsequent transfer by 1960 of all non-military space programs to NASA and military space programs to the Services. With the transfer of the space programs ARPA lost a major area of responsibility, and roughly 50 percent of its budget. Ballistic missile defense remained as a large-scale programmatic effort, becoming nearly 80 percent of the Agency's remaining budget.

After its Sputnik-generated jump-start, ARPA faced shifting priorities beginning in the early sixties. The confrontation and competition with the Soviet Union reached extremes with the Cuban Missile Crisis; the United States and the Soviet Union were rapidly building up their respective nuclear missile inventories, giving increased stimulus to ballistic missile defense. This also was a time when the United States and the Soviet Union were in the process of determining whether and in what way to come to grips with nuclear weapons testing. The arms control negotiations surrounding the Limited Test Ban Treaty, signed in 1963, were of direct importance to the White House, and a subject of great controversy.[2] ARPA's VELA program, although relatively small in comparison to DEFENDER, drew considerable attention from ARPA Directors Ruina and Sproull.[3] The

2 See York, op. cit., 1989.

3 Barber, op. cit., p. V-7.

national importance of these two areas clearly made them compelling. One was the subject of treaty negotiations to which ARPA directly contributed, and the other involved a decision on whether or not to deploy a major new weapons system, fraught with policy and technical issues and implications.

However, ARPA's level of involvement in both of these areas was sharply curtailed within a few years, due largely to exigencies of the policy arena. The decision to deploy the "light BMD" system substantially reduced ARPA's BMD effort. The imperative underlying the VELA work became much reduced with the Limited Test Ban Treaty. DARPA work continued to the point when the 150-kiloton threshold test ban was signed in the mid-1970s. Subsequent emphasis of VELA has been on improving the accuracy of seismic yield estimates so treaty thresholds can be lowered.[4]

ARPA was directly affected by the Vietnam War. The effect of the war was pervasive and impacted ARPA both in terms of its research programs and more indirectly, but profoundly, in terms of the relative de-emphasis of advanced R&D in DoD, and the growing negative relationship between ARPA's university base of research and the needs and interests of military R&D. ARPA's involvement in the war was largely due to the proponency of William H. Godel (Deputy Director of ARPA), who actively sought this involvement, despite somewhat negative views of ARPA Directors Ruina and then Sproull, under whom he worked. However, there was strong interest in OSD, and Congress as well, for increased R&D for application to the Vietnam war. Moreover, this need was emphasized by the advice of highly respected advisory groups, such as the DDR&E's Limited War RDT&E Task Group chaired by physicist Dr. Luis Alvarez and reporting to then DDR&E Dr. Harold Brown in 1961.[5] This combined interest and funding from Congress, and the imprimatur of the DDR&E, supported by high-level advice, were important underpinnings for ARPA's involvement.

However, the tenor of the Vietnam period became one of true beleaguerment as ARPA was caught directly in the anti-Vietnam sentiment sweeping the universities, and an anti-academic propensity of the military and DoD, who were increasingly questioning the value of the heavily academic mode of much of the research supported by ARPA. The crescendo of this sentiment was the "Mansfield amendment" in 1969, which explicitly limited the scope of military funding in basic R&D to those projects having "a direct and

4 Note from Dr. S. Lukasik, 3/12/91.

5 S. Deitchman recollection of his work with this Task Group, of which he was Executive Secretary.

apparent relationship to a specific military function or operation."[6] The Amendment although short-lived, left a lasting impact on the involvement of DoD in basic research. Due to the extremely tight budgetary situation at the time, ARPA already had been applying "strict relevance criteria" to its research.[7] Thus, the amendment essentially reinforced what already was being practiced. However, it provided a basis of constant pressure to focus on shorter term, more explicitly applications-oriented research.

It is evident that, given these pressures and trends, in the early 1970s ARPA and DoD management were highly concerned about the Agency's future. It is important to clarify that ARPA was not the only organization affected by these trends and developments. Indeed, the overall environment surrounding defense was not conducive to advanced research and development during the first half of the 1970s. At the time many organizations involved in Defense R&D were similarly uncertain about their fate. Some, such as the Weapons System Evaluation Group and the Defense Communications Planning Group, were abolished, and others faced major re-focusing, while many went though a period of reduced support during which they risked becoming moribund.[8]

Some of this can be seen as a function of time, as most of these organizations under scrutiny began during the immediate post-World War II era and had reached a point of maturity in which they had exhausted their initial missions, achieved their original objectives, or events had so changed that they were decreasingly relevant. Thus, ARPA's situation in the early 1970s can be seen as the result of a broader, more general stock-taking and introspection following the withdrawal from Vietnam, which also was accompanied by a decreased sense in some quarters of the value and need of advanced technology for defense, and a strong (and essentially mutual) negative relationship that had developed between the academic and defense communities.

As the effects of the post-Vietnam era were becoming most pronounced in budgetary and programmatic terms, there was a vigorous interest within the Office of the Secretary of Defense to rebuild the Defense R&D base, with an appreciation of a renewed

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6 Public Law 91-121 (1970 Military Procurement Authorization Act), Section 203.

7 S. Lukasik, quoted in Barber, p. VIII-21.

8 Interestingly, at the same time a Blue Ribbon Panel Report on DoD Reorganization, which advocated creating the position of acquisition executive, suggested that DARPA assume responsibility for *all* basic research in DoD. See Barber, p. X-20-24.

imperative for doing this.[9] The Soviet Union had during the late 1960s embarked on an intensive buildup of their ICBM force that achieved parity with U.S. strategic capabilities. By introducing MIRVed warheads in the early 1970s, the United States had maintained a quantitative balance in warheads, but the Soviet Union began to put MIRVs on their ICBMs by 1975, and pursue an RV race with the United States. Similarly the Warsaw Pact was in the midst of a conventional force modernization program that had resulted in a substantial improvement in their tactical capabilities against NATO forces in Europe.[10] The buildup of Soviet capabilities starting in the mid-1960s, which had coincided with the U.S. involvement in Vietnam, resulted in a major reappraisal of U.S. security and the ability of the defense R&D capabilities to meet the challenge.[11]

The mid-70s thus created a heightened emphasis on R&D as a leveraging capability for the United States to respond to an intensive buildup of Soviet tactical and nuclear forces. The advanced technology R&D program that was started at this time was oriented toward achieving (regaining) a qualitative superiority that would overcome the Soviet buildup. The emphasis of this program was on sensing and processing technologies which were enabled by the U.S. leadership in electronics technologies. and in other technologies that enabled the development of low-observable ("stealthy") systems. As shown in Volumes I and II, DARPA played a key role in this set of DoD programs. This "third wave" of technology development started coming into the field 10 years later. By the mid-1980s the main elements of these efforts were starting to move into procurement, with such systems as the B-2 strategic bomber, the F-117A fighter, the gradual implementation of the follow-on forces attack concept as the DARPA-developed PAVE MOVER radar transitioned into JSTARS, and other elements of the ASSAULT BREAKER concept began to be fielded as the ATACMS ballistic missile system for standoff attack of tactical force columns.[12] Interestingly, these systems started to appear just before the threat that drove their development collapsed, and now a different world environment has begun to emerge

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9 See the staement of Dr. Malcolm R. Currie, DDR&E, to the Committee on Armed Services, House of Representatives, March 5, 1975, in which he emphasizes "technological initiative" as a response to the efforts of the Soviet Union "to achieve military parity and go on to superiority." See *Hearings on Military Posture and H.R. 3869, Department of Defense Authorization for Apporiations for Fiscal Year 1976 and 1977*, pp. 3762-3763.

10 See *United States/Soviet Military Balance*, Library of Congress, Congressional Research Service, January 1976, especially pp. 13-29, for discussion of asymmetries at this time in the United States vs. the Soviet Union in the NATO arena.

11 Currie, 1975, op. cit.

12 See Volume II, Chapter V.

with markedly different national security emphases. These systems were used with excellent results in the Persian Gulf War.

In some sense the situation today is analogous to the end of World War II, in that the technology being developed to meet the threat of the day is only emerging and has not yet been fully realized when the adversary capitulates. There is evidence that the developments initiated by DARPA's technology thrusts of the late 1970s, which then moved toward acquisition in the 1980s, were seen by the Soviet military as creating "a revolution in military affairs,"[13] which raised fundamental concerns for them on how to respond to "significant advances in high technology" given the "inability of the Soviet economy to support a technological modernization of the armed forces."[14]

DARPA figured prominently in many of the technologies that created this situation for the Soviet Union. In the late 1980s DARPA continued to build upon the thrusts of the mid-70s and early 1980s to explore new areas of possible leverage--e.g., Strategic Computing, SLCSAT, LIGHTSAT. Moreover, Congressionally mandated programs in submarine technology and Armor-Antiarmor were directly aimed at bringing DARPA into those areas where the Soviet Union was seen as having substantial capabilities and which Congress was not satisfied were being addressed by Service R&D programs. Thus, as the Soviet Union retrenched from its forward posture, and turned its attention to its internal cohesion, DARPA was exploring an array of technologies that, if developed further into applications, could extend the leadership position of the United States in military technology. Although much of the technology research now being pursued may be useful to meet other threats, the DoD, and the country, have yet to define those threats well enough to determine which R&D efforts are of priority.

From this standpoint it appears that DARPA is now at the cusp of another critical and fundamental period in defense research and development, in which the role of advanced technology is in question, as well as the areas of its application and the time horizon for its need. This is a new era in which priorities are being recast, while at the same time the resources for pursuing these priorities are substantially reduced.

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13 Chief of General Staff Marshal Nikolay Ogarkov, quoted in D. L. Smith and A.L. Meirc, "Ogarkov's Revolution: Soviet Military Doctrine for the 1990s," *International Security Review*, July 1987, p. 869.

14 Matthew O'Brien, "Soviet Military Reform," Institute for Defense Analyses, Working Paper, November 1990.

DARPA has experienced such periods of refocusing before. There are, however, some characteristics which differentiate this emerging period from DARPA's prior experiences. Figure III-2 depicts what we have labeled as "strategic drivers" at the three critical points in time: 1958 when ARPA was created, 1975 when DARPA embarked on a broad set of thrusts, and today. Given the changes in the world situation, DARPA faces important decisions on its programs and objectives. We comment further on these concerns in Section VI, Implications for the Future DARPA Program.

In this context it is perhaps insightful to consider the comment of Dr. Currie, then the DDR&E, in 1975:[15]

> For decades we have based our military and economic vitality on technology. Today we find ourselves in an uncertain world. We view with concern growing dependence on other nations for effective operation of our economy, and we see a determined adversary led by people who understand the role of technology, who achieved apparent parity in raw power and who insist they can and will create a position of military and economic strength favorable to their country. In this increasingly competitive, often hostile and rapidly changing world, Americans seem to have only one real choice. Clearly our national well-being cannot be based on unlimited raw materials or on unlimited manpower and cheap labor. Rather it must be based on our ability to multiply and enhance the limited natural and human resources we do have. Technology thus appears to offer us our place in the sun--the means to insure our security and economic vitality.

While there has clearly been a major transformation in the world over the 15 years since Dr. Currie's statement, the issue as it relates to DARPA is what role the Agency should now play in furthering technology for the enhancement of the security of the country. In 1975 DARPA was given a major role, which it actively helped to define. It would appear today that leadership is required to define the strategy that will continue to provide the future technological basis for the security and vitality of the United States.

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[15] Currie, 1975, op. cit., p. 3760.

1958	1975	1990-91
• Communism and the USSR the enemy; containment and massive retaliation the strategy • DoD being reorganized in centralized format (Act of '58) • War in Vietnam beginning • SPUTNIK energizes nation • U.S. push to excel in technology, get into space	• Vietnam ends in disgrace and dissension • Major-systems R&D had been neglected • USSR had made startling progress in weapons, organizing the WP; NATO nuclear monopoly gone • U.S. perceived need to catch up and be leader again; turning its attention back to NATO • Powerful new technology emerging: Computers and solid state devices, all kinds • Age of smart air-surface weapons had begun	• USSR threat perceived gone when WP evaporated and Germany unified (could be temporary, but public not worried). USSR and E. Europe currently present mainly economic and internal political problems, ethnic strife. • U.S. no longer dominates world economy and technology; economic concerns, including economic competition with EC and Japan, predominate • Major U.S. budget issues (deficit, taxes, how much defense) in face of social and economic problems • Reversed flow of much advanced technology: military --> civilian then; civilian --> military now • Parts of Third World coming up strong militarily; Third World unstable, rambunctious, especially in Middle East/SWA, potentially in Korea, South Asia, China • U.S. military standdown via shrinking defense budget, including R&D, while undertaking major military buildup toward war threat in ME/SWA.

Figure III-2. Strategic Drivers at Turning Points in DARPA's Evolution

IV. ENDURING DARPA TECHNOLOGY PROGRAM AREAS

A. INTRODUCTION

Despite the perturbations in funding and political factors to which DARPA over time has had to respond, there is an identifiable core of research areas in which DARPA has persisted and made major contributions. This section identifies three such enduring areas of DARPA technology research, providing a view of the relative emphasis as these evolved, and what appeared to be the main factors bearing on their evolution and their effect. Among the most notable technology areas evidencing a constancy of focus in DARPA are: sensing and surveillance, computers and information processing, and directed-energy systems concepts and research.

These more enduring DARPA technology research areas might be described as being indirectly supporting, if not tangential to, the "mainstream" of defense R&D, the development and fielding of weapons and weapons platforms. This was not the main role originally conceived for ARPA as a "special projects" organization. Indeed Secretary McElroy's views were extremely expansive on the "Special Project Agency" being the central source for "the vast weapons systems of the future."[1] McElroy's idea was to take concepts ("will-of-the-wisps") "and carry them through to a point where there can at least be a determination of their feasibility and what their probable cost might be . . . a function that extends beyond the immediate foreseeable weapons systems of the current or near future . . . to centralize the research and development of weapons system or group of weapons systems which do not have any obvious Service connection in their inception . . . R&D quite far down the road including testing." Of course, much of DARPA's R&D did extend to platforms and weapons systems concepts--ASSAULT BREAKER and STEALTH being two particularly salient examples. These were very large-scale system demonstration programs. In addition, from time to time, DARPA has supported less ambitious, more incremental development of platforms, e.g., HIMAG and weapons such as TANK BREAKER.

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1 R. Barber, op. cit., p. II-5.

While the development of such systems and weapons is a major dimension of DARPA's mission and program, the Agency has fulfilled another highly significant role--the persistent pursuit of nascent technology areas of great potential and broad (but initially not well-determined) application. This aspect of DARPA is a special feature, which merits explicit attention.

B. SENSING AND SURVEILLANCE

After the passing of its role in the space program, ARPA research centered initially on ballistic missile defense. This wide-ranging research area extended ARPA into a vast array of technology research and development including missile phenomenology, concepts for "destruction mechanisms" that included very exotic approaches such as lasers and particle beams, advanced systems for sensing and detection (e.g., ESAR), and advanced missile and propulsion systems. Advanced sensing and detection have been a particularly enduring and fruitful aspect of the early ARPA DEFENDER assignment. DARPA's involvement in advanced sensing technologies encompassed radar, optical, and infrared sensing. Each of these was pursued initially under the DEFENDER program, but DARPA's role in both extended throughout the subsequent history of the Agency, albeit with a changing focus and technological emphasis over the years.

1. Radar

Since its inception, DARPA has played a significant role in radar technology. ARPA took on (from initial Air Force work) the development of ESAR, which was the first 2-dimensional phased array radar; its successor, the FPS-85, is still operational today as part of the Air Force SPACETRACK system. The successes and limitations of the ESAR program were key factors in the subsequent establishment of a broad-based phased array component and techniques program.[2] This, in turn, helped to improve all U.S. phased array developments including the Navy's AEGIS. ARPA's radar technology work in this area is acknowledged to have "directly influenced a whole new generation of advanced Service radars."[3]

Under DEFENDER, ARPA also had a parallel effort in over-the-horizon (OTH) high frequency radar. This research had a payoff in the early 1960s with the Air Force 440L early warning system, which remained deployed until retired in 1975. The OTH

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2 Volume I, p. 6-6.
3 Barber, op. cit., p. V-20.

program had a number of additional impacts, including key information useful for development of CONUS and Navy air defense. ARPA also developed several wide bandwidth radar systems under PRESS (Pacific Range Electromagnetic Systems Studies), which not only were instrumental in the re-entry measurements aspect of DEFENDER, but are still in use today f r Air Force, Army, and SDI R&D efforts, and operationally as part of the Air Force's SPADATS system.

ARPA's first major radar development effort outside of DEFENDER (important far beyond its modest cost) was the sophisticated, foliage-penetrating "Camp Sentinel" radar for detecting intruders. This radar was developed and fielded in Vietnam, as part of Project AGILE. Under the ASSAULT BREAKER program DARPA played a major role in the development of the PAVE MOVER airborne tactical surveillance and tracking radar. This became the basis for the Air Force JSTARS that is currently being procured.

The legacy of this broad range of advanced radar developments is the SDI ground based radar and the long-range imaging program that is currently being pursued, that draws upon and integrates much of the knowledge accumulated in DARPA's continued and large-scale investment in radar technology. It is important to emphasize that DARPA's enduring support across the domain of advanced radar technology permitted the development of several parallel paths and produced an infrastructure and knowledge base extending across a range of applications.

2. Infrared Sensing

DEFENDER also conducted a comprehensive assessment of the utility of infrared (IR) sensing systems for BMD early warning in its TABSTONE program. The results of ARPA's TABSTONE analyses influenced the decision to proceed with IR early warning satellites, implemented by the Air Force as SEWS or the DSP.[4] IR sensing continued in ARPA, after a short hiatus, particularly with interest in the problem of night vision, which was a key issue during the Vietnam conflict.[5]

DARPA's involvement in infrared technology, under Heilmeier, escalated sharply and ultimately became a major thrust under the TEAL RUBY program, with adjuncts such

4 The AMOS telescopes also made major advances in infrared technology for astronomical research and this program served as "a unique test bed for focal plane arrays developed by DARPA," Volume I, p. 10-9.

5 See Heilmeier, op. cit., p. 4916, on ARPA's involvement in FLIR development (some in conjunction with the Army Night Vision Laboratory).

as HALO and HI-CAMP. DARPA was an important contributor to the development of early IR scanners, used in Vietnam, and subsequently in the advancement of focal plane array technology. Today there is continuing emphasis on infrared technology with emphasis on tactical applications.

3. Other Sensing

DARPA pursued major developments in sensing in other areas as well. The VELA program of nuclear test detection entailed the development of a vast array of detection instruments, such as x-ray and gamma ray detectors, for satellite surveillance, and major advances in seismic sensing. The early ARPA effort to obtain worldwide seismic data has had a major impact on seismology and earthquake prediction. Sensing for ASW has been another area of persistent DARPA concern. Although it already had some earlier involvement at a low level, ARPA was directly assigned a program in ASW in 1971 by the DDR&E. ARPA explicitly searched outside for promising ideas to pursue, leading to the LAMBA towed array, embodied in the current Navy SURTASS system. At the same time ARPA began a non-acoustic ASW sensing program to find other detection approaches, including optical and radar sensing from aircraft. These ASW efforts were beset by limitations in information processing, which became one of the motifs for DARPA's information-processing research.

C. INFORMATION PROCESSING

Closely related to the sensing technology developments are the problems associated with processing and understanding what has been sensed. From the very earliest DEFENDER efforts, there was concern about the ability to process the data from the sensing systems that were being developed.[6] However, the initial impetus and motivation for ARPA's involvement in information processing came from a different direction--command and control--and from DoD's experience with the earlier Air Force SAGE project. It was from this original concern that Dr. J.C.R. Licklider, who had worked on SAGE, was brought to ARPA, bringing with him a distinct vision of the needs for fundamental advances in interactive computing to realize the full potential of computers. The advances that have been made through DARPA's sustained efforts in information

6 Barber, op. cit., p.III-54. For example, there was significant work in computer control and processing with advanced (for the time) solid-state components associated with the ESAR phased array program. See Volume I, p. 6-2.

processing have had substantial military impact--not only on command and control, but well beyond, into processing the tremendous amount of data that advanced sensing systems of the type pioneered by DARPA produce. Moreover, the sensing and information-processing technology areas are clearly complementary, becoming increasingly integrated, and DARPA has played a key role in furthering their inter-relationship. Finally, DARPA's achievements in computer time-sharing and networking have influenced the directions of information-processing management and transfer in the entire industrial world, military and civilian.

The contributions made by DARPA to information science and technology, depicted in Figure IV-1, have ranged from supporting fundamental conceptual work at the basic level (such as artificial intelligence and image understanding), through the support of new approaches for microelectronics technology of the VLSI program, to the development of large systems or hardware configurations of new computing or information-processing technologies, such as the ARPANET packet-switching network, the ILLIAC IV parallel processing mainframe, and, most recently, the massively parallel computers, such as the Connection Machine, based on novel architectures that sprang from DARPA's support.

At the heart of Licklider's interest was inter-active computing, which led to considerable focus on a few academic centers, most notably, MIT, Stanford, and Carnegie-Mellon, where the faculties were most interested in exploring the development of such a concept. ARPA's support for artificial intelligence (and related speech recognition and image understanding) is perhaps one of the greatest testaments to the Agency's patience in the pursuit of fundamental research problems that have the promise of breakthrough potential. In essence, ARPA was one of the earliest sponsors of this field through its support to Machine-Aided Cognition (MAC), and it remained an area of continuous DARPA support.

After a decade of support for primarily basic, mostly academic research, DARPA pushed this area toward applications and demonstration, first under Dr. Lukasik with the speech understanding and ASW multi-array signal processing programs and then even more so when Dr. Heilmeier became Director. This stressed the state of the art, leading to some frustration. However, at the time, the limitations of computing capabilities to perform artificial intelligence computations became a major driving force for DARPA's emphasis on fundamental advances in computer architectures and systems in order to achieve step-level gains in processing power. These will be discussed further below.

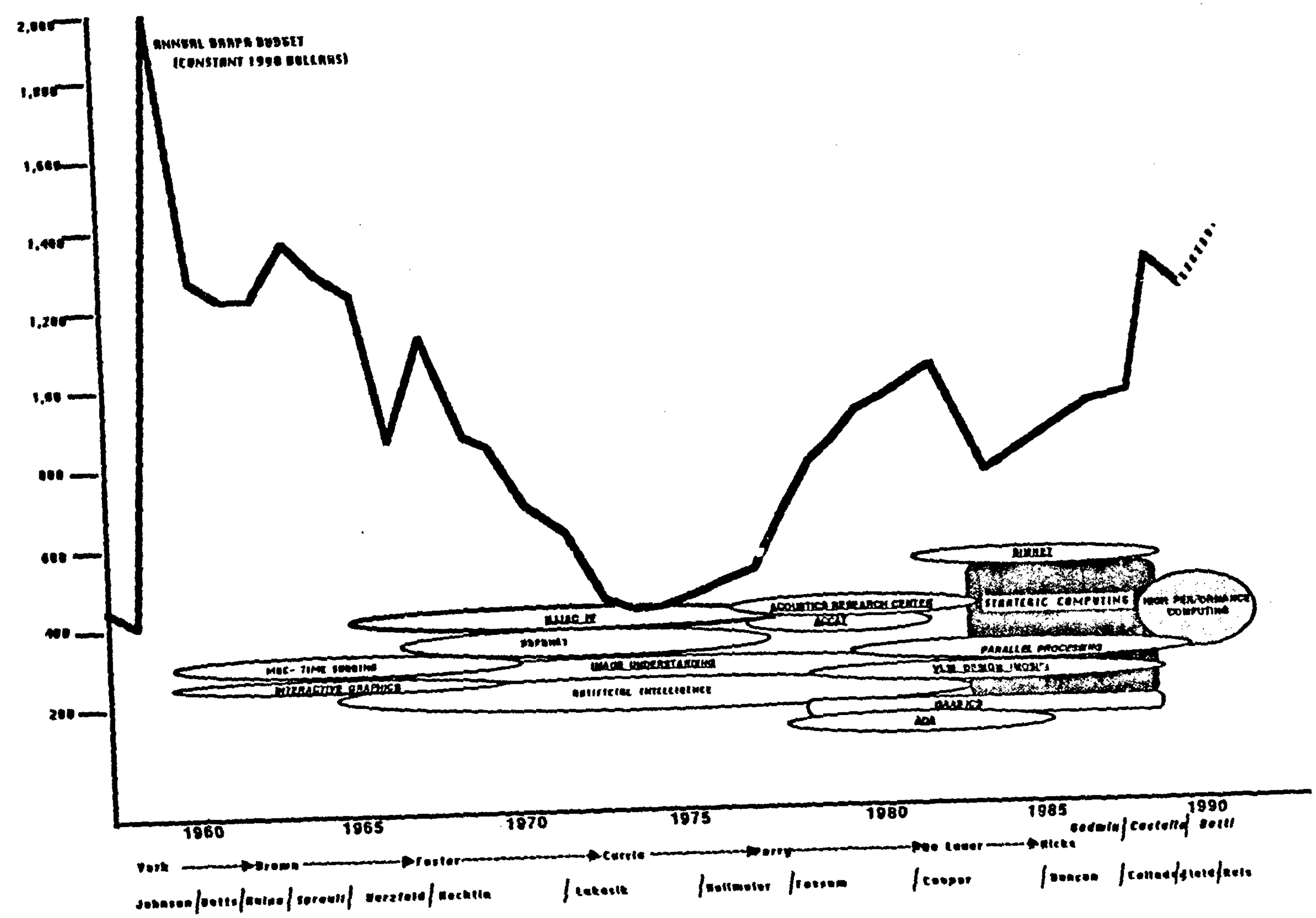

Figure IV-1. Major DARPA Programs Related to Information Processing

Following ARPA's early-on efforts in interactive computing and artificial intelligence, ARPANET began to emerge in the late 1960s as a major focus of attention. ARPANET in essence took the next step beyond time-sharing of computers--networking among computers. ARPANET created an enabling capability for linking computers which was seen as a "revolutionary change in telecommunication technology."[7] With the maturity of ARPANET in the early 1970s, with its military application leading to the Defense Data Network (DDN), its transfer to civilian use and commercialization, DARPA began to focus on other aspects of information processing. This included the application of AI to command, control, and communications, anti-submarine warfare, and the development of Ada. Interest in networking and sharing information in large numbers of computers also led to the SIMNET facility, applicable to combat training and testing of new weapon and tactical concepts in a simulated combat environment.

The focus on developing fundamental advances in computer architectures to achieve substantially greater processing capabilities began in the late 1970s and led to the VLSI program that revolutionized the microelectronics field. This led to an explosion of computer architecture ideas, which were realized with the support of the DARPA-sponsored MOSIS facility. DARPA's support to academic centers throughout the 1980s for a broad program of advanced computer architecture research produced such concepts as powerful networked work stations that are today major commercial successes and massively parallel computers that are directly useful to a wide range of defense, as well as civilian, applications.[8]

The Strategic Computing Program began in 1985 to consolidate the advances made in information-processing technologies and to stimulate their application to the vexing concerns of infonnation processing for such areas as sensing and surveillance, autonomous vehicle operation, and command and control. Through the advances made with sustained DARPA support, the ambitious vision of Licklider for revolutionizing information processing and applying it to problems of "human cognition" are being progressively realized.

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7 Volume I, pp. 20-26.
8 Volume II, Chapters XVII and XVIII.

D. DIRECTED ENERGY

From its inception DARPA has been involved with efforts to pursue very advanced weapons concepts, particularly for ballistic missile defense, that have gone far beyond the normal or accepted weapons technologies or concepts of the day. In its earliest days ARPA's DEFENDER program set about exploring innovative, long-term, but plausible alternative concepts for missile defense. Directed energy weapons are one of the key outcomes of this effort.

1. Lasers

Soon after the publication of the laser concept in 1958, ARPA began a research program to make a laser weapon actually work. The ARPA motivation was that the laser concept offered the possibility of delivering large quantities of photon energy to a target. After successful demonstration of the first operating laser at Hughes in 1960, the ARPA program greatly expanded to include investigation of lasers, laser excitation and power systems, and propagation of high-energy laser beams through the atmosphere. The Army and the Air Force had similar programs at this time; the Army also had a program to develop low-energy laser range finders. By the early 1960s, a JASON review and other studies indicated that the high-energy laser ABM weapon technology then available would be impractical. Because of the potential of the concept, if it could be made to work, the ARPA program continued with emphasis on fundamentals. Also, as the Vietnam war effort increased, some effort was made to design and produce small, light laser range finders and target designators.

In the mid- to late 60s the infrared (IR) gas dynamics laser (GDL) was invented at AVCO. Because of its prospective capability this invention generated a new laser program at ARPA. ARPA and Service GDL research were then integrated into the DoD GDL program conducted by a special group under DDR&E with ARPA as the lead. IR optical component technology was developed in these programs, much of it under ARPA materials program sponsorship. This IR technology spun off to form the basis of today's industrial laser welding capability.

In the early 1970s ARPA, together with the Navy, built the state-of-the-art testbed MIRACL IR laser and optical system. This was the highest powered and most advanced experimental system at the time. ARPA also joined the Air Force in building the Airborne Laser Laboratory (ALL). The MIRACL has been used in lethality experiments by SDI.

While the MIRACL program was still ongoing, the invention of the excimer ultraviolet laser and conversion led to hopes for a laser at short wavelengths. This led to a new, doubly motivated DARPA program. One part of this program was toward high-energy, blue-green lasers as a weapon, and the other, joint with the Navy, toward a ground, aircraft, or space based laser system for communication with submarines, which became SLCSAT. Besides some possible operational advantages, the possibility that the SLCSAT might replace the Navy's ELF system has been attractive to some in Congress. Both parts of the program involved advanced technologies, e.g., atmospheric compensations for the ground based laser. The ground based laser part of this program was transfered to SDI, and the aircraft and space based laser part was transferred to the Navy. However, subsequent work has shown the high potential cost of a SLCSAT system and the preferability of a solid state instead of an excimer laser for an aircraft based communications application. DARPA and the Navy have commenced a new joint program towards development and demonstration of such a system. DARPA interest in a space based laser for ABM also began after MIRACL. Despite the advantages of short wavelengths for space optics, the efficiencies and high powers of IR lasers remained the dominant consideration, and DARPA concentrated on building a compact, high-energy gas laser, the Alpha laser. This program has been transferred to SDI.

2. Particle Beams

Investigations of the possibility that an intense, high-energy beam of charged particles might bore its way through the atmosphere and destroy an incoming ICBM RV were begun by ARPA soon after its inception. This possibility had been brought to the attention of DoD by a group of high-energy accelerator physicists before ARPA had been formally established. Early ARPA work on this concept involved a number of top-notch theoretical and experimental physicists, who quickly determined that a sufficiently intense and narrow high-energy charged particle beam might indeed be able to bore through atmospheric gases, if various instabilities of the beam could be overcome. An experimental program was set up at Livermore under N. Christopholis, using parts of an accelerator which had been built for the ASTRON thermonuclear fusion device. Progress was slow, but in the mid-60s a comprehensive review requested by Dr. C. Herzfeld, then ARPA director, reaffirmed the potential of the charged particle beam as a possible ABM weapon. In the early 1970s, however, ARPA terminated the program, concerned that the elaborateness of the "fixes" to beam instability were making it increasingly unlikely that a weapon would be realized, concerned that a new costly accelerator would be required for experiments, and, overall, concerned that the technical complexities involved in developing such a system would be excessive. However, the Navy picked up a modification of the particle beam as a possible "soft kill" antimissile weapon, and continued development for a few years until Congress decreed that because of continuing Army interest, DARPA should be responsible for the program. This responsibility has continued, and the new accelerator, the Advanced Test Accelerator (ATA), was built under this program and has begun to be used for atmospheric experiments. The ATA has also found use in free electron laser experiments which were supported under SDI.

E. CONCLUSION

We have reviewed three areas in which DARPA has had sustained involvement and substantial impact. These research areas demonstrate a more enduring aspect of the DARPA mission: to explore areas that are only partially understood and not well defined, and that offer the potential for major technological breakthroughs. Generally speaking, the manner in which the results of research into such technology areas will affect actual defense weapons, platforms, or other applications is only very generally or even vaguely understood.

A major venue for pursuing such work has been the universities. Indeed it is noteworthy that DARPA has had a number of clear successes associated with academic research:

- ARECIBO
- Over-the-Horizon (OTH) Radar
- Interdisciplinary (Materials) Research Laboratories (IDLs)
- Information-Processing Technology--MAC, VLSI.

The close link between universities and DARPA was severely damaged by the Vietnam experience, but over the past decade considerable progress has been made in re-forging this vital connection. Notably, most of the major advances that recently have emerged in DARPA's support of advanced computer architectures sprang from a beneficial nexus of university research, commercial development and exploitation of this research, and the subsequent application to defense technology needs.

Persistence to some degree is a function of the broader support that DARPA has had for initially pursuing research in various areas. DEFENDER, materials research, and information processing were early assignments to ARPA, and defined areas where the efforts of others were seen (at high levels) as needing either refocusing or stimulation. DEFENDER begat ARPA's sponsorship of high-energy weapons research, and, with VELA, its broad sweeping involvement in sensing and surveillance. Clearly, as shown in the preceding chapter, the security needs and policy priorities of the day influenced and affected the evolution and focus within these broad technology areas. Moreover, as some of these areas have matured, there were efforts, sometimes explicitly forced by events or individuals, to apply results that markedly changed the character and scope of ARPA's involvement. Most notably, in several of these areas, there were identifiable, significant spin-off effects, in terms of military and civilian application, commercial success, and the opening up of new avenues of inquiry that go well beyond what could be anticipated at the time.

In looking forward to the future, and assessing DARPA's contributions, it is perhaps useful to recall Dr. Von Neumann's adage: "For progress there's no prescription: one can only state characteristics--intelligence, flexibility, and persistence."

V. DARPA PROGRAM INITIATION AND MANAGEMENT

A. INTRODUCTION

This section provides our perspective on why and how first ARPA, and then DARPA, programs have been conducted relative to their influence, impact, and success.[1] We have an advantage in having reviewed in detail over 60 projects conducted over the Agency's 33 years. In addition, this study has allowed the authors to discuss and review DARPA's broader programmatic interests and concerns with others, including former DDR&Es, ARPA and DARPA Directors, Office Directors and Program Managers, as well as others, both in and out of Government, who have been involved with the Agency's efforts as both participants in and consumers of its research product. From this accumulated information we have been able to synthesize a perspective about the impact and influence of DARPA's work that may be of use to DARPA management as it approaches the definition and management of its programs in the future.

This study was designed initially to seek and document the ways in which ARPA and DARPA work had influenced defense technology and technology more broadly. The selection of projects and programs that were examined therefore emphasized those that would generally be considered as having had relatively important impact and thus be thought of as relatively successful. The historical research and analysis did not make an explicit effort to review major program failures. Our review has shown, however, that the projects and programs considered in this study have had a wide range of both degree and type of influence and effect. By many measures, they did not all achieve similar success or

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1 There have been efforts throughout DARPA's history to review how the agency has conducted its research and to suggest recommendations on how it might improve various aspects of its management and practices, for example, *Report of the Defense Science Board 1981 Summer Study on Technology Base*, November 1981, USDRE, Washington, D.C., and *The Defense Advanced Research Projects Agency's Technology Transfer Process*, National Security Industrial Association, December 1985. Also, specific studies of major programs have been done with attendant "lessons learned," such as "The Life and Times of TEAL RUBY," by Edwin Schneider, October 1983, based on a DARPA commissioned "rebaselining" study performed by Aerospace Corporation in 1982. In the course of this study we have reviewed most of these prior assessments. Given the particular nature of our study, we see our perspective as complementary, having drawn on a historical review of some depth and detail, as well as direct discussions with several of the key management figures within and outside of DARPA.

all have great impact. Much can be learned from where projects or programs did and did not succeed, and why. In our view the programs reviewed in this study give an ample variety to afford such insights, without dwelling on some specific acknowledged failures.

Moreover, the authors were able to draw on the results of a brief review, undertaken by IDA with contributions by many former ARPA and DARPA Directors, DDR&Es and USDREs, of DARPA efforts and prospects in the prototyping assignment that was given to DARPA after the Packard Commission report.[2] This review, too, examined the conditions making for success or failure in DARPA efforts, and oriented the judgments toward the question of whether the results were transferred in some form to some user or users.

Our observations about the conditions that led to success and the circumstances in which success was not total, have been used to draw conclusions about key aspects underlying DARPA project or program achievements or accomplishments. For the reasons discussed just above, we do not believe the initial "success-oriented" selectivity of projects and programs in this history affects these overall conclusions very much. Where generalizations should be accepted with caution, this is pointed out.

B. UNIQUENESS OF DARPA'S MISSION

In presenting this perspective it is important, in our view, to state clearly the nature of DARPA's role and mission. ARPA was created to fill a unique role, a role which by definition and in its inception put it into contention and competition with the existing Defense R&D establishment. As the *Advanced Research* Projects Agency, ARPA was differentiated from other organizations by an explicit emphasis on "advanced" research, generally implying a degree of risk greater than more usual (non-"advanced") research endeavors.

Former ARPA Director Dr. Eberhardt Rechtin emphasizes that research, as opposed to development, implies unknowns, which in turn implies the possibility of failure, in the sense that the advanced concept or idea that is being researched may not be achievable or feasible with the current knowledge and capabilities. If one knew in advance that the

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2 Richard Van Atta, Seymour Deitchman, et al., *DARPA's Role in Prototyping Defense Systems*, Institute for Defense Analyses, March 27, 1990.

concept were achievable with little or no risk of failure, the project would not be a *research* effort, but a *development* effort.[3]

DARPA has over its history grappled with how to interpret or pursue *advanced* research, both in contrast to the broad array of research being conducted within and for DoD, and relative to its perception of the needs at the time. Its official mission states:

> DARPA shall:
>
> 1. Pursue imaginative and innovative research and development projects offering significant military utility.
>
> 2. Manage and direct the conduct of basic and applied research and development projects that exploit scientific breakthroughs and demonstrate the feasibility of revolutionary approaches for improved cost and performance of advanced technology for future applications.[4]

It is clear from our review of DARPA's history that within the scope of this mission the emphasis and interpretation of *advanced* research have varied, particularly in terms of (1) the degree and type of risk and (2) how far to go toward demonstration of application. At times with changing circumstances the agency has had to re-assess its project mix and emphasis due to determinations both internally and within the Office of the Secretary of Defense regarding the appropriate level of risk and the need to demonstrate application potential. In a sense these somewhat contradictory imperatives serve as the extreme points on a pendulum's swing. As DARPA is pulled toward one of the extremes, often by forces beyond itself, including Congressional pressures, there are countervailing pressures stressing DARPA's unique characteristics to do militarily relevant advanced research.

Risk has several dimensions: (1) lack of knowledge regarding the phenomena or concept itself; (2) lack of knowledge about the applications that might result if the phenomena or concept were understood; (3) inability to gauge the cost of arriving at answers regarding either of these; and (4) difficulty of determining broader operational and cost impacts of adopting the concept. As answers about (1) become more clear through basic research, ideas regarding applications begin to proliferate, as do questions of whether and how to explore their prospects. DARPA is at the forefront of this question and has the difficult job of determining whether enough is known to move toward an application and, if so, how to do so. At times this can be very controversial, as researchers may feel they do

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3 Discussion with Dr. Eberhardt Rechtin, August 1989.

4 Statement by Dr. Victor H. Reis, Director, DARPA, before the Subcommittee on Research and Development, Armed Services Committee, House of Representatives, April 23, 1991.

not know enough to guarantee success and are concerned that "premature" efforts may in fact create doubts about the utility and feasibility of the area of research, resulting in less funding and (from their perspective) less progress. DARPA, however, has a different imperative than the researcher to strive to see what can be done with the concepts or knowledge, even if it risks exposing what is not known and what its flaws are. This tension is endemic in DARPA's mission and can put it at odds with the research community that it sponsors.

At the other end of the spectrum, as projects demonstrate application potential, DARPA runs into another set of tensions, not with the researcher, but with the potential recipient of the research product. Given that the ideas pursued are innovative, perhaps revolutionary, they imply unknowns to the user in terms of how they will be implemented and how this implementation will affect their, the implementor's, overall operations. To this end the potential users seek to reduce their uncertainty, in what is a highly risk-intolerant environment, by encouraging DARPA, or some other development agency, to carry forward the concept until these risks are minimized. Achieving transition is increased by this additional risk reduction, but it also entails substantial additional cost and raises the issue of mission boundaries.

Generally the solution for transitioning to practice appears to be well-thought-out advanced planning with explicit definition of responsibilities and expectations and continuous interaction between DARPA as the advanced researcher and the ultimate receiving organization. Evidence has shown, for example, in the SIMNET program, that this can be very effective. Yet, there are times when the nature of the development is so revolutionary that its implications for existing operational practices are such that smooth, seamless, non-contentious transfer is unlikely. In such cases, success is likely to be slow, and to require intervention at the highest levels in DoD and extraordinary management measures to achieve.

As DARPA's history has shown, research projects, even explicit failures, often produce the additional capabilities and knowledge that, with persistence, can lead to eventual application or achievement. DARPA is one of the very few organizations, in Government, or even outside of Government, where such persistent research, tolerant of failure, in search of a success path, is permitted. From this perspective, it is important to appreciate the decisions that have been made to take on risky ideas, why they were taken on, and how they were managed. Through its history, for various reasons, DARPA has varied the nature of the risk, as well as the scope and kind of programs it has pursued. We make an effort here to understand the factors underlying the choices that were made, and to provide some insight based on this understanding.

C. INITIATION OF ARPA AND DARPA PROGRAMS

In the beginning of the Agency, ARPA programs generally were initiated by assignment from higher authority. ARPA was in essence a "special projects" organization with assignment from the White House to work on "presidential issues." This early program charter for such major issues as ballistic missile defense and nuclear test detection continued as the primary motif for the Agency through the mid-sixties. By the late 1960s ARPA research became much more "self-initiated" with considerable interaction with the DDR&E and increasingly with the military Services. Although assignments to DARPA have usually come via the DDR&E, and later the USDRE, they may have been initiated in the first instance with some higher authority--the President acting at the suggestion of the PSAC, the Secretary of Defense, or the Congress, making a request of the Secretary of Defense, and similar routes. In some cases, such as Project AGILE and work on STEALTH, the formal assignment from above came after the project or program idea was broached by DARPA.

Even during the formative years when the Agency was focused on a few "presidential issues," many project ideas and initiatives emerged from within the Agency and were brought to it from other sources, such as Service laboratories, other Government agencies, and from universities. More importantly, the general scope of the "presidential issues" established broad charters for areas of research, and most of the ideas to be pursued within these areas originated from a large variety of sources and directions including Agency initiatives, proposals made by other agencies, or as a result of unsolicited proposals from sources outside the Government. As often as not, two or more of these stimuli operated at once. Most assignments to DARPA have been defined broadly enough to permit great latitude in initiating projects generally applicable to the assignment. The Services, or their laboratories, would know of the assignment to DARPA or of work that Agency had initiated. Whether the Services were participants or not, they often felt free to propose that DARPA assume responsibility for some area of work that looked promising, but that the Service was not able or willing to fund for any of a number of reasons. And, as the work progressed, or simply in view of DARPA's reputation for funding promising but high-risk projects, industry and universities have approached the Agency for funding of specific proposals, in existing or new areas of work.

In most cases, whether projects have begun under the stimulus of an assignment to DARPA, or have been initiated by DARPA itself, there has usually been an ongoing stream of work outside of DARPA--in the Services, universities, industry, or in all of these--making the need for or potential interest in the results of a nascent project or program apparent. This stream of work also served as a source of background knowledge and expertise and interest in the community, on which DARPA could capitalize. That is, DARPA, by virtue of its work modes and its situation, usually found a diffuse, but fertile, base to build on when it undertook a task. On the other hand, it was often the case that the on-going work was seen by those at high levels in DoD or elsewhere (the White House or even the Congress) to be insufficiently supported or directed toward identified needs or concerns of national security importance, and that other defense research organizations were not appropriate for meeting the need.

The characteristics of the DARPA that most often led to the initiation of DARPA effort in such cases were (1) its readiness to accept a new idea; (2) its ability, by virtue of its charter and its role in Defense RDT&E, to take high risks in new areas of endeavor; and (3) its ability to turn around rapidly as a result of its short bureaucratic strings. At the same time, and for similar reasons, DARPA proved to be a convenient, or sometimes unique, mechanism to which the DoD could turn to initiate the effort. Such uniqueness may also have been part of the temporal circumstance--e.g., a Service might not be asked to undertake a multi-Service program because it was on record as favoring a different program direction, or similar reasons. Thus, it was often the case that, although some other agency existed that would have been physically able to take on the task, it either would not do so or found it institutionally awkward to do so at the time.

DARPA program origins have tended to converge from many sources, and it is thus difficult in most cases to trace them precisely. The data show that DARPA work on the projects examined has usually begun as a result of an initiative or suggestion from somewhere within the Government, rather than in direct response to proposals from outside the Government.[5] In about half the cases, there was a broad existing assignment to DARPA, and projects within the assignment began by any of the pathways mentioned. Often it was the case that DARPA's significant role was in bringing together disparate approaches or sources, some of which may have languished in other institutions, and galvanizing these into a more coherent program of research and/or development with

5 This is not to say that these ideas were not often stimulated by others from outside the Government.

explicit goals that could, at least in theory, be achieved. Whatever the source of a program or project, a key aspect of DARPA's taking on such programs was its risk-oriented, flexible, relatively unrestricted management, which gave it an ability to start an effort that the Services would have found daunting or simply did not believe in.

D. MANAGEMENT MAKING FOR SUCCESS

1. Management Characteristics

DARPA has been a small organization in relation to the size of its work program, and this has been one of the major influences on how it gets its work done and the success of that work ("success" has many forms, which will be discussed in the next subsection). DARPA has been characterized by an organizational structure and management approach that have both contributed to its success and in other instances, under different circumstances and requirements, proved to be a detriment.

The pluses of its management have included the following:

- DARPA has been able to attract very good people to manage its programs.

  DARPA's mission to pursue and support advanced research and its relatively unconstrained management environment have made it an attractive place for those who seek an unfettered place to explore technology and its application. DARPA has been seen as a place where one can see things done and make things happen with a freedom that is rare either in Government or industry. In contrast, in academic research the freedom is there, but not the funding and organizational wherewithal. Thus, DARPA has been highly attractive for the technological entrepreneur with a strong vision and desire to achieve.

  Two changes have had some impact on this characteristic of DARPA: (1) the more constrained environment caused by mounting pressures for DARPA to follow more traditional modes of contracting and management, and (2) the greater opportunities that have emerged in the private sector since DARPA's establishment to pursue advanced ideas in industry and to reap very high rewards for doing so. An additional concern is whether the talent pool of younger technically-exceptional individuals is being replenished, particularly in areas of interest to DARPA.

- Extensive autonomy for the project or program managers (PMs), and a short command chain with few if any intermediaries from the PM to agency management.

This has speeded technical and administrative decision-making and allowed the PM to exercise the judgment of his expertise, while the Director could insure supervision, trouble-shooting, planning response and early coupling to other DARPA and Service programs when appropriate, all without long time delays and the misunderstandings that could attend the use of many intermediaries.

- DARPA has been able to draw on the whole U.S. technical community.

One feature of the independent and venturesome nature of DARPA is that it has fostered linkages with the university and broader non-defense-specific technology base, which is more difficult in more restrictive or focused defense research. DARPA's prominent role in promulgating and sustaining non-traditional research areas, such as materials science and computer science in university environments and providing a means to move their preliminary concepts into increasingly defined applications, has been one of its hallmarks. Given its charter to pursue areas non-traditional to defense applications, it has developed an openness to foraging and exploring developments in a wide variety of venues and at times has become the primary means for supporting ideas that otherwise had little prospect of funding. Perhaps a victim of its success in this regard, DARPA is being looked upon by some as the funder for advanced, ambitious concepts, even where the link to defense is not readily evident, and for more mundane technology research and applications for which there is no other Federal funding agency. In the future DARPA faces some difficult decisions as to where in the broad technology infrastructure it draws upon to devote its energies and resources.

- Rotation of people with a lot of energy and ideas, as the program has changed.

This has permitted the organization to draw on the best talent in the country to manage its initiatives, and to suit the people to the tasks much more easily than if it had to keep shifting the same staff from one task to another. In particular, in some of the key programs, such as those that led to the successes in the computing field, it has recruited and then allowed and encouraged the people with the ideas to indulge their knowledge and their enthusiasms to carry their ideas to fruition.

- Contracting through the Services.

DARPA could use well-established contracting facilities without carrying the overhead of its own administrative organization for the purpose, and yet has been able to shorten the decision time in competitive procurements and in implementing contracts. This has become increasingly difficult as the procurement laws and regulations have demanded more explicit competition, but DARPA is still able to turn around faster than the Services can, even in the new environment.

- A mix of civilian/military staff.

  This has helped to bring an operational and Service flavor into much of the output, even where the Services may have had reservations, initially or throughout a project, about accepting or participating in the work directly.

The very factors that carried the seeds of success also occasionally have made DARPA project management difficult and could even have led to failure of some of the efforts. This has been particularly the case for large-scale system demonstration projects. Sometimes the work became so complex, and included so many elements, that one or a few individuals could not easily check up on and exercise effective management control over the contractor efforts. Also, letting the risk-takers have a relatively free hand has on occasion led to insufficient or tardy checks on poor judgments and perhaps wasted money. While DARPA has generally kept these problems under control, its decentralized management, with emphasis on individual initiative, or the very speed that made for success in other cases, could allow a project to move a long way down a mistaken road, and spend a good deal of money, before corrective action or cancellation could be effected.

We emphasize that DARPA has generally managed its programs very effectively. It is critical that extreme care be taken in any efforts to "improve" management control within DARPA or over DARPA. It is important to avoid saddling DARPA with restrictive management controls and procurement procedures to "ensure" no mistakes are made, that it becomes too encumbered to provide its essential and fundamentally different type of R&D support for DoD. Over time DARPA has become increasingly restricted by processes and procedures in conformance with more standard contracting organizations. Greater accountability and oversight have been achieved, and perhaps some greater degree of procurement fairness as well. Already there are concerns among those within and associated with DARPA that such procedures and controls have overly constrained DARPA's responsiveness. It is thus imperative that any additional management strictures be very carefully considered and limited so as to not impair DARPA's unique capabilities and approach. Attendant to this is another concern: that DARPA not become weighed down with mandated or expedient programs outside its high-risk, high payoff *advanced* R&D, that by their very definition require management and procurement approaches beyond those practiced by DARPA.

The ways in which DARPA's work has been conducted with the Services have also influenced Service attitudes toward the work and acceptance of its output. In cases where programs were joint, and tightly coordinated with Service interest, such as SIMNET, the ultimate outcome tended to be Service assumption of responsibility for the completion of the program or use of its output in another program, because that was intended from the start. In cases where the Service asked DARPA to undertake an effort, but did not participate itself (at least initially), the relationship and willingness of the Service to assume ultimate responsibility for the output often have depended heavily on interpersonal relationships between the DARPA principals and the Service managers who might be affected by and would have to deal with the output.

For those projects DARPA has undertaken, either by assignment or on its own initiative, that challenged Service doctrines or ongoing programs, the Services could either exert powerful opposition or remain indifferent. Our research has chronicled a significant number of such programs, starting with ARPA's very first assignments in ABM (Project DEFENDER) and space, that show the difficulties that arise when DARPA is given or takes on research that contradicts or confronts existing Service (or other DoD component) programmatic or operational domains. Major programs throughout the Agency's history have elicited strong negative response from potential "user" communities, who were uncomfortable or unhappy with the injection of contrary or fundamentally challenging concepts. Examples include major aspects of the AGILE program during the Vietnam conflict and the ASSAULT BREAKER standoff weapons concepts for attacking follow-on forces.

In these cases interpersonal relationships have been important, often involving the very highest levels of both OSD and the Services. When doctrinal issues have arisen, the Service may have opposed an effort even though the people involved behaved cordially toward each other personally. In such cases ultimate transfer was often problematical, and the intercession of the DDR&E/USDRE, and even the Secretary of Defense, sometimes was needed. When such intercession attended the program initiation, the Service generally could not ignore the program, and its ultimate acceptance of at least some of the output would be encouraged.

The presence of military professionals on the DARPA staff has helped DARPA's understanding of Service problems associated with the program and of the context into which the output would fall, and with later Service acceptance of the programs. Conversely, as in the examples of Ada and ASSAULT BREAKER, DARPA sometimes had to move the people who had conceived and worked on a project or program into a better (more authoritative) position, usually within OSD, to work on implementation with the Services.

2. Persistent Management Issues

A number of management issues have persisted throughout DARPA's history; they must be considered as "in the nature of the beast." These are:

- Knowing when something has or has not reached the end of the line. That is, making an appropriately timed decision to eliminate, transfer, or continue a project, since making it too soon could spell the difference between success and failure in the transfer, and waiting too long could needlessly use resources or even miss the key transfer opportunity.
- Transferring good ideas/projects when they challenge ongoing Service ways of doing things, doctrines, or requirements. Perhaps the prime example of the difficulty in such cases was the fact that the director of the Tactical Technology Office (TTO) was appointed head of a specially created office in OUSDRE specifically to help implement some of the ideas generated in the ASSAULT BREAKER and other tactical projects.
- Preventing large system programs from eating up smaller advanced research project budgets. By their very nature, high-risk *system* projects carry many unknowns, which could become sinks for funding at critical project stages when cancellation would as yet be out of the question; the money would have to be found somewhere, and justifying smaller programs in the competition could be difficult.
- Reconciling assigned new missions with Agency strengths and weaknesses. Some prime recent examples of this difficulty were the assignment of prototyping responsibility to DARPA, and the assignment of "productivity" related programs, such as SEMATECH, MIMIC, and STARS.

  The prototyping assignment grew out of the Packard Commission's work, and the search for a mechanism to determine feasibility of new systems without the costly (in time and money) processes of requirements establishment and early program development by the Services. DARPA experimented with the establishment of a separate Prototyping Office, but the prospect of extensive activity that might involve large system prototypes was daunting. In the end, it was determined that the intent of the Packard Commission had been to limit the DARPA contribution to totally new ideas that could be tested and demonstrated outside the necessity to build complete systems. It was but a short step to the conclusion that the kind of activity thus called for was, as has been noted earlier, characteristic of the work DARPA had always done, and the Prototyping Office was disestablished.

  The assignment of the group of "productivity"-focused tasks, SEMATECH, and others, created several definitional problems for DARPA. First, most of these programs were already in existence outside of DARPA and relocated into DARPA as a management re-organization decision. Second, the research generally undertaken in such programs is generally not advanced research, but mostly incremental in nature. Third, the DoD was only indirectly a consumer of the work; production of hardware and software is generally done for DoD by vendors. Fourth, many of the concerns are much more broad in scope than DoD's explicit interests, extending to the overall industrial base of the country, and thereby raising political boundary issues. Fifth, the funding requirements for developing and implementing solutions to issues of such broad scope create major budgetary demands which conflict with DARPA's and DoD's direct mission requirements. These have all made the absorption of such an assignment and the definition of programs within them more difficult.

While all new assignments have not been equally troublesome to DARPA, and some have been eagerly sought by the Agency, these examples illustrate the difficulties faced by the Agency when the nature of the DoD problems, and subsequent DARPA tasking, has changed radically.

The following chart [Fig. V-1] summarizes, for the projects and programs described in the first two volumes (and in the order in which they are listed), their genesis and some of their key management characteristics. Some of the data shown also bear on the conditions making for success, which will be discussed next.

E. INDICATORS AND MEASURES OF SUCCESS

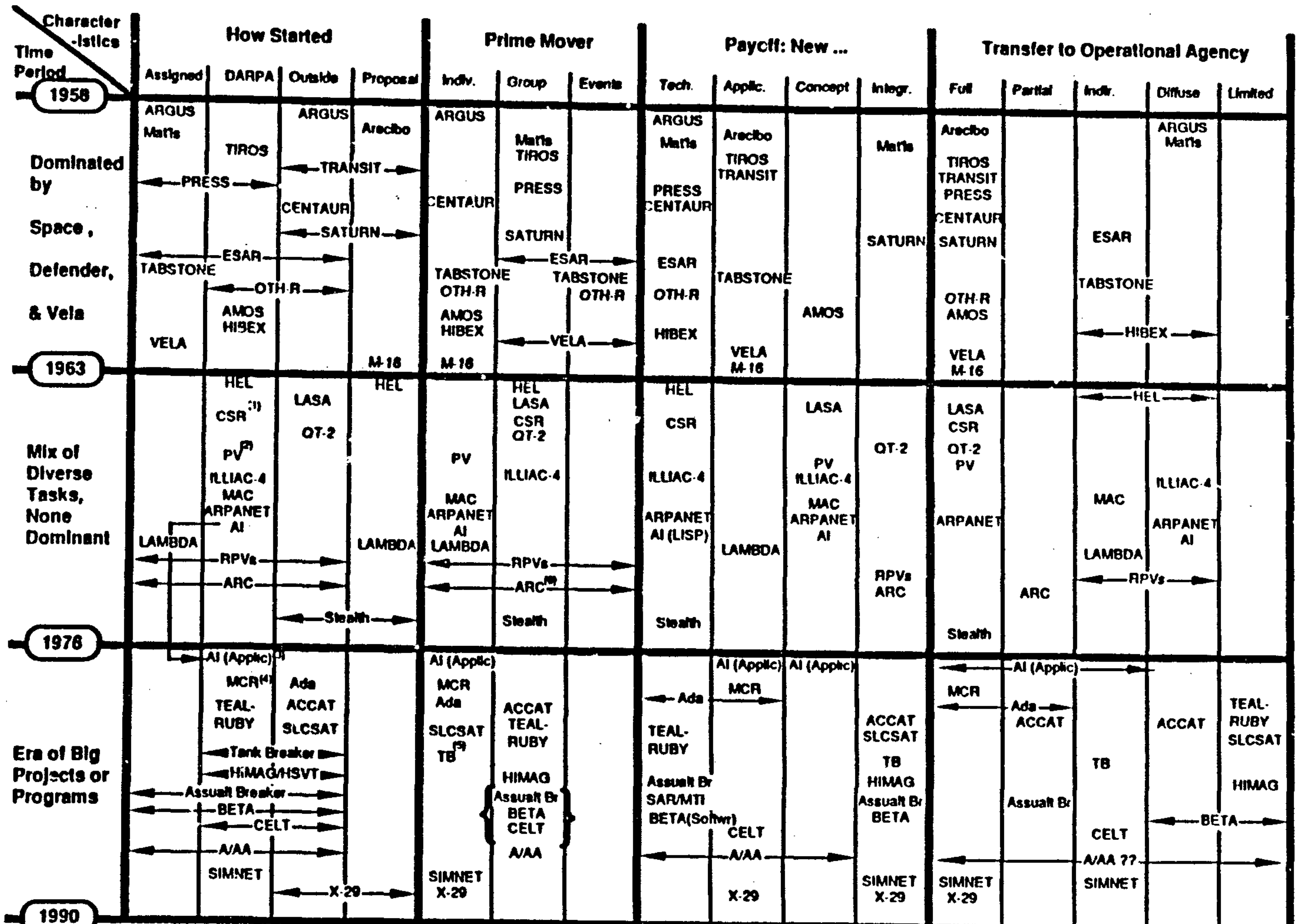
1. Aspects of "Success"

The following chart [Fig. V-2] lists the projects covered in rough chronological order, divided into the main periods of DARPA activity that were described in Section II. The genesis of the projects is shown in the first two columns in more detail than given just above. The second two columns list the nature of the payoff from the projects and how the payoff was conveyed into a user community.

Outcomes of ARPA and DARPA projects have included development or initial demonstrations of new technology, demonstrations of new applications of known technology, development and demonstration of new concepts of experimentation or operation, or integration of diverse technologies into new system concepts for the first time. Often more than one of these kinds of payoff could be achieved by the same project, as illustrated in the third column. The mechanisms by which the product could be adopted by user communities has been similarly diverse. In some cases, the results of projects or

Project	Need Existed	Better Mousetrap	Ongoing Stream	Individual's Push	Service(s) Aboard		Little/No Alt. Mechanism	Other
					At Start	Before End		
ARGUS	X			X	X		X	ARPA Originated
TIROS	X		X	X	X		(NASA)	
TRANSIT	X		X			X	X	
CENTAUR	X		X	X	X		(NASA)	
SATURN	X		X	X	X		X	
ESAR	X		X	X	X		X	HI RISK; ARPA Overrode Experts
TABSTONE	X		X	X		X	X	Confirmed Feasib. Over Svco Failure
HEL		X	X			X		
OTH RADAR	X		X		X			DARP*, Main Tech. Driver
AMOS	X					X	X	ARPA Originated
VELA HOTEL	X					X	X For Mgmt Reasons	Assigned To ARPA
VELA WWNSS	X		X				X For Mgmt Reasons	Assigned To ARPA
VELA LASA	X						X For Mgmt Reasons	ARPA Originated
AGILE: M-16		X		X		X		ARPA Initiative
AGILE: CAMP SENTINAL RADAR	X	X		(Lincoln Lab)		X		ARPA Originated
AGILE: OT-2	X			X	X			ARPA Originated
POCKET VETO	X			X		X	X (For Doctrine Reasons)	ARPA Originated
ILLIAC IV		X	X	X		(NASA)		
PROJECT MAC		X		X		(Indirect)	X	ARPA Initiative
ARPANET		X		X		(Indirect)	X	ARPA Initiative
AI		X	X			(Indirect)		ARPA Initiative
MORSE CODE READER	X		(Part of AI but new problem)	X		(Indirect)	X	ARPA Initiative
ACCAT	X		X		X		X	ARPA Initiative
LAMBDA	X			X		X	X	
SLCSAT		X		Individ. In Navy		Undecided	X	
TANK BREAKER	X		X			X	X	DARPA Initiative
HIMAG/HSVT-L	X		X	X	X			
MINI RPV		X	X			X		DARPA Feasib. Demo
ASSAULT-BREAKER	X		X	X		Significant Elements	X	DARPA Integration
BETA	X			X		X (Plus NATC)		Response to DSB
CELT	X		X		X			
ARMOR/ANTI-ARMOR	X		X	X	X		X (For Mgmt Reasons)	Response to DSB
PRESS	X		X		X		X	
ARECIBO		X			Transfer to NSF		X	University Proposal
ACOUSTIC RSCH CTR	X		X	X	X		Uniquely Positioned	
SIMNET	X			X	X		Uniquely Positioned	
HIBEX	X		X			X	X	DARPA Initiative
STEALTH	X			X	X			DARPA Initiative
X-29		X		X		X	X	
IDL	X					NSF	X	Assigned to ARPA
PENAIDS	X				By passed by MIRV		Part of DEFENDER	Assigned to ARPA
RET. FOR CAUSE			X		X		X	Air Force Initiative
TEAL RUBY		X	Key Elem. New		X			DARPA Initiative
ADA	X			X	X		X	DARPA Convenient Mechanism

Figure V-1. Summary of Projects and Programs in Volumes I and II



(1) Camp Sentinel Radar, AGILE
(2) Pocket Veto (Ballon-borne Radar), Adv. Sen. Off. Both AGILE and the ASO work were assigned to ARPA
(3) SUR, Expert Systems; Strategic Computing
(4) Morse Code Reader
(5) Tank Breaker
(6) Acoustic Research Center

Figure V-2. Projects in Chronological Order

programs, usually expressed in hardware, were transferred fully to a user. In cases such as the early boosters or the ARECIBO telescope, the transfer was complete. In a few cases, mainly the VELA program, DARPA maintained R&D responsibilities because there was no convenient agency to which to transfer it.

Other transfers have been partial, limited or indirect. Given the multifaceted nature of some projects, several of these characteristics apply to the same project. For example, only the radar and ground launched missile aspects of the ASSAULT BREAKER system concept were adopted by the Services; the air launched missiles, the warheads, and especially the close coupling between targeting and strike were not adopted. In some of the indirect transfers DARPA demonstrated the capability, but the user then redeveloped it *ab initio* in a different form; the DARPA feasibility demonstration was enough to encourage the development. In the case of ARPANET, the concept was disseminated so broadly after DARPA created the initial network that it has become part of the general technological "kit of tools" of the nation in the commercial as well as in the military area.

It is important to emphasize that the degree of transfer to a "using" community does not necessarily connote the degree of success of a project or program. For example, the X-29 swept-forward wing aircraft was built and is flying in Service experimental programs, and much will be learned from it. However, it will not by itself influence the field of aeronautics or of tactical aircraft design in a revolutionary way. However, such programs as the weather satellite TIROS, the long towed array (LAMBDA) that led to the SURTASS ASW sensor system, and TABSTONE, the space surveillance demonstration, have had a profound effect on how the country and the Services use technology. The transfer mechanisms in many of these cases have not been as "clean" as that for the X-29.

Moreover, success in transferring the hardware or knowledge gained in DARPA programs often depends on timing and the relationship to other events and programs. For example, SLCSAT has had some successful technical technology validation. However, whether the Navy adopts the system for communication with submarines will depend on the Navy's concepts of submarine operation in the new tactical and strategic world that is emerging in the aftermath of the Cold War and on the budget available for such purposes in the new environment. Similarly, adoption of the outputs from the armor/anti-armor program will depend on the rate at which the Congress and the DoD decide we should replace our main battle tanks and other armored fighting vehicles, and on the rate of transfer of advanced Soviet, U.S., and Allied tank technology to the Third World, where we expect much of our military action of the future to take place.

a. Military Applications Programs

Transfer or transition to an explicit "user" is more easily ascertainable for some types of programs than others. This to a large degree has depended upon whether the end product of the effort was oriented expressly toward a military application, where the "customer" is a Service or Defense agency, as opposed to those areas of R&D where the "customer" is less explicitly a military organization and the application less directly military. For military systems or platforms, such as a tilt-rotor aircraft, or a light ground combat assault vehicle, weapons, such as the TANK BREAKER anti-tank missile, or a military surveillance capability, such as the LAMBDA TOWED ARRAY for submarine tracking, the military nature of the research is usually very clear and the "customer" reasonably well defined. In these types of programs concerns about program transfer as the effort matures toward an application-capable system is a major aspect of the program's success.

Yet, for such applications, successful transfer is not just a matter of good R&D and good program management on the part of DARPA. Successful R&D and a well-run research program attentive to issues of transfer certainly may be prerequisites, but are not necessarily sufficient in themselves to effect successful transfer to a user community. Another major dimension to the success of transfer is the extent to which the intended "customer" desires and supports the program and its potential product. This aspect of transfer has often been at odds with and created conflict with DARPA's mission and how others have assigned programs to it. By their very nature "high risk" R&D programs of the type undertaken by or assigned to DARPA are often seen by potential customers as beyond *their* concept of what they need, even though others elsewhere in the hierarchy may feel that the need exists. Often such programs, if technically successful, challenge the operational premises of the user community and entail major overhaul of existing assets and capabilities. R&D which enhances the current mission and operations of the organization is one thing, R&D that obviates or obsoletes it is another.[6] DARPA, given the nature of its charter and the programs assigned to it, frequently has sponsored the latter type of research--making transfer a much more difficult proposition.

6 A key aspect of the Services' conservative tendencies in such matters is their recognition of the substantial training and manpower costs that are often associated with fundamental departures from current methods of operation and the time needed to become proficient in new approaches. Interestingly, DARPA through its SIMNET may have provided the potential for breaking through this training bottleneck.

DARPA has taken on military-specific advanced R&D that has been successfully and relatively easily transitioned to Service and agency communities. Not surprisingly, this generally has come about when the Service or agency itself was involved in the concept and worked with DARPA to initiate the program. Usually for such programs the level of risk and the degree of challenge they have entailed for the operating agency have been relatively low as compared to other DARPA programs. Smooth technology transition can be attained, but the degree of technical and operational challenge of such programs will almost of necessity be lowered to gain this ease of transition. The key question for DARPA and OSD is: Will the potential payoff be substantially improved by a smoother more incremental program, or will it be greater from a more challenging, higher risk program that incurs greater difficulties in subsequent transfer?

b. Technology Base Programs

Apart from the more militarily-specific technology development, DARPA has engaged in research into more broadly applicable and more generic technology areas--most notably materials, electronics, and computing--that are much less explicitly identified with a military customer base. These can be characterized as technologies that enable, underpin, or support a military capability, not specifically military in nature, yet are intrinsically important to achieving major gains in military performance. For example, gains in processing speed and throughput have been instrumental to advanced weapons capabilities such as MIRV or precision-guided munitions (PGMs), yet much of the underlying technology that enabled such advances is applicable to a wide range of defense and civil applications.

DARPA's role in this area began early in its history with the materials assignment in 1960 and the start of its information-processing sponsorship that was motivated by interest in improving command and control in the early 1960s. This work began with a heavy orientation toward "basic" research, and most of it was done either at universities or research laboratories. In the case of materials the purpose of the program at the outset was the establishment of an "infrastructure" capability in "interdisciplinary research laboratories" (IDLs) which fostered in essence a new discipline, materials science. In information processing, although not as specifically focused on developing a technical infrastructure as was the IDL program, the motivation and result were much the same. While certainly not the lone agent, DARPA can be credited with being *a* (if not *the*) major instigator of computer science as a technical discipline.

Moreover, DARPA played a leading role in providing opportunities for new directions to emerge more quickly from this area, through its sponsorship and support of information-processing programs that challenged the existing industry and academic perspectives and time frame. DARPA's role as a forcing function and an "opportunity incubator" directly stimulated those who were adventuresome and impatient with the existing industrial and academic strictures and provided both an outlet and support environment (such as MOSIS) which encouraged their efforts. In addition, DARPA's approach was explicitly to encourage the movement of development out from academic research into commercial application so that others, including the DARPA information processing community itself, could take advantage of the new technology developments.

In this sense, DARPA's ISTO and its base of research organizations were both their own providers and customers--a symbiotic group that "boot strapped" one another by providing enabling capabilities at one level (e.g., microelectronics) that permitted other levels (e.g., CAD-CAM) to improve substantially, which in turn improved the capabilities of the other. It is important to note that the linkages across DARPA programs were also extremely important, as the progress made by ISTO depended on advances in materials processing and device research fostered in the materials part of DARPA's Defense Sciences Office. Being on the leading edge of both of these fields and encouraging the interaction between researchers in these fields was a key aspect of DARPA's success. This interaction and coupling of research in different DARPA program areas, as well as within a program area through open program reviews, in essence orchestrating a symbiosis, has been an extremely potent feature of DARPA's management.

The ISTO computer-processing area began with a very ambitious, but not highly specified mission, embodied in Licklider's "vision," that was fundamentally broad in nature. It was explicitly understood that the development in this area had scope and ramifications well beyond explicitly military customers, but that success over time toward achieving the broad vision would result in potentially large payoffs to DoD's processing needs.[7] The sustained and varied technological achievements stemming from this broad view of mission has produced notable results affecting military and civilian information processing, and must be considered one of DARPA's most notable and enduring successes.

7 Licklider's vision of "man-computer symbiosis" is still a challenge, still beyond our current capabilities, and still underlies DARPA's ISTO program.

2. Qualities Contributing to Success

Recognizing that success comes in degrees that are often hard to define in all their subtlety, it is nevertheless possible to generalize from the data to a list of qualities of DARPA programs and projects that made for success. These qualities have included the following:

- **A need existed for what the output could do.** Sometimes the need was apparent and urgent--for example, the space systems, the long ASW arrays, ASSAULT BREAKER, advances in the armor/anti-armor area. Sometimes the question of need was controversial but the fact that the output would meet a well understood need in a revolutionary way became apparent when the product appeared--for example, ESAR/FPS-85, STEALTH.
- **A strong commitment by individuals to a concept.** Sometimes both the need and opportunity were latent, but were brought out by a few imaginative individuals, and the product turned out to provide a "better mousetrap"--examples are TIROS and ARPANET with its sequelae. The most notable example of this is the often referenced "vision" of Dr. J.C.R. Licklider, that set the motif guiding DARPA's sweeping contribution over a vast range of information-processing technologies.
- **Bright and imaginative individuals were given the opportunity to pursue ideas with minimal bureaucratic encumbrance.** This not only illustrates the importance of personnel turnover in DARPA's management style, it emphasizes that most of the credit for DARPA's successes must be ascribed to the efforts of the people who worked on the problems.
- **There was an ongoing stream of technical developments and evolution.** The DARPA role often lay in taking hold of an important problem and a perhaps unrecognized solution space in the ongoing stream, focusing it, demonstrating feasibility and utility, then giving it a strong push toward utilization that it would not have had otherwise.
- **DARPA management gave strong, top-level management support.** With the top-level, imaginative people developing challenging projects, management must provide an environment that tolerates risks, and encourages the undertaking of ambitious concepts; but management must balance this with adequate oversight and guidance to keep programs "within bounds" and to ensure that ambitious programs are reasonably attentive to the practicalities of the operational world. With very few exceptions, DARPA has provided this kind of management.
- **Explicit effort, taken early, to improve acceptance by the user community.** DARPA's programs often challenged the Service bureauc-

racies, but, for most successful programs, DARPA somehow managed to gain their support or, in the doctrinally difficult cases, at least their acquiescence along the way. This required, first, a strong commitment by DARPA management and an ability to interest others in OSD (the DDR&E or USDRE, or even the Secretary of Defense) and to work directly with the Service officials to work out the necessary agreements for the work. Second, DARPA's success required a demonstrated excellence of performance in projects or program outputs. In a nutshell, the user community either was eager for DARPA's support or, if they found the DARPA a challenge, they found they could not ignore the work.

All of the successful DARPA projects described in the first two volumes of this work and in the above two summary figures [Figs. V-1, V-2] displayed some or all of the characteristics described above. In all cases of almost total failure, most of these ingredients were missing.

The TEAL RUBY experience is instructive in this regard. By the ultimate measure, the program failed because it was cancelled before the planned satellite could be launched, it was overrun beyond tolerance, and, although the system exists, any future flight has probably been overtaken by events. Yet the program had many technical payoffs, and can be said to have advanced the state of the art of focal plane array IR detection considerably, defined previously unknown problems of the technology, and showed the directions to go toward their solution.

It is tempting to speculate that TEAL RUBY illustrates the limits of the very management style, described above, that has made so many other DARPA programs successful. In the early days of ARPA equally large projects were brought to completion, and many of the smaller projects through DARPA's history suffered similar vicissitudes and had overruns of similar proportions, if not magnitude. The national security environment at the time was willing to tolerate greater uncertainty and groping. For example, the DISCOVERER series of recoverable surveillance satellites suffered 12 failures before the first successful flight.[8]

TEAL RUBY was undertaken in a less forgiving time, and it pressed the technical state of the art significantly harder than many of the other ARPA and DARPA large-program successes. The DARPA style of using few people and short chains of command together with the willingness to plunge ahead in one direction until technical problems

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8 W. E. Burrows, *Deep Black*, New York, Random House, 1986, pp. 109-110.

required a new direction, has worked very well on projects that are below a threshold of tolerable uncertainty and that can be completed in a few years. In the TEAL RUBY case the cost uncertainty stressed the DARPA budget, causing the program to stretch out past the useful time of management continuity, and it entered into a period of large-budget austerity when tolerance for uncertainty had decreased. Thus, many of the factors that made for success in smaller or earlier projects tended to hurt this one.

TEAL RUBY is a single case and is complicated by a number of factors and circumstances that explain its particular fate. There are examples of other ambitious and controversial DARPA programs, even co-terminous with TEAL RUBY that have proven much more successful--for example ASSAULT BREAKER. There is a danger in saying that, because of the results of TEAL RUBY, DARPA should avoid large *system* projects that are high risk and have many unknowns. We believe this would go too far. Perhaps the lesson is that such high-risk system programs should not be undertaken unless: (1) there is clear understanding of both the cost and time implications, (2) these are commensurate with the need, and (3) there is a willingness to devote the management attention and resources to such a program, including the continued enlistment of strong Secretary of Defense support. Given their extraordinary demands, even in an organization which is focused on the extraordinary, such major large-systems programs should be few and undertaken with extreme attention to how they impact on the overall mission of DARPA and its ability to successfully manage them.

VI. IMPLICATIONS FOR PLANNING DARPA'S FUTURE

A. INTRODUCTION

This chapter addresses the question of the strategic directions facing DARPA from the perspective and insights derived from this study about past successful DARPA efforts. We explicitly do not propose a future DARPA strategy; rather, we lay out the conditions facing DARPA in light of the lessons from the past work, with the objective of articulating for DARPA management issues for DARPA's strategic planning.

This is a time of turmoil and policy reformulation in the nation. DoD and Congress are groping for new strategic directions. Defense budgets are tight, and barring any strategic surprises, perhaps emerging from the aftermath of the Persian Gulf conflict, they may keep declining until they reach a new, lower level in real terms than they have had since the Korean War. Moreover, along with the changing political-military environment and this constrained budget situation, there have been some fundamental changes in the economic and technological position of the United States in relationship to other countries, that present critical issues for DoD, and DARPA in particular. A most germane concern relative to DARPA's mission and its past history is how to sustain our accustomed high level of U.S. military-technological competence in a period of internationalization of our technology base, particularly with growing hostility to and shrinking of the Defense budget, and growing civilian sector dominance of key technologies. Complicating this situation is an increasing national debate on the role of DoD in meeting a range of civilian problems, from drugs to economic competitiveness.

As the DDR&E, Dr. Charles Herzfeld laid out a plan for Defense technology, several parts of which fit the DARPA mold very well.[1] These include: the emphasis on feasibility demonstration of very advanced technological ideas, before the Services attempt to enter routine development programs with them; continual renewal of *subsystems* to radically upgrade capabilities of long-life platforms; and the creation of "fieldable" prototypes of such devices and subsystems which the Services can test and refine before

1 Testimony of Dr. Charles H. Herzfeld, DDR&E, to the Subcommittee on Research and Development of the Committee on Armed Services, House of Representatives, April 23, 1991.

making commitments to expensive development programs. As the first two volumes of this history show, work of this kind has been DARPA's forte. The DDR&E's testimony also emphasized the need for a vigorous technology base program, of the type that generated many of DARPA's sensing and processing successes. Opportunities for impacts as broad as that of ARPANET or the multiple opportunities created by such programs as MAC and VLSI clearly should be a key aspect of DARPA's overall mission. Such developments resulted from a broader vision and a patience of commitment that is a rarity in both Government and industry. A key to DARPA successes has also been the ability to recognize and exploit technological opportunities *before* they become apparent to the rest of the world.

B. KEY ISSUES FOR DARPA STRATEGIC PLANNING

DARPA faces a difficult task in this critical time: Can DARPA find a sure path when the country and the DoD are groping? *This is a new situation in DARPA's history--when it got started, or when it changed direction, the country knew where it wanted to go. Today this condition doesn't hold; the country still has not found its preferred new military R&D directions.* DARPA may be in a position to help the country decide its directions in the areas of R&D affecting military technological capability in austere times. To deal with the current situation, DARPA faces decisions in the following areas:

1. Toward What Military Needs and Threats Should DARPA Orient its Work?

The strategic situation is not yet clear. DARPA made some notable and very responsive contributions to the Desert Storm operation in the Persian Gulf. It may turn out that this involvement will be the basis for defining a longer term program to support military requirements for such engagements. Such a program could take the form of a "Project AGILE" as well as efforts to develop fundamental advances in technological capability. In an era of "down-sizing" the military forces, what role should DARPA play? Should it turn its attention mainly to non-system needs, such as surveillance and training? Should it focus on alternatives to current high-cost systems, seeking affordability through technology? Should it be seeking to replace the high cost of troops and man-operated systems through development of more autonomous systems requiring less operational and logistic support? Given the diversity and uncertainty of the threat arena, what role should DARPA play in defining an alternative set of weapons appropriate for a specific potential threat arena--or should it focus on those aspects of technology that are more flexible and fungible across a range of possible applications, seeking ways to quickly develop special, specific applications if and when necessary? These are the types of questions that DARPA faces as it addresses the ambiguities of the current and projected threat environment.

2. What Technologies Should Be Pursued as Having Potential Fundamental Impact for the Future?

Are there technologies in the offing or languishing that could be important to the military forces in the future and that may be fertile for DARPA's style of exploitation and advance? Should DARPA concentrate more on a search mode to explore such prospects, particularly now that many of its major thrusts of the past 10-15 years have come to fruition? DARPA's success in some areas derived from visions of radical transformation of an area of technology application--STEALTH, Licklider's ambitious "man-machine symbiosis," Kahn's vision to achieve breakthroughs in computer architectures enabled by an explosion of microelectronics technology through the VLSI program, the reformulation of tactical warfare through the integrated stand-off weapon strategy of ASSAULT BREAKER. This is to say that technology development requires both bottom-up nurturing of the basic science and visionary concepts of need and application. DARPA must continue to play a leading role in both these domains as the integrating, galvanizing force.

3. What Should DARPA's Links With the Civilian World Be?

The Packard Commission, the DSB, and many other groups have emphasized the importance of using civilian technology to advance military systems while reducing their costs, but little demonstration of useful approaches has taken place. DARPA may be in an excellent position to pioneer such application in the projects it undertakes for DoD. Moreover, with increasing concerns that the domestic technology base in many "critical" technology areas is declining, DARPA's role in supporting the "technological infrastructure" may receive even greater emphasis. But this raises important issues of scope of funding and mission. DARPA has become to some the agency of last resort for technology support, with "national security" being quickly extended to cover nearly any technology. Yet, does not supporting an ever-increasing venue of technologies and for these an ever-broadening range of activities (often going well beyond what most would consider "advanced research") risk seriously compromising the fundamental role and effectiveness of DARPA? Establishing such boundary conditions, commensurate with meeting national security needs, is extremely difficult but necessary, particularly when budgets are more constrained.

Additionally, DARPA has over the past decade rebuilt its relationship with the nation's research universities, resulting in major technological achievements that have had profound effects on our military strength and our economic well-being. DARPA should search for opportunities to foster potentially revolutionary academic developments equivalent to its support of "materials science" and "computer science" in the past. Perhaps one such emerging field is opto-computing and processing. Perhaps earlier efforts to develop systematic planning and decision-analysis capabilities and situational simulations for planning and training would be appropriate to address policy options in today's complicated and fast-changing security environment. Such tools could be relevant for addressing broader business and other decision environments and may foster academic linkages in universities across several disciplines.

4. What Budget Leverage Is Associated With Different Approaches and Technology Areas?

What areas, of those under consideration for DARPA programs, are so big nationally that DARPA projects can make little difference? What areas might be so sensitive to a key thrust that modest effort can make a huge difference? DARPA's overall budget has risen to well over $1 billion, based largely on the pursuit of several key, large-scale thrusts, many of which have come or are coming to fruition. At times in the past DARPA's budget declined substantially when major program thrusts were transitioned to others--a hallmark of DARPA's success. As DARPA re-orients itself today, given the changing threat, the new technological challenges, and the changing environment of technology development, what is the basis for establishing the scope of funding? DARPA's funding should not be based on some generic concept of size, essentially an endowment to technology pursuit; rather, it should be programmatically based--sized and scoped to specific technology pursuits founded on an articulated program of exploration and a vision for development. Intrinsic to such a programmatic perspective is a strategic plan linked to broader perspectives of national security requirements.

Closely related to the issue above is the issue of how big any project or program should be. DARPA successes emerged from both large and small programs. The larger programs will carry a greater risk of overwhelming the technology base work that has been very important to DARPA's influence on the world over the years. But the extent to which the latter should be a concern will depend on decisions about what kind of DARPA comes next, to be discussed below.

5. How to Appropriately Control Investment Risk, and yet Pursue Sufficiently Ambitious, Potentially High-Payoff Programs?

DARPA must realistically assess the level of prospective risk in any area--not risk of technical success or failure, but risk of malinvestment of precious resources without commensurate payoff, whether there is technical success or failure. A program could consume a big part of DARPA's budget with little direct payoff, like TEAL RUBY; it might make little difference in the long run, like the X-29; or a modest investment might have a potentially huge payoff, like the investments in LAMBDA, STEALTH, or ARPANET. The problem is to be able to anticipate, with some reasonable chance of being correct, the category of any nascent DARPA project and to make program decisions accordingly. By their very nature some of the areas DARPA pursues require large-scale funding, and may require several years to see results. But such programs require careful management and scrutiny and must avoid becoming self-fulfilling "blank check" programs, and becoming too independent or too "important" for application of management's oversight. There is a tendency for protagonists of a technical area to lay claim to resources due to their own, well-meaning definition of the area's intrinsic importance. DARPA must balance these against larger criteria and priorities, and revisit these with some frequency.

6. How to Evolve Programs in Areas of Success?

Advanced computing has been one of the most successful areas of DARPA endeavor for much of its history. The earlier projects, some of which became quite large in scope, were run in a very decentralized manner with extensive university participation. DARPA established Centers of Excellence at the universities, which defined the details of the programs and which DARPA encouraged to exchange information and maintain fertile fields of ideas through frequent meetings of the researchers characterized by open exchanges of results and plans. Even at the time, the issue of balance between that mode and the more structured system approaches to the large EEMIT projects had to be resolved. The pattern in the computing area changed with the Strategic Computing Program, and current DARPA efforts in High Performance Computing involve large hardware as well as software programs with major industry participation. The field itself is now much bigger with many more players, the DARPA programs are more visible, they require more hands-on management by DARPA, and their dual-use nature in today's commercial competitive environment could work against the openness that characterized the earlier programs. The issues for DARPA attention and decision now are how, under these changed circumstances, to maintain an environment as conducive to success as was characteristic of another era; whether the changed nature of the field and management style may militate against the kinds of major successes that could be achieved when the field was more "virgin territory," and to identify correctly what today's "virgin territory" is in the new environment.

C. DARPA OF THE FUTURE

The planning envelope or "strategic space" within which DARPA can make decisions regarding the nature and thrust of its programs is defined by a spectrum of possible options. DARPA, and the higher level management that must consider DARPA as a key DoD resource, must determine what kind of organization DARPA should be in light of the uncertain future. Over its history DARPA has fulfilled several roles, and most of these simultaneously, but with very pronounced differences in emphasis. Over the course of its history DARPA has been:

(1) The singular focus for very large-scale, broadly chartered, nationally important technology applications programs--space, DEFENDER.

(2) The progenitor and chief supporter of major areas of basic science and generic technology with potential military, as well as civil, consequences--lasers, seismic technology, computer information processing, materials research.

(3) The developer of specific, large-scale system concepts and prototypes (weapons and non-weapons) including their operational demonstration--ASSAULT BREAKER, STEALTH; ILLIAC IV.

(4) The supporter of very unusual, highly experimental, extremely advanced concepts for weapons, systems, and capabilities--particle beams, autonomous vehicles, image and speech understanding systems.

(5) The developer of operational systems and capabilities for direct application to an existing military conflict or mission--AGILE, and most recently the DART system used in Desert Storm.

(6) The funder of research aimed at improving the technical capabilities of an industry to produce a "critical" defense-related technology--SEMATECH.

(7) The supporter of the acquisition of fundamental knowledge needed to better understand phenomena related to a potential defense application--ARGUS, PRESS, ARECIBO.

While this list could be elaborated further, the key point is that DARPA has pursued a wide range and scope of technology research and development activities that, at the time and under the circumstances, were seen by its management, and those they report to, as being germane and appropriate. The most important underlying aspects of these endeavors was (1) their potential for making a difference for a perceived national security need, and (2) the need for a special place to pursue the work. When the need itself lost its imperative or was resolved (sometimes aided by the success of DARPA's own efforts), or the work had so evolved (again often with the success of DARPA's effort) that the special place was no longer needed or appropriate, DARPA's involvement either ended or re-focused. DARPA's success generally can be measured by its transitioning an effort or program and moving on to the next problem. When it has had sustained involvement it generally has been in (1) enduring problems that are of great significance, but very difficult, and (2) technology areas that were just emerging where additional research has created new opportunities as the area has evolved.

From this perspective the options for DARPA's future posture emerge as a range of differing emphases that DARPA needs to consider as it frames a strategic perspective. The range of possible emphases include:

- **A focus mainly on technology exploration or search aimed primarily at generic application**

  With such ambiguities pervading the security arena, DARPA's most effective role might be on fostering generic technologies and the overall technology base, rather than concentrating resources on specific technology applications. It seems to be the case that many of the thrusts of 15 years ago have essentially played out and reached fruition, the threat environment is uncertain, and the prospect of new acquisitions to employ major technological breakthroughs is low; therefore, it would be reasonable for DARPA to concentrate on a technological search mode, similar in some ways to the early- to mid-1970s. In essence this perspective suggests that on-going thrusts should be carefully assessed to determine whether they are reaching diminishing returns and carrying on mainly through their own momentum, and whether they still meet the primary criteria for DARPA investment: high potential for making a difference in an area of defense importance and requiring the special offices of DARPA.

- **A focus on shaping the advanced defense technology base**

  DARPA could become the focus for advancing the DoD technology base and bringing it to the point where DoD could capture new directions of development of military systems from the DARPA output. In this strategy, DARPA would concentrate in the 6.1 and 6.2 areas; it would establish links to the DoD laboratories and any mechanism newly established to manage them, and it would, by judicious support of projects and programs at the labs and in industry, pave the way for advances in new areas of military technology. It could be product oriented or process oriented. In the former, it would concentrate on building "fieldable" prototypes and small systems or subsystems for new technology insertion in existing systems, following the DoD technology strategy. If it were to concentrate on process rather than product technology, it might have extensive programs in such areas as software engineering, manufacturing technology to reduce costs of highly variable, few-at-a-time procurements, and other approaches to productivity increase under the new conditions in which DoD must operate. Overall, such efforts might deal with existing or new kinds of systems and subsystems, or both. There is, in fact, no reason (other than budget availability, perhaps) why DARPA could not, in this mode, have extensive efforts in both the product- and process-oriented directions.

- **A special-mission-oriented DARPA**

  In keeping with its early history, DARPA could be re-focused as a special-mission organization, designed to meet fundamental challenges facing the DoD and possibly the country, to which the DoD has been asked to contribute. Such initiatives could include:

  - Great expansion of simulation networks for training military forces and evaluating new equipment in combat-like situations, while saving much of the cost of doing so in the field (but not all of that cost, because field operations to some degree are indispensable to real-world performance evaluation).

  - "Foraging" for civilian technology applications to military systems, as recommended by the DSB, and demonstrating those applications in prototype military equipment of suitable size and character.

  - A program to advance our national "technological security" in the manner of High Performance Computing, SEMATECH, High Definition Systems, and other high-technology "seed corn" related to defense technology development and applications. Underpinning this thrust is an understanding that the technology posture of the United States, relative to that at the time of DARPA's inception, has changed fundamentally, and requires new ways of DARPA and DoD overall to interact and cooperate with other Federal departments to meet the challenge. In fulfilling such a role, programs would have to be structured and executed in such a manner as to ensure that in the process DARPA does not become excessively distracted or disabled from pursuing its main mission--developing advanced technology for the nation's defense.

Clearly, all these modes of operation need not be considered as mutually exclusive. DARPA could, subject only to budget limitations, undertake to follow elements of several or even all of these options in a program tailored to suit conditions and the desires of its higher level sponsors and the national need. But it would have to be careful not to make the mix so random that no clear management and program pattern shines through. The history examined here shows that DARPA efforts had their greatest success when there was a clearly defined sense of mission and direction in the Agency and DoD.

To assume any of these modes of operation, DARPA and the DoD would have to define DARPA's role and its current mission to enable it. DARPA has served many and multiple purposes for the DoD through the years. Together with the Secretary of Defense, and perhaps other important Government leaders including members of the Congress, the President and his Science Advisor (for all of whose participation there is ample precedent), DARPA is now at an appropriate point to spell out the aspects of a redefined and refocused mission and program.

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