LETTERS

Alternative to Peer Review?

Rustum Roy's alternative to the peer review system (Editorial, 27 Mar., p. 1377) is simple and beautiful; but it has a design flaw similar to that of the early superhighways whose capacity was determined by measuring preconstruction point-to-point demand without considering the added demand that would result from the convenience of having the superhighway. The current (verifiable) proliferation of refereed journals and the concomitant (and less readily demonstrable) lower quality of accepted papers are surely due, in part, to the use of numbers in deciding promotions in academe. Given this precursor, it is not difficult to envision the added impetus toward low-quality publication, and (worse yet) the ultimate impact on quality of graduate degrees that would result from attaching dollars to such quantitative measures as numbers of refereed papers and numbers of graduate degrees. Inefficient as it is, imperfect as it is, the peer review system has a qualitative component based on informed judgment which we cannot afford to lose.

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I read Roy's editorial with disbelief. In order to reduce the work load associated with peer review of grant applications, he proposes basing research support on a formula linking the funding level to "productivity" measured solely by numbers of publications, Ph.D.'s, and so forth. There is no mention of the *quality* of the research these numbers would represent. Roy's proposal would not promote good science, but it would promote the proliferation and duplication of shorter, less significant research publications; it would also encourage institutions to cheapen their degree requirements to turn out more (perhaps less qualified) Ph.D.'s. If individuals or institutions chose to maintain standards and not succumb to the demands of the formula, their funding would be drawn away by those who did.

I agree that peer review requires significant effort, but its very value is in assessing quality rather than counting papers. If research funding were based on the formula suggested by Roy, a serious decrease in quality would be guaranteed.

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While peer review as currently practiced may not be the most desirable or efficient form of resource allocation, Roy's proposal for a productivity-based formula of block grants seems even less desirable because it raises more questions than it answers.

First, with respect to training, there are a great many institutions receiving public research funds which have no or minimal graduate programs. Those institutions supporting training, in turn, often base the size of their enrollments on stipends available, not program caliber. Both of these considerations immediately would distort any formula for training. Furthermore, the question of overabundance is ignored. Is it wise to base increased funding upon the continued production of unusable talent? These issues are not easily addressed by productivity figures alone.

Second, a funding formula based on publication is certain to exacerbate the already too real problem of fragmentation of data and duplicate reporting. How many more gratuitous coauthorships would appear, and how will junior authorship be counted? What body will decide what an adequately refereed journal is in a given field? Who will want to publish in nonrefereed forums?

Third, a formula based on a history of past support from a number of grouped sources ignores the fact that most research dollars are targeted toward highly specific goals. Within the National Institutes of Health alone, for example, some congressionally mandated spending extends to the program level within a single institute. Roy may be in a department and discipline where research falls into a single identifiable category, but this is the exception, not the rule. How would funding agencies be able to carry out their mandates, and who would be accountable for the pursuit of categorical research?

Fourth, using the amount of industrial and private support received by a department to allocate public dollars does not seem justifiable, since in many areas of research the amount of private support is minimal. A small private contribution, therefore, could disproportionately divert public resources. This is especially true where institutions have sought to perform contract work for private firms.

It is easy to criticize a well-characterized system and to recognize its inefficiencies. It is much more difficult to propose a comprehensive, viable alternative. There is no a priori reason to believe that an administrative formula and substitute peer review by journal referees and institutional committees is structurally superior to the current sys-

tem. Roy's assertions that intellectual efforts will be saved, training and conduct of science will be improved, and the system will be more conducive to innovation are far from evident facts.

Peer review as currently practiced does cost money, is time-consuming, and probably can be improved. It also, however, is firmly based on merit. Calls for its demise should be reconsidered until the scientific community is certain of all the real problems, and of the alternatives to be proposed. Is the real problem the cost of peer review, or the lack of funds to support meritorious projects? Care must be taken that the cure is not worse than the disease. Too often, when legislatures are made aware of problems, they propose quick solutions which may not be what concerned individuals had in mind.

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Roy suggests a formula which gives a weighted sum of four numbers: A, the number of advanced degrees awarded by the unit; B, the number of papers published; C, the amount of federal and state money awarded; and D, the amount of industrial money the unit had obtained. I suggest that formula-based funding is not as good as peer review for several reasons.

First, the former attempts to substitute numbers for scientific judgment. It is not enough to know how many graduates an institution has produced; one must know if they were any good. It is not enough to know how many papers were published in refereed journals; one must know if the journal has high standards and, more important, if the papers were good or just examples of the least publishable unit. Without informed evaluation the numbers are worth very little.

The proposed alternative also ignores the fact that different kinds of research require different amounts of money. In reviewing grant proposals, I see price tags that vary from $20,000 to more than $1 million. The scientific merit is not predictable from the cost; why should the amount of the award not reflect the variability of need? If, by the suggested formula, some laboratories will receive more than they need, then some will surely receive less.

A third difficulty is the need to vary the values of the coefficients by which the various factors are weighted. For instance, some fields do not receive any research support from industry or state mission agencies. This absence may be due to the irrelevance of the branch of science to any social or industrial mission, or it may reflect shortsightedness on the part of these grantors. In either case, such research would be underfunded by the use of a formula such as Roy's.

A fourth problem is the heterogeneity of the departments and research units; they often contain good workers and some who are not so good. If the funds go to the unit, rather than to the investigator, it is likely that intradepartmental politics, friendships, and enmities will influence how the funds are apportioned.

Finally, it should be emphasized that the currently used system of peer review already weights productivity very heavily. The "track record" of the applicant is an important factor in the evaluation of any proposal, but not the only one. . . .

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. . . Roy does not discuss a key issue: Who would decide the distribution of funds granted to a given university under a scheme such as he outlines? Does he suggest that department chairmen or deans should be entrusted with this responsibility? In that case we should lose an invaluable asset which distinguishes our system from that of European countries, the independence of principal investigators. It is hard to place a price tag on this feature, but contact with foreign scientists convinces me that the morale factor, due to the opportunity of junior faculty members to "run their own show," is basic to the success of academic science in America. It cannot be replaced by reserving "approximately 15 percent of the funds for entering (young) faculty."

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. . . The most dangerous aspect of Roy's proposal lies in the extreme concentration of power in the hands of the program manager. Which young faculty will get a slice of that reserved percentage of available funds? Which special initiatives will be deemed worthy? The knowledge, insight, wisdom, and prescience required of such a manager to perform at anything like the level of the peer review system would make walking on water seem like a prebreakfast constitutional. Even the brightest and best intentioned single individual could not perform this task. The system Roy suggests harks back to the simpler days of the 1950's, when a knowledgeable manager could supplement his own expertise in obtaining a comprehensive and balanced picture of a field by informally phoning "a select group of active scientists." This sort of procedure in the 1980's is unlikely to achieve a similar balance, given the much larger size of most fields. The very individuals best qualified for this impossible job are in fact precisely those who are most deeply engaged in pushing back the frontiers of knowledge, and least likely to view such a full-time task as rewarding or fulfilling. . . .

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. . . All of the factors listed by Roy are presently a part of the judgment undertaken in deciding on the granting of research support albeit without the use of a formula. The present procedure, time-consuming and cumbersome though it may be, does also allow for a judgment of the quality of the past performance and future promise. This judgment of quality, carried out in the best and most unbiased manner possible, is an absolutely essential ingredient in maintaining the health of U.S. science. . . .

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. . . "The alternative funding mechanism" based on the merit formula discussed in the 27 Mar. issue is likely to lead to the demise of the still strongest research system in the world. It is true that there are real problems with the system as it currently exists, but perhaps instead of creating an entirely new beast without peer review, we should restructure the present system and do some streamlining. There are many ways to reduce the burden on the current system: (i) cut out redundancy within and between granting agencies; (ii) limit the number of proposals an investigator can submit in a given time period; (iii) cut out the $100 per day consulting fee (scientists should be flattered to serve on review panels—we get reimbursed anyway by listing our service when we are up for promotion); (iv) perform fewer expensive site visits; (v) establish regional review panels, rather than bringing everyone to Washington; and (vi) look more closely at investigators who have established empires with many grants and ensure that money is indeed not being obtained from different sources to fund the same research. I daresay that if some

of the above were initiated, the taxpayers would be saved a lot of money and the peer review system would remain intact.

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I sense in all the above letters an aura of the ancien régime. None shows an awareness of the emerging issue given form by Milton Friedman (*1*): Why should the public support science at all? I answer this implicitly in my formula.

Morawetz's point (included also by Easter) is an essential detail I could not cover in the space of the editorial. The key American science funding innovations were funding from multiple sources and funding directly to the principal investigator. Provision for preserving those could be achieved if the National Science Foundation (NSF) or the National Institutes of Health required (with appropriate flexibility) *something like* a 70 (to principal investigators), 20 (to departments), and 10 (to institutions) split. While Easter has done me the justice of reading the editorial, he seems to miss the very purpose of the coefficients. They are there for agencies (as representatives of the public) to decide most of the issues he raises; not what science should be done, but what should receive *public* support.

The letters by Berns, McCreery, Kalt, and Liebman are excellent examples of the reactions of much of the science community to innovation on our own turf: they merely defend the status quo. I presented a philosophical rationale for a formula system based on value to society, as an alternative to the so-called peer review system, for the efficient and continued funding of nonmission research in the United States. None of the authors debates these major advantages. None notes the balance of four factors and the flexibility of the formula in being able—if need be—to accommodate some of their own proclivities.

However, two egregious but very common errors in the arguments of Berns, McCreery, Kalt, and Liebman need to be laid to rest. First is the notion that the peer review system is in some mysterious way linked with the progress of science and is responsible for the "still strongest research system in the world," and, second, that peer review is able to find or define "quality."

Since most of the fundamentals of quantum mechanics, organic synthesis, and DNA structure managed to be discovered without the blessing of peer review, the basic claim is without foundation. In spite of the different funding systems of Japan, Germany, the Soviet Union, and Britain, which also seem to do good science and technology, these claims persist. What is the best science? Is it better science to win two more Nobel prizes than to teach 4 to 5 years of physics, chemistry, and mathematics to most citizens of a country? None of the letters notes that systems without peer review also flourish in the United States. Some of the premier research institutions in this country, such as the Office of Naval Research (ONR) and the Defense Advanced Research Projects Agency, have supported basic research of the very highest quality without peer review at all. They rely on their own judgment of the investigators' competence and track record. A comparative study of "quality" of ONR- versus NSF-supported research might be definitive.

McCreery and Liebman make unsupported claims that "peer review" is actually able to predict the quality of *research not yet done*. To the contrary, as every journal editor knows, we cannot even judge the quality of *completed work* by peer review. Three sets of three reviewers can give mutually conflicting results. *Physical Review Letters* (*2*) has explicitly acknowledged these difficulties in its historic about-face on peer review of papers.

None of the letter writers mentions the extensive work of Elton and Rodgers (*3*), which has demonstrated the ability to duplicate the results of tedious, expensive "peer review" of departmental quality by simple, quantitative measures (including number of degrees). Despite Liebman's suggestion, where is there any quantitative study to show that a peer review process does better in producing research than even, say, a lottery selection among qualified applicants?

Finally, the concerns of McCreery, Liebman, and Kalt about a predicted lowering of standards by peer-reviewed journals (and by all universities) leaves them hoist by their own petard. If the peer reviewing of *completed* work is so unreliable and easily manipulated, how does one expect it to be accurate and honest as a predictor? They also do not mention that the literature explosion and the multiple authorship they decry happened in the heyday of "peer review." These trends will continue; their effect on the formula I propose is trivial. Since they affect but one term, and since everybody would be working by the same rules, any such trends would be easily normalized out.

The letters and telephone calls I have

received, many from distinguished colleagues, suggest a real readiness for change. We look forward to some analysis and ideas from the NSF, the National Institutes of Health, and the General Accounting Office with respect to a comparison of funding mechanisms.

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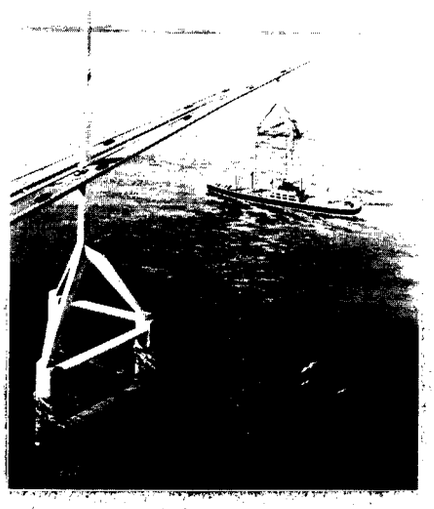
References

1. N. Wade, *Science* **210**, 33 (1980); *Newsweek* **97**, 99 (18 May 1981).
2. R. K. Adair and G. L. Trigg, *Phys. Rev. Lett.* **43**, 1969 (1979); D. Lazarus, *ibid.* **45**, 1605 (1980).
3. C. F. Elton and S. A. Rodgers, *Science* **174**, 565 (1971); *Am. Psychol.* **27**, 197 (1972).

Bridge Design

In his review (15 May, p. 787) of *Long-Span Bridges* (New York Academy of Sciences, 1980), William Zuk does not mention the longest cable-stayed bridge design ever made as a possible future development.



In 1969 M. M. Bascom and I did a rather detailed design study of a cable-stayed bridge for the Strait of Gibraltar in which the cables were suspended from a series of aluminum towers mounted on tension-leg platforms in water to 130-foot depths. Some 15 towers would have been required to cross the 8.2 nautical miles (13 kilometers). The largest ships would have passed easily under the deck and between the spans as shown in the accompanying photo. The design study was accepted by the sponsors, but the prospects of paying traffic were not sufficient to proceed further at that time.

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