RESEARCH MANAGEMENT REVIEW

The Journal of the
National Council of University Research Administrators

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Volume 6 Number 2 Fall 1992
Editor’s Preface

With the change in administration in Washington, we see what appears to be new directions in funding for research and educational programs at colleges and universities. Programs with titles such as; Defense Conversion, The Advanced Technology Program, Strategic Research Initiatives, High Performance Computing and Communications, and the Student Public Service Program, to name a few, have been promoted as the solution to the economic and social problems facing the nation. With the limitations on Federal agency budgets set by Congress and the unknown cost associated with the yet to be announced health care initiative, it is difficult to know what funding will be available for these new programs. We all have observed that the reduced levels of funding coupled with increased earmarking and charges of mismanagement of research funds at colleges and universities has led to calls for more accountability and tightened rules and regulations on the administration of awards.

The first article is the Keynote Address presented at the 34th annual meeting of NCURA in which Jay Moskowitz, the Associate Director for Science Policy and Legislation, recapped the planning effort that had been initiated by Bernadine Healy as Director of the National Institutes of Health. The Institutes were feeling the constraints of leaner budgets and Healy had set about overseeing the development of a long range plan for the future of the NIH. A key element in the plan was the proposed cost management efforts, particularly those directed to control of indirect costs. The NIH concerns, as well as those of some Congressmen, about indirect costs played an important role in the revisions to A-21 that recently were released. Unfortunately, we may not yet have seen the last of the indirect cost issue as Congress looks to possible ways to fund agency programs while staying within the confines of the budget agreement. Limiting reimbursement of indirect costs or further caps on elements of the rate were possible scenarios considered when the NIH was developing its cost management plan.

The second article is based on a presentation made at the Southeastern Regional Meeting by Howard Gobstein that attempts to provide us with some insight into what might be the elements of the Clinton administration’s science policy. What is interesting in this article is that many of the supposedly new programs really appear to be extensions of initiatives undertaken by previous administrations. Additionally, the author alerts us to the increased concerns about conflict-of-interest regulations that are likely to be forthcoming even though the science policy appears to be emphasizing closer ties between research performers and industry to
enhance the competitiveness of the US commercial sector.

The third article is a comprehensive review of the various methods that Federal agencies have used or can be applied to evaluate the impact of research. The content of the article will be useful to all research administrators if the Federal agencies seriously endeavor to follow the suggestions made in the report of Congressman Brown’s House Committee on Science, Space and Technology and the FCCSET and PCAST reports. Additionally, the description of various methodologies available to perform an impact assessment should prove useful for many institutions struggling with evaluation of programs and departments.

Finally, our last article attempts to summarize how the role of the research administrator has been changing because of the increasing complexity of regulations surrounding the administration of research awards that colleges and universities have come to expect as a source of revenue in this era of shrinking budgets. With the uncertainty of future funding levels and the ever increasing regulatory requirements, the observations contained in this article are surely to be repeated in the years ahead.
Keynote Address at the NCURA 34th Annual Meeting

JAY MOSKOWITZ

Editor’s Note: This paper was presented as the Keynote address at the 34th ACURA Annual Meeting, Washington, D.C., November 9, 1992.

INTRODUCTION

Thank you for that warm introduction. I am delighted that we could share a few moments together.

As a member of the NIH Senior Staff and an NIH career employee for over 23 years, I have been able to witness the genesis and the fulfillment of biomedical research. When I see a child afflicted with cystic fibrosis have a birthday yet one more year and those devastated by rare diseases being treated by a new drug in the armamentarium against their egregious symptoms, I am reminded that our research enterprise ushers in a new dawn, paving the way for tremendous, almost unimaginable, opportunities to improve the health status of our citizens and people around the world.

Following the words of Erasmus, at NIH and at your universities and laboratories, we are “building up a library which has no other limits than the world itself.” This library of knowledge about human kind and its interaction with nature has been sought since the beginnings of recorded history. Only now are we finally uncovering the secrets of the human body and answering the mysteries that have perplexed scholars throughout the centuries.

This information is vital for the present, and it is our legacy to the future. In fact, my colleague Walter Massey, director of the National Science Foundation, was recently quoted as saying that the United States will go down in history as the scientific Medici. He said that “Other cultures have contributed painting or wonderful classical music to mankind. The United States will have contributed an understanding of the world around us.”

At NIH we are well aware of the responsibility this entails. Our institutes, centers and divisions, in conjunction with more than 1800 grantee institutions, are participating in a biomedical research enterprise that will help Americans address some very pressing health care problems-diseases such as cancer, AIDS, Alzheimer’s disease, sickle-cell anemia, heart disease, rare diseases such as Battan’s Disease, and many, many others.

Jay Moskowitz is Associate Director for Science Policy and Legislation at the National Institutes of Health.
The approximately 29,391 research grants, center awards and various career program awards that we were able to fund in FY 1992 equalled an unprecedented health care braintrust devoted to making the future healthier and safer than the past. The National Council of University Research Administrators, and your individual universities and laboratories, continue to be full partners in our work. However, some skeptics hypothesized that our relationship has been strained in recent years, especially as the country learned of “inappropriate” spending of research dollars through indirect costs, and as we as an enterprise have witnessed increasing competition for ever scarcer research funds. That number of awards will drop to 28,711 in 1993. I am here to tell you that NIH recognizes your concerns and the indispensable need for a continued, strong, vibrant, and supportive partnership between our campus and your institutions. To quote the Director of NIH, Dr. Bernadine Healy, “The Nation is NIH’s campus.” As we address the issues that confront us, we must do so with an unshakable determination to remain united in our efforts to benefit every man, woman and child in this country.

THESIS

Biomedical and biobehavioral research is our road to a healthy and economically sound future. Our society must not abandon science and medical research in difficult times, but rather embrace it more closely, and in fact, celebrate it. The biomedical research enterprise must become, and remain a top priority for both the public and private sectors. We must directly address the health concerns of our citizens and demonstrate that their investment in research is justified. We must be held to high standards of performance and to strict standards of accountability. We must continue to earn the public’s trust.

As partners, we have much to share in this enterprise. I would like to brief you on just a few of the exciting activities underway at NIH:

* First, I will discuss the NIH Strategic Plan, which I believe will help us utilize more productively our resources at NIH now and in the future.  
* Second, I will mention our Cost Management Plan and our efforts to control both direct and indirect costs.  
* Third, I will discuss briefly some of the ethical and social issues that require NIH to assess the non-scientific impact of our research.
Then I will discuss the recent reorganization of the Alcohol, Drug Abuse and Mental Health Administration (ADAMHA), our new Women’s Health Initiative, and Minority Health Issues.

Finally, I will suggest some ways that you could help us.

THE STRATEGIC PLAN AND BIOMEDICAL RESEARCH AT NIH

Under Dr. Healy’s innovative and dynamic leadership, we have initiated a strategic planning process that takes us well beyond the yearly political cycle of budget proposals. We have found that we must do more to ensure the strength and vigor of our complex biomedical research enterprise. We want—we need—to achieve predictability and stability in our programs, as well as to capitalize on the extraordinary opportunities available in the sciences and medicine at this point in our history. So we have undertaken a bold and creative planning process to outline our goals and to direct our resources to help fulfill those goals.

Of course, planning and priority setting are not new concepts at NIH. Key strategic decisions formulated by Vannevar Bush and the post-war Office of Scientific Research and Development in the 1950s have shaped our present research capability and are in part responsible for NIH’s achievements. We want to build on this past success.

In our strategic planning process, two important principles have guided our efforts: first, that there will be no finality to the strategic planning—it must be an ongoing process, in which, to take some poetic license, we will generate living and breathing documents; and second, that our plan should not be viewed as a rigid blueprint—it must serve as a compass to guide our discovery.

With these two principles in mind, we have identified six trans-NIH objectives which have been much discussed by the extramural community, especially in five meetings held around the country. The first objective is to ensure that critical science and technologies in the most fundamental areas of biology are advanced as priorities across NIH. These areas are:

- molecular medicine.
- structural biology.
- immunology and vaccine development.
- biotechnology and bioengineering.
- integrative and cellular biology.

Investment in critical areas of science and technology primarily through investigator-initiated research will set the stage for improving health,
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reducing health care costs, and bolstering the nation’s economic well-being. This objective relates in part to investing in the patients of tomorrow.

The second objective is to strengthen the capacity of our nation’s biomedical and behavioral research enterprise to respond to current and emerging trans-NIH critical health needs in:

- nutrition.
- prevention.
- behavior.
- health of women, minorities, and the underserved.
- environment.

The individual institutes and centers, with their focus on specific diseases and human health, are central to and the agents for accomplishing this objective that relates to the patients of today.

The third objective is to provide for the renewal and growth of the intellectual capital base essential to biomedical research. A critical need exists for research training and career development. We can only be as creative and successful as the scientists who make up our enterprise. Ensuring fairness and equity of opportunity at NIH is central to efforts to enhance the human resources base of biomedical research.

Our fourth objective is to sustain and renew the capacity that is necessary to advance the Nation’s ability to conduct research that promotes critical science and technology and addresses the Nation’s health needs. This requires physical and methodological infrastructures that include instrumentation, research resources, support services, laboratory facilities and enabling technologies.

The fifth objective is to secure the maximal return on the public investment in our biomedical research enterprise. Stewardship of public resources requires that we provide efficient and responsible managers, quality management systems, and integrity and fairness in the conduct of our business.

Our sixth objective is to work to continually earn the public’s respect, trust and confidence as we carry out our mission. This is vital in a time when science itself has been repeatedly called into question, especially by those who lack science literacy and those who reject scientific research and its tools. Although NIH already ranks among the top three most respected governmental agencies, this respect cannot be taken for granted. We must hold ourselves to the highest standards.

The development of this plan is the result of extensive input from you, your faculty, and leaders in biomedical research. Each institute, center, and division of NIH will be responsible for implementing specific initiatives, all of which will be pertinent to their individual research missions.
We will continue to reaffirm the high-quality, investigator-initiated research which has been a hallmark of NIH. And, working with the advisory councils and program advisory groups, we are also committed to maintaining the balance and diversity of our research portfolio. At the same time, we will emphasize certain areas of great opportunity, such as molecular medicine, which Science magazine recently called part of “Biotech’s Second Generation.” And we will continue to emphasize research on health of women, minorities and the underserved to ensure that our present public health needs are being met.

THE COST MANAGEMENT PLAN AND EFFORTS TO CONTROL INDIRECT COSTS

I mentioned just a moment ago that we are concerned about costs, and that we have a plan to help maximize our research investment. We cannot afford to be careless about the impact of our program administration on the costs of research. For instance, a one percent increase in administrative costs can cost approximately $20 million-money that would otherwise fund new research projects, young investigators, or important new clinical trials.

Careful and prudent efforts to control rising costs are vital to the continued public support of biomedical research. The public, through their elected representatives and the executive branch, through many channels, have demanded that public money be used wisely and for its intended purposes. If we cannot control rising costs and guarantee that public funds are managed properly, then we risk losing the public support for biomedical research, the hard-won integrity of our research institutions, and the financial support that is the life-blood of our research.

I believe that you and I want the same results. We want a system that is fair and not arbitrary. We want to create real and effective incentives to cut research overhead. We want a simpler system that is more uniform across a broad range of grantee institutions. And we want to clarify what types of research expenses should be treated as direct or indirect costs.

The Cost Management Plan is our effort to further establish the wisest use of financial resources to effectively manage costs and fund the optimal number of grants. We believe that we share three goals in common.

- First, NIH must support only the best, peer-reviewed, biomedical research to sustain progress in combating disease and improving human health.
- Second, NIH must make use of limited Federal resources through cost management and containment to support, in a balanced manner, the
full range of funding mechanisms, such as investigator-initiated projects, research and resource centers, and research training.

- Third, there must be stability and predictability in the aggregate funding level for research in order to maintain the momentum in the advancement of biomedical knowledge generated by past and current investments in biomedical research.

Using these goals as a framework, NIH has developed a financial management plan to ensure the future viability of the biomedical research enterprise within the available appropriated funds. Therefore, we are doing the following:

- First, the average length of research project grants has been adjusted to four years. This preserves the important gains in average length of awards during the last decade (from 3.3 to over four years) and still ensures that a sufficient number of competing awards are made each year. If, however, on the basis of further analysis and experience, NIH and the community decides to increase the average duration above four years, we will update this financial management plan to indicate how and if this can be achieved within overall budget constraints.

- Second, we recognize the award of non-competing grants at committed levels is the cornerstone of our plan. To preserve this stability, the cohort of non-competing grants, on the average, will not exceed four percent over the prior budget period, taking into account one-time non-recurring costs.

- Third, we implemented cost-management measures to ensure that the average cost of the competing research grant project portfolio, on an Institute-by-Institute basis, increases at a rate no greater than the Biomedical Research and Development Price Index.

Of course, we continue to look for additional ways to be cost effective and cost efficient, and we must continually evaluate our current efforts to make sure that they work and do not undermine the biomedical research enterprise. This will require ongoing cooperation and communication between NIH, you, and the rest of the research community.

In the Health and Human Services (HHS) Report on Indirect Cost, just sent by Secretary Sullivan to the Director of the Office of Management and Budget (OMB) some of the following suggestions have been made:

- Develop a Federal data base of indirect cost information to include such data as rates, indirect cost components (proposed and
negotiated), research square-footage costs, and research full-time equivalents and salaries.

- Establish a standardized direct cost base upon which indirect costs are calculated.
- Specify standard classifications of costs between direct and indirect cost categories.
- Eliminate, or at least minimize, the use of special studies or strengthen the methodology for conducting such studies.
- Establish multi-year predetermined rates as the predominant methodology as a matter of policy in OMB cost principles.
- Establish threshold rates within the current system for certain types of costs.
- Revise the eligibility criteria to permit organizations with direct costs of $10 million or less under Federal grants and contracts to use the short-form method permitted in the current cost principles.

ETHICAL ISSUES

The implications of our partnership and joint research efforts extend far beyond the costs of research and the data we generate. The ethical, social, and legal aspects must also be considered and weighed from the beginning-by us-as well as by the relevant disciplines and by the public.

The human genome project is literally forcing you and me-and people all over the world-to understand and evaluate the social implications of this new generation of research. The advantages of this research are obvious-the mapping of these genes will help us understand inherited disorders and, eventually, how the environment influences these disorders. Such information could lead to new strategies for the prevention and treatment of more than 4,000 diseases of known genetic origin and to a much better understanding of other diseases that have genetic and multifactoral components, including infectious diseases, cancer, depression, and hypertension.

But the human genome project is doing more than changing the course of medicine. It may change our social behavior. In fact, the ethical and legal implications may be just as profound as the medical possibilities. We face new and important responsibilities with this knowledge. As Daniel Kevles and Leroy Hood have noted in their recent book, The Code of Codes: "the quest for the biological grail will, sooner or later, achieve its end, and... it is not too early to begin thinking about how to control the power so as to diminish-better yet, abolish-the legitimate social and scientific fears."

Privacy is one such issue. Already there is an unprecedented amount of genetic screening and monitoring underway in the workplace.
Corporations are asking detailed questions about family background and are making determinations about the likelihood of high health care costs, reduced attendance and lower productivity.

Genetic information is a powerful indicator, but also a very private, personal matter. It may be of interest to others for a variety of reasons—not all of them good. So we believe firmly that individuals must have control over the acquisition and disclosure of their own personal genetic information. Because genetic analyses are not foolproof predictors, not crystal balls, and they simply provide probabilities—we believe that it is the right of individuals and their families to determine the relative value of this information and control its access by others. Genetic information has the potential to be used to stereotype or categorize and expose them to loss of access to social and economic opportunities. We must be concerned about possible discrimination on the basis of genotype.

THE NIH RESPONSE

The NIH Human Genome Program sets aside five percent of its funding to identify and explore the ethical, social and legal implications of its research and to pursue methods of addressing these issues. The program established an Insurance Task Force—which is a group representing the insurance industry, consumer groups, geneticists, health policy analysts, and insurance policy makers—to establish industry guidelines and public policy options regarding use of genetic information.

Under Dr. Healy’s leadership, we are seriously committed to finding the proper use of genetic information. We have established a program for Science Policy Studies within the Office of the Director to examine these difficult issues more closely. The programs may attempt to anticipate some of these issues and concerns, functioning as an “early warning system.” Surely it could identify fields of biomedical research that may warrant accelerated development because of scientific opportunities or health emergencies. If there are areas that you have identified as emerging within this context, please let me know. We will be developing a formal agenda for this program soon.

MERGER WITH ADAMHA

As many of you know, the research components of ADAMHA (Alcohol, Drug Abuse and Mental Health Administration) have been transferred to the National Institutes of Health and the service components have been consolidated into a new agency, the Substance Abuse and Mental Health Services Administration (SAMHSA). The reorganization of ADAMHA provides the opportunity for both researchers and service
providers to thrive in their separate, though closely related, professional cultures. The three research institutes, and their exacting, sophisticated and important work, will now be even closer to the parallel work in fundamental biology, molecular medicine and neurosciences. There will be new and unprecedented opportunities for collaboration. By joining NIH, these research institutes will attract more earnest and demanding public attention.

But I want you to know that we are aware of the need for strong linkages between the addiction and mental disorder institutes and the service components in SAMHSA. We will forge and maintain those linkages, greatly assisting in the development of needed mechanisms for meaningful technology transfer. All partners-both SAMHSA and the research institutes at NIH-are committed to building upon the existing relationships between research and services. Through collaborative agreements, interagency contracts, and memoranda of understanding, we will strive to ensure an enduring and mutually productive relationship that benefits the research community and our citizens in need of improved care, and those at risk for addictive and mental disorders.

WOMEN’S HEALTH INITIATIVE

We are also very excited about our Women’s Health Initiative. There are serious and dangerous informational gaps in what we know about women’s health. So we have just begun a 14-year, $629 million project to generate the data we need in such areas as the impact of low-fat diets, supplements of calcium and vitamin D, and hormone replacement therapy on heart disease, cancer, and osteoporosis in women 45 years of age and older. We will also use community-based studies to look at ways to help women adopt more healthful behavior. We need to promote good eating habits, regular exercise, regular breast screening examinations, and smoking cessation. In addition, we will be utilizing the largest observational study of women ever done-involving 160,000 women-to determine the predictors and markers of disease.

This initiative will reach out to women of diverse racial, ethnic and socioeconomic backgrounds. Studies in the past did not focus on the needs of older women, and did not adequately include women from minority or underserved populations.

Last month we announced a $140 million, 15-year contract to the Fred Hutchinson Cancer Research Center in Seattle, which will function as the clinical coordinating center for the initiative. There will be a total of 45 clinical centers participating in the Women’s Health Initiative. The first 15 of the 45 clinical centers will be named early in 1993, while the remaining 30 will probably be named early in 1994.
NIH PROGRAMS FOR MINORITY SCIENTISTS

The NIH is committed to increasing the numbers of minority scientists represented in biomedical research careers. The NIH Office of Minority Programs was established for the purpose of supporting research to improve minority health. In March of this year, Dr. Healy announced plans to the Congress for a NIH Minority Health Initiative, a $45 million effort aimed at improving health in minority communities and attracting minorities into careers in medicine and research. Dr. John Ruffin, Director of the Office of Minority Programs, leads this effort.

The Minority Health Initiative will channel much needed support to research projects targeting those diseases and conditions which disproportionately afflict minorities. These disorders include AIDS, noninsulin-dependent diabetes mellitus, lupus, heart disease and stroke. Another key component of this initiative is to attract minorities into biomedical research careers. The Office of Minority Program’s long-term strategy calls for support of a variety of activities that focus on increasing the numbers of minority biomedical scientists. The office is moving quickly to establish several new projects in the area of minority health training. Recent efforts have focused on creating pilot or “start-up” training projects at all levels of the educational pipeline-from pre-college to post graduate levels.

HELP FROM THE COUNCIL

So far I have talked about what we are doing at NIH. But I have also stressed the need for partnership. First, we simply must have your full cooperation and partnership in efforts to ensure the public of the integrity that exists in the administration of the public funds. You and I are the stewards of the public’s trust. We must restore an environment of excellence in research administration, free from conflicts of interest, scientific misconduct, and misuse of Federal funds.

Second, we need your help-individually and through the Council-to maintain and enhance the priority placed on biomedical research. These are tight fiscal times. We have now entered a crucial era in terms of Federal fiscal policy toward support of biomedical research. Many years ago with champions like Claude Pepper, John Fogarty and recently Lowell Weicker, biomedical research was called the favored child of Congress. Yet, as one commentator reminded us, “NIH is not the only game in town.” It was unfortunate to note that in this and past political campaigns, while health care has received major attention, biomedical research was rarely mentioned as part of the solution to reducing national
health care costs despite the economic benefits that it can provide. We must reverse this erroneous image and restore biomedical research as a priority to the American people.

Third, as new discoveries rapidly change the medical landscape, I hope that the National Council of University Research Administrators will work closely with NIH to address ethical and legal issues, and to quickly translate this research into improvements that will help our citizens. Your credible leadership will be an invaluable asset as we seek to inform and involve your universities and laboratories.

Finally, I hope that we can retain and stimulate the interaction between the NCURA and NIH. Your viewpoint is essential for us to remain on the cutting edge. The Strategic Planning process that included 2000 members of the biomedical community is proof-positive of this. Let’s increase this traffic between you and the NIH campus.

CONCLUSION

In this presentation I have spoken of the promise of the future and how we hope to use the resources at NIH to discover information to enhance the practice of medicine and to improve the health status of our people.

Frankly, our joint research effort symbolizes a profound respect and recognition of the value of good health and long life. As Galileo observed so long ago, “Science can only grow.” Each scholastic contribution, each achievement in the laboratory, adds more knowledge to the edifice of medical science.

But we must work to maintain and expand our research partnership in biomedicine. And this partnership must include efforts to educate each other and the public about the need for this research and its proper applications. As Vannevar Bush once reminded a war-weary nation, “Science, by itself, provides no panacea for individual, social, and economic ills. It can be effective in the national welfare only as a member of a team.” He went on to say, “Without scientific progress no amount of achievement in other directions can insure our health, prosperity, and security as a nation in the modern world.” As we walk together into a new century, filled with wonder and promise, biomedical research will help to shape and define our world. We will, together, be on the vanguard of tomorrow.
Science Policy In the New Administration

HOWARD J. GOBSTEIN

Editor’s Note: This paper is adapted from a presentation given at the NCURA Southeastern Regional Meeting in Nashville, Tennessee, May 13, 1993.

Abstract. It is premature to describe the science policies of the Clinton Administration. The policies are still developing in this Administration’s first year as the Executive branch and the Congress begin negotiations on the President’s legislative proposals. Actual policies can be identified only after the Administration acts in promoting these policies. Clues for the eventual science policy are assessed: vignettes suggesting the evolution of these policies in previous Administrations, past and present Congressional interests in this area, the present policy-making context facing the government and, finally, aspects of the nascent science policies of the Clinton Administration are described.

SEEKING THE CLINTON ADMINISTRATION’S SCIENCE POLICY

It is much too early to define the Administration’s science policies. They are incomplete. Thus far, we have initial, somewhat idealistic descriptions of policies without a sense of the real priorities that result from deliberations with Congress to see what the Administration will support strongly and where it will yield.

Recall the sizable increases proposed annually for the National Science Foundation by the previous Administration in keeping with its stated policy to double the budget of the National Science Foundation in five years, by 1993. However, as financial constraints prompted Congress to slice substantially at NSF proposed increases, NSF budgets were supported “...with only a fraction of the vigor of Administration efforts to forestall congressional cuts in the superconducting supercollider and space station."

The tradeoffs by the Clinton Administration have just begun. And there will be plenty of opportunity for the President to exhibit his trademark flexibility and political smarts to retailor his thinking to suit the situation and the audience.

Howard J. Gobstein is the Vice President of the Association of American Universities (AAU)
A Washington analyst, who keeps a very healthy skepticism about these things, describes the situation as of May, 1993, thus:

Not sure what to make of President Clinton’s first 100 days, in spite of all the media analyses? Good, that shows you’re a normal, rational person. The fact is, the political situation in the capital is so confused right now that nobody can fathom it. And anyway, the first 100 days may not matter all that much. As you wait to see what the next 100 days bring, it may be useful to keep the following things in mind:

1. The media are right about at least one thing: Clinton really does have a focus problem; however, he may have begun to realize this.

2. The American people haven’t given up on the new Administration, but they’re basically in a very grumpy, anxious mood and nobody knows when their patience will run out.

3. Everybody in Washington, D.C., is acutely conscious of this and extremely nervous about it.

4. Members of Congress (both parties) are especially nervous because they haven’t yet developed a good, collective sense of how to operate in this new political climate, and they’re going to behave erratically until they figure that out (don’t worry, they’ll figure it out sooner or later; they always do).

5. It’s fruitless to try to predict the course of legislative events in a situation like this.

6. Such situations are not necessarily conducive to sound policymaking.

7. For better or worse, this is how representative democracies work.

If it is premature to be definitive about this Administration’s real policies, as reflected by its actions, at least we can discern clues. For our present purposes, we will concentrate on the so-called generic science and research and development and touch only briefly on space, defense, health and energy policies. Also, we shall spare the details of the agency budgets as we are dealing with broad policy here.

As a basis for our analysis, let’s keep in mind some general characteristics of the Clinton Administration:

- believes in the positive force of an activist government as a means of solving problems and enhancing the quality of life for the nation’s citizenry;
- is incredibly eager and hard working and they want to tackle all problems at once;
- believes in partnerships, linkages, collaborations among business, governments, national laboratories and universities;
- recognizes the importance of blending policy with politics to accomplish things, but are just learning how to put this into practice;
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* includes technological junkies who understand what they are talking about, truly believe in the benefits of new technology and the stimulating role of government.

(In what other Administration would a President refer to his Vice President as understanding “the gestalt of the gigabit” and have the VP espousing the rationale for government involvement [in developing the high speed national information network] as: “no private player has the wherewithal to finance the research and development into the new generation of switches, software and algorithms that are necessary to produce the very high-capacity network that we’re talking about. You would see the emergence of interim steps, but you would not see the country, as a whole, hurdle the entry barrier to a broadband network without government leadership.” And, that was not written by a great speech writer, that was part of an interview!)

As guides in our search for the Clinton science policies, we shall assess several sets of clues:

1. What science policies have preceded this Administration?
2. What is the context for present policymaking?
3. What are the science policy goals of members of Congress?
4. What science policies might we find in this Administration?

What has preceded the Clinton Administration’s Science Policies?

A few vignettes:

1. It is the beginning of a Presidential term of office, the economy is somewhat sluggish, there are a good number of scientists and engineers out of work, and international economic competition is becoming a concern seemingly due, in no small part, to lagging technological innovation by domestic corporations. There is rising interest in protecting the environment and a feeling that the government might harness science and technology more effectively to contribute to resolving some of the nation’s problems. Major projects such as the New Technology Opportunities Program (proposing significant government funding for civilian technology development), the Experimental Technology Incentives Program and the Experimental R&D Incentives Program (both “ETIP” and “ERDIP” attempted to determine how the government could stimulate increased industrial innovation) were conceived by the Executive Office of the President for the National Science Foundation, Department of Commerce, and what, at that time, was called the National Bureau of Standards. Few people, apart from science policy junkies, would recognize this as the foray
Science Policy in the New Administration

of a republican administration, predating the RANN program, 20 years ago, at the beginning of Nixon’s second Administration. Although no significant federally funded projects ever began (except for many reports and studies) these proposals represented the nascent steps toward contemporary science policies for economic development.

2. Economic growth is, again, very slow, productivity is low, there is great concern over the seeming lack of ability of U.S. industries to compete internationally, particularly basic industries, such as, automobiles and steel. Interest grows in a new partnership between government and industry to address these issues. The Administration responds and a new initiative is launched, focussed on the Department of Commerce and the Office of the President’s Science Advisor. There was a domestic policy review of Industrial Innovation involving most major federal agencies and resulting in, at least, a foot more of bookshelf coverage. Part of the outcomes were the establishment of a new Center for the Utilization of Federal Technology, and, with the assistance of Congress, four new “generic technology” centers were to be established at a university or other non-federal site, funded jointly by government and industry. State and regional corporations for Industrial Development were established to assist small businesses, especially start-ups bringing innovations to market. A strong effort was made to increase basic research at universities as being the underpinning of technological innovation, particularly the basic research in “strategic” areas. This action reversed the stagnation in university research funding from the mid 60s to the mid 70s. In a legislative triumph that has just begun to cause reverberations in the early 90s, the Bayh-Dole Act was passed which enabled universities to take title to patents for discoveries resulting from federally-funded research. With the ownership of patents, universities were able to grant exclusive licenses, initially to small businesses, and eventually to all corporations, to promote the transfer of federally-funded research. Finally, there was an attempt to establish a joint government-industry-university Cooperative Automobile Research Center. These programs were all part of President Carter’s policies.

As these brief descriptions suggest, government stimulation of civilian research for commercial use was toyed with, and began to be tested in the 70s and 80s. During the next 12 years of Presidents Reagan and Bush, there was something of a hiatus in the incrementally advancing notion of the role of government in promoting civilian-applied science and working with industry to further the economy. However, even though laissez-faire was the pronounced rule, and indeed, was a major emphasis of economic policies, funding for basic research continued to grow. Of course, there were some jittery times, like when in the early 80s social science research
and science education funding at the NSF were almost entirely wiped out, but they survived, after strong action by the academic community.

A continuation of the rationale for federal support for basic research, developed fully under Carter, recognized that the private sector would not adequately support basic research because the returns were not quickly known or recovered, nor were they sufficiently appropriated by an individual industrial funder. However, it was commonly recognized that the overall return to the nation from this research was significant. Thus, the NSF and the NIH grew rapidly during these years: in 1982, HHS obligated $2111 million for R&D to universities and colleges (primarily through NIH), by 1990, this increased to $4775 million. Similarly, NSF increased from $690 million to $1305 million during this same period. (All figures in current dollars.) Additional growth was sparked for NSF by a legislated objective agreed to by the White House, to double the support for the NSF with the five years beginning in 1988. Furthermore, in the late 8Os, the Administration demonstrated consistently strong resolve in initiating and supporting growth in funding for the construction of the SSC. Some would suggest this exhibited a sense of pride in U.S. leadership in high energy physics—others saw a connection to construction jobs going to Texas.

Slowly, by the second Reagan Administration, attention began expanding from basic research. After much debate, federal financial support was given to a consortium of companies for collaborative research in microelectronics through the Defense Department, thus forming Sematech. Although DOD-supported research (mostly through spinoffs from ARPA) long had been the “back door” method for government to support commercial technology development, the formation of Sematech moved the government even closer to admitting a role to directly fund industry development for significant commercial use. Antitrust policies were relaxed to promote industrial research consortia, although the federal government shied away from direct financial support in virtually all other cases. Thus, there was creeping technology policy, not industrial policy—for gosh sakes, not using those dreaded words.

The late 8Os also saw the advent of vastly increased multi-agency research programs, under the auspices of the newly invigorated FCCSET, with initiatives, such as the one dealing with high-speed computers, communications and materials. Finally, in the closing years of the Bush Administration, with much Congressional activity egged on by doses of reports by groups such as the Council on Competitiveness, the Department of Commerce finally began having a role, with the funding of the Advanced Technology Program (ATP), in the newly renamed National Institute for Standards and Technology (NIST).
What is the context for present policymaking?

I believe four attributes of present circumstances will be the most significant influences on the ultimate science policy of the Administration:

1. Soviets have gone Chapter 11. The end of the cold war ends the former defense rationale to invest in science. The developing rationale features an expanded definition of the nation’s security to include economic considerations even heavier than military.

2. Unfortunately, many of our major companies, such as IBM, and the defense contractors have appeared close to Chapter 11 as well. They are laying off workers literally by the tens of thousands—many of them highly technically skilled workers. Thus, a major focus of the government will be to create sufficient employment opportunities with adequate pay to support the rapidly expanding civilian emphasis of the workforce.

3. THE FEDERAL DEFICIT—presently it’s about $300 billion annually out of a $1.5 trillion budget. Remember, this is the annual INCREASE in our national debt. It may be coming down, maybe, gradually, but it looms over everything that government attempts to accomplish. Thus, as the federal government faces one of the most significant economic transitions in decades, its actions are severely hindered by past borrowing. The interest on the debt had been the fastest growing segment of the budget, now accounting for about 15 percent of the budget. If this annual sum of $230 billion is as incomprehensible to you as it is to me, consider an equivalent in a currency we all can understand easily—the annual interest is equal to:

- 10 NSF’s, plus
- 5 NIHs, plus
- 3 SSCs, (completely built!)
- Plus, the complete space station (before the reconfiguration),
- And a hefty student-grant program, the likes of which we have never seen.

If this appears incredible and dangerous to you, you are correct. The level of debt as a percentage of Gross Domestic Product (GDP), is now approaching that of the early 1950s when the nation was just beginning to pay off its WWII and Korean War financing. In 1950, national publicly held debt was a stratospheric 82.4 percent of GDP, it began dropping rapidly to 58.9 percent by 1955 and achieved levels in the upper 20s during the 70s. Then came the rapid runup of the 80s, by 1990, it was 44.1 percent; the estimate for 1993 is 53.5 percent.

Increased spending proposed for favored programs, such as science, have already butted up against spending limits—the resolution of which is uncertain at this time. The wrestling over increasing and revamping taxes has
begun in earnest with debates over the first Clinton budget plan this summer. Over all of this hangs the deficit. It is something that has to be addressed, or it will assuredly ruin the nation’s fiscal solvency for future generations.

4. To remind Washington politicians of these difficulties, there is the political climate which includes that staid, quiet gentleman from Texas who is spending millions of dollars out of his own pocket to tell anyone who will listen that he does not wish to become President. I suggest that his 20 percent-plus following in popularity polls does not go unnoticed in Washington.

Additionally, the Republicans have rapidly learned how to be a cohesive opposition, specifically in the Senate, and particularly if they are not duly consulted. They have demonstrated that they know a filibuster cannot be stopped without defects from their party.

As if these four attributes of present circumstances weren’t enough of a handful, other issues will certainly arise, with unknown, but maybe tremendous effect, such as, if the U.S. gets drawn into a regional conflict in Bosnia or a rekindling of hostilities in Iraq; if the debate over health care takes over all attention in DC during the fall; if trade disputes with other nations, particularly Japan, cannot be resolved; if welfare or other issues jam the agenda, then S&T can easily get crowded out of the equation for either time or money.

What are the science policy goals of members of Congress?
Whatever the constraints to increased federal investments, there is some Congressional momentum in support for virtually all the Clinton S&T policies, at least in theory, since virtually all of these efforts were begun earlier, by Members of Congress:

- defense conversion initiatives including one-half billion dollars for research, manufacturing extension, and training, were begun over the past two years in Congress as a means of assisting the economy in recovering from the closure of many military bases and the rapidly decreasing employment in the defense-related industries;
- ATP/technology extension centers were part of legislation over the last several years to keep the economy internationally competitive, continuing science and technology policy themes and federal programs that were begun over the past two decades;
- similarly, the High Performance Computing and Communications (HPCC) and other FCCSET initiatives in materials, manufacturing, biotechnology began years ago, by prior administrations, with assistance and encouragement by Congress.
Other policies of the Administration provide a complementary and supportive environment for a more activist science and technology policy. Secretary of Commerce Ron Brown was recently cited as believing that the proper role of government in the economy is no longer an issue. Government and industry need to work together with very active federal involvement. It remains to be seen how pending tax, health care, trade and other policies reflect this perspective.

Finally: What science policies might we find in this Administration?

We have now summarized the attributes of the Clinton Administration as activist, hard working and more than eager to apply science and technology to the nation’s economic problems. We have noted the development of themes from past Administrations-basic research increases, along with an evolving role for the federal government in direct support of applied research for commercial use. Present circumstances demand creative solutions to the enormous economic problems we face, particularly, due to very strong budget constraints. Science and technology policies will undoubtedly play an increasing role, especially picking up on the Congressional momentum established over the last several years.

Early indications of this Administration’s policies:

1. Support for science and technology is important—it is an investment in future progress. Undoubtedly, there will be increased funding, perhaps larger than in any other area of the budget. However, perhaps these times of fiscal constraint will not allow increases as rapidly as in the past.

   It was not always clear if universities would share in the increased investment—our institutions did not have a notable place in the original economic plan of the campaign along with technology initiatives. It was not until October 1992 that a single sheet appeared with the campaign’s blessing for university research. A key indicator of the Administration’s seriousness in promoting research will be how well they shepherd their budget proposals in the appropriations bills’ deliberations which begin in earnest in June.

2. Support for research funding agencies will be uneven, at least for fiscal year 1994. NSF, NIST and ARPA are the clear proposed winners, tying their efforts to strategic research initiatives. The Administration proposed an increase of barely three percent for NIH, two-thirds of which was similarly targeted to specific initiatives. White House Science officials responded that this should not be interpreted as a lower priority for biomedical research—NIH would increase more next year. Basic energy sciences is up a mite. DOD basic research (6.1) funding also increases in the
proposed budget with at least a $100 million increase proposed for URI for another round of multidisciplinary centers. Additionally, a $50 million renewal of a university instrumentation program is buried in the DOD budget proposal. The Administration continues to support the space station at a reduced cost design. Its support was crucial in gaining House support in late June. Administration support for the SSC was not quite as strong, but perhaps it would not have mattered since the House vote in opposition was lopsided.

3. Support for identifiable strategic, or otherwise targeted research efforts, are key. Most of NSF’s and NIH’s proposed increases are tagged in this manner, leaving other more generic research areas with either flat or declining funds. On the positive side, such increases add to the base budget of each agency and undoubtedly much fundamental, investigator-initiated research is performed under the rubric of strategic or targeted packaging. However, alternatively, if we sell too much of science as directed or strategic, we might inadvertently undercut the basis of support for areas of science that are not easily categorized in this manner, and we might be setting ourselves up for the inevitable evaluations that always occur for strategic efforts. What has all this money produced? Marketing fundamental research entirely as strategic efforts could backfire if, five years from now, the fundamental research has not produced what others might have expected.

4. There will be far more interest in partnerships and collaborative research across sectors. Most of the major program initiatives such as ATP, defense conversion, CRADAs, etc., require these linkages as an effort to stimulate quicker commercialization of research results.

5. There is a down side to the increasing view of university research as economic property and immediately vital to commercialization. Years of encouragement of universities to market their research results and enter into increasing collaborative efforts with industry-have also stimulated those who question whether entrepreneurial souls, affiliated with universities, are personally benefiting too much from these activities. Conflict-of-interest regulations are still being drafted in several federal agencies for university researchers. Then, there are those who question whether federally-funded university research ought to involve or be shared with foreign commercial interests. You will be hearing much of these and other issues related to university intellectual property in future years.

6. University infrastructure—has it been lost as an issue? Although there continues to be glimmers of interest in the Congress, it is highly unlikely that direct funded facilities programs will be renewed anytime soon. However, at the risk of sounding risque, I think that the reason this
Administration settled the recent questions over indirect costs was due to their understanding of its role in supporting university research facilities.

Have we defined the state of the Administration’s science policies? There is still far too much legislative trading to be done before the real priorities of this Administration become clear. For now, it is safe to characterize the Administration’s policies by, perhaps, taking just a little liberty with the words of that great political analyst Groucho Marx: These are my policies, if you do not like them, I have others.

REFERENCES

3 For the best comprehensive analysis of R&D in the President’s budget, the reader is referred to the annual series published by the American Association for the Advancement of Science.
Federal Research Impact Assessment Methods

RONALD N. KOSTOFF

Abstract. This paper describes the practice of Federal research impact assessment. Evaluation of research impact is described for three cases: research selection, where the work has not yet been performed; research review, where work and results are ongoing; and ex post research assessment, where research has been completed and results can be tracked. Qualitative methods (such as peer review), retrospective methods (such as projects Hindsight and TRACES), and quantitative methods (such as cost-benefit analysis and bibliometrics) are described. While peer review in its broadest sense is the most widely used method in research selection, review, and ex post assessment, it has its deficiencies, and there is no single method which provides a complete impact evaluation.

The views expressed in this paper are solely those of the author and do not represent the views of the Office of Naval Research.

INTRODUCTION

A recent Congressional Task Force report on the health of research stated, as one of its two recommendations:

“Integrate performance assessment mechanisms into the process using legislative mandates and other measures, to help measure the effectiveness of federally funded research programs.” In addressing the difficulty of implementing this recommendation, the report stated further: “More daunting than political resistance to performance assessment are the technical obstacles. Because policy-oriented assessment has not been a part of the research process in the past, its implementation must be both gradual and flexible. There are some initial efforts underway”. The reference in the Task Force report for these ‘initial efforts’ is the text of a presentation at a management conference.

The present paper is a summary and update of this presentation. It starts by identifying the many facets of research impact, then focuses on

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strengths and weaknesses of selected major techniques used in practice by the Federal government to assess research impact. If the Congressional Task Force report is one indicator of the direction that increased research accountability will take, then it becomes extremely important for research administrators, managers, and policymakers to understand the problems and practices of research impact assessment.

Research is the pursuit and production of knowledge. Measures of the expressions of knowledge resulting from research (such as papers, patents, students educated) must of necessity provide an incomplete picture of the research product. The concluding hypothesis that will permeate the remainder of this paper is that the greater the variety of measures and qualitative processes used to evaluate research impact, the greater is the likelihood of converging to an accurate understanding of the knowledge produced by research.

Impact of a research program involves identifying the variety of expressions of knowledge produced, as well as the changes which these expressions effect on a multitude of different potential research targets (other research areas, technology, systems, operations, other organizational missions, education, social structures, etc.). While some impacts may be tangible (new instruments developed, new research fields stimulated, students trained in new disciplines), many may be intangible (e.g., a designer of equipment may receive new insights from having attended a research seminar), and difficult to identify, much less quantify.

Evaluation of research impact is further complicated by the different perspectives and motivations of the assessors. The quantitative approaches require interpretation by the assessors, and the qualitative approaches rest on the purely subjective judgements of the assessors. The importance of a research program represents a weighting of its quantitative and qualitative impacts on the different potential targets of research. Yet this weighting is dependent on the multiple perspectives of the assessors, including technical, organizational, and personal perspectives and the interplay among these perspectives is not always obvious. Thus, not only is the impact of the research on each of its potential targets dependent on some unknown function of the multiple perspectives of the assessors, but the value and relative ranking of the targets depends on these multiple perspectives as well. Selection of technical methodologies, measures, and assumptions by the assessors may be driven significantly by organizational and personal motivations.

Understanding and measures of the impact of research are desired by research sponsors at every stage of the research cycle, including research topic identification, research selection, research management and evalua-
tion, and research termination/transition and ex post analysis. Research impact evaluations are of potential use to sponsors in: “Deciding whether to continue or end the program or to increase or decrease its budget; changing the program, or its management, to improve the probability of success; altering policies regarding the procurement, conduct, or management of research; and/or building support with policy makers and other constituencies of the program.”

There are many bibliographies containing the large number of methods developed to evaluate research conduct, impact, and benefits. A relatively small fraction of the methods are actually used in practice by Federal research sponsors and evaluators. Of those used in practice, only a small fraction of the results of impact studies are reported in the published literature, and an even smaller fraction are accepted by the final Federal decision-makers. While a number of the methods in practice actually used by Federal research sponsors to measure impact will be described in the remainder of this paper, one objective will be to focus on the strengths and weaknesses of these selected methods, rather than simply provide a ‘shopping list’ of techniques.

**RESEARCH IMPACT EVALUATION TECHNIQUES**

Luukkonen-Grunow, and Averch, provide summaries of major research evaluation methods used throughout the world. The three main categories, in frequency of usage order, are: Peer Review, Non-Quantitative Case Study and Anecdotal Approaches, and Quantitative Methods. Specific variants of each of these methods are described briefly, and selected examples of the more prominent applications in the U. S. are, presented.

**Peer Review**

Peer review of research represents evaluation by experts in the field, and is the method of choice in practice in the U.S. Its objectives run the gamut from being an efficient resource allocation mechanism to being a credible predictor of the impact of research.

The latter issue of peer review predictability directly affects the credibility of technological forecasting. While studies have been done relating reviewers’ scores on evaluation criteria to proposal outcomes, the author is not aware of reported studies that have related peer review scores/rankings of proposals to downstream impacts of the research on technology, systems, and operations. This type of study would require an elaborate data tracking system over lengthy time periods which does not exist today.
Thus, the value of peer review as a predictive tool for assessing the impact of research on an organization’s mission (other than research for its own sake) rests on faith more than on hard documented evidence.

Peer review problems include: partiality of peers to impact the outcome for non-technical reasons, including the organizational and personal reasons mentioned previously in the discussion on multiple perspectives; an ‘Old Boy’ network to protect established fields; a ‘Halo’ effect for higher likelihood of funding for more visible scientists/departments/institutions; reviewers differ in criteria to assess and interpret; the peer review process assumes agreement about what good research is, and what are promising opportunities; the costs of peer review are considerable but ignored in many cases. These potential problems should be considered during the process of selecting research impact assessment approaches.

Many studies related to peer review have been reported in the literature, ranging from the mechanics of conducting a peer review, to examples of peer reviews, to detailed critiques of peer reviews and the process itself. In addition to descriptions of peer reviews and processes contained in the reviews and surveys referenced above, other examples of processes and critiques exist.

While these reported studies present the process mechanics, the procedures followed, and the review results, the reader cannot ascertain the quality of the review and the results. In practice, procedure and process quality are mildly necessary, but nowhere sufficient, conditions for generating a high quality peer review. Many useful peer reviews have been conducted using a broad variety of processes, and while well documented modern processes may contribute to the efficiency of conducting a review, more than process is needed for high quality. There are many intangible factors that enter into a high quality review, and before examples of reviews are presented, some of the more important factors will be discussed.

High quality peer reviews require as a minimum the conditions summarized from Ormala: 1) The method, organization and criteria for an evaluation should be chosen and adjusted to the particular evaluation situation; 2) Different levels of evaluation require different evaluation methods; 3) Program and project goals are an important consideration when an evaluation study is carried out; 4) The basic motive behind an evaluation and the relationships between an evaluation and decision making should be openly communicated to all the parties involved; 5) The aims of an evaluation should be explicitly formulated; 6) The credibility of an evaluation should always be carefully established; 7) The prerequisites for the effective utilization of evaluation results should be taken into consideration in evaluation design.
Assuming these considerations have been taken into account, three of the most important intangible factors for a successful peer review are: Motivation, Competence, and Independence. The review leader’s motivation to conduct a technically credible review is the cornerstone of a successful review. The leader selects the reviewers, summarizes their comments, guides the questions and discussions in a panel review, and makes recommendations about whether the proposal should be funded. The quality of a review will never go beyond the competence of the reviewers. Two dimensions of competence which should be considered for a research review are the individual reviewer’s technical competence for the subject area, and the competence of the review group as a body to cover the different facets of research issues (other research impacts, technology and mission considerations and impacts, infrastructure, political and social impacts). The quality of a review is limited by the biases and conflicts of the reviewers. The biases and conflicts of the reviewers selected should be known to the leader and to each other.

Peer Review of Proposed Programs

The two largest Federal sponsors of research are the National Institutes of Health (NIH) and the National Science Foundation (NSF). The NSF peer review process of research proposals illustrates how potential research impact influences selection of new research areas. In the NSF process, proposals received are assigned to program officers for review. The program officers select external peer reviewers and using mail, panel, or mail & panel approaches, have the proposals assessed and rated. The program officers then perform their own assessment of the proposals and forward their recommendations to higher levels. These recommendations are rarely overturned.

From the 1987 version of the NSF Brochure, Information for Reviewers, reviewers use four criteria to assess the proposals: research performance competence; intrinsic merit of the research; utility or relevance of the research; and effect of the research on the infrastructure of science and engineering. Research impacts are evaluated through the second, third, and fourth criteria.

The second criterion, intrinsic merit, incorporates impact of the proposed research on other research fields in its definition and is a measure of the nearer term impact of the proposed research. The third criterion, utility, addresses the extent to which the work could contribute to an extrinsic goal such as a new technology. The fourth criterion, infrastructure, incorporates impact on the nation’s research/education/human resource base.

In the NIH process, proposals are sent to an initial peer review group,
composed mainly of active researchers at colleges and universities, where they are reviewed for scientific and technical merit. After receiving a priority rating from the peer reviewers, the proposals are then sent to a statutorily mandated advisory council for a program relevance review. After the council members recommend action to be taken on the proposals (usually concurrence with the peer group recommendations, but sometimes special action\textsuperscript{26}) the Institute staff rank the proposals and initiate a funding strategy.

The review criteria established by Public Health Service regulations and provided to the peer reviewers are: significance and originality of the proposal from a scientific and technical point of view; adequacy of the methodology to carry out the research; qualification and experience of the principal investigator and staff; reasonable availability of resources; reasonableness of the proposed budget and duration of the project; and other factors, such as human subjects, animal welfare, and biohazards. It appears that only the first criterion, significance, relates to impact, and can include the relatively near term impact on allied research fields. Broader impact and relevance issues appear to be the purview of the advisory councils. The council members are asked to assess the quality of the initial scientific review as well as the proposal’s relevance to Institute research program goals and broader societal health-related matters.

The Office of Naval Research (ONR) does not have formal peer review of individual research grants, but leaves the choice of peer review to its scientific officers. It requires a competitive process among internal Navy organizations (claimants) with external reviewers for those accelerated program proposals which constitute about 30% of the total ONR program. \textsuperscript{2,31-33} The claimants that win the competition then go to the technical community (if their charter is extramural) and advertise their areas of interest for proposals, or, if their charter is intramural, perform the work in-house.

In the competition, all the accelerated programs proposed by the claimants are categorized into areas of similar science\textsuperscript{31} and the proposals in each area are evaluated by a panel of experts external to ONR. The written portion of the evaluation requires numbers and comments for factors related to research quality and Navy relevance. The factors on the scoresheet relating to potential research impact estimation are Research Merit (RM), Potential Impact on Naval Needs (PINN), and Potential for Transition or Utility (PTU). The Research Merit criterion incorporates the potential impact of the research, if successful, on allied research areas. The Potential Impact on Naval Needs criterion deals with downstream impact of the proposed research on naval systems and operations. The Potential for Transition or Utility criterion incorporates the potential
nearer term impacts of the proposed research. Transition refers to the actual transfer of research programs to development and Utility refers to other mechanisms by which a program’s results would be transmitted to, and used by, the technical community.

A key component of the process reported in Kostoff 31 was the use of mixed levels of reviewers on the panels to evaluate the different potential impacts of research. The panels included bench-level researchers to address the impact of the proposed research on the field itself; broad research managers to address potential impact on allied research fields; technologists to address potential impact on technology and the potential of the research to transition to higher levels of development; systems specialists to address potential impact on systems and hardware; and operational naval officers to address the potential impact on naval operations. The presence of reviewers with different research target perspectives and levels of understanding on one panel provided a depth and breadth of comprehension of the different facets of the research impact that could not be achieved by segregating the science and utility components into separate panels and discussions. The interplay among reviewers coming from different perspectives allowed each reviewer to incorporate elements of other perspectives into his/her decision-making process.

A multiple regression analysis showed RM to be the most important factor in determining the bottom line score. 2 PINN did not weigh as heavily in the reviewers’ bottom line score as did PTU. The reviewers weighed nearer-term impact more heavily in their bottom line decisions, as evidenced by the higher correlations of PTU. Since the study also showed that the bulk of the proposed Accelerated Research Initiatives was viewed by the reviewers as basic research, and since the (possibly far) downstream naval impact of basic research may not be evident in many cases, it is not surprising that the more identifiable near-term impacts, such as transition to exploratory development or utility of results by other researchers, would affect the reviewers’ bottom line decisions more than the longer term impacts.

Peer Review of Existing Programs

There are many approaches used by research sponsoring organizations to conduct periodic peer reviews to monitor the quality and potential impact of ongoing research. 5-10, 13, 23, 25, 29, 31, 34 This section focuses on selected peer review approaches which reflect the state of the art in the technical community and pays special emphasis to how research impact is incorporated into the peer review process. The first case study is the U.S. Department of Energy (DOE) review of its Office of Basic Energy Sciences (BES), and the evolution of that approach into present DOE practice.
The second case study focuses on the ONR methods used to review extramural and intramural programs. The third and fourth case studies relate to the annual review of the National Institute of Standards and Technology (NIST) by the National Academy of Sciences (NAS), and the annual review of the DOE national laboratories by the field offices.

In 1981, the DOE performed an assessment of existing projects funded by its office of Basic Energy Sciences. Out of approximately 1200 active projects supported by BES, a randomly selected sample of 129 projects was reviewed by panels of scientific peers. The projects were grouped by areas of similar science, and the reviews were conducted on 40 separate days by 40 separate expert panels, with an average of four members and three projects per panel. The reviewers were, for the most part, bench level scientists independent of the DOE.

The reviewers were asked to rate seven factors for each project: Team Quality (TQ), Scientific Merit (SM), Scientific Approach (SA), Productivity (P), Importance to Mission (IM), Energy Impact (EI), and Overall Project Quality (OPQ). The three evaluation factors on the scoresheet which related to potential research impact were SM, IM, and EI. SM incorporated the potential impact of the research on allied research fields. IM covered the types of ways in which a research project could contribute to the Nation’s energy needs. EI was the probable impact of the research project on energy development, conservation, or use.

After the scoring by the panels was completed, all possible linear regression models (ranging from six-factors to one-factor) were used to relate the OPQ rating factor (essentially the reviewers’ bottom line score on each project) to the other rating factors for the 129 projects. The six-factor model produced a correlation coefficient of 0.89, which meant that the six-factors selected constituted the bulk of the considerations which the reviewers used to score the OPQ rating factor. In fact, the best three-factor model derived to predict the OPQ rating factor score, consisting of TQ, SA, and IM, produced correlation coefficients within three percent of those obtained with the complete six-factor model.

An updated version of the BES evaluation approach is used by the DOE Office of Program Analysis to conduct peer review assessments of DOE research and development. Now, after a panel has completed the evaluation of all the projects assigned to it, the members are asked to identify research needs or opportunities available to the DOE research program. With this updated version, DOE initiated in 1992 a detailed review of all projects supported by BES.

Each of ONR’s review processes has a major peer evaluation component adapted to meet the particular needs of the organizational unit under
review. The two reviews described here are those of ONR’s two largest claimants, the Research Programs Department (RPD) and the Naval Research Laboratory (NRL).

The RPD sponsors extramural basic research mainly at universities, and presently consists of 13 Divisions organized along science disciplines. Two separate groups contribute to the one day annual review of each Division. One group is the Division’s Board of Visitors (BOV), which represents academia, industry, and non-ONR government. The majority of the BOV are members of the research community, but the BOV will include representatives from the technology development community and the operational Navy where appropriate. The other group contributing to the review is the Research Advisory Board, the senior management of the RPD whose backgrounds span a wide range of scientific disciplines. For the review, the Division Director overviews the total Division, including programs, accomplishments, new opportunities, and management issues. The Division’s program managers describe their programs in detail, including the impact on science of their accomplishments, potential or ongoing transitions of their programs to development programs, some bibliometric measures such as publications, and potential impacts on the Navy if successful. The reviewers fill out comment sheets, focusing on scientific merit, technical approach, and potential naval impact, and later discuss their findings with the RPD management.

Almost all of the NRL’s programs are intramural, and it conducts full spectrum research in 60 task areas. On average, about 20 task areas will be reviewed per year, with four or five of these task areas reviewed using external reviewers, and the remainder reviewed by an internal NRL management group called the Research Advisory Committee (RAC). The external review group represents academia, industry, and non-NRL government. The RAC consists of NRL senior management whose backgrounds span a broad range of science disciplines.

The Coordinator of the task area reviewed by the external panel overviews the task area and investment strategy. Then, the principal investigators of the task area describe their work in detail, including the impact of their science accomplishments on the task area and allied science fields, transitions to more applied categories, bibliometric measures such as publications and presentations, and potential impact of their research on the Navy.

The reviewers fill out comment sheets, focusing on scientific merit, technical approach, and potential naval impact, and afterward visit and review facilities. The reviewers draft a report and meet with ONR management and members of the RAC to present their preliminary findings.
The remaining task areas are reviewed in detail by the RAC.

NIST is reviewed annually by two external groups, a general policy and management review, and a detailed technical review. The Visiting Committee on Advanced Technology reviews general policy, organization, budget, and programs of NIST. The Committee submits an annual report which includes reviews of progress in NIST’s science, engineering and technology transfer programs.

The Board on Assessment of NIST Programs, under the auspices of the National Academy of Sciences (NAS), performs a detailed technical review. Seventeen panels of reviewers (about ten people per panel) from industry and academia conduct program reviews based on two or three-day site visits at NIST facilities. The panels address variants of research quality, and because of NIST’s unique charter in supporting competitiveness, pay particular attention to technology transfer, industrial coupling, and emerging technologies. While quantitative indicators of research impact are not addressed in the panels’ annual report, impacts of the research on technology and competitiveness are addressed extensively. Recommendations for improvement in these impact areas are provided.

The DOE has nine contractor-operated multiprogram laboratories. Each contractor’s laboratory management performance is evaluated annually by the DOE Field Office (FO) to which each laboratory is assigned. The FO prepares an appraisal plan for the laboratory, which focuses on laboratory performance in four areas: (1) institutional management performance, which includes different aspects of overall lab management; (2) programmatic performance, which includes R&D achievements; (3) operations support performance, which includes technical functions which support mission objectives; (4) administrative performance, which includes business management functions.

In the programmatic performance areas, sources of input include DOE program officials, other agencies having substantial work at the laboratory, and FO program managers. For this annual review, DOE will utilize information from its own program advisory committees on the adequacy and impact of the laboratory’s R&D efforts in relation to the overall DOE program. Furthermore, DOE will use the reports of the scientific peer review committees established by the contractor, which provide an assessment of the quality of the laboratory’s R&D programs.

There appears to be no formal requirement for using teams of external reviewers for the technical programs as in the ONR and NIST reviews; rather, most input seems to come from the sponsors. Estimations of research impact appear to derive from the DOE program advisory committees and peer review assessments, which may be reflected in the annual appraisal.
To summarize the peer review section, the methods described include criteria which address the impact of research on its own and allied fields, as well as on the mission of the sponsoring organization. The most intensive use of peer review appears to be the NSF/NIH processes for assessing proposals, and the NAS annual review of NIST. Nearer-term research impacts typically play a more important role in the review outcome than longer-term impacts, but do not have quite the importance of team quality, research approach, or the research merit.

**RETROSPECTIVE METHODS**

In the evaluation of research impact, a spectrum of approaches may be considered. At one end of the spectrum are the subjective, essentially non-quantitative approaches, of which peer review is the prototype. At the other end of the spectrum are the mainly quantitative approaches, such as evaluative bibliometrics and cost-benefit. In between are what can be termed semi-quantitative, or retrospective, approaches.

Retrospective methods make minimal use of mathematical tools but attempt to draw on documented approaches and results wherever possible. In practice, some of these approaches (namely, studies of accomplishments resulting from sponsored research programs) are widely used by the research sponsoring organizations. Three retrospective methods are discussed: Projects Hindsight and TRACES (retrospective approaches), and accomplishments books.

Project Hindsight was a retrospective study performed by the Defense Department in the mid-1960s to identify those management factors important in assuring that research and technology programs are productive and that program results are used. The evolution of the new technology represented in each of the 20 weapons systems selected 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.

It was found that RXD Events from the directed (problem specific) basic research category emerged in systems development about nine years after their conception, while it took twenty or more years for some events from the undirected category to impact development. Since Project Hindsight concentrated only on the post World War II contributions of science and technology on the selected systems, it did not treat in any depth the contribution from undirected basic research, since many of those events predated the time span of the project.

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. Project Hindsight data showed that systems applications, rather than new science, inspired science and technology for advanced systems.

The most obvious limitation of Hindsight relating to basic research is the time frame. Most of the RXDs occurred in the 1950s, with few in the '40s and '60s. Since many fundamental research projects could require more than two decades for their results to impact systems, the cut-off on time span could have precluded the inclusion of research impacts. A more serious limitation of the study derives from defining the RXDs as identifiable advances which impact the final system directly. The 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.2915

In 1967, the National Science Foundation (NSF) instituted a study to trace retrospectively key events which 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.

Key ‘events’ in the research and development history of each innovation selected were identified. The study showed that non-mission research provided the origins from which science and technology could advance toward innovations. It also showed that approximately 70 percent of the important events were non-mission research, 20 percent mission-oriented research, and 10 percent development and application. The number of non-mission events peaked significantly between the twentieth and thirtieth year 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.

Thus, the TRACES time frame extended 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. In neither case were indirect impacts of basic research given formal credit, although the TRACES study did allude to non-mission research as “a fund
of knowledge against which withdrawals can be made to achieve innovation at a rate satisfactory to society. 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. For each innovation studied, the significant events (important activity in the history of an innovation) and decisive events (a significant event which 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; and 4) certain institutional factors, such as internal R&D management, availability of funding, management venture decision, etc.; 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 focusing on decisive events as above, the following important exogenous factors were identified: 1) the technical entrepreneur (a major driving force in the innovative process); 2) early recognition of the need; 3) government funding (more generally, availability of financial support, from whatever source); 4) the occurrence of an unplanned confluence of technology (confluence of technology occurred for some innovations 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.

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. 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 which 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.

In a modern version of the TRACES study, the National Cancer Institute (NCI) initiated an assessment to determine whether there were certain research settings or support mechanisms which 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. The study concluded that all the research settings and support mechanisms contributed significantly to the “Advances” with no single mechanism or setting represented disproportionately. Papers on the “TRACES” were extremely highly cited—eight times as frequently as expected.41

Hindsight and TRACES approaches require substantial resources, and are thus performed relatively infrequently. A more common vehicle used by research sponsoring organizations to display the impacts of funded research on science, technology, and the organization’s mission is the accomplishments book. Standard accomplishments books present descriptions of sample 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 other sectors. These books do not usually include quantitative estimates of impact.42,43
The approach taken by the Office of Energy Research, DOE, for one of its component organizations, the Office of Health and Environmental Research (OHER), was to present selected accomplishments in different research areas over the 40-year history of OHER. This technique allowed impacts and benefits of the research to be tracked through time. In some cases, such as development of a capability to predict the travel and dispersion of hazardous substances (space debris, nuclear weapons tests byproducts) released into the atmosphere, the benefits could not be quantified. In other cases, such as development of the flow cytometer and centrifugal fast analyzer, the benefits could be quantified. Costs of these programs, or subprograms, were not provided, and it is therefore difficult to relate the benefits, where stated, to the costs. In many of these programs, the chain of events leading to an innovation was very long (as TRACES and Hindsight have shown), required many performers and organizations, and in some cases required simultaneous multiple sponsors. Allocation of costs and benefits to the multiple performers and sponsors over time appears extremely difficult.

Recently, the Institute for Defense Analysis (IDA) produced a massive three-volume set describing the accomplishments of the Defense Advanced Research Projects Agency (DARPA). Of the hundreds of projects and programs funded by DARPA over its then (1988) 30 year lifetime, 49 were selected and studied in detail. 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. Most of the projects supported were technology or systems development rather than basic research, but many of these projects 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. Finally, success in transferring 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 the Submarine Laser Communication Satellite (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 and software products which resulted.

Similar to the other retrospective approaches described above, the IDA report describes supporting R&D for the projects examined, but does not account (in the author’s opinion) sufficiently for benefits resulting from indirect impacts of research. In the time evolution of development charts at the end of each project writeup, a few critical events/technologies which 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 should be viewed as complementing the subsequent DARPA effort, and should share in the benefits with DARPA.

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 accom-
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.

Hindsight, TRACES, and, to some degree, the DOE and DARPA accomplishments books used a historiographic approach and looked for significant research or development events in the metamorphosis of research programs in their evolution to products. They 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; and (4) while the costs of the research and development were not quantified, and the benefits (typically) were not quantified, the research and development were worth the cost.

**QUANTITATIVE METHODS**

**Bibliometrics**

A recent comprehensive review of bibliometrics shows the sparsity of bibliometric studies for research impact evaluation reported by the Federal government. The reason for this is due in part to the following problems with publication and citation counts:

- Publication counts: indicates quantity of output, not quality; non-journal methods of communication ignored; publication practices vary across fields, journals, employing institutions; choice of a suitable, inclusive database is problematical; undesirable publishing practices (artificially inflated numbers of co-authors, artificially shorter papers) increasing.
- Citations: intellectual link between citing source and reference article may not always exist; incorrect work may be highly cited; methodological papers among most highly cited; self-citation may artificially inflate citation rates; citations lost in automated searches due to spelling differences and inconsistencies; Science Citation Index (SCI) changes over time; SCI biased in favor of English language journals; and same problems as with publication counts.

In response to Cawkell's claims that "citation anomalies have little effect—they are like random noise in the presence of strong repetitive signals," MacRoberts stated the Federal concerns about bibliometrics eloquently: "When only a fraction of influences are cited, when what is cited is a biased sample of what is used, when influences from the informal level of scientific communication are excluded, when citations are not all the
same type, and so on, the ‘signal’ may be repetitive, but it is also weak, distorted, fragmented, incoherent, filtered, and noisy”.

Another reason for limited Federal use can be inferred from Narin\textsuperscript{51}, where studies on the publication and citation distribution functions for individuals are reviewed. The conclusion drawn, from studies such as those of Lotka, Shockley, De Solla Price, and Cole and Cole\textsuperscript{51}, is that very few of the active researchers are producing the heavily cited papers. How motivated are funding agencies to report these hyperbolic productivity distributions for different programs in the open literature, especially since many questions exist as to the accuracy and completeness of the bibliometric indicators? This conclusion raises the further question of the role actually played by the less productive researchers (as measured by publication and citation counts): is the productivity of the elite somehow dependent on the output of the less influential, or is the role of the less productive members that of maintaining the stability of the research infrastructure and educating future generations of researchers?

Macroscale bibliometric studies characterize science activity at the national, international, and discipline level. The biennial \textit{Science and Engineering Indicators} report\textsuperscript{30} tabulates data on characteristics of personnel in science, funds spent, publications and citations by country and field, and many other bibliometric indicators. Another study at the national level was aimed at evaluating the comparative international standing of British science.\textsuperscript{52} Using publication counts and citation counts, the authors evaluated scientific output of different countries by technical discipline as a function of time.

There is little evidence that the results from such studies have much influence on policy or decision-making; i.e., the allocation of resources. As Martin et al point out in their conclusions, there is potential benefit for a country to understand its position vis-a-vis that of its competitors in different science areas, in order to be able to exploit opportunities which may arise in those areas. However, which indicators are appropriate and how they should impact allocation decisions are open questions.

With the notable exception of the NIH\textsuperscript{9}, few Federal agencies report use of microscale bibliometric studies to evaluate programs and influence research planning in the published literature. The NIH bibliometric-based evaluations included the effectiveness of various research support mechanisms and training programs, the publication performance of the different institutes, the responsiveness of the research programs to their congressional mandate, and the comparative productivity of NIH-sponsored research and similar international programs.

Two recent papers\textsuperscript{41, 53} described determination of whether significant
relationships existed among major cancer research events, funding mechanisms, and performer locations; compared the quality of research supported by large grants and small grants from the National Institute of Dental Research; evaluated patterns of publication of the NIH intramural programs as a measure of the research performance of NIH; and evaluated quality of research as a function of size of the extramural funding institution. Most of the NIH studies focused on aggregated comparison studies (large grants vs: small, large schools vs. small schools, domestic vs. foreign, etc).

Patent citation analysis has the potential to provide insight to the conversion of science to technology. Much of the Federal government support of the development of patent citation analysis was by the NSF although there is little published evidence now of widespread Federal use of this capability. Some recent studies have focused on utilization of patent citation analysis for corporate intelligence and planning purposes. However, as Pavitt cautions, it is not yet clear to what extent the 'other publications', cited in patents, reproduce basic or applied research, from universities or corporate laboratories. In addition, a high proportion of technology is not patented, because it is kept secret, because it is tacit and non-codifiable art, or because-as in the case of software technology-it is very difficult to protect through patents.

Despite these limitations, bibliometrics may have utility in providing insight into research product dissemination. For example, in a recent series of presentations to large Federally-funded laboratories, the following suite of bibliometric studies was proposed: 1) Examine distribution of disciplines in co-authored papers, to see whether the multidisciplinary strengths of the lab are being utilized fully; 2) Examine distribution of organizations in co-authored papers, to determine the extent of lab collaboration with universities/industry/other labs and countries; 3) Examine nature (basic/applied) of citing journals and other media (patents), to ascertain whether lab's products are reaching the intended customer(s); 4) Determine whether the lab has its share of high impact (heavily cited) papers and patents, viewed by some analysts as a requirement for technical leadership; 5) Determine which countries are citing the lab's papers and patents, to see whether there is foreign exploitation of technology and in which disciplines; and 6) Identify papers and patents cited by the lab's papers and patents, to ascertain degree of lab's exploitation of foreign and other domestic technology.

While it was also recommended that the lab compare its output (papers/citations normalized over disciplines) with that of other similar institutions, this quantitative comparison should be approached with great
caution. A recent comparative bibliometric analysis of 53 laboratories clustered the labs into six types (Regulation and Control, Project Management, Science Frontier, Service, Devices, Survey), and stated that “comparisons of scientific impacts should be made only with laboratories that are comparable in their primary task and research outputs”. The report concluded further that 1) Bibliometric indicators and scientific publications are not the only outputs that should be measured, but the other types of outputs differ for different labs; 2) Bibliometric indicators are not equally valid across different types of laboratories; and 3) Bibliometric indicators are less useful for the evaluation of research laboratories involved in closed publication markets.

Co-occurrence Phenomena

Modern quantitative techniques utilize computer technology extensively, usually supplemented by network analytic approaches, and attempt to integrate disparate fields of research. One class of techniques which tends to focus more on macroscale impacts of research exploits the use of co-occurrence phenomena. In co-occurrence analysis, phenomena that occur together frequently in some domain are assumed to be related, and the strength of that relationship is assumed to be related to the co-occurrence frequency. Networks of these co-occurring phenomena are constructed, and then maps of evolving scientific fields are generated using the link-node values of the networks. Using these maps of science structure and evolution, the research policy analyst can develop a deeper understanding of the interrelationships among the different research fields and the impacts of external intervention, and can recommend new directions for more desirable research portfolios.

Little evidence of Federal use of these techniques (co-citation, co-word, co-nomination, and co-classification analysis) has been reported in the open literature. However, as computerized databases get larger, and more powerful computer software and hardware become readily available, their utilization in assessing research impact should increase substantially. These techniques with extensive references, especially co-word, are discussed in more detail in Appendix III of Kostoff.

Cost-benefit/Economic Analyses

A comprehensive survey examined the application of economic measures to the return on research and development as an investment in individual industries and at the national level. This document concluded that while econometric methods have been useful for tracking private R&D investment within industries, the methods failed to produce consis-
tent and useful results when applied to Federal R&D support.

Cost-benefit analysis has limited accuracy when applied to basic research because of the quality of both the cost and benefit data due to the large uncertainties characteristic of the research process, as well as selection of a credible origin of time for the computations. As an illustrative example, an incremental cost benefit analysis was performed on the fusion-fission hybrid, a fusion-driven fission reactor.61 This study ignored fusion hybrid research expenditures before 1980 (sunk costs). For the parameter ranges chosen, it was shown that there was a broad region over which hybrid development could prove cost-effective. However, had this same analysis been done in 1934 (around the beginning of identifiable basic research for fusion), using the same cost and benefit streams as in the 1983 study plus adding costs incurred between 1934 and 1980 and present-working back to 1934, then the result would have been much different from the 1983 study.

In the 1983 study, the problem was treated deterministically; uncertainties or probabilities of success of the different parameter values being achieved were not taken into account. The real problem, which pervades and limits any attempt to perform a cost-benefit analysis on a concept in the basic research stage, was the inherent uncertainty of controlling the fusion process. This translated to the inability to predict the probabilities of success and time and cost schedules for overcoming fundamental plasma research problems (e.g., plasma stabilities and confinement times); no credible methods were available. Thus, the main value of the cost-benefit approach was to show that the potential existed for positive payoff from the hybrid reactor development, that there was a credible region in parameter space in which controlled fusion development could prove cost effective; what was missing was the likelihood of achieving that payoff.

A more recent study weighed the costs of academic research against the benefits realized from the earlier introduction of innovative products and processes due to the academic research.62 A survey of corporate R&D executives showed that an average of seven years elapsed between a research finding and commercialization, and that commercialization would have been delayed an average of eight years without academic research. A cost-benefit analysis using this survey data showed a very high social rate of return resulting from academic research.

However, the data were not validated independently by a TRACES (or other document-based) type of analysis of a sample number of the products and processes. The time between the research findings and commercialization is very short compared to the results of Hindsight or the TRACES studies, and is more in line with the lag time between the end of basic research and commercialization shown by Hindsight/TRACES. Use of a
shorter lag time in the discounting process increases the benefit/cost ratio and the social rate of return. While the method is innovative, a more objective data source would provide higher confidence in the computed rates of return.

To summarize the quantitative methods section, few Federal agencies report (in the published literature) use of bibliometrics to evaluate programs and influence research planning. Cost-benefit and other economic approaches have been reported in the published literature over the years. The foundation on which these approaches rest needs to be strengthened to improve their credibility. As Averch\textsuperscript{63} states, after describing the huge social rates of return to investments in hybrid corn reported by Griliches\textsuperscript{64}: “In general, economists compute high social rates-of-return to most kinds of research. The rates, in fact, are usually much higher than those computed for other kinds of public investment. So there is a puzzle as to why research investments do not increase until their marginal return just equals returns from other public investments.”

CONCLUSIONS ON RESEARCH IMPACT EVALUATION TECHNIQUES

Three generic types of research impact approaches used by the Federal government were described (peer review, retrospective, and quantitative methods). Peer review is the method used most frequently. All methods examined have their unique shortcomings. A fundamental problem is that many research impact targets exist. These include impact on: research field itself; allied research fields; technology; systems; operations; education; etc. The strength of the specific impact of the research on each of these targets and the weighting assigned to the value of the research impact on each of these targets depends on the technical, organizational, and personal perspectives of the reviewers. For example, while research proposal X may have a very strong potential impact on technology Y and a very weak impact on graduate student education, if the evaluators selected for a particular review are organizationally and personally inclined to assign high importance to graduate student education, then research proposal X will suffer accordingly. The many available dimensions which derive from these different perspectives serve to complicate the evaluation process. Much of the research evaluation community has come to believe that simultaneous use of many techniques is the preferred approach. However, there is little evidence of multiple techniques used by the Federal government in impact assessment, especially bibliometrics to support peer review. This area is ripe for exploitation.

A recent study\textsuperscript{11} summarizes quite well the use of research impact
assessments by the Federal government. “Since 1985, no breakthrough methods of any variety have been invented that more definitively reveal the ex post scientific or social value of past research investments. . . the evidence is sparse that there is much payoff to public or private sector R&D administrators from making greater use of them. . . R&D administrators do use ex post evaluations for political and organizational purposes, for example, to convince sponsors that they are interested in rational decision processes and that they are funding good work. However, the research evaluation literature between 1985-1990 contains very few demonstrations that evaluation makes any difference at all to the critical decisions about the level and allocation of scarce scientific and technical resources.”

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Research Administration Reconsidered

STEPHEN L. HANSEN AND CLIFFORD L. SHISLER

Abstract. The rapid expansion of grants, the importance of grants as a source of revenue, and the close regulation of grants have changed the purpose and role of research administrators. Instead of serving the traditional role of a "mediator-expediter" among the faculty, the university, and the sponsor, research administrators have become regulators of the grants process. This change has had a significant impact upon the profession of research administration, affecting the kinds of jobs available in research administration, the characteristics of the people who fill those jobs, and the nature and content of our professional meetings.

Raymond Woodrow, reflecting upon his long career in research administration at Princeton University, wrote that the research administrator's tasks are to nourish a climate for research, to develop policies and support systems for research, and to provide "organizational arrangements that will help research to flourish in a university." The purpose of the research administration office, he argued, is to "facilitate research rather than direct it." Ken Beasley, in 1970, described the research administrator as a "mediator-expeditor" among the university, the faculty, and the sponsor.

Research administration, however, has changed in the last decade. The growing importance of grants as a source of revenue for universities and the expanding complexity of the regulation of grants have upset the delicate balance among the university, the faculty, and the funding source. The resulting new relationship among these groups has changed not only the purpose but also the profession of research administration.

In the past, the research administrator focused on facilitating the grants process and mediating the interests of the university, faculty, and sponsor. The first aspect of our role was to protect the university by making sure that reports were submitted, that accounts were audited, and that commodities were purchased according to university rules. A second responsibility, dealing with the research sponsor, was fairly simple. Research Administrators made sure proposals were submitted on time, helped faculty

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negotiate changes after the award was made, supported the submission of final reports, and nurtured a cordial relationship between the university and the agency.

The Research Administrator’s third area of responsibility, working with faculty, was somewhat more complex than working with the sponsor. We focused on facilitating the proposal submission process and providing post-award support to the faculty. Research Administrators helped faculty with submissions, arranged internal support, negotiated with the agencies with whom cordial relationships had been established, and intervened when a faculty member was having difficulty in managing a grant. Our relationship with the three constituencies was professional, cordial, and conciliatory. Research Administrators truly functioned as facilitators and mediators.

The relationship among the university, the sponsor, and the faculty member changed dramatically in the last ten years. This change has had a significant impact on both our role as research administrators and our profession.

The university has become increasingly dependent upon external funding to carry out its mission. It is less and less possible, as institutional resources decline, for a university to provide the necessary personnel and equipment to conduct research projects. By 1987, federal support accounted for 25 percent of all revenues for colleges and universities. Additionally, the prestige derived from grants has become important in most universities’ agendas. Ernest Boyer, in Scholarship Reconsidered, observed that “too many campuses are inclined to seek status by imitating what they perceive to be more prestigious institutions.” Hence some institutions embrace grants, particularly research grants, as a means for achieving status. Boyer quotes one administrator, for example, as saying that his goal was to put his university among the “top twenty or certainly in the top fifty” research institutions. Such rankings are conspicuously recorded in the Chronicle of Higher Education’s list of top 100 institutions receiving federal R&D grants. Thus, the university has come to depend on external dollars not only to maintain its mission but also to enhance its reputation. Grants, therefore, are no longer simply viewed as an extramural source of funding for the university. They are seen as essential for sustaining the academic endeavor.

These changes in the purposes of grants for the university have directly affected the role of the faculty member. Faculty can no longer depend on the university to fully fund research, so the pursuit of dollars to continue the research activity, employ staff, and equip laboratories has become an end instead of a means. Equally important, because of the revenue and the prestige of grants, university administrations pressure faculty to pursue sponsored programs. The need to acquire grants is so great that Donald K.
Hess, Vice President for Administration at the University of Rochester, rhetorically asked in his keynote address to the 1988 National Council of University Research Administrators (NCURA) Annual Meeting whether universities now look upon research as a business and as an opportunity for revenue.\(^5\) As evidence of the pressure felt by faculty, Boyer noted that 58 percent of the faculty stated that research grants were important in earning tenure but that only 24 percent of the faculty at four-year institutions received federal research support in the 12 month period of his survey.\(^6\) According to Boyer, this pressure helps explain dissatisfaction among the professoriate with the current reward system.

Driven by the need for revenue and by the expectations of the university administration, faculty have found that their interests are no longer equally balanced with those of the university and of the sponsor. Faculty cannot realistically refuse to seek grants. They have lost, consequently, the ability to make choices and, hence, the ability to protect their interests.

At the same time that the university’s interests began to override and diminish those of the faculty, the federal government changed the nature of the entire partnership. Originally, Washington viewed grants as long-term investments in research. As long as this purpose governed grant-making, faculty were able to set the agenda of what research to attempt and how to conduct it. As early as the mid 1970s, however, Washington began to move from supporting faculty initiated research to “purchasing” research. In “purchasing” research, the government viewed grants as a means to buy a product or a solution to a particular problem. Less interested in long-term investments in research, Washington began to control and dictate the research agenda.\(^7\) Furthermore, as the items on Washington’s “shopping list” changed, say, from cancer to AIDS, or from agriculture to economic development, so did the allocation of grant funding. Because of a need for revenue, university administrations pressured faculty to pursue these new lines of funding.

Additionally, during the 1980s, the federal government changed the nature of its partnership with universities by embarking upon a crusade not only to control the research agenda, but also to regulate the behavior of higher education. Between 1987 and 1990 alone, Congress passed 25 major new regulations and revisions to existing regulations.\(^8\) More recently, Washington imposed on the grants process additional regulations dealing with such concerns as misconduct in research, the drug-free workplace, and debarment and suspension. Many of these regulations, student loan default and drug free workplace certifications, for example, have no direct relationship to the activity funded by the grant. But because these restrictions are part of the grant condition, the universities accept the obligation
of complying with these regulations. According to the Association for the Study of Higher Education, “relationships with government have been marked by increasing bureaucratization and control.” Grant regulations force the universities to act as a surrogate for the government, enforcing Washington’s values and standards of behavior. The universities, therefore, have become agents of the federal government instead of partners.

The responsibilities of protecting the institution and complying with federal regulations abrogated the research administrator’s relationship with the faculty. Rather than nurturing a climate for research and facilitating the process, we have become regulators of the grant process. This change in function from facilitator to regulator has changed the profession of research administration. The level of education, the specialization of jobs, the content of our professional meetings, and certain demographic characteristics of our profession all reflect the shift in our purpose and role.

In 1983, a survey of the National Council of University Research Administrators (NCURA) membership indicated that 31 percent of the 1,066 respondents held doctoral degrees. By 1990, however, the percentage of NCURA members with doctoral degrees had fallen to 23 percent. Similarly, of the new members joining the Society of Research Administrators (SRA) from its beginning until 1985, 23 percent held doctorates. From 1986 until the present, however, only 17 percent of the new members possess a doctoral degree. This decline illustrates two trends in research administration resulting from the change in our role. First, with the emphasis now upon compliance with regulations and with the diminished role of the faculty in the grants “partnership,” there is less need for doctoral-prepared research administrators. In the past, the research administrator’s relationship with the faculty was critical in effectively performing the role of a “mediator-expediter.” Research administrators who themselves had earned the doctoral degree, conducted research, published, and taught were true colleagues, and as such, were able to help nourish and stimulate the climate for research. With the grants process and the research agenda directed by federal sponsors, the collegial relationship between faculty and the research administrator becomes less important. Likewise, as the pressure to obtain external funding increases, the research administrator’s role in nourishing the climate for research is less significant.

The decline in the number of doctoral degree-prepared research administrators illustrates a second trend. The expansion of regulations has created the need for specializations in the profession. According to a report from the Pew Higher Education Research Programs, “each new federal program carries with it substantial monitoring requirements that often lead to the establishment of new internal bureaucracies.” Health and safety reg-
ulations are a prime example. Most research universities have had to increase their staff of health and safety inspectors fivefold or better. Consequently, the number of individuals has grown who, for example, may be responsible solely for compliance with human subjects regulations or animal care. Likewise, the number of research administrators who may spend full time on contract negotiations, industry relations, indirect costs, grant accounting, or other specializations, such as the various aspects of patents, copyrights, and licensing, has grown. The regulations governing the grants process have increased to such proportions and have become so complex that it is necessary in many institutions to have departmental level research administrators. Clearly, the level of knowledge in each specialization now required by the grants process and the decline in the significance of the research administrator's relationship with the faculty have reduced the need for Ph.D.-trained research administrators.

Certain other changes in NCURA's membership also reflect the change in the role of research administration. In 1983, 34 percent of the members were female. By 1990, women represented 55 percent of the membership. SRA has experienced the same type of change with females now constituting over half of its members. The change in the percentage is a result of a growth in the membership related to new positions in research administration needed to manage specialized functions. The fact that women are filling these positions reflects, in part, the general opening of positions in higher education to women. It also reflects, however, the creation of more specialized jobs in research administration not requiring doctoral degrees, and the subsequent employment of women in these lower paying positions.

Related to these changes in the profession is the movement for certification. The specialization of jobs and the lack of need for a doctoral degree have given impetus to the call for the credentialing of research administrators through a certification process. By expanding and specializing our jobs in response to outside influences, we have fragmented research administration thereby complicating our career paths. There is no clear progression, for example, from a compliance specialist to a vice president for research. Regardless of one's position in the debate over certification, many perceive certification as a means of mitigating the effects of fragmentation and specialization.

The change in the role of the research administrator is also reflected in the content of NCURA's annual meetings. In 1986, for example, NCURA offered 12 workshops. Full-day sessions covered general topics while the half-day workshops examined standard issues such as faculty incentives and governmental relations. There were workshops on FAR regulations and on compliance. The single half-day session on compliance
covered all legal and ethical compliance issues including biosafety, hazardous materials, and radiation safety along with human subjects and animal care. Concurrent sessions focused on grant development, funding opportunities, industry relationships, and faculty development.

By 1991, the focus of the annual meeting had changed. Appropriately entitled “The Changing Climate for Research,” the meeting offered 23 workshops. Topics now examined were technology transfer, conflict of interest, biological and chemical safety, intellectual property, subcontracting, and clinical trials. Reflecting the new environment, a full-day workshop focused exclusively on regulatory compliance as distinct from ethical compliance. The concurrent sessions followed the same trend of specificity. Topics included materials transfer agreements, working with faculty entrepreneurs, scientific integrity, international agreements, and revisions to OMB A-21.

The contrast between the two meetings illustrates well the change in our roles. Instead of emphasizing grant opportunities and how to work with the faculty, 1991 topics focused on the new specific issues associated with the process of grants.

In 1967, A. Eurich warned universities of the seductive nature of grants.16 The lure of money, he wrote, could start a university down a treacherous and slippery path which in the end might mean the loss of its ability to determine its own mission. While it is unclear whether universities have arrived at that final point, it is certain that the environment in which research administrators operate has changed. This change provokes important questions and issues that we, as research administrators, must confront.

If the present trend of more federal involvement and control continues, research administrators will have to commit a greater portion of their resources to manage compliance and regulations, and the trend toward specialization of functions within research administration will accelerate. Additionally, as more administrative effort is dedicated to managing federal regulations, other changes may occur. University research policies will become driven by the need to comply with federal demands rather than by the need to support a creative environment for research. Ultimately, it will mean that the research administrator’s purpose, as described by Woodrow and Beasley, to create a climate for research will be lost.

If we believe it is important to preserve our original purpose, then we must accept the responsibility of helping to protect the research environment. We must make our institutions aware of the changes that have occurred over the past 15 years and the consequences of those changes. If we fail to guard the research environment, then we are merely contributing to our universities’ slide down the path Eurich warned us to avoid.
REFERENCES

9 Lindsay and Neumann, p. iii.
13 Data from the Society of Research Administrators Executive Office.
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