Letters

NIH Fraud Guidelines

The editorial "Fraud in science" by Daniel E. Koshland, Jr. (9 Jan., p. 141), states that the National Institutes of Health procedures for dealing with possible misconduct in science (*1*) "seem admirable and appropriate." We take exception to two recommendations made by NIH for awardee institutions, first, to item (b), "protecting the privacy of those who in good faith report apparent misconduct" (*1*, p. 24) and, second, to item (i), "if the possible misconduct is not substantiated undertaking diligent efforts where appropriate to restore the reputation of those under investigation" (*1*, p. 24).

Concerning item (b), we believe this policy is morally wrong and contravenes the rights a citizen of the United States might reasonably expect. We know of no civil court in which an individual is prevented from knowing the identity of his accuser. This should not be the policy of universities, which are supposed to be centers of enlightenment and education. The policy suggests that anonymous informants should be condoned and that, by providing the descriptor "in good faith," some university official will have the wisdom and background to judge whether the individual is acting in good faith.

We believe the following. Those of us who have made a career in science value our integrity and that of our confreres above any other single trait; we rigorously apply this trait to our own research and in evaluating that of our peers; we have a moral obligation to report any suspected errors or suspected dishonesty to the individual whom we suspect, and then, if our doubts are not assuaged, to university officials. We must as a community demand the most rigorous and correct behavior of ourselves and of our peers; at no time should we hide behind a cloak of anonymity, and in no way should NIH or the university encourage or condone such an action.

Point (i) is equally troubling because it implies that regardless of efforts to protect an individual who has been accused, the conduct of an investigation will become general knowledge and will damage the individual during its course. There should be no need to "restore the reputation" of one who is unjustly accused, because in this nation we are innocent until proven guilty. If an investigation is conducted correctly and responsibly, then this recommendation is superfluous.

The federal regulations make no recommendations about how an institution is to carry out its inquiries. If we accept the federal role model, the local institutions will appoint a "Misconduct Policy Officer" or MPO. In considering this we note with amusement that the federal official designated as chief MPO is the Deputy Director for Extramural Research and Training, or DDERT. We do not believe the university should appoint a comparable official whose role will be to deal with "dedirt." Rather, we believe any alleged instance of misconduct should be considered by a panel of peers who are impressed with the solemnity of their task and who have access, as does the accused individual, to all facts and all persons involved in the process of accusation.

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1. *NIH Guide Grants Contracts*, **15** (No. 11) (18 July 1986).

Response: The National Institutes of Health is appreciative of comments by Koshland and by Rosen and Hoffman. The document "Policies and procedures for dealing with possible misconduct in science" published in the *NIH Guide for Grants and Contracts* (18 July 1986) represents interim policies. As stated in the *Guide* (p. 23), the section on awardee responsibilities will be published separately in the *Federal Register* as a notice of proposed rulemaking. The date of publication will be printed in the *Guide*, and we invite comments from others in the scientific community.

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SSC and SDI

I have been following the public debate about the Reagan Administration's proposal to build a 52-mile proton accelerator with considerable interest and some bemusement. In particular, I am more than a little surprised that essentially all of the debate has centered around issues of "big versus little science." Little has been said regarding the Administration's real motives in pursuing such a project.

Given Reagan's history of funding for fundamental research (and other indications that he is not above a certain duplicity at the hands of the "military men"), why have no questions been raised when he announces that unless we build this mammoth accelerator, we will fall irrevocably behind in the subatomic particle race, and when he emphasizes that the results from experiments using the accelerator would have no practical applications?

To my eye, this is pure smoke screen. It seems obvious that any funding for the Superconducting Super Collider is further funding for the Strategic Defense Initiative. The very construction and testing of such an accelerator would seem to involve perfecting technologies that are directly applicable to space weaponry.

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Late Cretaceous Atmospheric Oxygen

An additional note should be added to the comments by Cisowski and Fuller and Argyle (Letters, 17 Oct. 1986, p. 261) on the remarkable finding by Wolbach *et al.* (Reports, 11 Oct. 1985, p. 167) of a Cretaceous-Tertiary soot layer. The amount of reduced carbon sequestered in Middle to Upper Cretaceous organic-rich shales (*1*) might have been balanced during the Cretaceous by a surplus of photosynthesis-derived atmospheric oxygen. For calculation purposes, we make the conservative estimates that (i) 7.22×10^7 square kilometers of the Cretaceous sea floor received substantial black shale deposition (equal to about 20% of the present area of the sea floor); (ii) the average thickness of these organic-rich sediments is 40 meters; and (iii) the average total organic carbon of these sediments is 1.0%. We also assume that the remainder of the global carbon cycle was unable to absorb the surplus oxygen. When one uses these approximations, the net increment to the atmospheric oxygen reservoir is 1.54×10^{20} grams, or about a 3% shift in the composition of the atmosphere toward oxygen. Normal atmospheric oxygen content (about 21%) is generally assumed to have been near present atmospheric levels for at least the last 100 million years (*2*). Watson *et al.* (*3*) show that even wet vegetation is highly flammable when atmospheric oxygen levels reach 25%. An increase in atmospheric O_2 levels to 24% during the Late Cretaceous would have dramatically increased the flammability of moisture-containing vegetation and the sustainability of wildfires and extended the maximum possible distance from

suitable vegetation of an oceanic impact that could still ignite wildfires. An impact may have provided the energy necessary to bring oxygen from a (possibly metastable) level of 24% back down to 21%, a value that seems to reflect a balance between photosynthetic production of hydrocarbons and combustion of terrestrial vegetation.

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REFERENCES

1. P. C. De Graciansky *et al.*, *Nature (London)* **308**, 346 (1984).
2. P. Cloud, *Paleobiology* **2**, 351 (1976).
3. A. J. Watson, J. E. Lovelock, L. Margulis, *Biosystems* **10**, 293 (1978).

Nucleic Acid Database Management

We would like to comment on the database management model presented by the nucleic acid database groups at the joint National Institutes of Health–European Molecular Biology Laboratory workshop "Future databases for molecular biology," recently held in Heidelberg. This model was designed to cope with the explosive increase in the amount of nucleic acid sequence data expected in the coming years. The model should be commended because it calls for the direct participation of the research community in the input and verification of the data. The plan is for the database groups to develop and provide software that allows the investigators to prepare their own data for submission to the databases; quality control mechanisms that automatically check the data will be designed into the software.

Under present policies the preparation and verification of data is the most time-consuming task; because this work is performed by research scientists, it is also expensive. By relegating the role of data preparation to the research community and concentrating on software development, the database groups hope to develop a management system that can cope with the increasing amount of data at minimal cost. For the past 10 years, we have been using software to maintain the quality of the National Biomedical Research Foundation–Protein Identification Resource Protein Sequence Database. Such software can save a considerable amount of time and reduce staff. However, it has limitations, in that it can check only those aspects of the data specified by the designer.

Scientific information is fundamentally different from the data usually treated by computer science in that its nature changes as the understanding of the research community increases. New data often present new problems, and the software must be continually upgraded. We fear that if the nucleic acid database groups stop examining the data, they will not know what changes need to be implemented in software and the resultant software will not be up to the task.

The participants in the workshop presented a visionary picture of the use of future sequence databases as powerful research tools; for example, they could be used to identify relations between sequences, predict structure and function, and allow the detailed chemical mechanisms of gene control and activation to be modeled. Powerful organizational structures could map the interrelations between the data and allow these mappings to be used in generating new information. The resulting information resource would be more than just the sum of its parts. Such uses require highly organized databases in which the relations between each entry and the rest of the database are clearly defined. Quality control in such a structure involves not only validating the information in each entry, but also ensuring the correct representation of the interrelations between the data. The model presented by the nucleic acid database managers does not contain a mechanism for generating or maintaining these organizational structures.

Contributing research scientists most certainly are more knowledgeable about their data than are the database producers; often, however, the scientists do not appreciate the relation of their data to the overall structure of the database. That is the role of those maintaining the database, whose task will consume an increasing amount of our resources.

It was brought out quite clearly at the workshop that the amount of money being spent to maintain the databases is only a tiny fraction of that being spent to generate the data. We must seriously ask ourselves why we are willing to spend so much money generating data but are unwilling to spend enough money to ensure that the data are properly stored and organized and that all possible information is extracted. If the databases can become the powerful research tool envisioned by the workshop participants, it will certainly be worth the investment.

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Saturated Fat Avoidance

I would like to clarify the quotes of what I said to Gina Kolata on the subject of the avoidance of dietary fat (Research News, 23 Jan., p. 436). In countries such as the United Kingdom or the United States, replacement of much of the saturated fat from dairy and other land animal products by other commonly available foodstuffs will reduce plasma low-density lipoprotein cholesterol, which in turn will reduce the likelihood of premature death from coronary heart disease. Such changes are of substantial value and are incorporated into many sets of dietary guidelines. Some experts believe such changes will also help avoid cancer, but the evidence in this respect is less secure than the evidence that they will help avoid coronary heart disease. Hence, I said that when such changes are recommended then it should chiefly be because they *will* help avoid heart disease rather than because they *may well* avoid cancer. Whether or not they also have any substantial effect on cancer, the determinants of plasma low-density lipoprotein cholesterol are collectively of an importance comparable with that of tobacco, and collectively they and tobacco still account for about half of all British deaths in middle age.

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Correction

In Bernard D. Davis' article "Bacterial domestication: Underlying assumptions" (Policy Forum, 13 Mar., p. 1329), the word "no" was erroneously omitted from the last sentence of the first full paragraph in column two on page 1334. The sentence should have read, "But this view builds on a parallel between genetic engineering and the physical technologies, rather than on a much closer model, with very different predictions: the domestication of wild organisms, resulting in enormous benefits and no catastrophes."

Erratum: The opening sentence of Igor B. Dawid's review (6 Feb., p. 695) of *Gene Activity in Early Development* by Eric H. Davidson was incorrectly printed. The first paragraph of the review should have read as follows.

"As in the two earlier editions of Davidson's standard work, much of this new book is devoted to discussion of quantitative work on RNA accumulation and distribution during development. Davidson also discusses in detail spatial regulation of gene expression in the embryo and its relationship to lineage determination, providing a strongly comparative point of view and generating broad and well-balanced interpretations of large bodies of connected facts. The result is an excellent book that represents the field from a personal yet broadly convincing vantage point."