11-1-1995

The Advanced Research Projects Agency: A Discussion of Evaluation Techniques and Overview of the Agency's Enchanted Past and Uncertain Future

Timothy W. Swett

University of Nebraska at Omaha

Follow this and additional works at: https://digitalcommons.unomaha.edu/studentwork

Please take our feedback survey at: https://unomaha.az1.qualtrics.com/jfe/form/SV_8cchtFmpDyGfBLE

Recommended Citation

This Thesis is brought to you for free and open access by DigitalCommons@UNO. It has been accepted for inclusion in Student Work by an authorized administrator of DigitalCommons@UNO. For more information, please contact unodigitalcommons@unomaha.edu.
The Advanced Research Projects Agency
A Discussion of Evaluation Techniques and Overview of the Agency's Enchanted Past and Uncertain Future

A Thesis

Presented to the

Department of Economics

and the

Faculty of the Graduate College

University of Nebraska

In Partial Fulfillment

of the Requirements for the Degree

Master of Arts

University of Nebraska at Omaha

by

Timothy W. Swett

November 1995
THESIS ACCEPTANCE

Acceptance for the faculty of the Graduate College, University of Nebraska, in partial fulfillment of the requirements for the degree Master of Arts, University of Nebraska at Omaha.

Committee

<table>
<thead>
<tr>
<th>Name</th>
<th>Department/School</th>
</tr>
</thead>
<tbody>
<tr>
<td>E. Lee</td>
<td>Economics</td>
</tr>
<tr>
<td>John</td>
<td>ISQA</td>
</tr>
<tr>
<td>Arthur Diamond</td>
<td>Economics</td>
</tr>
</tbody>
</table>

Chairperson: Arthur Diamond
Date: November 26, 1995
Abstract

Government often has been maligned as a source of funding for innovative research due to stories of failures caused by factors such as bureaucratic micromanagement, overly-restrictive regulation, and lack of customer focus. While basic research may be regarded as a public good, and therefore within the domain of government funding, applied research is often considered best left to the private sector. One government agency sometimes mentioned as an exception to the rule of government mismanagement in applied research is the Advanced Research Projects Agency (ARPA).

Founded in 1958, ARPA has compiled an impressive list of accomplishments in the area of military technology. Ironically, the agency is known equally well for the commercial spin-offs of its research, particularly in the area of computer technology. These commercial successes have led many in Washington, D.C. to believe the agency's research agenda should be expanded to focus explicitly on "dual-use" technologies, with the intent of benefiting both the commercial and military sectors simultaneously. Reflecting this mission expansion, in recent years ARPA also has been viewed as a funding instrument to prop up America's high-technology industries against foreign competition.

Surprisingly, no study has attempted to quantify ARPA's impact on technology in relation to its investment. This paper lays the groundwork for such an effort by surveying the most promising research evaluation methods, reviewing the sources of information available to support an evaluation, and identifying the potential pitfalls. Through a survey of articles related to ARPA, and a review of a study commissioned by the agency in the late 1980s, the paper identifies factors potentially responsible for ARPA's well-known list of achievements. Finally, the study warns that ARPA's recent direction may have taken it off the trail-blazing path it once traveled.
Acknowledgments

I have been blessed with a terrific advisor, Prof. Art Diamond. I can never thank him enough for all of his work and his tremendous patience. His calm encouragement carried me through several times of panic! I also owe a debt of gratitude to my committee members, Prof. Kim Sosin and Prof. Justin Stolen, for their generous service in reviewing my draft and providing insightful advice along the way. More than anyone else, I thank my wonderful wife, Heather, who suffered through many evenings apart from her husband. Her understanding and encouragement are unsurpassed. Finally, I thank God for answered prayer, and for once again demonstrating that I never have cause for worry.
# Table of Contents

Table of Contents ..................................................................................................................................................1
Introduction ...........................................................................................................................................................2
Chapter 1: A Brief History of Government-Funded Research ............................................................................6
  1. Ratio Models ................................................................................................................................................11
  2. Economic Models ......................................................................................................................................12
  3. Bibliometrics ............................................................................................................................................15
  4. Peer Review .............................................................................................................................................18
Chapter 2: Evaluating Research and Development ..........................................................................................9
  1. Ratio Models .............................................................................................................................................11
  2. Economic Models .....................................................................................................................................12
  3. Bibliometrics ...........................................................................................................................................15
  4. Peer Review ............................................................................................................................................18
Chapter 3: History of ARPA...............................................................................................................................24
  Figure 1: DARPA Budget (Nominal $) ....................................................................................................28
  Figure 2: DARPA Budget (1990 $) ........................................................................................................28
Chapter 4: Difficulties in Quantifying ARPA's Success .................................................................................32
Chapter 5: The Secrets of ARPA's Success .......................................................................................................37
  1. Funding Research is Job One ................................................................................................................38
  2. ARPA Does Not Perform Its Own Research ......................................................................................39
  3. Small Size ...............................................................................................................................................39
    Figure 3: ARPA Organizational Structure ............................................................................................40
    Figure 4: ARPA Staff Size ....................................................................................................................41
  4. Rotation of Highly-Qualified Personnel .................................................................................................42
    Table I: Tenures of ARPA Directors (Source: ARPA, unpublished) ....................................................43
  5. One Customer: DOD ..............................................................................................................................44
  6. Authority to Focus on High-Risk Research ..............................................................................................45
  7. Freedom From Micromanagement .........................................................................................................46
  8. Concentration of Resources on Centers of Excellence ........................................................................47
Chapter 6: Dangerous Trends..............................................................................................................................48
  1. Funding Research is Only One of the Jobs - The SEMATECH Case ....................................................48
  2. Expanding Size .......................................................................................................................................56
  3. Changing Composition of Personnel .....................................................................................................58
    Figure 5: ARPA Military/Civilian Staff Mix .........................................................................................60
    Figure 6: Military Presence on ARPA Staff ..........................................................................................60
  4. Loss of Customer Focus .........................................................................................................................61
  5. Increase in Micromanagement ................................................................................................................65
  6. Resources Distributed Geographically: The TRP Case ........................................................................66
Chapter 7: Conclusion .........................................................................................................................................67
Appendix A: Approximate ARPA Investments, through 1988, in Projects Selected by IDA ......................69
Appendix B: The AIDS Patent Project Search Engine ..................................................................................70
Appendix C: ARPA Staff Size by Year ...........................................................................................................71
Glossary ............................................................................................................................................................72
References ..........................................................................................................................................................73
Introduction

Many have maligned government agencies as a source of funding for technology, arguing that government projects are all too often driven by concerns other than efficiency, effectiveness, or public welfare. The historical failures of government attempts to manage technology are well documented (Squires 1984; Eads and Nelson 1971; Griliches 1987). A particularly interesting example is provided by George Eads and Richard Nelson. They studied the commercial airline industry after World War II and found that, by 1970, 80 percent of the world's commercial airline fleet was built in the United States. This happened even though U.S. manufacturers had to fund their own research and development, while the British government paid for up to fifty percent of the development costs of new aircraft and required their own airlines to buy them. In another study, Zvi Griliches examined trends in productivity for a sample of 652 manufacturing firms and found that, over the 1967-1977 time period, the greater the proportion of total research funded by the companies, rather than the government, the greater the value-added of that research (Griliches 1987).

Critics of government-funded research and development (R&D) argue that government failures demonstrate the need to leave technological funding to the private sector, where the potential benefits of investments in new technology are likely to be weighed more carefully against the costs and risks involved.

A primary obstacle to efficient government funding is, of course, the ever-expanding bureaucracy which seems so prevalent in government institutions. Understanding this phenomenon of constant expansion of the government and its agencies requires a review of Buchanan and Tullock's theory of Public Choice (1962). Political theorists often assume the individual working in the public sector acts not in his or her own self-interest, but in the public interest. Indeed, proponents of socialism rely on the
idea that personal motives for private gain can be replaced by the motivation to serve the social good.

At the very foundation of democracy, however, is the reliance upon competition among individual actors in the economy, all of whom operate under the same set of constraining laws. In the democratic system, therefore, the concept of individuals acting in their own self-interests is crucial to the operation of economic markets. Is it then reasonable to expect people to act in their own self-interests in their private-sector transactions, but in the interest of the so-called "public good" when employed by the government? Buchanan and Tullock and the adherents to the theory of Public Choice do not think so. They argue that those working for the government will pursue their self-interests in both their public and private lives. Buchanan and Tullock further argue that no clear-cut definition of the "public good" exists in the first place. Because people rarely agree on what constitutes this nebulous public good, the assertion that the majority of politicians strive for such a goal is rather meaningless (Buchanan and Tullock 1962).

One way for politicians to act in their own self-interests is to garner publicity for themselves as they champion causes which are disproportionately beneficial to their own constituencies. In so doing, they hope to secure the loyalty, in the form of reelection votes, of an admiring constituency. Obviously, if all politicians voted independently, only the rare issue of apparent benefit to the majority of the country would receive enough support to become law, as each legislator would vote in favor of only those issues beneficial to his or her own constituency. In reality, however, legislators form coalitions and engage in a ritual of quid pro quo which ensures that most, if not all of them, are able to amass a treasure chest of pork for their respective constituencies. This, incidentally, provides a partial explanation for the staying power, or apparent advantage enjoyed by incumbents, as well as the Herculean effort required to decrease the federal budget.
Cutting an item out of the budget may risk the crumbling of coalitions, the future withdrawal of support for budget items favored by other legislators, and "hell to pay" at the ballot box. Justifying their own pet programs while blaming the rest of the Congress for not balancing the budget is, unfortunately, a much safer option for most politicians.

One government agency sometimes mentioned as an exception to the rule of government mismanagement is the Advanced Research Projects Agency (ARPA), previously known as the Defense Advanced Research Projects Agency (DARPA). Edward E. David Jr., President Nixon's science advisor and a former Bell Laboratories executive, claims, "Among all the outfits that dispense public money, this one has produced the most" (Perry 1991, 65). Interestingly, the innovations for which ARPA is best known are the commercial applications of its research and development, often referred to as "spin-offs" (Brown and Wilson 1993). ARPA is most famous for its role in the development of the computer industry and as the sponsor of many important innovations in that industry. The agency has made its mark with such developments as automatic teller machines (ATM), computer graphics, the Internet (formerly the ARPANET), supercomputers, Artificial Intelligence (AI), expert systems, parallel processing, and even the computer "mouse" (Flamm 1987; Kitfield 1990). In addition to spinning off computer technology, ARPA has spun off entire companies, such as Silicon Graphics, Sun Microsystems, and MIPS Computer Systems. In an article about commercial spin-offs from defense spending, one writer for Fortune magazine even proposed that ARPA "may have done more for U.S. competitiveness than any other organization" (Perry 1991, 65). Ironically, ARPA probably is known less for its equally-impressive list of military accomplishments, which include "stealth" technology and "smart weapons," the Strategic Defense Initiative (SDI), the National Aerospace Plane (NASP), night vision goggles, advanced sensing devices, and a myriad of other military advances.
With such an impressive list of successes the question then begs to be asked, has ARPA proven itself to be significantly more efficient at funding technology than other agencies? If ARPA has been more successful at funding technology than other government agencies, then we might endeavor to identify and explain the reasons for ARPA's success, and thereby provide criteria for future government funding of scientific research.

Chapter 1 provides a brief history of government funding of scientific research, and traces the roots of pork barrel spending in the area of science funding. Chapter 2 reviews some of the common approaches to evaluating research and development, with an emphasis on those techniques most likely to apply to ARPA. Chapter 3 provides a brief review of ARPA's history. Chapter 4 addresses the problem of evaluating the agency's success and discusses the most comprehensive study on ARPA to date. Though no definitive answers exist, and detailed analysis is lacking, available evidence suggests the conclusion that ARPA has enjoyed greater-than-normal success in funding technology. Chapter 5 identifies some of the explanatory factors in ARPA's record of success, while Chapter 6 highlights some disturbing trends and concludes with a word of caution concerning the future of the agency.
Chapter 1: A Brief History of Government-Funded Research

Large-scale government funding of research and development began during World War II. Prior to that time, the Department of Agriculture had administered research funds, but few other agencies had played a major role in funding research. Focused on military requirements during the war, government-funded research was tremendously successful at advancing the state-of-the-art in military weaponry. This funding led to the invention of radar, sonar, the proximity fuse, and the beginning of the atomic age. Government-funded research during this period also paved the way for several health-care advances, including penicillin and DDT. Focused on these successes, President Roosevelt and many of the nation's top scientists resolved that the government's role in funding research should continue (Martino 1992).

Since its inception in 1887, the Department of Agriculture has followed (and still follows) a set formula of disbursing research dollars "to assure that each state, and almost every congressional district, gets its share" (Martino 1992, 21). The department has not attempted to evaluate individual proposals before doling out its research funds. Instead, it has distributed its research funds, according to certain formulae, to ensure geographic equity (Martino 1992; Huffman and Evenson 1993). Ironically, economic analyses have found that the rates of return on agricultural research "have exceeded the normal rates of return on investment in the economy" (Schultz 1981, 105). One explanation for this success is that, because optimal agricultural practices differ across different regions and climates, distributing research funds geographically may be the best approach. Such a method is not, however, likely to be very efficient in those areas of scientific research which are not geographically dependent.
Post-WWII agencies chose not to follow the Agriculture Department's example, but instead distributed research funds on the basis of peer review. Researchers submitted proposals to the respective agencies and a peer review panel decided which proposals were most worthy of funding. This system contradicted the desires of Vannevar Bush, the head of the Office of Scientific Research and Development during the post-war Roosevelt administration. Bush advocated a system by which all government research grants would be administered by a single government agency, the National Science Foundation. Fortunately, the fact that research ended up being funded by multiple agencies probably helped prevent pork-barreling for a long while. Since the numerous agencies had to compete for proposals from top scientists, they had to earn good reputations (Martino 1992).

According to Joseph Martino, pork-barreling in science funding really did not become prevalent until 1983, when Washington lobbying firm Cassidy and Associates obtained direct appropriations for labs at Columbia University and Catholic University. Congress directed the Department of Energy (DOE) to spend the money without even asking the DOE, or anyone else in the scientific community, whether the project had any merit. Martino goes on to explain:

In 1983 and 1984 together, Congress appropriated $100 million for laboratories and research projects solely on the basis of lobbying by the recipients. There were neither requests by the government agencies directed to spend the money nor congressional hearings on any of these appropriations (1992, 2-3).

In 1985, this trend continued as Congress, without any hearings, ordered the DOE to spend $56.5 million for projects at nine universities. After the Department of Defense (DOD) established the University Research Initiative to sponsor defense-oriented research
at universities, Congress directed which universities would receive the funds. Within seven years, annual pork-barrel appropriations for science grew from a few million to a quarter of a billion dollars (Martino 1992).

Obviously, pork barrel spending is a suspect in some of the failures of government-funded research over the past decade, but what about the decades and even centuries prior? If Martino is correct in his assertion that pork barrel spending in the area of science funding is a rather recent occurrence, then some other factor must be responsible. In general, those who criticize government funding of science refer to government funding and targeting civilian technologies. Actually, the federal government has fared pretty well at funding military technology. The dominance of the American military machine and the desirability of American military hardware bears witness to this success. Instances of government failures have occurred when the government has decided that the market has overlooked a great opportunity and that it needs to push ahead in technological areas which the private sector has avoided. The lack of commercial investment in military R&D, which is explained in Chapter 3: History of ARPA, creates rich opportunities for ground-breaking research by government agencies. In the private sector, with a multitude of buyers and sellers, if no one is willing to invest in a particular area of applied research, the reason can be presumed to be an insufficient expected reward for the level of risk involved. If so, the phenomenon of government agencies experiencing failure in these areas should come as no surprise.
Chapter 2: Evaluating Research and Development

Government research managers are interested in determining the value of the research they support for at least two reasons: to help them improve the management of their programs, and to demonstrate to their bureaucratic superiors, to Congress, and to agency clientele groups that their programs have produced benefits that justify their cost (Roessner 1993, 192).

Evaluating Research and Development efforts is almost always a touchy subject. Dr. Alain Barbarie, in his essay on the evaluation of Canadian R&D (1993), identifies three primary difficulties in performing such evaluations. First, those outside the scientific community are justifiably uncomfortable with evaluating the work performed by scientists. Those in the position to make decisions regarding the fate of research organizations often are not scientists themselves, and may feel unqualified to pass judgment. Second, scientific research is very difficult to evaluate in terms of a traditional cost-benefit analysis, due to the fact that the results of scientific research may not be seen for many years. Also, the impact of a single research project may not be identifiable, as it combines with many other research projects to impact an innovation. Third, scientists themselves often resist the evaluations. As Dr. Barbarie explains,

The scientific community in the Federal Government tends to be reluctant to have R&D work evaluated by any criteria other than those of quality of research. Preservation of the status quo is obviously a strong incentive; however, many deeply believe that science, if done well, is always worthwhile and that outsiders have neither the right nor the necessary qualifications to question it (1993, 156).

Incidentally, the reluctance to evaluate R&D on other criteria is not peculiar to the public sector. In a recent article, Bell Laboratories' scientist and Nobel laureate Arno Penzias recounts a story of how researchers at his company were encouraged by management (who, at Bell Labs, are also researchers) to build the world's most powerful laser diode. They succeeded in doing this and for their efforts received much praise, including the "best
paper“ award at a professional conference, but they failed to meet the customer requirement for a producible and usable laser diode. These scientists failed to consider the customer's criteria for success. Evaluated from the customer's perspective, this effort would be regarded as a failure, yet the scientists considered it a rousing success. Penzias acknowledges the scientific value of producing a record-breaking laser, but ponders whether a much more valuable product might have resulted had the researchers included compatibility with existing fabrication methods and other customer requirements as constraints in their optimization effort (1995).

Despite these difficulties, evaluation of government-funded R&D has become a hot topic around the world. As a result of conferences sponsored by the Commission of the European Communities in the 1970s and 1980s, all European Community research projects were evaluated regularly by the mid-1980s. Japan also became serious about research evaluation in the early 1980s, and in fact commissioned the Asahi Research Center to survey the techniques in use throughout the world. Though the United States has been less enthusiastic about pursuing formal evaluation programs, the U.S. Congress has pushed for them, and the Office of Naval Research (ONR) has led the way in establishing such programs (Cozzens 1993). The benefit of establishing a formal review process is that reviews can be conducted on research in progress to identify any weaknesses to the program managers and allow them to make adjustments before going too far in the wrong direction. The reviews also might be useful in evaluating the overall success of federal agencies and identifying common traits of successful programs. A multitude of evaluation approaches are available, but four of the most pertinent to ARPA are ratio models, economic models, bibliometrics, and peer review.
1. Ratio Models

Ratio models are the most common method of evaluating the return on research and development programs. This is often done by expressing the outputs of an R&D program as a ratio to the R&D inputs used to produce them. Some of the ratios are based on subjective estimates. For instance, one might estimate the ratio of the net future value of stealth technology (a DARPA product) to the funds spent to develop that technology. Since most new military aircraft programs will likely incorporate signature reduction, or stealth, to some degree, such an estimation would require estimating the value of stealth to all the unknown implementations in the future. This would resemble an approach used by Alcoa, which prepares estimates of the impact on sales revenues and cost savings of each technological breakthrough resulting from its R&D expenditures (Link 1993). These impacts are then discounted to present values and added together, and the sum is divided by Alcoa's total R&D expenditures to provide a measure of the overall return on R&D. The credibility of the resulting ratio depends, of course, on the accuracy of the impact forecasts and the assumed discount rates.

Other ratios are based on objectively-measured quantities. As an example, an appropriate quantitative ratio might be the number of weapon system innovations (new weapon systems, improvements to existing systems, or innovative ways of employing existing systems) divided by the agency's total budget. Though appealing for their simplicity, ratio models have several weaknesses. First, a problem common to all ratio models is the question of when to calculate the ratio. Because outputs may occur over many years, if the evaluation fails to consider outputs far enough into the future, it will underestimate the true ratio. Second, ratios can reward quantity at the expense of quality. For example, if an agency's success is judged by a ratio of the number of innovations to the agency's budget, its researchers will have incentive to produce marginal innovations in
great quantity rather than a few revolutionary innovations. Simply counting the number of innovations would not account for the value of each one. Finally, a ratio of outputs to R&D expenditures is flawed because R&D is only one of the inputs used to produce a given set of outputs. The profit from the sale of an automobile, for example, is due partially to the R&D investment, but is also due to the labor and capital inputs. This factor leads to the potential for overestimation of the value of R&D (Link 1993).

2. Economic Models

Another method of evaluating R&D is through the use of economic models. This method includes internal rate of return (IRR) and net present value (NPV) calculations, as well as the production function approach. From the field of accounting, "IRR is defined as that discount rate which equates the present value of a project's expected cash inflows to the present value of the project's expected costs" (Brigham and Gapenski 1994, 394).

\[
\sum_{t=0}^{n} \frac{CF_t}{(1 + IRR)^t} = 0
\]

- \( t \) = time period
- \( n \) = number of time periods
- \( CF_t \) = cash flow in period \( t \) (inflows positive, outflows negative)

IRR is used in financial management as a determinant of whether or not a proposed investment is expected to generate a positive net return. A project is pursued only if its IRR exceeds the expected cost of capital. Using the IRR approach to evaluate ARPA research would require estimating the monetary inflows to those organizations or companies which produce weapon systems using the knowledge gained from particular ARPA research efforts and solving for the IRR, which equates the present value of that stream of estimated inflows to the present value of the ARPA investment. One important caution to consider when comparing IRRs for different projects is that the method can
lead to unreliable results. Because the projects' net present value profiles\(^1\) can cross, the IRR approach for comparing projects may lead to different answers, depending on the market interest rate used.

Alternatively, a market interest rate (which is known, in the case of *ex-post* evaluations) could be substituted for IRR in the above equation, with the dependent variable becoming the net present value. Then, the resulting equation could be solved for NPV:

\[
NPV = \sum_{i=0}^{n} \frac{CF_i}{(1 + k)^i}
\]

- \(k\) = market interest rate
- \(CF_t\) = cash flow in period \(t\)

The benefit of this calculation, over the IRR approach, is that the NPVs of various projects can be compared.

Like the ratio models, these approaches also ignore the contribution of other inputs (such as labor, capital, management structure, experience, etc.) by the weapon system producers. A third approach, using production functions, uses time-series data on capital, labor, and technology inputs to estimate the value of each input to the total output.

\[
Q = f(K, L, T)
\]

- \(Q\) = quantity or dollar-value of output
- \(K\) = capital input
- \(L\) = labor input
- \(T\) = technology input

---

\(^1\) A net present value profile is simply a graphical representation of net present values versus varying interest rates. The rate at which two net present value profiles cross is called the crossover rate. At interest rates below the crossover rate, one project has the higher NPV, while at interest rates greater than the crossover rate, the other project has the higher NPV (Brigham and Gapenski 1994).
Several assumptions must be made regarding the mathematical form of the transformation equation, but the positive side is that only time-series data on the dollar-value of output and the size of labor, capital, and R&D expenditures are needed to estimate the marginal contribution of R&D to the organization's output (Link 1993).

Since such data are often available in the corporate arena, economic models may be quite useful to the firm. In the case of federally-funded research, however, a fundamental problem arises due to the lack of a readily-identifiable and measurable output. While a firm may limit its concern to the impact on profitability, for instance, the government must be concerned with the net impact on social welfare of its funded research (Stiglitz 1988). Hence, when such an approach is used to evaluate federal R&D evaluation, the independent variable often must be expressed in such vague terminology as "net social benefit." A similar approach (in terms of the forecasted variable) involves the use of consumer surplus analysis, which relies on an estimation of the future supply and demand for the commodity to be produced by the government expenditure. As one writer cautions, however, "with extremely uncertain future demand and supply curves, consumer surplus analysis, when used in the R&D context, rapidly degenerates into advanced quantitative guesswork" (Roessner 1993, 194). In ARPA's case, the problem is no less significant. The dependent variable cannot be expressed in terms of a monetary value. ARPA's research is not meant to improve profitability or market share, which are clearly measurable for the private firm, but to enhance the national defense posture, which is rather difficult to quantify. Due to this difficulty, "the use of quantitative methods to aid decisions involving applied research is expected to be more problematic, and limited, in government than in business" (Roessner 1993, 183).
3. Bibliometrics

Bibliometrics is the study and analysis of scientific output using publication-based data. A common form of this approach involves citation counts, in which the number of citations of a certain research effort serves as a proxy for the importance of that research. Because publications are the media through which scientists share ideas, the more an article is cited, the more likely the research underlying the article has contributed to the advancement of knowledge. For this reason, article citations are best used in evaluating basic research (Melkers 1993). Considering, however, that only about 10 percent\(^2\) of its research is classified as basic research (Agres 1989), article citation is not likely to be the best approach for evaluating ARPA.

One simple, and related, approach to evaluating the productivity of an agency might be simply to count the number of patents it garners over a given time period. In theory, since patents serve to establish property rights, the more patents an agency generates, the more innovations it has produced. Unfortunately, such an approach would fail to convince critics such as Harvard Professor Manuel Trajtenberg, who writes, "patents exhibit an enormous variance in their 'importance' or 'value,' and hence, simple patent counts cannot be very informative of innovative 'output'" (1990, 172). Instead, patent counts are merely an indicator of the amount of R&D inputs. Trajtenberg's argument is that any agency, with a large enough staff and enough financial resources, can generate a collection of patents. Simply counting them, therefore, does not indicate the quality of the research funded or performed by the agency. Trajtenberg elaborates:

---

\(^2\)This 10 percent figure is based on a claim by Agres (1989, 40) that basic research at DARPA totaled about $100 million of the agency's $1 billion budget. Whether this percentage has remained constant over time is not clear.
...the mere counting of patents at any level of aggregation cannot possibly render good value indicators: simple patent counts assign a value of one to all patents by construction, whereas their true values exhibit a very large variance. Furthermore, there is substantial evidence to the effect that the distribution of patent values is highly skewed toward the low end, with a long and thin tail into the high-value side (1990, 173).

Griliches echoes this finding, citing European data which reveal that the majority of patents are of "no or little real value while at the same time a much smaller fraction of patents is associated with really large economic returns" (Griliches 1987, 31).

A more enlightened approach involves the use of patent citations. When a patent is issued, accompanying documentation lists a variety of pertinent information, including citations to previous patents on which the newly patented innovation builds. These citations then demonstrate the contribution made by the new patent. In other words, "the granting of the patent is a legal statement that the idea embodied in the patent represents a novel and useful contribution over and above the previous state of knowledge, as represented by the citations" (Jaffe 1993, 580). The idea behind patent citation analysis is that, while a patent in itself does not necessarily signify an important innovation, citations to that patent in other patent applications indicate that it is generating follow-on ideas. More citations to a given patent, then, indicate relatively greater impact (Melkers 1993). While this type of research was once too cumbersome to be practical, patent applications are now available electronically, making the technique more feasible. Sources of electronic patent applications include DIALOG and the PATDATA database, available through BRS (Trajtenberg 1990; Jaffe 1993).

Unfortunately, patent citation analysis is still relatively expensive. The DIALOG database, though accessible, is also proprietary and costs $90 per hour of access time. Other sources for patent information include commercial patent research services, such as
CHI Research, which use their own proprietary databases. These sources exist primarily to serve the needs of those interested in obtaining new patents, a process which requires in-depth research into prior patents to determine the incremental contribution of the new patent. For those interested in patenting innovations to establish legal property rights, the cost of citation searches is justifiable as a cost of doing business. For academic research, on the other hand, the costs can be prohibitive.

The home page of the U.S. Patent and Trademark Office (USPTO)\(^3\) will have a new feature, scheduled to become active on 9 November, 1995, which will provide free online access to over twenty years of patent bibliographic text data. According to Mr. Jim Hirabayashi, in the Information Products Office of the USPTO, the data will come from the front pages of the patent applications, and will contain such information as the patent number, issue date, filing date, title, inventor, city, state, assigned owner, patent classification code, examination information, name of legal representative, U.S. patents cited, foreign patents cited, other publications cited, an abstract, and Patent Cooperation Treaty information. The search engine is the same one currently used for the AIDS Patent Database,\(^4\) which also is available from the USPTO home page. At this time, the search engine is not very sophisticated, providing little capability for statistical analysis of patent citations (see Appendix B). Another project, led by Adam Jaffe at Harvard and funded by the NSF, is collecting information from USPTO and creating a database for researchers, though Hirabayashi did not think it would be available to the public.

Hirabayashi was skeptical that a search of patents for ARPA or DARPA references would be successful, because the patent owner is not identified to that level of detail. According to Hirabayashi, ARPA patents probably would be listed simply as DOD patents. Thus, to perform a patent citation search, a list of ARPA's patent numbers or

\(^{3}\) http://www.uspto.gov/

\(^{4}\) http://patents.cnidr.org/welcome.html
titles would be required. The USPTO does not perform patent citation analyses, and is therefore unable to provide any specific guidance, but Hirabayashi recommended researching the work of Francis Narin, of CHI Research, who has been very active in the field.

One caveat in using citation analysis is that it may be biased toward scientists doing mainstream research (Lindsey 1989). Those operating outside the mainstream are not likely to be cited very often unless their research produces breakthrough innovations which influence the mainstream. This is important to consider, because ARPA tends to work far outside the mainstream, and its innovations are primarily useful in the development and use of military weapon systems. Unless an ARPA innovation results in a spin-off, therefore, it may not create a significant number of patent citations. This situation would not, however, indicate that the innovation was any less important than innovations which garner more patent citations. This caveat actually is applicable to evaluations of government labs, in general. According to Papadakis and Bozeman, because most government labs generate only one or two patents per year, "the substantial majority of R&D laboratories in the government system do not pass the germaneness or volume tests which would allow patents to be used meaningfully in R&D evaluation" (1993, 117).

4. Peer Review

Due to the difficulties and limitations associated with evaluating research using the previously-identified approaches, about half of the federal research laboratories choose to evaluate their own research programs with yet another method, peer review (Bozeman 1993). This method involves selecting a team of experts in the area to be evaluated, and

---

5 With the exception of its information processing work, which, arguably, has produced breakthrough innovations.
bringing them together to review the research effort and evaluate it on several predetermined criteria. Traditionally, peer review has been used prior to the research, as a tool for evaluating potential research projects and selecting those to pursue. A common criticism of peer review in this context is that it often leads to selection of very conservative, low-risk projects (Friedman 1981; Squires 1986). One of the reasons for this is that individual reviewers "evaluate scientific contributions in relation to their own (very different) cognitive and social locations" (Martin and Irvine 1983, 76). In other words, reviewers are sometimes reluctant to give a good review to research which contradicts their own research or thinking. Obviously, low-risk, conservative projects are incongruous with the very nature of ARPA, so peer review should be used judiciously in the project-selection process at the agency. As an evaluation tool for completed or in-progress projects, however, peer review may be the most logical choice. According to the Committee on Federal Laboratories Task Force on Performance Measures for Research and Development (1975), peer review is the "generally recognized best procedure for evaluating research and development" (Bozeman 1993, 83).

In addition to the problem mentioned above, peer review evaluation has a couple of other drawbacks. First, due to national security considerations, research at ARPA is extremely sensitive. This situation is similar to that faced by industrial laboratories. Because research performed by industrial labs is often proprietary, only about 17.5 percent of them use peer review to evaluate their own research (Bozeman 1993). Companies spend large sums of money on research to produce new products, and are thus averse to revealing their research findings to peers from outside their organizations. Similarly, ARPA must protect its research findings from foreign governments. Consequently, any peers selected to review ARPA research would need to possess the requisite security clearances. This being said, the security clearance problem is not necessarily a fatal
stumbling block. Other federal researchers have the necessary clearances, as do many of the scientists employed by defense contractors.6

The second problem is actually more serious. Studies of Canadian R&D evaluations have revealed that those conducted prior to the 1980's generally served the interests of the researchers, not upper management. As a result, any research considered to be of good scientific quality was considered a success regardless of the outcome. Addressing this situation, in 1986, the Office of the Comptroller General of Canada (OCG) produced a Discussion Paper entitled Evaluations of Research and Development Programs, which specified the issues to consider in such evaluations. As a result of this paper, in the late 1980s, Canadian government evaluations began to ask such questions as:

Do the research activities undertaken constitute a legitimate role for government at this time? Do the conditions that gave rise to the program still prevail? Are the objectives consistent with the current government policy priorities and goals? . . . Does the R&D program have a clearly defined clientele? To what extent are the clients satisfied that the R&D program is producing or is likely to produce results which will be useful to them? (Barbarie 1993, 158-9).

Questions such as these give the R&D programs more of a customer focus, increasing the likelihood that research will be conducted with an end in mind, rather than simply for the sake of science. The lesson for ARPA is that, when establishing peer review panels, the criteria by which those panels will evaluate R&D effectiveness must be carefully considered. "Good scientific quality" and interesting research are not acceptable substitutes for revolutionary advances in the art of war.

6 Though another problem could be created by hiring peers who work for the very companies likely to compete for the resulting weapon systems
Unfortunately, no studies exist which attempt to quantify ARPA's effectiveness at funding technology, and the agency itself does not maintain the requisite data even to attempt such a study. One ARPA official was able to provide such information as a list of directors and the annual budgets over time, but more specific information was not available, since the agency does not employ an historian, and has no procedure for collecting key management data. In the absence of an existing comprehensive assessment, and lacking the means to conduct such a study, an alternative is the analysis of unsolicited opinions of those who have written about ARPA in the public forum. Purists will not accept this as a real evaluation method, of course, due to its lack of scientific rigor, but without any other workable approach, some value must be credited to the opinions of experts and industry insiders. While ARPA has received praise from the press and from defense experts, the existence of admirers does not prove widespread respect. A reasonable proxy for the aggregate opinion of the defense industry may be gleaned, however, from a simple tally of the positive and negative opinions expressed in a sample of articles written about ARPA projects over the years. If, for instance, a thorough sampling of articles finds praise for a given project significantly more often than criticism, the project might reasonably be considered a success. Scouring the technical journals for mention of a given project and keeping a tally of the results might not be the most enjoyable of tasks, but it would provide interesting data for further research. Over time, after a significant number of the projects have been assigned peer acceptance ratings, the resulting database would either provide support for, or an argument against, claims that the agency has been a model of success. Obviously, such an approach lacks the scientific rigor of some other approaches, but it may be the best approach available at this time.
These are just a few of the techniques available for evaluating R&D impacts. Only those most applicable to evaluating overall program effectiveness for an agency such as ARPA are presented here. A truly comprehensive evaluation, of course, is likely to require evaluations of the individual projects. One interesting technique involves the use of case studies, with the objective of tracing the pattern of events in the development of a technology (Kingsley 1993). This technique would be most useful in evaluating ARPA's role in a single innovation. For instance, ARPA directors often boast of their agency's role in the development of stealth technology. In reality, however, radar cross section (RCS) reduction techniques had been applied in the design of the Lockheed SR-71 Blackbird in the 1950s. Additionally, the ideas for the stealth technology incorporated into the F-117 had been published by a Russian scientist prior to ARPA's involvement (Rich and Janos 1994). ARPA's role in stealth was basically limited to funding the technology demonstration programs which led to the Advanced Cruise Missile (ACM), B-2 bomber, and the F-117. To be sure, these were great technological accomplishments, but the breakthrough innovations actually occurred much earlier. In fact, using the case study method, one might be able to show that ARPA actually impeded the development of stealth by declining to fund its early pioneers (Van Atta, Reed, and Deitchman 1991).

Provided enough data could be located, textual databases could be used, in theory, to perform Co-Word analyses, and thereby trace research impacts. This is a technique which has become more promising with the advancement of computers and data-storage devices (Kostoff 1993a). In the case of ARPA and its research, however, the required data is too sparse. Because of this factor, and due to the large amount of groundwork required prior to engaging in Co-Word analysis, this technique will not be pursued here.

The researcher has a rather large selection of evaluation methods from which to choose. All have certain shortcomings, and are therefore likely to be indeterminate when
used alone. Accordingly, "the greater the variety of measures used to evaluate research impact, the greater is the likelihood of converging to an accurate understanding of the knowledge produced by research" (Kostoff 1993b, 164). Unfortunately, federal agencies rarely use multiple techniques to evaluate their research, but instead choose to stick to a single, favorite technique (Kostoff 1993b). This circumstance may be due to the widely-held opinion that in-depth evaluations of R&D have not, to date, proven to be extremely useful or reliable. Until studies begin to reveal compelling justification for pursuing aggressive R&D evaluation, the situation is not likely to change. As a result, making judgments about agency success in funding research and comparing one agency to another is often rather difficult and inconclusive:
Chapter 3: History of ARPA

"I want an agency that makes sure no important thing remains undone because it doesn't fit somebody's mission." - Secretary of Defense Neil McElroy, 1958 (Gansler 1989, 238)

If American defense contractors were allowed to market their wares without restriction on the world market, the federal government probably would have little need for investing in defense R&D. Because of the intense competition among governments to field the most powerful militaries and the most sophisticated weapon systems, any innovation resulting in a military advantage would be tremendously valuable to the producer. Defense contractors would rush to invest in R&D and would compete for the top scientists necessary to develop such innovations. For obvious reasons, of course, the U.S. government is not willing to allow American companies unrestricted access to the world arms market. As a result, defense contractors operate within a monopsonistic market. They have, essentially, only one customer: the DOD. If they should develop a new weapon system or pertinent innovation on their own, they would be at the mercy of U.S. politicians to recoup their investment. Those elected officials might decide not to allocate the funds to purchase the firm's product, no matter how innovative it may be. The incentive to stay ahead in the international competition for military supremacy would not be enough to ensure the sale, because those same politicians could prevent the product from being sold to anyone else. Self-directed defense R&D is, therefore, extremely risky.7

This is the reason why the federal government must fund organizations like ARPA, the Office of Naval Research (ONR), and the National Research Laboratories.

---

7 Northrop learned this lesson the hard way when they developed the F-20 Tigershark without a commitment by the Defense Department to buy the aircraft. Though the F-20 met the Air Force needs for a lightweight, inexpensive, single-seat fighter, Northrop was left holding the bag.
ARPA was founded in 1958, in response to the Soviet launching of Sputnik. Separate from the three Service components, ARPA was to act as DOD's research arm and invest in high-risk, potentially high-payoff technology. In their 1991 report on DARPA, the Institute for Defense Analyses (IDA) divided the agency's history into 3 major periods (Van Atta, Deitchman, and Reed 1991). From its beginning in 1958 through the mid-1960s, ARPA enjoyed generous funding as it primarily concentrated on several large projects to which it had been directed by the Executive Branch. Once those programs became sufficiently mature to be transferred to other agencies, however, ARPA took on a completely new look. Real funding levels declined in the second period, which included the years from the mid-1960s to the mid-1970s. With the transfer of the "presidential issues," the agency gained the freedom to pursue projects of its own choosing, but lost much of its reason for being. The critical need for the agency became less obvious, and some even pushed to end its charter. In spite of this erosion of status, however, the agency not only survived this uneasy period, but also produced some of its most important research in the area of computer science. The third period dates from the mid-1970s to the present, and witnesses rising budgets as the agency takes on several large high-risk projects, begins to indulge in programs seemingly outside of its charter, and is pulled in several new directions by politicians apparently intent on crafting industrial policy through selective appropriation.

From the start, ARPA concentrated on several large "presidential issues:" space, ballistic missile defense (BMD, the DEFENDER program), and nuclear test detection (the VELA program). These, after all, comprised the justification for forming the agency. Even in the formative years, however, ARPA funded ground-breaking research in computer science. In 1961, the predecessor to the agency's Information Processing Techniques Office (IPTO) was organized, and J.C.R Licklider was hired as the first
director. Licklider had worked with Leo Beranek on speech problems at Harvard during World War II, and the two had later joined Richard Bolt and Robert Newman on the faculty at MIT. Those close ties continued when the group left MIT to form the consulting firm of Bolt, Beranek, and Newman (BBN), a company which would become ARPA's prime contractor in building ARPANET, the predecessor of today's widely-renowned Internet. The mission of the IPTO was to build "Centers of Excellence" for basic research in computer science that would improve military command, control, and communications (C^3). The first years of ARPA were a period of generous funding, since the executive branch had high expectations for its chosen projects (Flamm 1987).

The mid-1960s through the mid-1970s marked a distinctly different period. During these years, ARPA transferred the civilian space programs to NASA, and most military space programs to the Services. It also transferred DEFENDER to the Army, leaving VELA and the new AGILE program, which funded counter-insurgency R&D for the war in Vietnam, as the only remaining "presidential" projects. These transfers marked major transitions (DEFENDER alone had consumed 40-50 percent of the total budget from 1965-67) and left significant holes in the fabric of ARPA (Van Atta, Deitchman, and Reed 1991). The program reductions did, however, provide the opportunity for a number of new exploratory research programs in such diverse areas as computer processing, behavioral science, and advanced materials. With a 1965 computer research budget of $14 million, which grew by 1973 to $39 million, ARPA (renamed DARPA in 1972) funded research in such areas as computer graphics, display systems, interactive computing and timesharing systems, and artificial intelligence (AI). In 1962, ARPA had negotiated a large-scale contract for an MIT-based project with a timeshare focus. Project MAC (Multiple Access Computer/Machine Aided Cognition) initially was funded at $2 million per year, with that amount increasing to $4.3 million in 1969 and remaining as high
as $3 million in 1973. DARPA became the Defense Department's dominant source of computer research and development in the 1970s, and led the world in its timesharing research. According to Kenneth Flamm, "of 12 general-purpose timesharing systems catalogued in a 1967 survey article, six . . . were sponsored by DARPA" (1987, 58).

The early research into timesharing systems laid the foundation for follow-on research on networks and the establishment in 1969 of ARPANET. For the initial study of the basic concepts of "packet-switched communications," DARPA had again turned to MIT (Flamm 1987). Work on ARPANET began in 1966, when Robert Taylor, a former director of ARPA's computer research program and partner in Bolt, Beranek, and Newman, asked the ARPA director for money to pursue the idea. Immediately, $1 million was transferred into the ARPANET project, with BBN as the prime contractor. In 1968, Taylor joined original IPTO Director Licklider in writing an influential paper suggesting that computers could serve as communications devices, and envisioning a computer network "that would create new communities of scientists separated by geography but united by technology" (Kantrowitz and Rogers 1994, 57).

Though overall funding levels from the mid-1960s to mid-1970s were fairly constant in nominal terms (figure 1), inflation caused a steady decline in the real purchasing power of ARPA research budgets (figure 2).
Figure 1: DARPA Budget (Nominal $)

Figure 2: DARPA Budget (1990 $)
The declining real budgets were the result of uncertainty about the mission of the agency, following completion of some of the "presidential" programs. Some people believed ARPA had served its purpose and no longer needed to exist. Fearing the agency would become unfocused after completing its initial projects, Congress emphasized the need for joint Service-ARPA programs and passed the 1972 Mansfield Amendment, which further emphasized the importance of ARPA sticking to defense-specific research. In fact, this emphasis was reflected in the addition of "Defense" to the agency's name in 1972, creating DARPA (Van Atta, Deitchman, and Reed 1991).

The third major period dates from the mid-1970s to the present, and involves large projects in several high-risk areas, many of which were outgrowths of the earlier exploratory research. Within the third period, three sub-periods also are apparent. From 1975 to 1977, DARPA, under the leadership of Dr. George Heilmeier, launched several new large-scale demonstration projects, such as STEALTH, Space-based infrared surveillance (TEAL RUBY), standoff follow-on forces attack (ASSAULT BREAKER), and experimental aircraft (X-29, X-Wing). In an effort to prevent these large-scale programs from completely absorbing the agency's resources, Dr. Heilmeier separated them into a separate office called the Experimental Evaluation of Major Innovative Technologies (EEMIT). Dr. Robert Fossum became the DARPA Director in 1977, and although he was concerned about all the technology demonstration projects launched by his predecessor, he was stuck with them (Van Atta, Deitchman, and Reed 1991).

In 1981, Dr. Robert Cooper took over as Director and, following the previous pattern, began transferring many of the EEMIT programs (STEALTH, ASSAULT BREAKER, TEAL RUBY, X-29) to the Services. He eliminated other, less promising EEMIT programs. Simultaneously, the Strategic Technologies Office (STO), created in

---

8 No information is available concerning which projects were eliminated.
the early 1970s to pursue several small, exploratory-research projects, was transferred to
the newly-formed Strategic Defense Initiative (SDI), along with several related EEMIT
projects. In total, the programs transferred to SDI represented about 25 percent of
DARPA's budget. In place of these projects, DARPA launched several new aviation and
naval technology programs and gave birth to the Strategic Computing Program, an effort
to leverage computing advances into defense applications. From 1986 to the present,
DARPA transferred several more key programs to other agencies, as Dr. Cooper
attempted to re-focus the agency on technology research, rather than system
demonstration. Under Dr. Cooper's leadership, ARPA devoted funds to increased
research on undersea warfare and a new program, LIGHTSAT, to develop small satellites
for tactical Command, Control and Communications (C^3). Meanwhile, the Office of the
Secretary of Defense (OSD) introduced an entirely new focus, defense manufacturing, and
assigned DARPA several new "productivity-" oriented programs, such as the
semiconductor manufacturing consortium (SEMATECH), microwave/millimeter-wave
monolithic integrated circuit chips MIMIC, and Software Technology for Adaptable
Reliable Systems (STARS) (Van Atta, Deitchman, and Reed, 1991). Though justified as a
means of reducing the cost of future weapon systems, these programs were (and are)
extremely controversial, due to their weak fit with ARPA's mission and their striking
resemblance to de facto industrial policy/corporate welfare.

Continuing this trend of expanding the role of ARPA, in the Defense Conversion,
Reinvestment, and Transition Assistance Act of 1993, the Clinton Administration
introduced the Technology Reinvestment Project (TRP). The TRP is managed by an
oversight committee called the Defense Technology Conversion Council, which consists
of ARPA, the Department of Energy/Defense Programs (DOE/DP), the Department of
Commerce's National Institute of Standards and Technology (NIST), the National
Science Foundation (NSF), and the National Aeronautics and Space Administration (NASA). Demonstrating its admiration, the Administration selected ARPA to chair that council. The goals of TRP include softening the blow of the defense draw-down on small businesses and reconsidering military specifications to allow cheaper, commercially-available components to be used in weapon systems when possible. While reasonable arguments may be made in support of such an effort, the selection of ARPA to lead it seems to demonstrate either a lack of understanding or a willful neglect of the agency's strengths.

Historically, according to Agres, about half of the ideas for DARPA-sponsored research come from outside the agency, with the rest originating at DARPA. Research is concentrated in several key areas, with about 10 percent of the projects classified as basic research. Consistent with its roots, ARPA is still a major player in computer science research. In fact, as of 1989, ARPA supported the education of almost half of the American graduate students in computer science (Agres 1989, 40).9

9 This statistic probably does not hold true today. Though the 1994 Defense Appropriations Bill increased funding for ARPA's Computing Systems and Communications Technology program by $426 million, it also directed a $900 million cut in the program's university research funding.
Chapter 4: Difficulties in Quantifying ARPA’s Success

Though few can dispute the noted accomplishments of ARPA, quantifying its overall success is rather difficult, as explained in Chapter 2. The mission of ARPA is to fund advanced research with a potential for high payoff for the Department of Defense, to keep America's armed forces on the forefront of technology. From that perspective, to the extent ARPA can take credit for America's military preeminence, it has been quite successful, at least in terms of accomplishing its mission. Assessing its efficiency, however, is more difficult. Though many defense experts have credited ARPA as a success in funding innovative technologies, they have failed to make the case that the agency has done so in an efficient manner. How much, for instance, is stealth technology worth to the United States? How many dollars worth of security does that technology provide? Without answers to such questions, we cannot assess definitively whether the results of research funded by ARPA warrant the costs of funding the agency.

Perhaps due to the difficulty of answering these questions, ARPA's supporters often judge its success based on the commercial payoffs from its defense research. Justifying the agency's existence solely by the value of its research to the commercial sector is, however, a risky approach. As Fortune writer Bruce Steinberg notes,

Military-industrial policy is not particularly efficient. Soldiers aside, defense spending will create around 1.2 million jobs between now and 1987. But economists calculate that the same investment in the civilian economy could produce roughly 25% more jobs (1984, 42).

The writer was arguing, not that government should invest in the civilian economy instead of in national defense, but that expenditures on defense should be based on national security requirements, not on the potential economic benefits of those expenditures.
Implied is the argument that government should leave economic investment to the private sector, where risk and return considerations determine the allocation of financial resources.

Even ignoring the above argument, the approach of quantifying ARPA's success by the value of its programs to the commercial sector is impractical, due to measurement difficulties. One problem is that "programs that some have tagged as 'failures,' others have seen as major accomplishments. Some of the 'failures' may have had significant influence elsewhere, while some 'successes' may have had little downstream effect" (Van Atta, Deitchman, and Reed 1991, I-1,2).

Another problem is the difficulty of determining what might have happened without ARPA funding. This line of reasoning gets into the idea of premature industries. A premature industry, according to University of Chicago economist Robert Fogel (1960), is one in which the public and private interests are not united. Investment in such an industry comes from the public coffers because the private sector does not yet see the opportunity for profitable investment, given the perceived level of risk. By this definition, the computer industry was a premature industry. Although the Department of Defense, through the Office of Naval Research (ONR), and the National Security Agency (NSA) funded most of the early (through 1950s) research in computer science, and ARPA continued the high level of funding throughout the 1960s and 70s, that fact does not mean the industry developed purely because of government funding. All that is known is that, as a result of ARPA and other DOD agency funding, the computer industry developed sooner than it otherwise would have. Certainly, the early years of the computer industry were dominated by the demands of the Defense Department. The private sector did not yet see the value of investing in this area, as is reflected by the fact that many of the early computer firms went out of business when their government funding dried up (Flamm
1987). Could this indicate that the industry developed at the expense of other industries, which could have benefited more from the research dollars? Critics of "big government" think so. Unfortunately, an objective answer to that question is unlikely, due to the number of assumptions which must be made prior to answering it. Even without government funding, however, most would agree the computer industry eventually would have emerged.

The most extensive study of the impacts of ARPA's research was conducted by the Institute for Defense Analyses (IDA), under DARPA contract. DARPA was approaching its thirtieth anniversary and wanted to document the impact of its research in those first thirty years. In this study, IDA collected all the information it could find on a list of 55 ARPA projects. The first two volumes of the study contain histories of each of the projects, while the third volume attempts to draw conclusions concerning the impact of ARPA's research agenda. Overall, the study is very complimentary of ARPA's accomplishments, though because of its broad scope, no attempt is made to analyze the efficiency of any of the projects.

A few sources of bias also are apparent. The first is due to the close relationship between ARPA and IDA. When ARPA was established, its original staff came directly from IDA. In the years following, ARPA worked directly with IDA on some of its projects. For IDA, therefore, to criticize ARPA, especially in its formative years, would mean criticizing itself. The second source of bias is due simply to the stated purpose of the study. DARPA paid IDA to document the impacts it had made over its first thirty years, and to draw out recommendations for the future. Had IDA criticized DARPA too strongly and minimized the impact of its research, its report is unlikely to have been

\[10\] Such as a 1984 basic technology program to deal with infrared signature reduction, and the ADA common programming language development effort (Van Atta 1991, 10-6, 15-6).
viewed as an acceptable product. A third source of possible bias is found in the project-selection process employed by IDA:

The projects studied were selected by the IDA project team and DARPA management working together, based on two criteria: (a) their importance, judged on the basis of evidence in attestation and documentation; and (b) the expected availability of data. The data available would have to contain sufficient information to permit elucidation of DARPA's role and contribution, tracing the paths of technical events through ultimate use, assessment of the impact and spin-offs of the output, and clear enough records to permit evaluation of lessons learned from the outcome (Reed 1990, 3).

Not only did the IDA researchers work with DARPA management to determine which projects would be considered, but they even used as a baseline the project list management had prepared on the occasion of DARPA's twenty-fifth anniversary celebration. The two criteria mentioned above should have guaranteed that only the most successful projects were selected, since they both would tend to make the baseline list even more biased toward successful projects. The first criterion, that the projects be judged important, on the basis of documented evidence, biases the project list to the most successful projects because they are the ones most likely to be publicized and considered important. The second criterion, data availability, is understandable from the perspective of the researchers, but is, nevertheless, equally biasing. IDA had to rely on the data provided by DARPA. The likelihood of the agency providing more data on its successes than its failures only stands to reason.

To estimate the potential project-selection bias, please refer to Appendix A. This appendix is the result of a project-by-project enumeration of the amount of DARPA investment in each, as reported in the IDA study. In the first volume and through about half of the second volume, the investment figures are provided in the summary section for each project. About halfway through the second volume, however, the figures become
more difficult to identify. For those projects missing investment figures, or with incomplete figures, estimates are found in the column to the right of the investment figures. The far-right column in Appendix A notes any assumptions. The study documents about $3.7 billion in investment. Including several generous estimates, that figure rises to about $5.2 billion. Comparing these numbers to the total DARPA budgets from 1958 to 1988 (approximately $11.7 billion), the projects reviewed in the IDA study represent between thirty-two and forty-four percent of DARPA's total budgets. Certainly, these estimates leave ample room for project selection bias. Nevertheless, the study is impressive in scope and may be quite useful as a starting point for follow-on analysis.
Chapter 5: The Secrets of ARPA's Success

Whatever the truth about the agency's absolute measure of success, anecdotal evidence (i.e., the list of successful projects the agency has funded, and the opinions of noted defense and research experts) suggests that ARPA has been more successful at funding technology than other government agencies. Agres called DARPA a governmental role model for funding technology and remarked, "in the corporate world, they would be venture capitalists, and highly successful ones at that" (Agres 1989, 39). According to the IDA study, ARPA's success has been made possible by eight primary strengths (Van Atta, Deitchman, and Reed 1991):
1. Small size in relation to the size of its work programs
2. Flat organizational structure
3. Program managers (PMs) are empowered to make decisions and commit agency funds
4. Contracting is performed through the Services
5. Ability to access the entire U.S. technical community
6. Rotation of highly-qualified personnel
7. Ability to attract good people to manage its programs
8. Mix of civilian and military staff

Another, less comprehensive study of the agency was conducted by Michael Davey of the Congressional Research Service (CRS), and identified a similar list of factors responsible for ARPA's success (1993):
1. Small staff
2. Flat organizational structure
3. Program managers are given the authority to fund or terminate projects
4. Two-thirds of ARPA's contracts are administered by the Services
5. Ability to access the entire U.S. technical community
6. Ability to make quick decisions to fund innovative ideas
7. Few rules and regulations
8. Usually funds a project only long enough to determine its feasibility
9. Large research budget

My own review of the literature on ARPA led me to select eight readily-identifiable factors which have led to ARPA's success: (1) funding research is job one, (2) ARPA does not perform its own research, (3) small size of the agency, (4) the rotation of highly-qualified personnel, (5) dedication to a single customer: DOD, (6) authority to focus on high-risk research, (7) freedom from micro management, and (8) concentration of resources on Centers of Excellence.

1. Funding Research is Job One

According to its official mission statement (as of 1991), ARPA exists to:

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 (Van Atta, Reed, and Deitchman 1991, V-3).

Funding research has been ARPA's only function over most of its existence. Other agencies fund research "on the side," as an additional function. Traditionally, ARPA has done nothing but fund research. This setup has minimized distractions and allowed ARPA to be very selective, concentrating on funding the best research possible. As a result, ARPA typically funds only about 5 percent of the proposals submitted from outside the agency, while other federal agencies fund 30-40 percent of proposed research projects.
ARPA even has been able to avoid a significant amount of administrative overhead by delegating two-thirds of its contract administration responsibilities to the Services (Davey 1993). As a result of this intense concentration on its narrow mission, ARPA has had the opportunity to become quite proficient at spotting the research programs with the most promise.

2. ARPA Does Not Perform Its Own Research

ARPA has no labs of its own. The agency merely administers funds. It may fund research at universities, at industrial laboratories, or at government laboratories. As a result, ARPA has the flexibility to fund the most promising research in the country, without organizational or geographical bounds. In contrast, other defense-related research and development efforts take place within government laboratories. If those labs wish to pursue research outside their areas of expertise, they must either try to hire the top researchers from the private sector (which is often difficult, given the compensation gap between what industry or academia can offer and what government can offer), or attempt to conduct the research with their own resources, which may not be sufficient.

3. Small Size

"Large hierarchical organizations tend to be remarkably efficient mechanisms for the suppression of new ideas and alternatives." - Former Secretary of Defense James Schlesinger (Gansler 1989, 218)

Public Choice theory, explained in the Introduction, is equally applicable to the behavior of politicians at the agency level. Although these government officials generally are not permitted to use their positions to enhance their own financial status, they are
often quite amenable to serving their own self-interests in other ways. One way is by continually increasing their respective agencies' budgets and staffs, while simultaneously expanding their roles and missions, thereby increasing the individual bureaucrat's sphere of influence. This sort of behavior, sometimes labeled "empire building," leads to layer upon layer of bureaucracy being added to organizations over time. In contrast, ARPA has maintained a very small staff, and operates in a relatively flat organizational structure (figure 3).

Figure 3: ARPA Organizational Structure
Thirty-seven years after its creation, the agency still employs less than 200 staff members, with about half of them serving as program managers (Kitfield 1990). Each of the program managers is responsible for a $10 to $50 million block of funds, with about 80 percent allocated for a single project or set of projects (Davey 1993). Although the agency commands an impressive overall budget of about $1.5 billion, DARPA's combined budgets from 1958 to 1990 still fell short of DOD's single year budget for research, development, testing, and evaluation (Kitfield 1990). This relatively small size fosters low overhead costs and limits bureaucracy. Asked whether DARPA could expand in size and take on more responsibilities, controversial former director Craig Fields replied, "no, DARPA can't get much larger in personnel and in budget and still be effective" (Agres 1989, 42). This is notable, considering the widely-held opinion that Fields lost his position at ARPA due to his efforts to expand the agency's mission. Despite his zeal to get involved in other areas, even he realized that the effectiveness of his agency depended on it staying small.

![Figure 4: ARPA Staff Size](image-url)
4. Rotation of Highly-Qualified Personnel

"There is nothing more important... The technical people are solid. They have international reputations in their fields. Their personality is to be entrepreneurs." - Craig Fields, Former Director, ARPA (Agres 1989, 42)

One plausible reason why ARPA directors have avoided the quest for power in terms of increasing staff and budgets is that few of them have stayed around long enough to benefit from such power (see Table I). The phenomenon of relatively short tenures is true throughout the organization. Due to its prestige, ARPA is able to attract some of the brightest minds in the country, from industry, academia, and government, at the height of their careers. Most of the engineers are in their 30s and 40s. Many even accept a reduction in pay to work for the government agency. These employees have current knowledge and fresh ideas for innovative research. They generally come to ARPA for an average stay of 3 to 5 years and then return to their previous employers (Agres 1989). As explained in the IDA study:

DARPA has been seen as a place where one can see things done and make things happen with a freedom that is rare in either 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 (Van Atta, Deitchman, and Reed 1991, V-7).

Because they do not stay long and because they are so intent on "getting things done," ARPA's employees do not have time to "build empires" and become entrenched in bureaucracy.
<table>
<thead>
<tr>
<th>Director</th>
<th>From</th>
<th>To</th>
<th>Years</th>
</tr>
</thead>
<tbody>
<tr>
<td>Johnson, Roy N.</td>
<td>7 Feb 58</td>
<td>13 Nov 59</td>
<td>1.8</td>
</tr>
<tr>
<td>Betts, Austin W.</td>
<td>8 Dec 59</td>
<td>15 Jan 61</td>
<td>1.1</td>
</tr>
<tr>
<td>Ruina, Jack P.</td>
<td>16 Jan 61</td>
<td>5 Jul 63</td>
<td>2.5</td>
</tr>
<tr>
<td>Sproull, Robert L.</td>
<td>3 Sep 63</td>
<td>4 Jun 65</td>
<td>1.8</td>
</tr>
<tr>
<td>Herzfeld, Charles M.</td>
<td>6 Jun 65</td>
<td>30 May 67</td>
<td>2.0</td>
</tr>
<tr>
<td>Rechtin, Eberhardt</td>
<td>21 Nov 67</td>
<td>26 Dec 70</td>
<td>3.1</td>
</tr>
<tr>
<td>Lukasik, Stephen</td>
<td>11 Apr 71</td>
<td>17 Dec 74</td>
<td>3.7</td>
</tr>
<tr>
<td>Heilmeier, George H.</td>
<td>5 Mar 75</td>
<td>3 Dec 77</td>
<td>2.8</td>
</tr>
<tr>
<td>Fossum, Robert R.</td>
<td>4 Dec 77</td>
<td>5 Apr 81</td>
<td>3.3</td>
</tr>
<tr>
<td>Cooper, Robert</td>
<td>29 Jul 81</td>
<td>11 Jul 85</td>
<td>4.0</td>
</tr>
<tr>
<td>Duncan, Robert C.</td>
<td>18 Nov 85</td>
<td>21 Dec 87</td>
<td>2.1</td>
</tr>
<tr>
<td>Colladay, Raymond S.</td>
<td>31 Jan 88</td>
<td>28 Apr 89</td>
<td>1.2</td>
</tr>
<tr>
<td>Fields, Craig I.</td>
<td>1 May 89</td>
<td>6 May 90</td>
<td>1.0</td>
</tr>
<tr>
<td>Reis, Victor H. (acting)</td>
<td>7 May 90</td>
<td>12 Nov 90</td>
<td>0.5</td>
</tr>
<tr>
<td>Reis, Victor H.</td>
<td>13 Nov 90</td>
<td>3 Dec 91</td>
<td>1.1</td>
</tr>
<tr>
<td>Denman, Gary L. (acting)</td>
<td>4 Dec 91</td>
<td>14 Mar 92</td>
<td>0.3</td>
</tr>
<tr>
<td>Denman, Gary L.</td>
<td>15 Mar 92</td>
<td>3 Mar 95</td>
<td>3.0</td>
</tr>
<tr>
<td>Lynn, Larry (acting)</td>
<td>14 Mar 95</td>
<td>present</td>
<td></td>
</tr>
</tbody>
</table>

**Table I: Tenures of ARPA Directors (Source: ARPA, unpublished)**
5. One Customer: DOD

ARPA has focused on a single customer, the Department of Defense. This customer focus, along with the practice of funding only that research which holds promise for satisfying the needs of the Defense Department, is extremely important in explaining the development of the computer industry. Even before ARPA, the Defense Department funded most of the research in computer science. The reason for this funding was that the military had identified very real needs for computers, such as calculating ballistic trajectories in World War II. This kind of involvement is very different from targeting technologies. Government involvement in promoting the computer industry had nothing to do with selecting a technology for the private sector or boosting American competitiveness. Technology targeting, on the other hand, involves selecting technologies or supporting scientific research in the hope that something useful might be produced for someone. The idea that research aimed at solving identifiable problems is more likely to be fruitful than research funded to produce something of potential use for a group of unknown customers with unknown problems only stands to reason.

Another benefit of the concentration on one customer is that it reduces the difficulty of selecting projects to fund. Other agencies, such as the National Institutes of Health (NIH) and the National Science Foundation (NSF) must fund research without a specific customer because they exist only to support scientific research. To prioritize numerous proposals, they must rely on the peer review system, by which a panel of experts in the particular field judge the merit of competing projects. One of the criticisms of the peer review process is that it often stifles innovation (Friedman 1981). Research which threatens currently-held beliefs may be seen as a threat by the panel members. These reviewers, who are usually engaged in their own research, have a personal interest in the outcome of the peer review process. ARPA does not have to worry about this
problem because it makes its own decisions about which projects to fund, based on its knowledge of its one and only customer. Though program managers at DARPA may use peer review as a tool to help prioritize potential projects, the final decision is the responsibility of the program managers. The distinction is that ARPA, on the basis of clear objectives, purchases its research. Purchased research involves a quid pro quo relationship, which leads to a mutually-beneficial exchange. Supported research does not involve such a relationship, since the supporter is, in effect, a third party to the transaction, and does not understand the needs of its undefined customer (Martino 1992).

6. Authority to Focus on High-Risk Research

ARPA traditionally has maintained a tightly-focused objective, to fund advanced technology and thereby exploit technological opportunities which the rest of DOD has overlooked. While other agencies may shy away from innovative research due to fear of failure or the problems caused by the peer review process, ARPA's focus on high-risk, advanced research often pushes it toward high-payoff, innovative technology. Undoubtedly, the agency has funded many failures along the way (though its directors rarely talk about them), but it also has funded some spectacular successes. ARPA has been allowed to accept some failures because uncertainty is the very nature of research. The research effort attempts to remove the veil of uncertainty to reveal whether or not any opportunities for valuable development exist. ARPA is exposed to greater-than-normal risks because it seeks out innovations capable of revolutionizing the nature of warfare and giving American forces a distinctive edge. In fact, ARPA is the only federal agency with the mission of delving into such high-risk research (Agres 1989). Similar to financial
investments, technological investments also require a high degree of risk to achieve higher-than-normal returns.

7. Freedom From Micromanagement

"All DARPA does is fund research. But DARPA officials carefully select R&D projects, and then they nurture them with ample funding and little bureaucratic intervention or micromanagement" (Agres 1989, 39).

Traditionally, politicians have not interfered with ARPA's day-to-day operations. One factor which has provided the agency this freedom from micromanagement is the high-level of secrecy afforded many of ARPA's research projects. In most government-funded research, bureaucracy is inevitable, because government must account for how taxpayers' money is spent. For this reason, federal funding usually results in a large amount of paperwork and micromanagement. Another factor leading to this political involvement is the fact that legislators have an incentive to associate their own names with popular research so they may receive credit for that research (Martino 1992). Because ARPA's projects are often so highly classified, few people outside the agency know what projects are being funded, and are thus prevented from interfering. Indeed, former ARPA director Robert Cooper comments, "DARPA probably has the largest pot of unrestricted money in the government or even in industry to [fund research]. That's where its power lies" (Perry 1991, 65).

In addition to freedom from political micromanagement, Project Managers at ARPA have great latitude within the organization to make final decisions about funding and terminating projects. Working under very few rules and regulations, they have the authority to award money very quickly to those ideas they believe are promising (Davey 1993).
8. Concentration of Resources on Centers of Excellence

ARPA concentrates its resources on key areas in a few major research organizations with proven capabilities. This factor is closely related to the point mentioned earlier, that ARPA does not perform its own research. The authority of the organization to focus its resources so tightly is also related to the situation mentioned above, that the agency has remained relatively free from micromanagement. All too often, in other areas of government appropriation, politicians threaten to prevent certain projects from being pursued unless funds are allocated "equitably" across state lines and congressional districts. As a result, work is not always assigned to the most capable organization, but is instead allocated on the basis of political influence.

Artificial Intelligence research provides an example of the difference in ARPA funding procedures. ARPA always has been a major supporter of artificial intelligence. In fact, it typically has supplied about 55% of all government funding in this area. Even while disbursing these large sums of research money, however, the agency has concentrated its grants in a few major Centers of Excellence: Bolt, Beranek and Newman (BBN), Carnegie-Mellon University, MIT, Stanford University, SRI International, and Xerox's Palo Alto Research Center (Flamm 1987).
Chapter 6: Dangerous Trends

Despite the past successes of ARPA, the agency's future effectiveness may be in jeopardy. Shortly after the advent of pork-barrel funding for science, Congress began to look at ARPA as a new political playground. Politicians, impressed by the apparent success of the agency and, no doubt, by its sizable budget, began thinking of new jobs for ARPA. As in the story about the goose that laid the golden eggs, the results of this tinkering are threatening the health of ARPA, and may bring an end to 37 years of innovative research. Mirroring some of the historical strengths of the agency, several important weaknesses are now identifiable.

1. Funding Research is Only One of the Jobs - The SEMATECH Case

The 1986 Discussion Paper by the Office of the Comptroller General of Canada concerning evaluation of federal R&D established, among other things, "that quality of research work, while necessary, is not a sufficient condition for federal R&D program effectiveness, and that to be effective, quality research must be pursued in the context of a clearly defined and relevant purpose" (Barbarie 1993, 157). This is significant because, in addition to funding basic and applied research, ARPA, in its third period, has become heavily involved in other activities, such as government-industry consortia, prototype demonstrations, manufacturing methods, and manufacturing engineering education. This shift has occurred, not by accident, but by design. Indeed, President Clinton released a report in September 1992 which called for "reforming federal research and development programs to focus on critical technologies," and "leveraging the existing federal investment in technology to maximize its contribution to industrial performance" (Lee 1992, 59). The purpose of the agency no longer seems so "clearly defined and relevant" as it once was.
The consortium of Semiconductor Manufacturing Technology, SEMATECH, became one of ARPA's most significant projects in the third period of its history, and the forerunner of a new focus for the agency. As mentioned earlier, SEMATECH was justified as a means of reducing the cost of future weapon systems (Van Atta, Deitchman, and Reed 1991). The real purpose of this consortium, however, was no secret. The American semiconductor industry was in trouble in 1987. Less than 40 percent of the equipment used to make computer chips was being produced in America (Pope 1993b). The original fourteen industrial members of SEMATECH could have established the consortium on their own, given that anti-trust laws had been relaxed (Steinberg 1984). If government support was justified, then the government could have appropriated the funds in the form of a series of grants or loans. The Chrysler Corporation bailout could have been cited as a precedent for such an action. Instead, ARPA was forced to foot half the bill for an industry-government consortium dedicated to propping up the American semiconductor industry. This project stands in stark contrast to previous DARPA involvement in computer science research. When DOD funded key research in the early years of the computer industry, it was basically the only player. Almost no one else was interested in using computers, so very little private-sector research money was available. If DOD was to get the computers it needed, it was forced to pay for the research. Such was not the case in 1987, and is not the case today. DOD, though admittedly a major customer, is now only one of many customers of the computer industry.

Then DARPA director Craig Fields justified his agency's involvement in SEMATECH by pointing to the frightening possibility of all the American computer chip manufacturers going out of business (Charles 1989). American weaponry is so reliant on

---

11 According to Charles, "The Pentagon buys more than $4 billion dollars worth of semiconductors each year, about 27 percent of the industry's total U.S. production. Overall, electronics account for 36 percent of the Pentagon's $38 billion R&D budget and 17 percent of the total defense budget" (1989).
computers that our wartime effectiveness depends in large extent on the availability of semiconductors. If the American semiconductor industry should fold, reasoned Fields, the U.S. would be forced to rely on semiconductor shipments from Japan and other countries. Not only would such extended supply lines be undesirable, due to the reduction in timeliness, but the situation also would leave U.S. war-fighting capability vulnerable to attacks on other countries or on the supply lines themselves. This scenario does warrant some concern. Unfortunately, while the American government may have the noblest of intentions, such intervention recalls similar attempts by the British to prop up their airline industry. They and others have failed miserably at resurrecting struggling industries. Whether the government should have gotten involved in rescuing the American computer chip industry is, at best, questionable. In any event, industrial safety nets are definitely outside the purview of ARPA.

The fact is, SEMATECH was not intended to solve critical national defense problems with advanced research, but to prop up the American computer chip industry by subsidizing the industry's R&D efforts. This fact was confirmed when the consortium's chief executive, Robert Noyce, explained, "the first requirement of success is that it (SEMATECH) be industry-driven. The industry spends far more time and effort thinking about what needs to be done to promote its competitiveness than any other entity, so why wouldn't you believe its conclusion?" (Charles 1989, 18). Mr. Noyce is absolutely right. The industry should decide for itself what needs to be done to improve its competitiveness. That question should not be answered by the Defense Department. The industry also should determine how much investment is appropriate to stimulate that competitiveness and commit its own funds. Otherwise, the industry will not be able to conduct a proper risk versus return analysis, and will have less incentive to succeed on its own merits.
Several of SEMATECH's original fourteen members became disillusioned with the consortium and withdrew their membership during 1992 and 1993. LSI Logic Corporation announced its withdrawal in January 1992, citing a fundamental disagreement about the direction of the consortium, and concerns that SEMATECH was not the best place to invest its limited R&D dollars. LSI's disagreement was with the SEMATECH practice of granting money directly to the companies which produce computer chip manufacturing equipment. SEMATECH supporters pointed to LSI's shaky financial situation to minimize the significance of the withdrawal (Yamada 1992a), but could not make the same argument a week later, when Micron Technology, Inc. announced that it too was leaving SEMATECH. Micron was doing well financially, but cited the same concerns about the strategic direction of SEMATECH. A company spokesman said Micron "had hoped the consortium would focus more on ways to make advanced chips" (Yamada 1992b, A8) instead of funneling money into the pockets of the makers of computer chip manufacturing equipment. As Micron chairman and CEO Joseph Parkinson explained, "Sematech has gotten off the track of its original mission to develop the production process" (ibid.).

Coincident with Micron's announcement, Harris Corporation had announced it, too, was considering withdrawing from SEMATECH. A company spokesman at the time noted that Harris intended to stay a member as long as the returns from its SEMATECH membership justified the required investment (Yamada 1992b). A year later, in January of 1993, Harris Corporation announced that it had, indeed, withdrawn from SEMATECH (Pope 1993a). Simultaneously, other members began pressing the consortium to "align its research more closely with their own projects" (ibid.) instead of handing out their ducats to the equipment manufacturers. In such a competitive industry, companies struggled to justify giving their resources over to the consortium. In fact, SEMATECH had almost
lost another member, Rockwell International Corporation, in 1992, because of the poor return-on-investment performance statistics the company had generated. To prevent this occurrence, SEMATECH had "launched an entire new research program to keep Rockwell in the fold" (Pope 1992).

More bad news for SEMATECH arrived in an August 1992 Government Accounting Office (GAO) report, which announced the decision by the Defense Department to begin pulling its own money out of SEMATECH in 1993. Ten of the twelve SEMATECH members told the GAO they would not increase their own dues to make up for the loss of Defense funding. Though Craig Fields and other supporters of the consortium argued for the need to continue federal funding, the justification was hard to find. American producers of computer chip manufacturing equipment had experienced a significant turnaround in the early 1990s. By early 1993, they controlled a 53 percent share of the world market, up from less than 40 percent in 1987 (Pope 1993b). Thus, the true goal of SEMATECH appeared to have been accomplished, and the consortium no longer appeared to need the large federal subsidy.

Irrespective of SEMATECH's need for the federal subsidy, in April 1993 it gained a partial replacement for its former patron. The Energy Department announced, on April 7, a five-year agreement worth $103 million to help the consortium continue funding the equipment manufacturers (Wall Street Journal 1993). The following year, in October, SEMATECH proudly announced a plan to "wean itself from its $90 million annual federal appropriation" (McCartney 1994, B6). The plan called for removing DOD funding after the 1997 fiscal year. No mention was made of the status of the Energy Department funding, though SEMATECH director Craig Barrett, who also served as chief operating officer for Intel Corporation, announced, "The semiconductor industry is doing well, making money, and can afford to pay its own way" (ibid.). Of course, Barrett did not say
the consortium *would* pay its own way, only that it could *afford* to do so. Indeed, the SEMATECH board stated its intention to replace the Defense Department funding by competing for federal research grants. At the time of the announcement, Congress already had approved the $90 million SEMATECH appropriation for fiscal year 1995 (ibid.).

In fiscal year 1996, a year scheduled to be the last in ARPA's sponsorship of SEMATECH (Lynn 1995), the question remains, "was SEMATECH a success?" From a return-on-investment perspective, the anecdotal evidence appears disappointing. Several of the members withdrew from the consortium, others struggled to justify their membership, and at least one was kept in only through the charity of a research program designed to accomplish that end. This record is particularly dismal, considering the fact that the companies put up only half of SEMATECH's funding. DOD covered half the cost of SEMATECH, yet the corporate members were able to appropriate returns on the total investment. Admittedly, the American semiconductor industry did make a comeback, but was that comeback the result of SEMATECH or its leadership at ARPA? Another possible explanation is found simply in recalling the changing market conditions during the relevant time period. The 1984-88 time frame played host to the Reagan defense buildup, during which the American semiconductor industry was unable to keep up with overall demand (Steinberg 1984). Because of defense procurement regulations, only American manufacturers were allowed to supply the demands of the Defense Department, creating a protected market and perhaps encouraging American companies to specialize in military requirements. Because of the size of the overall American market for semiconductors, Japanese manufacturers gained an opportunity to step in and supply the American commercial market, siphoning away market share. While American companies focused on the military customer, those Japanese companies may have been able to gain an advantage in meeting the requirements of the commercial sector. Thus, once the defense drawdown
began, American manufacturers may have found themselves at a competitive disadvantage. The federal subsidy to SEMATECH may have bought the American manufacturers enough time to regain their market share. If so, SEMATECH itself may actually have been irrelevant. This is an area ripe for further study.

Even if SEMATECH can claim responsibility for resurrecting the American semiconductor industry, the question remains, at what cost? What advanced research programs did ARPA forego in order to fund and manage SEMATECH? Again, supporting America's semiconductor industry may be admirable and even justifiable from the vantage point of preserving an industry important to national security and America's economy, but it cannot be classified as advanced research. The unique capabilities of the Advanced Research Projects Agency are, therefore, wasted on such efforts. Other government agencies are likely to be equally capable of administering a subsidy. Those other agencies are not, however, capable of filling ARPA's role in selecting high-risk research projects with a potential for revolutionizing the art of war.

If predictions by Craig Fields hold true, and ARPA is not able to refocus its efforts on advanced research for national defense, the agency may find itself involved in more projects like SEMATECH in the future. In 1989, Fields predicted the agency would be more active in the future in helping critical industries survive the threat of foreign competition (Charles 1989). Critical, in Fields's terms, is meant to imply not only those industries critical to national security, but also those industries considered critical to American economic growth. Indeed, the High Definition Television (HDTV) project was another industry already targeted for DARPA aid at the time of Mr. Fields's remarks. Although several military uses for advanced/flat-panel displays have been identified, the driving force behind DARPA's initial involvement was the perceived lead the Japanese owned in this area (ibid.).
ARPA's shift in direction did not occur without a struggle. The Bush Administration, opposed to the technology targeting idea, recognized the shift in DARPA priorities and fired director Craig Fields, who had spent an unusually lengthy 16 years at the agency. Surprisingly, one of Fields's many supporters was George A. Keyworth, the science advisor to President Reagan. Defending Fields, Keyworth expounded, "DARPA and Fields's record is a string of one brilliant idea after another" (Carey 1990, 31). Thus, the ideological lines are not as clearly drawn as one might expect. Though the Bush Administration put up a valiant fight, Congress won out. As a result, the 1993 Defense Conversion package included funds for such Congressionally-directed projects as the "High Definition Display Manufacturing Consortia" and the Manufacturing Extension and Manufacturing Engineering Education programs (Davey 1993).

Today, funding for flat-panel displays appears to be alive and well, in the form of the High Definition Systems program, and ARPA continues to fund SEMATECH for yet another year. Fortunately, statements before Congress by recent ARPA directors seem to indicate that the High Definition Systems program has been directed toward meeting military needs. As current ARPA director Larry Lynn explained before the Subcommittee on National Security, House Appropriations Committee,

FY96 primary efforts are focused on the development and demonstration of: power efficient displays for man-portable tactical information and cockpit applications; compact, high-brightness, large screen displays for command and control systems; and ultrahigh resolution displays for intelligence applications. The head mounted display program will shrink picture element size to allow over five million picture elements to be built in one square inch and provide high quality information to mobile warriors (1995).

Dr. Lynn identifies some exciting uses for high-definition systems which may very well help the U.S. military forces gain a technological edge on their adversaries. Whether this emphasis has come about as a result of a leadership decision to focus on ARPA's
traditional customer, or as a result of a smaller-than-expected commercial market is unclear. Another possible explanation is that ARPA directors may be trying to appease a Republican Congress which seems intent on cutting the size of government while preserving military capability. If so, this would help explain the 1994 Clinton Administration decision which directed ARPA to manage a $600 million program "to develop a significant US manufacturing capability for flat panel displays" (Aviation Week and Space Technology 1994, 80). Whatever the driving force might have been, the current program is, indeed, worthy of the advanced research label, and does hold promise for military innovation. The prospect of ARPA again being tasked with administering industrial policy in lieu of pursuing advanced research remains, however, a legitimate concern.

2. Expanding Size

Why was ARPA ever saddled with a project like SEMATECH, which appears to fall outside the agency's mission area? Senator Jeff Bingaman, D-N.M. explained candidly that ARPA was the only agency with the available funds (Charles 1989). Ideally, the decision of who should manage a particular research project should consider agency missions and strengths. The decision should not be made on the basis of available budgetary resources. Senator Bingaman's explanation is only partial, however. The real story goes back to legislation (S. 1233, 1987) introduced by Sen. John Glenn, D-Ohio, to create a whole new agency within the Commerce Department to act as a civilian version of DARPA. Many politicians wanted to cash in on the success of DARPA by duplicating it in the civilian/industrial sector. The legislation failed because of the cost and ideological opposition by the Bush Administration. Supporters of the idea did not, however, give up the fight to have civilian technology funded. In the absence of a civilian DARPA, they
simply created a small civilian technology office within the Commerce Department called the Advanced Technology Program (ATP) and simultaneously broadened the mission of its defense counterpart (Davey 1993; Kitfield 1990).

Since 1986, ARPA has begun to receive more funds than its directors request. From 1986 to 1990, for instance, DARPA's budget doubled, with the agency receiving more funds than it requested in three of those five years (this occurred, incidentally, during a period in which DOD's budget declined, in real terms). The significance of this aberration is that 1) the agency is growing in size, and 2) the agency is losing its freedom from micromanagement. Congress is pushing ARPA to fund dual-use technologies and government/industry consortia by allocating more funds to those areas than its directors request. In 1993, for instance, ARPA requested $10 million for high resolution display research, and was allocated $100 million. ARPA also received $581 million in appropriations earmarked for civilian "conversion initiatives" which the agency did not even request (Davey 1993). Congress, it appears, now has the responsibility for deciding which initiatives ARPA will fund. It should come as no surprise, then, that politicians are anxious to increase the agency's funding. In the opinion of former DARPA director Robert Cooper (1982-84), these forays into non-defense areas can serve only to distract ARPA from its critical primary mission:

In the case of SEMATECH, Congress plunks a $200 million-plus consortium in DARPA's lap, and tells the agency to manage it. Not only is that a big project for an agency the size of DARPA, but it is probably a mismatch with the type of person who is a program manager there. They tend to be venture capitalist types, looking for high risk, high payoff technologies, rather than major project managers. And that trend will change DARPA for the worse (Kitfield 1990, 26-28).
3. Changing Composition of Personnel

One of ARPA's great strengths has been its high turnover rate. While this characteristic may not be envied in many other organizations, it has helped ARPA by providing a continuous influx of highly-qualified scientists and engineers from the civilian sector. Typically, these people come in with the latest knowledge and fresh ideas and only stay long enough to oversee one major project. They bring a broad base of experience and talent from academia, industry, and the military. Recently, however, the situation has changed. More new scientists and engineers are arriving at ARPA from within government. In 1993, the composition of the ARPA staff was 80 percent government and 20 percent industry, just the opposite of the ratio in the 1970s.

Compounding the problem of having more people from inside the government and less from industry, those from within government are staying at ARPA longer. Former director Gary Denman expressed this concern in an address before the Subcommittee on Defense, House Appropriations Committee:

I am also concerned that the tour of duty for civil service program managers at ARPA is increasing. In 1978, 75 percent of the civil service program managers had been assigned to ARPA for less than four years, and only four percent had been with the Agency for more than eight years. Currently, 46 percent of the ARPA program managers have less than four years at the Agency and 19 percent have been in place for more than eight years. This is an unfavorable trend. The more stringent ethics regulations designed to close the revolving door between government and industry contribute to this trend; however, there are other factors involved. The recruitment of the best and brightest talent from industry is seriously hindered by the current disparity in pay and the lengthy and unwieldy government hiring process. This is particularly evident in attempting to recruit for the senior-level ARPA Office Director positions (1994, 17).

As Dr. Denman mentions, the changing personnel composition is the result of three primary causes. First, the salary differential between inside and outside the government has grown larger, increasing the penalty for the top people to serve a tour at ARPA.
Second, the government hiring process deters good people from applying. Dr. Denman, however, seems to minimize the importance of what I believe may be the most significant factor: the federal government's new "revolving door" legislation. These ethics regulations are intended to prevent people from profiting in the private sector from their government service. On the surface, the regulations seem reasonable. After all, who wants to see government employees profiting from their public positions? The unintended effect, however, is to keep ARPA from the full talent pool.

ARPA has a long history of rather entangled relationships with private-sector organizations. The personnel shuffle among ARPA, MIT, and Bolt, Beranek, and Newman is an obvious example. Did Robert Taylor, the former director of the agency's computer research program, act inappropriately when he asked his old friends at ARPA for money to build the first computer network? Was ARPA wrong for selecting BBN as a key contractor on the ARPANET project? Such relationships probably would not be allowed today, due to the mere appearance of impropriety, but they were essential to the success of the network, because they implemented the best talent in the country. In the past, civilian scientists may have been willing to accept a pay cut to work at ARPA if they believed the tour would enhance their careers. If, however, civilian scientists fear taking a position at ARPA will mark the end of their private-sector careers, the prestige of serving in the agency is bound to suffer. This problem, therefore, is not likely to be solved simply by increasing program managers' pay.

Another change which may be relevant is the shift in the military-to-civilian ratio on the ARPA staff. As shown in Figures 5 and 6, the military presence on the ARPA staff has declined steadily over the 37 years since the agency's inception.
Figure 5: ARPA Military/Civilian Staff Mix

Figure 6: Military Presence on ARPA Staff
This is disturbing because the military/civilian mix was one of the strengths IDA identified in Volume 3 of their study:

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 (Van Atta, Deitchman, and Reed 1991, V-10).

To the extent that the reduction in the military presence reduces the understanding of Service problems and impacts Service acceptance of ARPA programs, the agency's success rate (in terms of funding military innovations) may be expected to suffer. The next danger is closely related to the changing composition of personnel, and may be considered the reason for it.

4. Loss of Customer Focus

Many judge ARPA's success on the basis of the large number of commercial "spin-offs" the agency has spawned. This is ironic because the spin-offs have occurred in spite of the agency's focus on military research. If ARPA can create so many commercial successes by accident, many reason, it could create many more by directly funding commercial research. Such ideas have received even more attention since the end of the Cold War. Adding to this sentiment is a report by the Council on Competitiveness, a blue-ribbon panel of industry and academic leaders, which warned in 1991 that America was losing its technological edge in the global marketplace. The report placed significant blame for this perceived technological decline on excessive defense spending (Perry 1991). Combined with the public's increased sensitivity to the size of the nation's budget deficit, these factors have made Congress less willing to invest in research aimed solely at defense-related objectives. They instead have become increasingly more interested in funding "dual-use technologies;" that is, research targeted toward both the military and commercial sectors.
Many in Congress have grown impatient with waiting for spin-offs, and now believe more commercially-viable technologies may be generated by skipping the middleman (i.e., the defense sector) and directly targeting the various commercial industries. This new focus is reflected in the recent name change from DARPA to ARPA. Dropping "Defense" from the agency's name is meant to indicate a broader mission for the agency. In fact, the 1993 Defense Authorization bill required ARPA to "pursue imaginative and innovative research and development projects having . . . both military and commercial (dual-use) applications" (Davey 1993, 7). Jacques Gansler, in Affording Defense, defended the dual-use idea, explaining that "shifting the focus of defense R&D toward 'dual-use' technologies is not meant to replace private-sector funding of non-defense R&D; it is meant to complement it" (1989, 236). Unfortunately, this change necessarily leads to a diluted focus and a dissolution of the supplier-customer relationship. If ARPA is to fund research beneficial to everyone, how can it focus on anyone? How will it select which projects to fund without more definite objectives? DARPA knew its customer. Can ARPA ever know the needs of the amorphous private sector?

In 1993, President Clinton launched the multi-agency "Technology Reinvestment Program" for the purpose of fostering new dual-use technologies, simplifying the defense acquisition process, and easing the burden of the defense drawdown. If the project can achieve these goals, the benefit is expected to be in the form of a closer relationship between government and industrial research and development. This development ideally would lead to innovations useful to both the commercial and military sectors, and would help to hold down the cost of defense acquisitions. Teaming ARPA, the Department of Commerce's National Institute of Standards and Technology (NIST), the Department of Energy, Department of Transportation, National Science Foundation (NSF), and NASA, the Technology Reinvestment Program is headed by ARPA. Again, the goals are
laudable, and the program may prove worthwhile. The problem is that the Administration gave ARPA responsibility for directing the new project.

ARPA is well-suited for funding high-risk research to expand the envelope of defense technology. It is not, however, in any position to pass judgment about the commercial value of a technology or to rewrite the Defense Department's labyrinthine procurement regulations. With the federal government offering to pay for up to 50 percent of the cost of research selected by the Technology Reinvestment Program, proposals for funding are plentiful. Unfortunately, only a small percentage of the proposals can be funded, given the program's limited budget. Somehow, then, the government agencies must decide which companies and which industries will receive government funding and which will not. The danger, of course, is that such decisions will be made on the basis of political influences. Recognizing this unseemly situation, Sen. John McCain (R-AZ) told Deputy Defense Secretary William Perry, "We know there is intense competition for those funds. We would like to be assured that the funds are given on the basis of merit and not political influence" (Gregory 1993, 52).

Another concern is the issue of foreign participation in Clinton's defense conversion program. Most would agree that federal tax dollars should not go to foreign companies. Avoiding that outcome, however, is more difficult than many might imagine. In an increasingly global economy, a large number of successful companies are neither strictly American nor foreign, but are instead Multinational Corporations (MNC). Should American tax dollars assist companies with divisions in more than one country? ARPA's answer, responding to a nervous Congress, is "no." Funds will be restricted to companies with a "significant level of [their] research, development, engineering and manufacturing activities in the United States" (Gregory 1993, 53). Will that policy exclude the most successful companies, in favor of those that have not been able to expand globally? If so,
is ARPA not acting to circumvent market forces and prop up the relative failures? In effect, then, ARPA has taken on the role of a corporate welfare administrator. Somehow, that does not seem to fit in with President Eisenhower's original vision for the agency.

Considering the above concerns, one might question the justification for ARPA involvement in funding research to benefit the private sector. While military R&D may be justified on the basis of national defense being a public good, what is the rationale for funding commercial R&D with public money? Private companies fund research based on risk and expected returns. If risk is considered too great relative to expected returns for a private company to invest its own money, what is the rationale for the government taking money from its citizens to make such an investment? How can a government agency determine whether or not the private sector has under-invested in a particular technology? A broader focus also may present the opportunity for Congress to get more involved in selecting which projects will be funded. Many of DARPA's projects were highly classified, and therefore free of public scrutiny. ARPA will not have such a luxury when funding commercial research. Future research grants may well follow the example of the Department of Agriculture and be spread out equally among congressional districts. Alternatively, research funds may be allocated to certain geographical areas to appease powerful members of Congress, or purchase key votes on other legislation. Individual industries also will have a stake in how the funds are invested. As a result, lobbyists are likely to fight for equitable distribution of the research grants among industries. The net result could be to extinguish the flame of innovation at ARPA.
5. Increase in Micromanagement

Perhaps in response to criticism by the Council on Competitiveness, the Deputy Secretary of Defense decided in 1992 to require the Director of Defense Research and Engineering (DDR&E) to certify annually that DOD research funds (including those controlled by ARPA) are allocated optimally (Davey 1993). Will this requirement result in pressure on ARPA to avoid the high-risk research that has been the agency's hallmark? Undeniably, the answer is yes. In fact, the Institute for Defense Analyses evaluation found evidence that, even before the Council on Competitiveness report, ARPA operations were becoming increasingly restrictive:

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 (Van Atta, Deitchman, and Reed 1991, V-9).

The source of this phenomenon also can be traced to mandated programs outside the realm of "high-risk, high-payoff advanced R&D," because those programs require management and procurement procedures not suited to ARPA's primary role (Van Atta, Deitchman, and Reed 1991). The agency has become overly burdened in recent years with mandated programs in such areas as prototyping and improving manufacturing technology, which are not the advanced form of research for which ARPA was founded.
6. Resources Distributed Geographically: The TRP Case

It is the mission of the Technology Reinvestment Project (TRP) to stimulate the transition to a growing, integrated, national industrial capability which provides the most advanced, affordable, military systems and the most competitive commercial products. TRP programs are structured to expand high quality employment opportunities in commercial and dual-use United States industries and demonstrably enhance U.S. competitiveness. This will be accomplished through the application of defense and commercial resources to develop dual-use technologies, manufacturing and technology assistance to small firms, and education and training programs that enhance U.S. manufacturing skills and target displaced defense industry workers (ARPA 1993, 1-1).

While ARPA once focused its resources on a few Centers of Excellence, the Technology Reinvestment Program (TRP), started in 1993, provides an example of ARPA distributing its funds geographically, in a politically-acceptable fashion. Not only are the funds distributed throughout the country, but a portion of the funds are set aside for small businesses, through the Small Business Innovative Research Program. In this program, leadership in the field of research is less important than being a small company (i.e., less than 500 employees). In fact, should a small company be judged to "dominate" the field in which it submits a research project, that company will be disqualified (ARPA 1993)! Apparently, the idea of funding the best research is no longer paramount.
Chapter 7: Conclusion

The present study has provided an informal review of the literature useful in planning evaluations of the Advanced Research Projects Agency. It has suggested a few ideas for conducting evaluations of the individual projects, and has identified a plausible list of factors which have enabled ARPA to achieve the degree of success with which it is so often credited. Along with those positive factors, the review has identified six trends which may be threatening the future of the agency.

ARPA has funded many innovative research projects over the past thirty-seven years, and has developed a loyal following of admirers. Still, in thirty-seven years, only one comprehensive evaluation has been performed, and that was funded under ARPA contract and did not include any detailed analysis. Beginning with an agenda of large, high-risk presidential issues, ARPA's advanced research efforts are now intermingled with managing government-industry consortia and doling out money to displaced defense workers and small businesses. The agency is at a cross-roads in its history and may have lost its clear direction. In-depth analysis is long overdue, and should help the agency to capitalize on its prior strengths to ensure a bright future. Such evaluations will be especially challenging, due to the difficulty in obtaining necessary data, but need to be started.

Patent citation analysis may offer some promise as an evaluation tool, provided the researcher can find the identification numbers of patents generated by ARPA. Finding these identification numbers is likely to be a very difficult task, however. The researcher may have more success in searching on-line databases of journal abstracts to assess the opinions of experts regarding particular areas of ARPA-funded research. This would provide a test of the assertion that ARPA is highly-respected in the scientific community.
The most promising technique for future research efforts involves the use of case-studies. This approach may be useful in evaluating ARPA's contribution to key technological innovations. As mentioned earlier, ARPA's role in stealth technology may not be as significant as the agency claims, since much of the actual research was performed outside of ARPA's purview and prior to the agency's involvement. Case studies of other ARPA programs may reveal similar findings. Performed for several programs at various points in time, the case study approach also might reveal whether ARPA's impact has changed over the years.

The factors identified in Chapter 5 seem to be responsible, to some degree, for ARPA's apparent success in funding research. A thorough test of this hypothesis, however, will require a similar evaluation of other "peer" agencies, such as the National Institutes of Health, the National Science Foundation, and the Naval Research Laboratory. Only when such evaluations are accomplished, and comparisons can be performed, will the evidence be sufficient to determine conclusively which characteristics lead to efficiency in funding successful research.

12 See page 22.
**Appendix A: Approximate ARPA Investments, through 1988, in Projects Selected by IDA**

<table>
<thead>
<tr>
<th>PROGRAM</th>
<th>PROJECT</th>
<th>ARPA INVESTMENT (Millions)</th>
<th>COMMENTS</th>
</tr>
</thead>
<tbody>
<tr>
<td>SPACE</td>
<td>ARGUS</td>
<td>9</td>
<td></td>
</tr>
<tr>
<td></td>
<td>TITOS</td>
<td>14</td>
<td></td>
</tr>
<tr>
<td></td>
<td>TRANSIT</td>
<td>52</td>
<td>p3-8 (vol 1) says 28M and 24M for a total of 42M</td>
</tr>
<tr>
<td></td>
<td>CENTAUR</td>
<td>22</td>
<td></td>
</tr>
<tr>
<td></td>
<td>SATURN</td>
<td>101.5</td>
<td></td>
</tr>
<tr>
<td>DEFENDER (ABM)</td>
<td>ESAR</td>
<td>45</td>
<td>ESAR construction and testing, plus phased array technology program</td>
</tr>
<tr>
<td></td>
<td>HE LASERS</td>
<td>1000</td>
<td>750M for lasers, plus about $250M for space mirrors work</td>
</tr>
<tr>
<td></td>
<td>OFR RADAR</td>
<td>100</td>
<td></td>
</tr>
<tr>
<td></td>
<td>AMOS</td>
<td>102</td>
<td>initial cost: $12M; cost of later phases = $90M (10-9, v1)</td>
</tr>
<tr>
<td></td>
<td>PRESS</td>
<td>200</td>
<td></td>
</tr>
<tr>
<td></td>
<td>AURECIBO</td>
<td>19</td>
<td></td>
</tr>
<tr>
<td></td>
<td>HIBEX</td>
<td>25</td>
<td></td>
</tr>
<tr>
<td></td>
<td>UPSTAGE</td>
<td>26</td>
<td></td>
</tr>
<tr>
<td></td>
<td>PENAIDS</td>
<td>25</td>
<td></td>
</tr>
<tr>
<td>VELA (NUC. MON.)</td>
<td>V. HOTEL</td>
<td>150</td>
<td></td>
</tr>
<tr>
<td></td>
<td>V. UNIFORM</td>
<td>9</td>
<td></td>
</tr>
<tr>
<td></td>
<td>LASA</td>
<td>45</td>
<td></td>
</tr>
<tr>
<td></td>
<td>NORSAR</td>
<td>8</td>
<td></td>
</tr>
<tr>
<td>AGILE</td>
<td>AR-15/M-16</td>
<td>0.5</td>
<td>0.1 plus expense of field office in Vietnam (est. $100K)</td>
</tr>
<tr>
<td></td>
<td>CAMP SENTINEL</td>
<td>7</td>
<td></td>
</tr>
<tr>
<td></td>
<td>X-269-OT-2</td>
<td>0.25</td>
<td></td>
</tr>
<tr>
<td></td>
<td>POCKET VETO</td>
<td>12</td>
<td>$6M direct cost, plus $6 M for predecessor programs</td>
</tr>
<tr>
<td>INF. PROCESSING</td>
<td>ILLIAC IV</td>
<td>59</td>
<td>$31M development cost, plus $28M for &quot;shakedown and utilization&quot;</td>
</tr>
<tr>
<td></td>
<td>Project MAC</td>
<td>25</td>
<td></td>
</tr>
<tr>
<td></td>
<td>ARPANET</td>
<td>25</td>
<td></td>
</tr>
<tr>
<td></td>
<td>AI</td>
<td>120</td>
<td>130 Up to Strategic Computing Program (est. additional $130M)</td>
</tr>
<tr>
<td></td>
<td>Morse Code Reader</td>
<td>2</td>
<td></td>
</tr>
<tr>
<td></td>
<td>ACCAT</td>
<td>15.7</td>
<td></td>
</tr>
<tr>
<td></td>
<td>LAMBDAB</td>
<td>12</td>
<td>36 Not including MFA, FME, or OMAT (est. $12M each)</td>
</tr>
<tr>
<td></td>
<td>SLCSAT</td>
<td>150</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Interact. Comp. Graphics</td>
<td>0</td>
<td>100 No figure provided (est. $100M)</td>
</tr>
<tr>
<td></td>
<td>Image Understanding</td>
<td>72</td>
<td>Through 1988</td>
</tr>
<tr>
<td></td>
<td>ADA</td>
<td>0</td>
<td>5 No figure provided - ARPA played a mrg role (est. $5M)</td>
</tr>
<tr>
<td></td>
<td>SIMMNET</td>
<td>32.1</td>
<td>15% of $214M total</td>
</tr>
<tr>
<td></td>
<td>VLSI</td>
<td>0</td>
<td>100 No figure provided; MOSIS said to be low-cost because it was able</td>
</tr>
<tr>
<td></td>
<td>MOSIS</td>
<td>54</td>
<td>To leverage the VLSI investment (18-32, vol 2). Est. $100M</td>
</tr>
<tr>
<td>TACTICAL TECH</td>
<td>TANK BREAKER</td>
<td>35</td>
<td>Facility and staff: $30M; Proj. support: $24M (18-24, vol 2)</td>
</tr>
<tr>
<td></td>
<td>HIMAG/HVT-L</td>
<td>47</td>
<td></td>
</tr>
<tr>
<td></td>
<td>MINI-RPVs</td>
<td>15</td>
<td></td>
</tr>
<tr>
<td></td>
<td>ASSAULT BREAKER</td>
<td>155</td>
<td></td>
</tr>
<tr>
<td></td>
<td>PAVE MOVEMENT/AMOS</td>
<td>50</td>
<td></td>
</tr>
<tr>
<td></td>
<td>BETA</td>
<td>9</td>
<td></td>
</tr>
<tr>
<td></td>
<td>CELT</td>
<td>11</td>
<td></td>
</tr>
<tr>
<td>SURVEILLANCE</td>
<td>TEL RUBY/HCAMP</td>
<td>354</td>
<td>24M initial, 100M overrun, 230M 1982 baseline addl funds</td>
</tr>
<tr>
<td>AVIATION TECH</td>
<td>STEALTH</td>
<td>0</td>
<td>1000 No estimate provided- cited security concerns, Guess $1B</td>
</tr>
<tr>
<td></td>
<td>X-29</td>
<td>126.85</td>
<td>AO 3436; 8.85M, AO4168: 118M</td>
</tr>
<tr>
<td>NAVAL TECH</td>
<td>ARI</td>
<td>64</td>
<td></td>
</tr>
<tr>
<td>MATERIALS</td>
<td>Digital Gallium Arsenide</td>
<td>5.5</td>
<td>14.5 1977 Rockwell effort; $5.5M; no figures for later programs. Est. $20M total</td>
</tr>
<tr>
<td></td>
<td>IDUs</td>
<td>158</td>
<td></td>
</tr>
<tr>
<td></td>
<td>F-100 Engine RFC</td>
<td>13</td>
<td>$7M direct costs, plus $6M for related work</td>
</tr>
<tr>
<td>TOTAL</td>
<td></td>
<td>3700.4</td>
<td>1486.6 These projects represent up to $5.19 Billion of ARPA's 1958-88 budgets</td>
</tr>
</tbody>
</table>
Appendix B: The AIDS Patent Project Search Engine

The search engine used for the AIDS Patent Project\textsuperscript{13} is the same tool which will be used for the new system to be introduced by the USPTO on 9 November, 1995. This system will allow access to over twenty years of bibliographic text data from the front pages of patent applications.

The AIDS Patent Project has two search tools, one a free text search, and the other a two-field boolean search. The free text search tool is available on the AIDS Patents Search Page, http://patents.cnidr.org/pto/search.html, and consists of a form resembling the following:

Enter terms to find: _______________
Search on field: _______________
{Options: Full Text, Patent Number, Title, Abstract, Inventor Name, Assignee Name, Claims, Original Classification}

Enter terms to de-emphasize: _______________
Search on field: _______________ {Options same as above}

The second tool is available on the AIDS Patents Boolean Search Page, http://patents.cnidr.org/pto/bool.html, and consists of a form resembling the following:

Enter first term: _______________
Search on Field: _______________

{Options: Full Text, Patent Number, Title, Abstract, Inventor Name, Assignee Name, Claims, Original Classification}

Boolean operator: _______________ {Options: and, or, andnot}

Enter second term: _______________
Search on Field: _______________ {Options same as first field}

\textsuperscript{13} Home Page http://patents.cnidr.org/welcome.html
## Appendix C: ARPA Staff Size by Year

<table>
<thead>
<tr>
<th>Year</th>
<th>Civilian</th>
<th>Military</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>1963</td>
<td>109</td>
<td>60</td>
<td>169</td>
</tr>
<tr>
<td>1964</td>
<td>125</td>
<td>65</td>
<td>190</td>
</tr>
<tr>
<td>1965</td>
<td>149</td>
<td>61</td>
<td>210</td>
</tr>
<tr>
<td>1966</td>
<td>148</td>
<td>57</td>
<td>205</td>
</tr>
<tr>
<td>1967</td>
<td>153</td>
<td>54</td>
<td>207</td>
</tr>
<tr>
<td>1968</td>
<td>147</td>
<td>58</td>
<td>205</td>
</tr>
<tr>
<td>1969</td>
<td>136</td>
<td>61</td>
<td>197</td>
</tr>
<tr>
<td>1970</td>
<td>128</td>
<td>60</td>
<td>188</td>
</tr>
<tr>
<td>1971</td>
<td>122</td>
<td>59</td>
<td>181</td>
</tr>
<tr>
<td>1972</td>
<td>126</td>
<td>59</td>
<td>185</td>
</tr>
<tr>
<td>1973</td>
<td>118</td>
<td>55</td>
<td>173</td>
</tr>
<tr>
<td>1974</td>
<td>118</td>
<td>43</td>
<td>161</td>
</tr>
<tr>
<td>1975</td>
<td>120</td>
<td>33</td>
<td>153</td>
</tr>
<tr>
<td>1976</td>
<td>120</td>
<td>33</td>
<td>153</td>
</tr>
<tr>
<td>1977</td>
<td>113</td>
<td>32</td>
<td>145</td>
</tr>
<tr>
<td>1978</td>
<td>108</td>
<td>30</td>
<td>138</td>
</tr>
<tr>
<td>1979</td>
<td>117</td>
<td>29</td>
<td>146</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Year</th>
<th>Civilian</th>
<th>Military</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>1980</td>
<td>121</td>
<td>29</td>
<td>150</td>
</tr>
<tr>
<td>1981</td>
<td>123</td>
<td>29</td>
<td>152</td>
</tr>
<tr>
<td>1982</td>
<td>123</td>
<td>29</td>
<td>152</td>
</tr>
<tr>
<td>1983</td>
<td>131</td>
<td>32</td>
<td>163</td>
</tr>
<tr>
<td>1984</td>
<td>130</td>
<td>30</td>
<td>160</td>
</tr>
<tr>
<td>1985</td>
<td>125</td>
<td>25</td>
<td>150</td>
</tr>
<tr>
<td>1986</td>
<td>125</td>
<td>26</td>
<td>151</td>
</tr>
<tr>
<td>1987</td>
<td>139</td>
<td>26</td>
<td>165</td>
</tr>
<tr>
<td>1988</td>
<td>130</td>
<td>26</td>
<td>156</td>
</tr>
<tr>
<td>1989</td>
<td>145</td>
<td>28</td>
<td>173</td>
</tr>
<tr>
<td>1990</td>
<td>145</td>
<td>27</td>
<td>172</td>
</tr>
<tr>
<td>1991</td>
<td>140</td>
<td>26</td>
<td>166</td>
</tr>
<tr>
<td>1992</td>
<td>145</td>
<td>24</td>
<td>169</td>
</tr>
<tr>
<td>1993</td>
<td>158</td>
<td>24</td>
<td>182</td>
</tr>
<tr>
<td>1994</td>
<td>152</td>
<td>24</td>
<td>176</td>
</tr>
<tr>
<td>1995</td>
<td>158</td>
<td>24</td>
<td>182</td>
</tr>
</tbody>
</table>

Table II: ARPA Staff Size, by Year (Source: ARPA, unpublished)
<table>
<thead>
<tr>
<th>Glossary</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>AGILE</td>
<td>Vietnam-era counterinsurgency R&amp;D program</td>
</tr>
<tr>
<td>AI</td>
<td>Artificial Intelligence</td>
</tr>
<tr>
<td>ARPA</td>
<td>Advanced Research Projects Agency</td>
</tr>
<tr>
<td>ASSAULT BREAKER</td>
<td>Standoff follow-on forces attack program</td>
</tr>
<tr>
<td>BBN</td>
<td>Bolt, Beranek, and Newman</td>
</tr>
<tr>
<td>BMD</td>
<td>Ballistic Missile Defense</td>
</tr>
<tr>
<td>C3</td>
<td>Command, Control, and Communications</td>
</tr>
<tr>
<td>DARPA</td>
<td>Defense Advanced Research Projects Agency</td>
</tr>
<tr>
<td>DEFENDER</td>
<td>Ballistic missile defense program</td>
</tr>
<tr>
<td>DOD</td>
<td>Department of Defense</td>
</tr>
<tr>
<td>DOE</td>
<td>Department of Energy</td>
</tr>
<tr>
<td>EEMIT</td>
<td>Experimental Evaluation of Major Innovative Technologies</td>
</tr>
<tr>
<td>IDA</td>
<td>Institute for Defense Analyses</td>
</tr>
<tr>
<td>IPTO</td>
<td>Information Processing Techniques Office</td>
</tr>
<tr>
<td>IRR</td>
<td>Internal Rate of Return</td>
</tr>
<tr>
<td>LIGHTSAT</td>
<td>Program to develop small satellites for tactical C3</td>
</tr>
<tr>
<td>MAC</td>
<td>Multiple Access Computer / Machine Aided Cognition</td>
</tr>
<tr>
<td>MNC</td>
<td>Multinational Corporation</td>
</tr>
<tr>
<td>MIMIC</td>
<td>Microwave/millimeter-wave monolithic integrated circuit chips</td>
</tr>
<tr>
<td>MIT</td>
<td>Massachusetts Institute of Technology</td>
</tr>
<tr>
<td>NASA</td>
<td>National Aeronautics and Space Administration</td>
</tr>
<tr>
<td>NASP</td>
<td>National Aerospace Plane</td>
</tr>
<tr>
<td>NIH</td>
<td>National Institutes of Health</td>
</tr>
<tr>
<td>NIST</td>
<td>National Institute of Standards and Technology</td>
</tr>
<tr>
<td>NRL</td>
<td>Naval Research Laboratory</td>
</tr>
<tr>
<td>NSF</td>
<td>National Science Foundation</td>
</tr>
<tr>
<td>ONR</td>
<td>Office of Naval Research</td>
</tr>
<tr>
<td>PM</td>
<td>Program Manager or Project Manager</td>
</tr>
<tr>
<td>R&amp;D</td>
<td>Research and Development</td>
</tr>
<tr>
<td>SBIR</td>
<td>Small Business Innovative Research Program</td>
</tr>
<tr>
<td>SCP</td>
<td>Strategic Computing Program</td>
</tr>
<tr>
<td>SDI</td>
<td>Strategic Defense Initiative</td>
</tr>
<tr>
<td>SEMATECH</td>
<td>The Consortium of Semiconductor Manufacturing Technology</td>
</tr>
<tr>
<td>STARS</td>
<td>Software Technology for Adaptable Reliable Systems</td>
</tr>
<tr>
<td>STO</td>
<td>Strategic Technologies Office</td>
</tr>
<tr>
<td>TCP/IP</td>
<td>Transfer Control Protocol / Internet Protocol</td>
</tr>
<tr>
<td>TEAL RUBY</td>
<td>Space-based infrared surveillance program</td>
</tr>
<tr>
<td>TRP</td>
<td>Technology Reinvestment Program</td>
</tr>
<tr>
<td>USDA</td>
<td>U.S. Department of Agriculture</td>
</tr>
<tr>
<td>USPTO</td>
<td>U.S. Patent and Trademark Office</td>
</tr>
<tr>
<td>VELA</td>
<td>Nuclear test detection program</td>
</tr>
</tbody>
</table>
References


(Cited in Melkers, 1993).
_Evaluating R&D Impacts: Methods and Practice_. Edited by Barry Bozeman and

Appropriations Committee. 23 March.


Transaction Publishers.

Street Journal_, 5 October.

Melkers, Julia. 1993. Bibliometrics as a Tool for Analysis of R&D Impacts. In
_Evaluating R&D Impacts: Methods and Practice_. Edited by Barry Bozeman and

Papadakis, Maria. 1993. Patents and the Evaluation of R&D. In _Evaluating R&D
Impacts: Methods and Practice_. Edited by Barry Bozeman and Julia Melkers.


American Century_ 123 (Spring/Summer).

August.

---. 1993a. Harris Corp. Leaves Sematech; Pullout Is Third in a Year. _The Wall Street
Journal_, 6 January.


