Letters

Overhead Costs

The current structure for the allocation of federal research funds results in the funding of science entirely on its merit, regardless of the cost. It promotes quality science and productivity, but it does nothing to contain costs. Consequently, scientists and institutions are motivated to inflate direct and indirect costs. Daniel E. Koshland, Jr.'s editorial of 29 March (p. 1545) suggests changes in the administrative structure of indirect cost funding. I suggest we also consider a modification of our current funding scheme to promote quality science, productivity, and cost effectiveness at a "grass roots" level.

The peer-review system should remain as it is, with a review of the budget and assignment of priority scores. But after the review, the "cost" of the grant would be calculated as the total direct and indirect costs divided by the number of years of funding approved. This "cost" would be included in determining funding priority. For example, grants ranking in the top 5% would be funded regardless of the cost. Grants with a priority of 5 to 10% would be funded only if the "cost" were less than a specified amount.The scale would continue downward so that large budgets go only to the highest priority science. The exact algorithm could be adjusted to fund a given number of grants with the available funds, and it could have limits so that nothing lower than a certain priority is funded. (There would be limits on the number of low-priority grants one investigator or lab could hold to prevent scientists from dividing a big project into many little ones.)

Once scientists know that the chance of being funded increases by having smaller budgets, the size of the budgets on grant applications to the National Institutes of Health would likely decrease and more science, and scientists, would be funded. Productivity and quality would not be adversely affected because scientists already have strong personal motivations for maintaining both. The public and our legislators might become more supportive once they know that we scientists are doing our part to cut costs. More "small science" would be encouraged, and science and teaching would become a more attractive profession to bright students who are currently discouraged by funding "horror stories."

We should acknowledge and respond to our country's fiscal limitations and get our own costs under control before we complain about a lack of funds or ask for more money. With the proper motivation and structure, we can use existing funds to support more of the best scientists and the best science.

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Koshland's continuing campaign to revise the method of charging overhead costs to research projects is important. However, the specific technique that he favors—a "universal overhead rate"—is not a good solution.

It would be better to create separate "cost pools" for administrative expenses, for use of facilities, and perhaps for libraries and other important cost categories, and to charge research projects with costs based on an overhead rate developed for each pool. The charge for facilities use, for example, would reflect the proportion of the facilities costs that each project caused.

This method would result in widely different charges among universities and among various types of research projects within a university, and these differences would reflect real differences in the costs that projects of various types actually incurred. For example, the facilities charge for projects based on library research or questionnaires would be considerably lower than for those that use expensive equipment. There would be a little more bookkeeping than with a single universal rate, but most well-managed businesses use such an approach, and they would not do so if they thought the additional cost was less than the benefits. Moreover, the overall "indirect cost" rate, which the public usually associates entirely with administrative costs, would be dramatically reduced.

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Chimp-Language Wars

In her Research News article, "Déjà vu all over again: Chimp-language wars" (29 Mar., p. 1561), Ann Gibbons writes that the whole field of sign language studies of chimpanzees was "devastated" by a single article published in 1979 (*1*). But she does not note the large amount of evidence and debate that has appeared during the past decade. Our recent volume (*2*) summarizes the available evidence. We list here (*3*) all the reviews of this volume of evidence that we know to have appeared at this writing. Interested readers might wish to consult these reviews by distinguished biologists, anthropologists, and psychologists in internationally recognized journals. Not all of the reviewers have been equally favorable in their overall opinion of the book. Nevertheless, they agree that the scientific evidence is sound and that this field of research, which is very much alive, has not only survived Terrace's critique but has produced a significant body of additional evidence since 1979.

To us the "déjà vu" of the "language wars" is the traditional rejection of the modern view that the same basic laws govern the intelligent behavior of human and nonhuman beings. Jerome Bruner (*4*) put it this way:

> A third trend is also discernible: the bridging of gaps that before were not so much empty as they were filled with corrosive dogmatism. The gaps between prelinguistic communication and language proper as the child develops, the gap between gesture and word, between holophrases and sentences, between chimps signing and man talking, between sign languages and spoken ones, between the structure of action and the structure of language. I think that the renewal of interest in language as an interactive, communicative system has made these 'gaps' less like battlegrounds where one fights and dies for the uniqueness of man and more like unknown seas to be mapped."

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Big Science, Little Science

Amidst all the discussion and controversy about "big science," its effects on little science, and priorities for the support system for science and scientists, there has been little or no discussion or analysis of what constitutes big or little science and how money and effort are actually divided. It seems worthwhile to try to devise ways to measure the distribution, and I propose a possibly useful index of support.

As an example of what is generally called "big science," I examined the Superconducting Super Collider (SSC) in terms of the support per scientist per year that it will provide over its lifetime and compared it with two other cases. This seems reasonable because much of the argument is cast in terms of large projects allegedly consuming inordinate fractions of resources that otherwise would be dedicated to the support of individual scientists doing "small science."

The Department of Energy baseline estimate of the cost of building the SSC is $8.3 billion in "as spent" dollars, that is, in dollars of the year in which they will be spent (*1*). In 1990 dollars (without escalation), the total is about $7 billion. Upgrades to the detectors over the lifetime of the SSC and additional foreign contributions to the detectors may amount to about $1 billion. If we assume that the SSC will operate for 25 years, the total annual capital cost in 1990 dollars may be taken to be about $320 million.

When the SSC is built, its estimated operating cost in 1990 dollars will be $300 million per year of operation. Let us assume that there will be about 2500 investigators involved every year for the 25 years. (By "investigator" I mean a scientist, generally at the Ph.D. level, who has a responsibility for devising and carrying out research, alone or in a group, and would be considered capable of writing proposals and accepting research funds.) The big detector projects already involve a total of about 2000 investigators continually, and a number of other projects involve small detectors and other experiments.

The salaries and benefits of the scientific investigators who will use the SSC are not included in the above operating and capital costs. We can assume that the average cost of salary and benefits for an investigator will be $100,000 per year in 1990 dollars. The figures lead to an estimate of $350,000 per investigator per year.

This rough estimate of cost per investigator per year is in the range of the equivalent numbers for the General Motors Research Laboratories (GMR), which operate with a budget (including capital expenditures) that over the past few years has been in the range of $185 to $155 million. During this period the laboratories have supported about 500 investigators, with cost per investigator per year in the range of $370,000 to $310,000. GMR is generally engaged in small to medium-small research; investigators work individually or in small teams. There is no equipment comparable in scale to a large accelerator, but there is occasional access, as required, to proving grounds, a large wind tunnel, and manufacturing facilities for tests.

In 1989, the total expenses of the Woods Hole Oceanographic Institution, less those for its education program and for ship refits undertaken on behalf of the National Science Foundation (NSF)–supported oceanographic ship fleet, were about $54 million

(*2*). There are 128 members of the scientific staff in five scientific departments, 63 additional members of the technical staff who are considered to be investigators, and four scientists in the Marine Policy Center—for a total of 195 investigators (*3*). This gives an estimate of the cost per investigator per year of $277,000, not including the capital cost of the oceanographic ship fleet supported by the Navy and the NSF. The annual ship cost per scientist is $20,000, raising the total cost per investigator per year to slightly less than $300,000.

This rather crude estimate suggests that, on the basis of total cost per investigator per year, the SSC ranks as somewhat larger science than oceanography and about the same size science as a large (but not "big science") industrial laboratory. Similar estimates for other institutions could be obtained, but it is important that comparisons be made on the basis of total costs of support.

It has been suggested that the investigator count for the SSC is unfair because many of the investigators are not "doing real science," but are building the accelerator and the detectors and "really doing engineering." However, accelerator physics and detector physics are recognized by the scientific community as "real physics," and appear in the journals as such, because they *are* real physics, and very difficult physics at that. In any case, most experimental scientists spend most of their time preparing and building experiments and only a little time doing them. It is not clear that the division of effort among various kinds of activities is any different in small or big science.

It seems perfectly legitimate for scientists to band together to do science on a scale that is impossible to work at alone or in small groups. This is increasingly the case in many kinds of science. The individual investigator continues to be the key, but there are some things that individuals cannot do alone (and some things that cannot be done in teams); working alone does not necessarily confer a special legitimacy.

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