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Semiquantitative Methods for Research Impact Assessment

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ABSTRACT

Three types of semiquantitative methods used by the federal government in research impact assessment are presented. These include the classic retrospective method (Project Hindsight), another retrospective approach (Project TRACES and follow-ons), and accomplishments books used by selected research-sponsoring organizations (Office of Naval Research, Air Force Office of Scientific Research, Department of Energy Office of Health and Environmental Research, Department of Energy High Energy Physics Program, Defense Advanced Research Projects Agency). The strengths and weaknesses of each approach are discussed. One goal of all the studies presented was to identify the products of research and some of their impacts. In addition, the Hindsight, TRACES, and DARPA studies tried to identify factors that influenced the productivity and impact of research. The following general conclusions about the role and impact of basic research were reached:

1. The majority of basic research events that directly impacted technologies or systems were non-mission-oriented and occurred many decades before the technology or system emerged.
2. The cumulative indirect impacts of basic research were not accounted for by any of the retrospective approaches published.
3. An advanced pool of knowledge must be developed in many fields before synthesis leading to an innovation can occur.
4. Allocation of benefits among researchers, organizations, and funding agencies to determine economic returns from basic research is very difficult and arbitrary, especially at the micro level.

Introduction

In the evaluation of research impact, a spectrum of approaches may be considered [1–12]. At one end of the spectrum are the subjective, essentially nonquantitative approaches, of which peer review is the prototype [2–6, 9, 13–19]. At the other end of the spectrum are the mainly quantitative approaches, such as evaluative bibliometrics and cost-benefit [20–28]. In between are what can be termed semiquantitative approaches [11, 12].

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These semiquantitative methods make less, or little, use of mathematical tools but attempt to draw on documented approaches and results wherever possible. They have limited credibility in the analytic community, because the selection of results tends to be arbitrary rather than mathematically rigorous, and they are viewed more as anecdotal approaches than serious technical approaches. Nevertheless, in practice, some of these approaches (namely, studies of accomplishments resulting from sponsored research programs) are widely used by the research-sponsoring organizations.

In this paper, three types of semiquantitative methods used by the US federal government are discussed. First is the classic look-back method, as typified by Project Hindsight. While this study was done in the 1960s, its complexity and expense precluded follow-on studies of a similar level, and it remains the leading example of its genre. Second, another approach to look-back, as exemplified by Project TRACES, is reviewed. Here, the critical events leading to a major advance are identified. Third, the accomplishments documents approach, used by many, if not all, research-sponsoring organizations is examined.

Project Hindsight

Project Hindsight was established by the Defense Department in 1965 to identify those management factors important in assuring that research and technology programs are productive and that program results are used. It also attempted to measure the overall increase in cost-effectiveness in the current generation of weapons systems compared with that of their predecessors assignable to any part of the Defense Department's investment in research and science and technology [29].

As the name implies, the approach taken in Project Hindsight was retrospective. Twenty arbitrarily selected recent weapons systems and major military equipments were analyzed by (mainly DOD in-house) teams of technical specialists. Their task was to identify applications of science and technology that were not utilized in predecessor military systems designed to meet roughly the same requirements. The evolution of the new technology represented in each system was traced back in time to critical points called "research or exploratory development (RXD) events." The RXD event was the basic quantifying unit in the study and was defined as the occurrence of a novel idea and the subsequent scientific and engineering activity in which the idea was examined or tested. There could be one or two RXD events, or an extended chain of them, culminating in a device or component found in a particular system.

The teams of specialists identified 710 unique RXD events, conducted the historical traces, and described and documented the related activities in terms of the differential amount of knowledge that accounted in part for the increased cost-effectiveness of the systems analyzed (compared with their predecessors). Project Hindsight concentrated only on the post-World War II contributions of science and technology on the selected systems. Each study team was allowed about three months to complete its research on each system.

In treating the sciences, Hindsight distinguished (a) the basic research done to solve a specific assigned problem from (b) the basic research done to expand the frontiers of scientific knowledge. These were categorized as directed and undirected basic research, respectively. It was found that RXD events from the directed basic research category emerged in systems development approximately nine years following their conception, while it took 20 or more years for some events from the undirected category to impact development. Thus, the Hindsight study did not treat in any depth the contribution from undirected basic research, since many of those events predated the time span of the project [29].

Before discussing the methodology further, some of the critical findings will be summarized. The identification of the RXDs was found to be fairly simple, and time limitations permitted only a fraction to be uncovered and examined. The results of research in science were most frequently exploited when the investigator responded to recognized needs of the engineering community. A high probability of utilization involved awareness on the part of the scientist concerning who in the engineering community needed the knowledge, and on the part of the interested engineers as to which specific scientist was working on the problem.

The greatest identified payoff in terms of ideas leading to enhanced weapons systems resulted from research in technology—and then, where the research scientist or engineer was intimately aware of problems of the applications engineer. The real difference in performance between a weapon system and its predecessor was usually not the consequence of one, two, or three scientific advances or technological capabilities but was the synergistic effect of 100, 200, or 300 advances, each of which alone was relatively insignificant. These hundreds of diverse advances must then be fitted and adjusted for a unified operational weapon system. The characteristics of each advance must be carefully interfaced with those of other advances. Project Hindsight data showed that systems applications, rather than new science, inspired science and technology for advanced systems.

While criticisms of a project of the complexity and scope of Hindsight are certainly possible, given the alternatives (or lack thereof), Hindsight was probably a reasonable first step in assessing the impact of applied research and technology development on weapons systems. The obvious question is whether the Hindsight approach and conclusions were appropriate for evaluating the impact of basic research on weapons systems, or whether the study ground rules and constraints contained built-in biases against basic research.

The most obvious limitation of Hindsight relating to basic research is the time frame. A reading of the Hindsight report appendices shows that most of the RXDs occurred in the 1950s, with few in the 1940s and 1960s. Because many fundamental research projects could require more than two decades for their results to impact systems (especially two decades ago when dissemination of results did not have the benefit of today's communication channels and systems), the cutoff on time span could have precluded the inclusion of research impacts. If an updated Hindsight study were performed, the time problem could be alleviated by increasing the retrospective time span allowed. Thus, the time-span problem is not a flaw or limitation of the generic retrospective process, but rather is associated with the particular Hindsight implementation.

A more serious limitation relates to the RXD approach. The RXDs are identifiable advances that draw upon the pool of technical knowledge in existence at that time. But the pool of knowledge is continually increasing, and the components of this pool are highly interrelated, both directly and indirectly. For example, advances in basic materials understanding may be dependent upon advances in physics, chemistry, mathematics, computer technology, laser technology, computer algorithms, and so forth. Some of these impacts are direct; most are indirect.

Thus, any RXD could theoretically be shown to be impacted directly or indirectly by small (or in some cases large) advances in the component basic research of the knowledge pool. While the direct or indirect impact of any one basic research component on any one RXD may be small (if it were large and within the time span, it would have been identified as an RXD), the total direct and indirect impact of this basic research component on *all* the RXDs may not be small. *These cumulative indirect and direct* *impacts of basic research are not accounted for by the Hindsight methodology, and in fact are not taken into account by any of the retrospective approaches published or in use today. A recent study [30] that examined impacts of research on other research and technology through direct and indirect paths using a network approach showed that the indirect impacts of fundamental research can be very large in a cumulative sense.* For Hindsight, the indirect impacts would have been even larger if the actual larger number of RXDs had been examined.

The Hindsight conclusions relative to the impact of basic research have to be seen in perspective. The conclusion to be drawn from the study is that fundamental research had little direct impact on selected weapons systems (whose degree of design conservatism, which could impact implementation speed of revolutionary concepts, was not stated or evaluated) in a time-period threshold two decades before weapon system implementation. Had the time-period threshold been expanded, and indirect impacts of the basic research been incorporated into the study, then a conclusion could have been drawn about the total impact of the basic research on weapon systems. However, had the question about impact been raised from the basic research component viewpoint, and an appropriate study been done (of which Hindsight would have been one part), then conclusions could have been drawn about total impact of the basic research component on all technology and systems, of which the Hindsight weapons systems were one part.

TRACES

THE ORIGINAL TRACES STUDY

In 1967, the National Science Foundation (NSF) instituted a study to trace retrospectively key events that had led to a number of major technological innovations (Technology in Retrospect and Critical Events in Science—TRACES). One goal was to provide more specific information on the role of the various mechanisms, institutions, and types of R&D activity required for successful technological innovation [31].

The study performers, Illinois Institute of Technology Research Institute (IITRI), chose, in their view, a representative cross section of R&D for study and treated each case in as standard a manner as possible. The five innovations selected were magnetic ferrites, video tape recorder, oral contraceptive pill, electron microscope, and matrix isolation. Key "events" in the R&D history of each innovation were identified, an "event" being defined as the point at which a published paper, presentation, or reference to the research was made. The R&D activities on the five tracings were grouped by category of research (mission, nonmission), type of institution, date of event, and so forth, to bring out some of the factors that entered into the transition from nonmission research to innovation.

The study showed that nonmission research provided the origins from which science and technology could advance toward innovations. It also showed that, of the 341 key R&D events judged to be important to the evaluation of innovation, approximately 70% were nonmission research, 20% mission-oriented research, and 10% development and application.

The number of nonmission events peaked significantly between the 20th and 30th years prior to an innovation, while mission-oriented research events and those in the development and application area peaked during the decade preceding innovation. For the cases studied, the average time from conception to demonstration of an innovation was nine years.

Ten years prior to an innovation, that is, shortly before conception, approximately 90% of the nonmission research had been accomplished; most nonmission research appeared completed prior to the conception of the innovation to which it would ultimately contribute. The tracings also revealed cases in which mission-oriented research or development efforts elicited later nonmimssion research which often was found to be crucial to the ultimate innovation.

There are a number of interesting comparisons to be made between TRACES and Hindsight. First, the TRACES time frame extends back sufficiently far to include many basic research results, while the Hindsight time span was able to include most development events, but excluded most basic research results. Hindsight traced the impacts on weapons systems, whereas TRACES examined the impact on single technologies. Thus, the Hindsight starting point, a weapons system, is one level higher (consists of many single technologies) than the TRACES starting point. Coupled with the fact that the Hindsight weapons systems had, on average, 35 events, and the TRACES innovations had, on average, 70 events, it is not surprising that the Hindsight events tended to be applied research or technology advances, whereas the TRACES events tended to be more basic research. In neither case were indirect impacts of basic research given formal credit, although the TRACES study did allude to nonmission research as "a fund of knowledge against which withdrawals can be made to achieve innovation at a rate satisfactory to society" [31].

TRACES FOLLOW-ON STUDY

In a follow-on study to TRACES, the NSF sponsored Battelle-Columbus Laboratories to perform a case study examination of the process and mechanism of technological innovation [32]. For each of the ten innovations studied (heart pacemaker, hybrid corn, hybrid small grains, green revolution wheat, electrophotography, input-output economic analysis, organophosphorus insecticides, oral contraceptives, magnetic ferrites, video tape recorder), the significant events (important activity in the history of an innovation) and decisive events (a significant event that provides a major and essential impetus to the innovation) which contributed to the innovation were identified. The influence of various exogenous factors on the decisive events was determined, and several important characteristics of the innovative process as a whole were obtained.

Based on frequency of occurrence of the highest rankings of the exogenous factors on the decisive events, the following rankings of importance were obtained:

1. Recognition of technical opportunity (motivation of the timely improvement of an existing product or process) ranked first among the exogenous factors.
2. Recognition of the need (motivation for solving the problem or meeting *the* need satisfied by the eventual innovation, rather than *any* technological need) ranked second.
3. Technical entrepreneur (an individual within the performing organization who champions a scientific or technical activity) ranked third.
4. Certain institutional factors, such as internal R&D management, availability of funding, management venture decision, and so forth, ranked fourth collectively, indicating the importance of the institutional environment to the innovative process.

Based on examination of characteristics of the case histories as a whole, rather than cusing on decisive events as asbove, the following generalizations were drawn:

1. The technical entrepreneur is a characteristic important in nine of the ten innovations, and is a major driving force in the innovative process.
2. Early recognition of the need was characteristic of the history of nine of the innovations.
3. Government funding was instrumental in direct support of seven of the innovations. More generally, availability of financial support, from whatever source, emerged as an important feature of the innovative process.
4. The occurence of an unplanned confluence of technology was characteristic of six of the innovations. Confluence of technology occurred for the other four innovations as well, but as a result of deliberate planning, rather than by accident.
5. Most of the innovations originated outside the organization that developed them.
6. Additional supporting inventions were required during the development effort for all the innovations studied to arrive at a product with consumer acceptance.

Over the full time span of the innovation, nearly 34% of the significant events were non-mission-oriented research (NMOR), 38% were mission-oriented research (MOR), 26% were developmental, and a few percent were nontechnical. Of the total events in the period prior to conception of the innovative idea, over half were NMOR and one-third MOR. In the bounded interval between first conception and first realization, 16% were NMOR, with the remainder split among MOR (43%), development (38%), and nontechnical events (3%). Many of the NMOR events in the bounded interval were in the nature of feedback or spin-off basic research prompted by the innovation. In the postinnovation period, when diffusion and improvement take place, 10% of the events were NMOR, 39% were MOR, and 45% were development.

The number of NMOR events peaked in the period three to four decades prior to the culmination of the innovation, whereas the number of MOR and development events peaked in the decade preceding the data of innovation. Half of the NMOR events occurred 30 years preceding innovation; half of the MOR events occurred in the 15 years prior to innovation; and half the developmental effort took place within the ten years preceding innovation.

The study authors recognized, to some degree, that the focus on specific events did not allow sufficient credit to be allocated to the indirect impacts of research. As they stated:

> This kind of analysis tends to underplay the role of NMOR in the innovative process, since it does not portray the importance of the general background of science necessary for the other categories of technical events. For example, MOR and developmental activities in insecticides would have been impossible without the antecedent totality of organic chemistry. Similarly, research on contraception depended on the basic science background of reproductive biology. As a further example, in the case involving grain improvement, Hybrid Small Grains and Green Revolution Wheat show a low percentage of NMOR events (20 percent), but these percentages would be higher if the early NMOR events credited to Hybrid Corn were also counted in their totals.

They correctly identified the absence of recognition given to specific supporting fields of research. However, they did not identify or attempt to account for the impacts of the fundamental research from many fields which resulted in the instrumentation, theoretical, and computational capabilities necessary for these supporting research fields to advance.

While the technical entrepreneur is viewed as extremely important to the innovative process, it does not appear (to the author) to be the critical path factor. Examination of the historiographic tracings that display the significant events chronologically for each of the innovations shows that *an advanced pool of knowledge must be developed in many fields before synthesis leading to an innovation can occur. The entrepreneur can* *be viewed as an individual or group with the ability to assimilate this diverse information and exploit it for further development. However, once this pool of knowledge exists, there are many persons or groups with capability to exploit the information, and thus the real critical path to the innovation is more likely the knowledge pool than any particular entrepreneur.* The entrepreneurs listed in the study undoubtedly accelerated the introduction of the innovation, but they were at all times paced by the developmental level of the knowledge pool.

A RECENT TRACES STUDY

In the mid-1980s, the National Cancer Institute (NCI) initiated an assessment to determine whether there were certain research settings or support mechanisms that were more effective in bringing about important advances in cancer research. The approach taken was analogous in concept to the initial TRACES study, with the addition of citation analyses to provide an independent measure of the impact of the Trace papers (papers associated with each key "event") and by adding control sets of papers.

Thirteen important "advances" (key "events") in cancer research were defined by a senior advisory panel of experts, and the key papers associated with these "advances" and in the historiographic research streams were identified. Both the support source and the institutional setting of the papers were analyzed. In addition to the Trace papers, three other sets of papers were developed to serve as comparison sets whose properties were contrasted with the Trace papers.

The study concluded that all the research settings, and all the support mechanisms (small and large grants, contracts, intramural NCI, and so forth) contributed significantly to the "advances," with no single mechanism or setting represented disproportionately. More specifically, NCI provided 37% of the acknowledged support for the Trace papers, there was a large amount of cooperative, multisponsor support for the Trace papers, and papers on the Traces, whatever the support mechanism, were extremely highly cited—eight times as frequently as expected [33].

While indirect impacts of research on the "advances" were not a goal of this study and were not evaluated, the additional methodology (mainly citation and cocitation analysis) used in performing the latest Traces incarnation could shed some light on indirect impacts. For example, one of the control sets of papers used in the study was termed Augmentation papers and consisted of closely related contemporaneous papers cited with the Traces papers and identified through cocitation techniques. Another of the control sets was called Science Base and consisted of papers cited by the Trace papers, representing the precursor knowledge upon which the selected major "advance" was dependent.

These two sets of papers provided some idea of the *direct impact* of other science fields on the cancer fields of interest ("advances"). If citation and cocitation analysis were done on the Augmentation papers and the Science Base papers, combined with word frequency and coword analyses of these paper sets [34], and the process repeated a few times, then many of the pathways through which indirect impacts on the "advances" occur could be identified, and the magnitude of the impacts perhaps quantified to some degree. The amount of data and analyses required would be large, but based on the results and conclusions of a recent network-based approach to evaluating indirect impact of research [30], the computational/analytical problem is of necessity large because of the potentially large number of pathways through which direct and indirect impacts of research can occur.

Accomplishments Books

BACKGROUND

Semiquantitative methods, such as Hindsight and TRACES, require substantial commitments of people, time, and dollars. Because of the large resource requirements, these types of studies are performed relatively infrequently. A more common vehicle used by research-sponsoring organizations to display the impacts of funded research on advancement of science, actual or potential impacts on advancement of allied science or technology, and potential impacts on the organization's mission is the accomplishments book.

This type of document tends to present descriptions of selected scientific accomplishments in sufficient detail for the reader to understand the science that was accomplished and have some idea of the potential importance of the research to mission, technology, and perhaps the commercial sector. The accomplishments books make no pretenses about being all-inclusive, nor do they usually include quantitative estimates of impact. The accomplishments are drawn from the different disciplines funded by the organization, and are meant to be portrayed as representative of the breadth of activity. A few of these books are described briefly; the books selected should be viewed as representative of the genre.

OFFICE OF NAVAL RESEARCH (ONR)

Every couple of years, the ONR produces a book of significant accomplishments, the most recent being in 1989 [35]. The accomplishments are categorized into four major areas, reflecting the ONR technical discipline management structure: physical sciences, environmental sciences, engineering sciences, and life sciences. Thirty-six accomplishments are described in the most recent incarnation, one per page, including topics such as observation of new quantum states in an optical fiber, spectroscopic studies of energetic materials, improved deep ocean mapping capability, and understanding initial stages of ship hull fouling. The reader of this document receives a synopsis of the many areas in which ONR is involved, and how these areas potentially can impact the Navy.

AIR FORCE OFFICE OF SCIENTIFIC RESEARCH (AFOSR)

The AFOSR accomplishments book is similar in structure and spirit to that of ONR. In a recent incarnation [36], the accomplishments were divided among the six technical disciplines that reflect the AFOSR management structure: aerospace sciences, chemical and atmospheric sciences, electronic and material sciences, life sciences, mathematical and information sciences, and physical and geophysical sciences. Twenty-five accomplishments were described, with more or less equal representation from each of the six disciplines. As in the ONR book, no quantification of impact was attempted.

DOE OFFICE OF HEALTH AND ENVIRONMENTAL RESEARCH (OHER)

A somewhat different type of accomplishments book was generated by the Department of Energy (DOE), Office of Energy Research, for one of its component organizations, the OHER [37, 38]. The approach taken was to describe the 40-year history of OHER and present selected accomplishments in different research areas from different points in time. This technique allowed impacts and benefits of the research to be tracked through time, and in some cases to be quantified as well.

Costs of these programs, or subprograms, were not provided, and it is therefore difficult to relate the benefits, where stated, to the costs. Some of the benefits, such as an improved knowledge base on which to set health regulatory standards, would be extremely difficult to quantify. In some cases, the report does attempt this quantification. For example, in discussing radiation standards, the report states:

> More stringent standards, which might have been necessary in the absence of knowledge gained through the research program, could have easily cost electric power consumers an additional $2 billion annually. [37]

Other examples of research accomplishments probably not amenable to quantification are presented throughout the report, such as development of a capability to predict the travel and dispersion of hazardous substances (space debris, nuclear weapons tests by-products) released into the atmosphere. No numbers are associated with this accomplishment.

There are examples of hardware, or products, that resulted from the research, and quantification is applied to some of these accomplishments. The flow cytometer and centrifugal fast analyzer were developed to help search for radiation effects on humans. These have evolved into commercial products, and the quantified benefit given in the report is "about 10,000 units are in worldwide use." In the second volume [38], benefits for the centrifugal fast analyzer are stated as "estimated savings of $30 to $90 million/year." The high-resolution gamma ray spectrometer was developed to distinguish between radioactive elements with emissions of similar energies. Today, it is broadly used to monitor the environment and in many research applications as well, and the quantified benefit in the first report is "based on the value of rapid analysis as compared with slower alternatives, the benefit to nuclear plant operation alone is estimated to be $20 million annually."

A detailed reading of this document uncovers the difficulties of trying to identify, assign, and quantify costs and benefits of basic research. As TRACES and other similar studies have shown, the chain of events leading to an innovation is long and broad. Many researchers over many years have been involved in the chain, and many funding agencies, some simultaneously with the same researchers, may have been involved. How should costs and benefits be allocated under such circumstances?

For example, in the second volume [38], the "original" funding for the centrifugal fast analyzer project was shared by the Atomic Energy Commission (AEC) and the National Institutes of Health, and later funding was provided by the National Aeronautics and Space Administration for a zero-G variant. How should credit for the benefits be shared among these three agencies? And what about all the fundamental research that led up to the invention of the analyzer? How should the benefits be allocated to the researchers and funding agencies that participated?

Again, in the second volume, in the section about iodine-131 therapy for hyperthyroidism, it is stated that the basic application of iodine-131 to toxic goiter diseases was developed between 1939 and 1941. The initial AEC involvement is reported in 1946 (when the AEC was formed) when iodine-131 from nuclear reactor fission products was shipped from Oak Ridge National Laboratories. The report states that "total estimated savings in treatment cost because of the use of iodine-131 could be as high as $280 million/year." How much of this amount should be credited to AEC research? All $280M? None (the initial innovation was completed before the AEC was formed)? Only the portion of the total benefits resulting from cheaper isotopes? These are difficult questions and are endemic to any study of basic research that tries to assign costs and benefits to particular innovations.

DOE HIGH ENERGY PHYSICS PROGRAM

Another historiographic-based approach to describing program accomplishments is that used by the DOE High Energy Physics Program [39]. The history and interrelatedness of the diverse elements of the program, followed by the wider applications of high energy physics, constitute this accomplishments book. One chapter is devoted to the impact of knowledge gained from high energy physics on the fields of astrophysics and cosmology. No quantification is attempted, since improved understanding of the universe does not lend itself to that type of analysis.

More practical benefits resulting from better understanding of high energy beams, as well as resulting from the devices, instruments, and technologies that were developed to perform high energy physics research, are presented at the end of the report. Here, the different applications are described (tumor treatment, medical diagnosis, ion implantation, materials research, x-ray lithography, radioisotope production, superconducting magnets, klystrons, and so forth), but no quantification is attempted.

DARPA TECHNICAL ACCOMPLISHMENTS

The Defense Advanced Research Projects Agency (DARPA) was established in 1958 in response to Sputnik. DARPA's initial primary focuses were (a) the "presidential issues" of space, (b) ballistic missile defense (Project DEFENDER) and nuclear test detection (VELA), and (c) avoiding future Sputniks as its broader overall charter. Over its lifetime, as its mission has been redefined and refocused, it has sponsored a wide variety of thrust areas, including the following major areas: (a) defense manufacturing, (b) nuclear test monitoring, (c) naval technologies, (d) materials and components, (e) sensors and surveillance, (f) command, control, and communications, (g) information processing, (h) ground systems and weapons, (i) air systems, (j) AGILE (counterinsurgency R&D), (k) high energy systems, (l) DEFENDER and space defense, (m) space systems.

Recently, the Institute for Defense Analysis (IDA) produced a massive three-volume set describing the accomplishments of DARPA [40]. Of the hundreds of projects and programs funded by DARPA over its then (1988) 30-year lifetime, 49 were selected and studied in detail. Two criteria were used by the IDA project team and the DARPA management collectively in selecting projects/programs to be studied: (a) the importance of the projects, judged on the basis of evidence in attestation and documentation, and (b) the expected availability of data. The focus of the 49 retrospectives documented was (a) What were the origins of each project or program? (b) What did DARPA itself do? and (c) What was the result, impact, and effect of the work DARPA supported?

The structure of the description of each accomplishment was (a) a brief overview of the history and accomplishment; (b) a detailed technical history of the project; and (c) observations on its success. At the end of each project description was a time evolution chart of the project. The actions/achievements of the different organizations involved in the project's evolution (preceding, paralleling, and succeeding DARPA's involvement) were shown as a function of time. The main DARPA involvement (DARPA project track) was highlighted, related DARPA actions or DARPA influence were shown, DARPA technology transfer was shown, and related actions by other groups was shown. At the end of each project write-up, the DARPA costs over the project life (where known) were identified and some estimate of the dollar benefits (where possible) was presented.

In general, the outcomes of DARPA projects have included development or initial demonstrations of new technology, demonstrations of new applications of known technology, development and demonstration of new concepts of experimentation or operation, or integration of diverse technologies into new system concepts for the first time. Often, more than one of these kinds of payoff could be achieved by the same project. Most of the projects supported were technology or systems development rather than basic research, but many were fed by basic as well as applied research. The qualities of DARPA-supported programs and projects that contributed to success can be summarized:

1. A need existed for what the output could do.
2. There was a strong commitment by individuals to a concept.
3. Bright and imaginative individuals were given the opportunity to pursue ideas with minimal bureaucratic encumbrance.
4. There was an ongoing stream of technical developments and evolution.
5. DARPA management gave strong, top-level management support.
6. There was explicit effort, taken early, to improve acceptance by the user community.

The degree of success and impact is more difficult to measure. In some cases, the results of projects or programs, usually expressed in hardware, were transferred fully to a user. Other transfers have been partial, limited, or indirect. Given the multifaceted nature of some projects, several of these characteristics apply to the same project. Finally, success in transerring the hardware or knowledge gained in DARPA programs often depends on timing and the relationship to other events and programs. The report provides an excellent example of the impact of exogenous events on the fate of SLCSAT, a project which has had some successful technology validation of satellite-submarine laser communication. Whether the Navy adopts the system for communication with submarines will depend on the Navy's concepts of submarine operation in the new tactical and strategic world that is emerging in the aftermath of the cold war and the budget available for such purposes in the new environment.

The impacts of the more fundamental DARPA areas of support, such as materials sciences and information processing, are more difficult to measure than impacts of the development-oriented projects, where transition to a defined user is somewhat clearer. The report defines DARPA's impact in these technology base areas as having stimulated an infrastructure and new disciplines. It identifies programs established at universities, interdisciplinary efforts initiated, projects in fundamental technologies accelerated by DARPA funding, and hardware/software products that resulted.

Similar to the other semiquantitative approaches described above, the IDA report does not (in the author's opinion) account sufficiently for benefits resulting from indirect impacts of research. In the time evolution charts at the end of each project write-up, a few critical events/technologies that preceded the DARPA involvement are shown, and then the DARPA contribution is highlighted. The existing pool of scientific and technological knowledge, which DARPA exploited very productively, was developed over many years by many diverse organizations and was a necessary condition for DARPA to achieve its successes and impacts. The people and organizations who developed this base of technology complemented the DARPA effort, and should be allocated a share of the benefits.

One of the major impacts of DARPA support, which could be quantified to some degree by relating costs to benefits, is that *projects were brought to fruition earlier than they would have been without DARPA support.* Areas such as gallium arsenide semiconductors, computer architectures (RISC, systolic array, symbolic processing, parallel processing, neural networks), the ADA language, to name only a very few, were accelerated greatly because of DARPA's involvement and support. Future DARPA accomplishments reports could relate the DARPA program (or specific project) expenditures (in a discounted sense) to the earlier realization of benefits (in a discounted sense) due to DARPA support to provide additional measures of the effectiveness of DARPA's funding.

Summary and Conclusions

A variety of approaches were presented which showed different types of impacts of research, but little or no quantification of impact was performed. Hindsight, TRACES, and, to some degree, the DOE and DARPA accomplishments books had some similar themes. All these methods used a historiographc approach, looked for significant research or development events in the metamorphosis of research programs in their evolution to products, and attempted to convince the reader that:

1. The significant research and exploratory development events in the development of the product or process were the ones identified.
2. Typically, the organization sponsoring the study was responsible for some of the (critical) significant events.
3. The final product or process to which these events contributed was important.
4. While the costs of the R&D were not quantified, and the benefits (typically) were not quantified, the R&D was worth the cost.

As the historiographic analyses (Hindsight/TRACES) of a technology or system have shown, if the time interval in which the antecedent critical events occur is arbitrarily truncated, as in the two-decade time interval Hindsight case, the impacts of basic research on the technology or system will not be given adequate recognition. As Hindsight and the different TRACES studies have shown, the number of mission-oriented research events peaks about a decade before the technology innovation. However, these studies have also shown that the number of non-mission-oriented research events peaks about three decades before the technology innovation, and eight or nine decades, or more, may be necessary in some cases to recognize the original critical antecedent events. Over a long time interval, the majority of key R&D events tend to be non-mission-oriented. Thus, future studies of this type should allow time intervals of many decades to insure that critical non-mission-oriented research events are captured.

Even in those cases when an adequate time interval was used, and critical non-mission-oriented events were identified, the cumulative indirect impacts of basic research were not accounted for by any of the retrospective approaches published or in use today. A recent study [30] that examined impacts of research on other research and technology through direct and indirect paths using a network approach showed that the indirect impacts of fundamental research can be very large in a cumulative sense. Future retrospective studies should devote more effort to identifying indirect impacts of research to enhance their credibility. While indirect impacts of research are much more difficult to identify than direct impacts, and the data-gathering effort is much larger and more complex, neglect of indirect impacts skews the results and conclusions relative to the value of basic research significantly. Use of some of the advanced computer-based technologies available today, such as citation analysis [33], could identify and document many of the pathways of the indirect impacts of research.

While some of the studies concluded that the technical entrepreneur was extremely important to the innovative process, it does not appear (to the author) to be the critical path factor. Examination of the TRACES historiographic tracings that display the significant events chronologically for each of the innovations, as well as the DARPA and OHER case studies and those of the other accomplishments books, showed that an advanced pool of knowledge must be developed in many fields before synthesis leading to an innovation can occur. The entrepreneur can be viewed as an individual or group with the ability to assimilate this diverse information and exploit it for further development. However, once this pool of knowledge exists, there are many persons or groups with capability to exploit the information, and thus the real critical path to the innovation is more likely the knowledge pool than any particular entrepreneur. The entrepreneurs listed in the studies undoubtedly accelerated the introduction of the innovation, but they were at all times paced by the developmental level of the knowledge pool.

A detailed reading of some of the studies that attempted to incorporate economic quantification showed the difficulties of trying to identify, assign, and quantify costs and benefits of basic research, especially at a micro level. As TRACES and other similar studies have shown, the chain of events leading to an innovation is long and broad. Many researchers over many years have been involved in the chain, and many funding agencies, some simultaneously with the same researchers, may have been involved. The allocation of costs and benefits under such circumstances is a very difficult and highly arbitrary process. The allocation problem is reduced, but not eliminated, when the analysis is applied at the macro level (integrating across individual researchers, organizations, and so forth).

While these approaches do provide interesting information and insight into the transition process from research to development to products, processes, or systems, the arbitrary selectivity and anecdotal nature of many of the results render any conclusions as to cost-effectiveness or generalizability suspect. Supplementary analyses using other approaches are required for further justification of the value of the R&D.

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