Letters

HETE: New Observations

In her article "Thromboxanes: The power behind the prostaglandins?" (Research News, 21 Nov., p 770), Gina Bari Kolata indicates that HETE, the lipoxygenase metabolite of arachidonic acid, has no known biological function. Recently, we have shown that HETE is at least biologically active as a chemoattractant for human polymorphonuclear leukocytes in vitro (*1*). Although the chemotactic role of HETE in vivo remains to be determined, it is clear that both of the endogenous arachidonate oxygenating systems of platelets can elaborate mediators of inflammation. These new observations suggest that the platelet is a locus for linking hemostasis with certain inflammatory events via selective oxygenation of arachidonic acid.

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References

1. S. R. Turner, J. A. Tainer, W. S. Lynn, *Nature (London)* **257**, 680 (1975).

X-ray Scanners

Arthur L. Robinson's Research News articles "Image reconstruction (I): Computerized x-ray scanners" (7 Nov., p. 542) and "Image reconstruction (II): Computerized scanner explosion" (14 Nov., p. 647) point to the critical societal issue emerging from the development, marketing, and use of these remarkable devices, of whether the ". . . cost will translate into a quantifiable improvement in health care. . . ." Yet Robinson shuns the dispassionate examination of the issue by stating, "But to those who have suffered through pneumoencephalography, the value of the CAT [computerized axial tomography]-scanner may be more obvious."

It is not axiomatic that the patients studied by the 200 scanners now operating in this country (many of them " . . . up to 16 hours a day, 5 or 6 days per week") would otherwise have been slated for pneumoencephalography. It is not known what portion of those patients truly require the detailed work-up for neurological disease which calls for that examination. For that matter, it is not known what portion are being sent for CAT-scan in lieu of consultation by a neurologist or even in place of a detailed work-up by a general practitioner. It is also not known how many physicians, taking on the investment opportunity which may accompany marketing of some of these devices, now find themselves saddled with a monthly mortgage payment that might create a conflict of interest which could influence usage.

While "full-fledged breakthroughs are rare," as Robinson states, quantum leaps in medical diagnosis or therapy do happen with enough frequency to suggest cautions worth paying attention to. Not uncommonly, such developments are hailed with a lack of discrimination, which leads initially to a sacrifice of resources, both human (in terms of morbidity and mortality of patients) and financial. The relative mix of morbidity, mortality, and dollar expenditure will vary with the particular "breakthrough," but it is the responsibility of both health care leaders and an informed public to avoid a cycle of enthusiasm, uncritical embrace, overzealous application, and ultimate retrenchment—with its inevitable embarrassing display of the irrationality of those who claim to embrace the scientific method, to say nothing of the prodigality. (In fairness, the fault is not to be laid entirely at the doorsteps of the physician and the hospital. Demands from patients on the one hand, and pressures from the industries that foresee expanding and profitable markets, on the other, must also be indicted as powerful sources in the untrammeled explosion of use that follows a "breakthrough.")

Perhaps it is reasonable to restrict the CAT-scanner to a few university hospitals and to insist upon the application of protocols for study of its proper use, on the grounds that society can afford another 2 or 3 years of wait more readily than they can afford the waste that uncontrolled use will likely bring over the same period. The problem will still remain, once such studies are completed, of whether the nation's physicians will follow the developed guidelines. It is likely that they will. Indeed, the likelihood that they will exercise prudent judgment, once "prudence" is defined, is much greater if, by that time, there is not a CAT-scanner gracing the diagnostic radiology suite of every hospital, clinic, and office in this country.

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Geothermal Energy Production

Years ago, when developing the use of tritium (radioactive isotope of hydrogen, 12-year half-life) for hydrologic purposes, my students and I measured the tritium in many different kinds of natural waters. Others, particularly coordinating with the International Atomic Energy Agency, have assembled a vast library of data on the surface waters of the world.

I now serve as a member of the University of California Energy Council, a body created to cooperate with the new California Energy Commission. The council has been reviewing the chances of generating energy by means of a "dry" geothermal method—that of drilling into an extinct volcano (if there is such) and injecting water, which should then reissue as steam. The Los Alamos Laboratory already has major efforts under way on this project, and Los Alamos, being operated by the University of California, is represented on our Energy Council.

It is becoming clear that there is nothing wrong with this "dry" geothermal method provided that the geochemistry is favorable. The fear is that the high-pressure, high-temperature conditions will cause the water to be so corrosive that it will plug the cracks making contact with the hot rock and thus block the flow of steam.

In this connection, I reexamined our early tritium data for natural water (*1*) and found evidence that the steam at the Geysers geothermal field near Calistoga, California (the site of a large geothermal electric power generating plant) is formed, at least in substantial part, from *rainwater of recent origin.*

By chance, an abrupt 50-fold increase in tritium concentration in rain occurred in Chicago and elsewhere in the Northern Hemisphere sometime between 2 and 19 March 1954, following nuclear weapons tests. The samples at the Geysers were taken on 23 March 1954 by D. E. White of the U.S. Geological Survey. Measurement revealed that the hot water was about 10

percent rainwater less than 2 weeks old. Four Nevada hot springs sampled on 11 March 1954 showed somewhat similar results. It therefore seems likely to me that this "dry" geothermal method, with its enormous potential, must be feasible.

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References and Notes

1. W. F. Libby, *J. Wash. Acad. Sci.* **45**, 301 (1955).
2. Contribution No. 3570 from the Department of Chemistry, University of California, Los Angeles.

NSF Funding Patterns

In his letter of 3 October (p. 10) Paul Gordon expresses serious doubt about the validity of the conclusions that I drew from my statistical study of the funding patterns of materials departments through the Metallurgy and Materials (M & M) Section of the National Science Foundation (NSF) (News and Comment, 22 Aug., p. 622). He cites my presumed "highly questionable use of the first-name citation index as a measure of the quality of university materials departments." However, in my study I did not *assume* a connection between first-author citation counts and scientific excellence but *found* a strong statistical correlation between first-author citation counts and success in the competition for M & M funding, both nationally (that is, disregarding departmental affiliation) and in intradepartmental competition. The only assumption made, therefore, was that, other things being equal, the proposal selections were made on the basis of scientific excellence. My statistics also indicate, however, that the funding experiences of faculty were critically dependent on their departmental affiliation; that is, things are *not* equal for different departments.

The data indicating a strong positive correlation of M & M funding with first-author citation counts are in agreement with the findings in an exhaustive study of the funding of chemistry departments through NSF's chemistry section (*1*). This study was recently discussed by Wilson (*2*). In (*1*) a positive correlation was found between peer review ratings and citation counts, over a 5-year period and not restricted to first authors; it was also found that "no proposals from very highly cited authors receive low ratings." This is in contrast to the result in my study that, apparently because of departmental biasing, materials departments with the highest citation counts per person receive the lowest M & M funding per person in spite of vigorous proposal activity.

In the same study of funding through the NSF chemistry section, a close correlation was found between the ranking of chemistry departments in the order of their citation counts per faculty member and the ranking of these departments by other measures, including the Roose-Andersen jury (*3*). This aspect of the study was summarized as follows: "Consideration of all the data at hand has led us to conclude that a ranking based on the departmental average of citations per faculty member is the preferred criterion for ordering chemistry departments based on use of their research results by the scientific community." Gordon's belief regarding the ten materials departments with the highest citation counts per faculty member "that at least four, and possibly six, of these departments would not come close to being rated among the top ten if the opinions of the . . . materials community were polled" is thus in contradistinction to the experience with respect to chemistry departments.

Admittedly, departments emphasizing undergraduate programs will not show up as well in citation studies as those concentrating on graduate education. On the other hand, M & M funding is not intended to support undergraduate education, and the supply of B.S. graduates referred to by Gordon is not affected by M & M funding. In any event, the issue is not very pertinent because I do *not* advocate funding of faculty in accordance with the citation ranking of their departments but, on the contrary, I question patterns of funding of faculty, in response to individual research proposals, which indicate bias on account of departmental affiliation.

Gordon's doubts about whether "science will serve the United States best by moving toward an elitist national scientific effort," which he believes to be specifically one of my assumptions, conflict with the policy on which NSF funding in response to individual research proposals is based. Although formula funding, which is independent of research proposals, is practiced widely in other advanced nations, the "elitist" goal that the best scientists shall be supported in preference to the less able ones is the raison d'être of our national policy of proposal writing and reviewing. This goal of "elitist" funding is judged to be desirable enough that, as a nation, we are willing to spend much time and money to achieve it. Perhaps the policy *should* be questioned, but presumably there is agreement at least within the NSF that it is sound and that the best scientists and the best departments should be supported with above-average funding.

In this connection, the issue of Materials Research Laboratory (MRL) funding raised by Gordon is pertinent. My statistics indicate that there is considerable bias in favor of MRL departments compared with non-MRL departments in the competition for M & M funding. Even so, because MRL departments tend to have high citation counts per faculty member, the overall bias against the most highly ranked departments also causes top MRL departments to be statistically underfunded with respect to M & M awards. Further, none of the departments that are top-ranked have been elevated to MRL status since the 1971 organization of the Division of Materials Research, to which both the M & M and the MRL sections belong. Rather, all new MRL departments are mid-ranked in my study, and it is these which statistically receive the most liberal M & M funding. Such patterns are hardly a reaction against "elitism." In effect, proposals to NSF are close to unsolicited offers to sell research in the national interest to be paid for by the taxpayers. Therefore an analogy to bidding on contract is apt, and we do not consider it "elitist" if, in accordance with the law, the lowest bidder receives the contract. Thus, subject to the avoidance of undue funding concentrations, for a given amount of research, the lowest bidder who can give the highest returns should receive the award. Statistical indications are that this ability is correlated with high numbers of first-author citations.

All in all, my data indicate the superimposition of strong departmental bias on a selection system which distributes funding *within* departments in accordance with scientific merit that is statistically correlated with high first-author citation counts. The existence of departmental funding "quotas," largely independent of the number and quality of proposals submitted, is implied in this result. The reference to an "old-boy network" attributed to E. Creutz (News and Comment, 22 Aug., p. 622) may reflect the fact that I had pointed out to him a statistical correlation between the size of such "quotas" and relations of the recipient departments with NSF through study or employment, or both.

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References

1. National Science Foundation, "Directors program review, 1973" (NSF internal report, National Science Foundation, Washington, D.C., 1973), pp. 21–34.
2. M. K. Wilson, *Annu. Rev. Phys. Chem.* **26**, 1 (1975).
3. K. D. Roose and C. J. Andersen, *A Rating of Graduate Programs* (American Council on Education, Washington, D.C., 1970).