

EU Science Funding Milestones Miss the Mark

As European scientists who have run large research teams for years, we are concerned about the increasing bureaucratic load for scientists, especially in applying for and handling European Union research funds. One of us has coordinated an EU large-scale facility for nine years, and the other has participated in several EU projects. We have each received (and still receive) funding both from government sources and from a wide variety of private grants. Collecting the funding from a multitude of sources can consume all of a scientist's time. We would like to open discussion on this undesirable development and propose a complementary funding policy.

In EU funding, the border between science and R&D work seems to be fading, as is evident from the increasing emphasis on detailed research plans that must include promises of well-defined scientific results, now called "deliverables," to be produced at clearly fixed time points. If milestones are not met, the reviewer teams, recruited from peer European scientists, can cut funding even in the middle of the granting period. However, history of science indicates that scientific discoveries are highly unpredictable. Thus goal- and milestone-oriented funding policy suits only R&D-type work, for which it was originally developed.

Scientific disciplines differ in many respects, but any ambitious and innovative research includes uncertainty, errors, and misjudgments. Clairvoyant scientists who claim to predict their ground-breaking experiments three to five years ahead are suspect, to say the least. Consequently, many of the planned approaches have to be abandoned or modified during the course of the research. Scientists certainly need goals, but those goals cannot be

reached the way a marathon runner would, by following well-marked roads with roadside milestones. Science is more like orienteering, in which the approaches and paths must be selected according to the ever-changing terrain.

The EU's increasing science bu-

reaucracy, requiring laborious and complicated paperwork both in the initial application and in the frequent project reports, has led to the emergence of science consultants: Universities and corporations offer know-how to interpret the deliverables, milestones, Gantt charts, Pert

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diagrams, and road maps. Obviously, only devoted science managers can keep track of the oversized research networks that the scientists feel obliged to create in order to compete in the funding game. These new professional groups decrease the total research budget available for scientists without improving the quality of science.

The increasing bureaucratic load also decreases the willingness of top scientists to volunteer for review panels of grant applications and for evaluations of research programs. Thus the reviews sometimes are not based on good scientific standards. Unfortunately, means to correct unfair decisions are practically nonexistent in EU funding, which clearly lags behind, for example, the US National Institutes of Health, where the applicants are advised on how to improve their application for the next round.

It might be wise to consider alternative and complementary funding approaches. One possibility is to improve methods for recognizing top research teams and scientists (not necessarily the ones who can, or have time to, write the best research plans) and to fund them with risk (similar to the venture capitalist model), as long as their productivity stays high. Previous achievements continue to be the best means to predict future success; according to our experience, looking in the rearview mirror works at all stages of research and thus is fair for scientists at all levels of professional maturity.

As considerable amounts of citizens' money are spent on research, everybody—including the scientist—has the right to expect that the money will be used wisely. Present funding practices should be examined and discussed openly. We propose that some large funding body study the effectiveness of different funding and evaluation approaches by applying both open-minded science measures and more strict R&D criteria, and then following the results for the long term. Such a study would also benefit national funding agencies.

To cite Richard Feynman: "The rate of the development of science is not the rate at which you make observations alone but, much more important, the rate at which you create new things to test." Such new things emerge only as research progresses; they are not known in advance. At each milestone, the scientist must study all possible directions, but doing

so requires freedom, continuity of funding, and time to think—without wasting energy on irrelevant tasks.

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Nuclear Pit Facility's Merits, Snags, and Timelines

In the June 2004 issue of PHYSICS TODAY (page 34), Jim Dawson reports on the American Physical Society (APS) discussion paper, published in April 2004, about a modern pit facility. That paper evaluates an MPF based on an overly optimistic combination of assumptions that obviate the need for planning such a facility. A parametric evaluation of the stockpile plan recently delivered to Congress and the current estimates of pit lifetimes by the National Nuclear Security Administration weapon laboratories show that continuous planning for an MPF is prudent risk management to meet national security needs.¹

The NNSA advocates managing the risks to national security by uninterrupted planning for an MPF while obtaining further information for acquisition decisions. For example, initial results from accelerated pit-aging experiments and from the inevitable aging effects of plutonium on weapon performance are expected in 2007. The APS position paper advocates a high-risk approach of deferring or curtailing the MPF project until such information is available. The NNSA plan for an MPF includes a series of major system-acquisition critical decisions in 2007, in 2009, and at the start of construction in 2012. Based on the current plan, the Secretary of Energy will not be making irreversible decisions on constructing an MPF until early in the next decade. That is well beyond the 2006 minimum date suggested by the APS position paper for making such decisions.

The APS paper is replete with examples of unrealistic optimism and factual errors. Some examples include discounting the challenges of upgrading production capacity to 80 pits per year at the Los Alamos National Laboratory TA-55 facility or rapidly enhancing production out-

puts in a future facility. Although a small capacity of 125 pits per year is the most likely path forward, the MPF environmental impact statement provides analyses for capacities of 125 to 450 pits per year, not because of a desire to maintain large stocks of undeployed warheads as suggested by the APS paper, but to ensure that the maximum potential environmental impacts of an MPF have been considered for National Environmental Policy Act compliance purposes. APS asserts that pits in storage at the Pantex plant in Amarillo, Texas, could be used as replacements and thus obviate the need for a new facility. However, that assertion lacks technical foundation because pits stored at Pantex will age similarly to pits that are stockpiled.

Deferral of MPF planning, which was suggested in the APS paper, would negatively affect plans for a responsive pit manufacturing infrastructure that may be pivotal to further reducing the number of stockpile warheads—a cost-saving move for the nation. Similarly, the capability to manufacture replacement pits eliminates weapon-performance uncertainties that result from plutonium aging and is consistent with maintaining the moratorium on nuclear testing.

Current plans afford numerous review opportunities until early in the next decade. National security for the US should be based on a prudent assessment of risks and not the overly optimistic and unrealistic evaluation contained in the APS position paper. Early planning and development are essential to avoid cost overruns and delays.

Reference

1. US Department of Energy, National Nuclear Security Administration, *Report to Congressional Defense Committees on "An Enhanced Schedule for the Modern Pit Facility"*, February 2004. Available at http://www.fcnl.org/pdfs/nuc_mpfreport.pdf.

Everet Beckner

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Dawson replies: For comment, we contacted Steve Fetter, chair of the national security subcommittee of the APS Panel on Public Affairs, and Frank von Hippel, POPA's chair.

Fetter and von Hippel comment: Everet Beckner, the National Nuclear Security Administration's