understanding the natural variability of ocean processes to distinguish it from human-generated variability was neither given high priority nor was it a sought-after outcome of the convention. Consequently, 5 years later, little has changed in nations' behaviors—business is as before, there is little new knowledge, an opportunity has been squandered, and unenlightened rhetoric has been elevated.

So why are S&T in foreign affairs important? As the Carnegie Commission pointed out, "Revolutionary advances in physics have led to diverse applications in weapons, energy, materials, and medicine, with extraordinary impacts on the quality of life and on economic and political relationships among countries. . . ." "Greenhouse gases, the AIDS virus, agricultural biotechnology, advanced energy systems, new pharmaceuticals, information technologies, and a host of other scientific and technological trends shape global competition and cooperation. . . ." "All must take bold and imaginative steps to adapt to a world in which the border between domestic and foreign affairs is crossed everywhere and most particularly by science and technology."

Unfortunately, the State Department today has neither the human resources, organizational structure, culture, nor funds to facilitate major S&T bilateral or multilateral efforts. As mentioned earlier, this situation has deteriorated even further since the commission's report in 1992, despite intentions to improve matters with the appointment of the new under secretary for global affairs. One indicator of this deterioration is the perception, if not the fact, that the State Department continues to undervalue its S&T counselors at our embassies overseas. As an early member of the advisory committee to the Carnegie Commission, my discussions with other participants at that time confirmed my view that S&T counselors assigned to our embassy staffs worldwide are most often not given a serious role in deliberations on important foreign affairs matters that have significant technical content. Many are good people with good intentions, but with poor resource support, often limited training for their assignment, and seen by seniors and peers as serving in noncareer-enhancing billets, their accomplishments are often modest at best.

Another indicator of deterioration and sign of disinterest in S&T is the fact that the number of scientifically qualified personnel at the State Department dedicated to the assistant secretary for oceans and international environmental and scientific affairs has dwindled. In the same *Science and Government Report* referenced earlier (*2*), the point was made that "[a]n unannounced reorganization has eliminated the State Department's senior position for international science, technology, and health, and redistributed those functions within a slimmed-down department bureau that's increasingly focused on global environmental issues." Yet, it is within this office, working under the newly established under secretary for global affairs, that much of the coordination of major S&T initiatives with other nations should be routinely monitored and overseen in close coordination with the appropriate government agencies. So, visible signs that such attention is actually being given to these monitoring and oversight functions, and at a sufficiently high level, are simply not there.

As a consequence of all this inattention, the United States is fast gaining a reputation among other nations as an unreliable S&T partner when launching major new research initiatives of potential benefit to the United States and the world. I question whether others will want to join us again at the start-up of any major collaborative research endeavor until we become more predictable. It is timely to do so now.

For starters, I would urge the White House and Department of State to do what the Carnegie Commission recommended 5 years ago. I heartily endorse these still valid and substantive recommendations. Most important, "The President should clarify the international responsibilities and priorities for S&T among mission agencies and ensure their overall coordination with foreign policy through the Department of State." Additionally, we should "[s]et plans for the long-term nurturing of human resources throughout the government, and especially in State, for work on global issues with a substantial scientific and technological character." The President's Council of Advisors on Science and Technology should catalyze necessary remedial action within the Executive Branch. Further, within the Legislative Branch, congressional committees with jurisdiction over science and foreign relations should help see that the commission's recommendations are carried out. It is time to get serious about S&T in foreign affairs.

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Peer Review: The Appropriate GPRA Metric for Research

Ronald N. Kostoff

The federal government is the largest single sponsor of fundamental science research today. Increased scrutiny of federal programs in the drive toward deficit reduction requires increased public accountability for the stewards of the government's research funds. The Government Performance and Results Act (GPRA) of 1993 (*1*) was passed to improve the accountability of government-funded programs by measurements of performance against planned targets. Federal agencies are required to initiate implementation of GPRA in fiscal year 1997; pilot projects (*2*) will help identify performance measures for different types of programs. However, it is extremely important that the tools used to enforce research accountability do not destroy basic research.

There are three major components to GPRA: strategic plans, annual performance plans, and metrics to show how well the annual plans are being met (*1*). Classical strategic planning derives from the military and commercial world, focuses on the application of knowledge toward a predefined goal rather than the search for knowledge, and assumes that the links between plans and targets are understood.

Annual performance plans are derived from production and service industries, where efficiency in the use of known resources to achieve well-defined targets over the performance period is the main goal. Revolutionary basic research, which has historically yielded some of the largest downstream payoffs, has an inherently large uncertainty and failure rate, and may take many years before results are forthcoming. This intrinsic long time scale, characteristic of basic research, conflicts with the short-term emphasis of much of the corporate world, where annual reports and requirements for quarterly financial performance shorten the production period for research results. This near term fo-

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cus on financial performance has essentially eliminated long-range high-risk fundamental research financed from corporate funds in most industries.

Metrics that gauge adherence to annual performance plans derive, in modern times, from the time- and motion-study component of industrial engineering. Again, these tools measure efficiency of the use of known resources to achieve specific goals over a set time period. At present, such output metrics are applied informally to research for purposes of academic analysis (*3*), and these analytical results may provide useful insights to research activity. Annual application of these quantitative indicators is more appropriate for measuring the short-term observable outputs that characterize activity and productivity (cars produced, papers published) than the long-term outcomes that characterize mission and societal impact (improving health, enhancing safety).

A major concern of researchers is that the short-term services and production orientation of the GPRA planning and metrics components could refocus the research away from long-range high-risk revolutionary science challenges to shorter-term low-risk evolutionary product-oriented goals. Annual application of these metrics to basic research in the formal bureaucratic sense of GPRA could convert the nature of the research being conducted from a quest for knowledge and understanding to a drive for output metrics. Uncertainties inherent in basic research bring into question the validity and credibility of any long-range plans to achieve specific goals, because long-term research effectiveness and impact will depend on economic, environmental, and geopolitical factors not evident during the research phase (*4*).

A more subtle concern is that application of the present GPRA approach to basic research may effectively yield the same results as government-imposed censorship. The requirements of federal agencies to display compliance with the GPRA metrics may reorient their selection of research proposals to maximize these arbitrary measures. Concepts that could improve understanding and the unification of science, but would not optimally satisfy the GPRA metrics, might no longer be proposed for federal funding because of lower funding probability. [I am reminded of Solzhenitsyn's views that the worst part of documents being censored was not that sections were rejected; the worst part was the loss of those ideas that were not even expressed and eventually no longer considered because of the knowledge that they would be censored (*5*).] Safe, short-term, low-risk evolutionary research would become the accepted practice. Basic research needs to be decoupled from "strategic" targets and GPRA metrics, and the scientific roadblocks and challenges alone should be the stimuli for research activity.

A more appropriate accountability approach for basic research is: (i) articulation of a rational investment strategy; (ii) long- and short-term retrospective studies that show the diverse benefits from past research and potential future benefits; and (iii) quality control of expert peer review. An organization's research investment strategy is a rationale for the prioritization and allocation of resources to address knowledge deficiencies that impede attainment of the organization's goals. Short-term retrospective studies show how recent research has affected fields of science, and may contain projections of future impacts of research on technologies, systems, and operations. Long-term retrospective studies of major innovations and outcomes in systems and technology show the origins of critical research and development advances in a broad spectrum of fundamental research performed many decades earlier (*4*). Expert peer review on a periodic basis will validate the soundness of the investment strategy and the importance of the research accomplishments and subsequent technology impacts.

Peer review properly designed to support GPRA would provide credible indication to the research sponsors of intrinsic program quality, program relevance, management quality, and appropriateness of direction, and has the potential to improve the quality of the research program as well (*6*). Before such a review process is implemented, a number of considerations have to be addressed.

The primary requirements of excellent peer review are the dedication of an organization's senior management to the highest quality objective review and the motivation of the review manager to conduct a technically credible review. In particular, the review manager selects the review process, criteria, and reviewers; guides the panel questions and discussion; summarizes reviewers' comments; and recommends follow-up actions. The selection of panelists by the review manager can substantially influence the review outcome.

Excellent peer review that provides an accurate picture of the intrinsic quality of the research being reviewed requires highly competent reviewers and no injection of additional distortions in the reviewers' evaluations as a result of biases, conflict, fraud, or insufficient work. Not only should each reviewer be technically competent for his or her subject area, but the competence of the review group should cover the multiple facets of research issues (specific research area reviewed, allied research areas, technology, systems, and missions). In addition, panel expertise should not be limited to subdisciplines of the program under review (which addresses the question of whether the job is being done right), but should be broadened to the area covered by the overall program's highest level objectives (which addresses the question of whether the right job is being done). Broadening the panel in this manner will ease introduction of new paradigms.

If GPRA reports are used to support the budgetary process, the results of different panels evaluating different technical disciplines must be normalized so that parametric comparison becomes meaningful. Biases, interpretation differences, scoring differences, different review processes, and the myriad of other causes for panel differences over and above intrinsic technical quality differences must be identified and mitigated. Differences in repeatability, reliability, and precision should also be identified and minimized.

Finally, peer-review costs, which include more than direct, out-of-pocket costs, should not be neglected in establishing a specific review process. With high-quality performers and reviewers, time and opportunity costs are high and represent the major contribution to total costs. The total review costs can be a nonnegligible fraction of total program costs, depending on the review frequency, the level of technical detail desired, and whether the programs are labor or hardware intensive.

In summary, peer review is the appropriate central evaluation mechanism for basic research under GPRA, but careful thought and planning will be required to implement a viable and credible peer-review process.

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7. The views expressed in this article are solely those of the author and do not represent the views of the U.S. Department of the Navy.