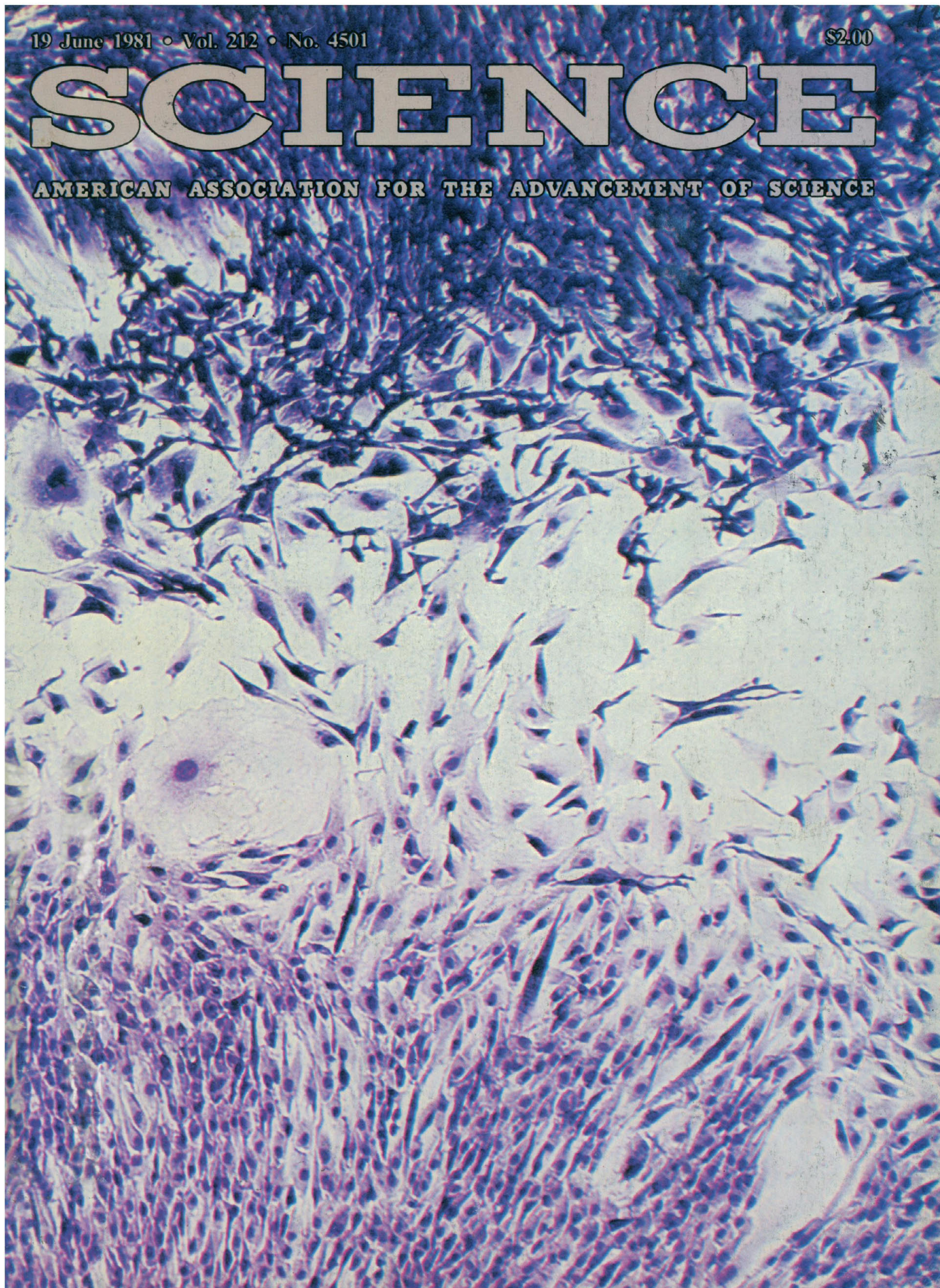


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A photograph of a Beckman TJ-6 centrifuge. The machine is a dark, boxy unit with a large circular lid that is open, revealing the interior rotor. The rotor is black and holds four sample tubes in a cross pattern. The tubes contain liquids of different colors: orange, blue, green, and red. The front panel of the centrifuge is dark and features several controls: a large white knob on the right for speed, a smaller white knob for time, and a power switch. The brand name 'BECKMAN' and 'Model TJ-6 Centrifuge' are printed on the front panel. The background is a light-colored wall with a repeating pattern of the text 'The TJ-6 is an excellent product. We're delighted we have one.'

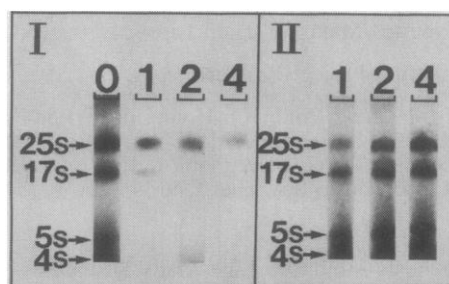
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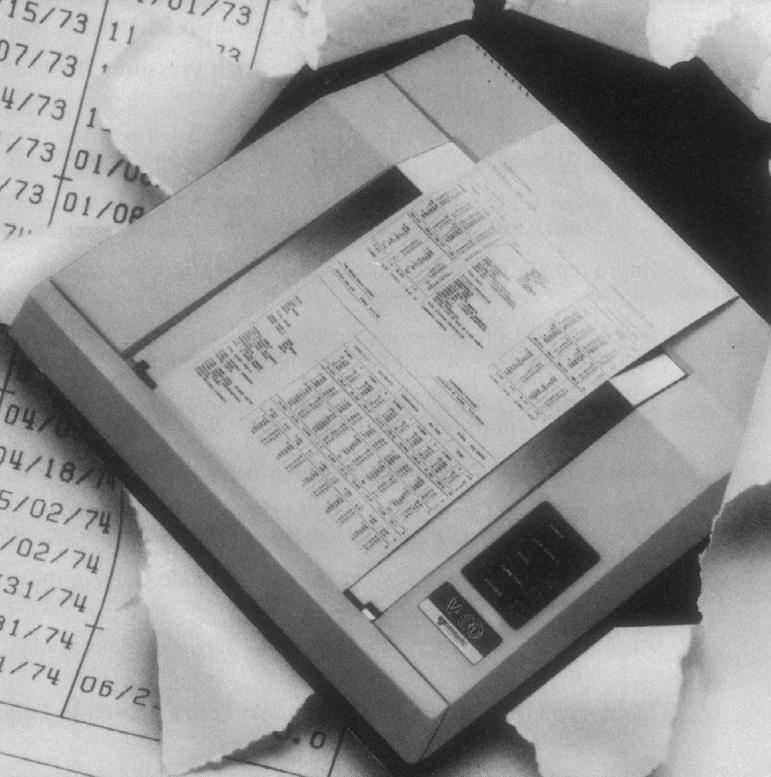
Herman Pollack
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COVER

Edge of a morphologically transformed colony (top) of Syrian hamster embryo cells contrasted with the edge of a normal colony. Morphologically transformed cells, which progress to neoplastic cells, are characterized by dis-oriented, basophilic cells with a decreased nuclear/cytoplasmic ratio. Transformed colonies are induced by many carcinogens, for example, benzo[a]pyrene and diethylstilbestrol. Diethylstilbestrol, however, transforms the cells without measurable gene mutation at two loci (about $\times 47$). See page 1402. [Chester Reather, Johns Hopkins University, Baltimore, Maryland]

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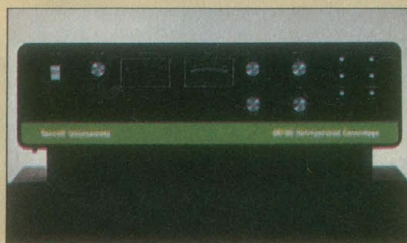


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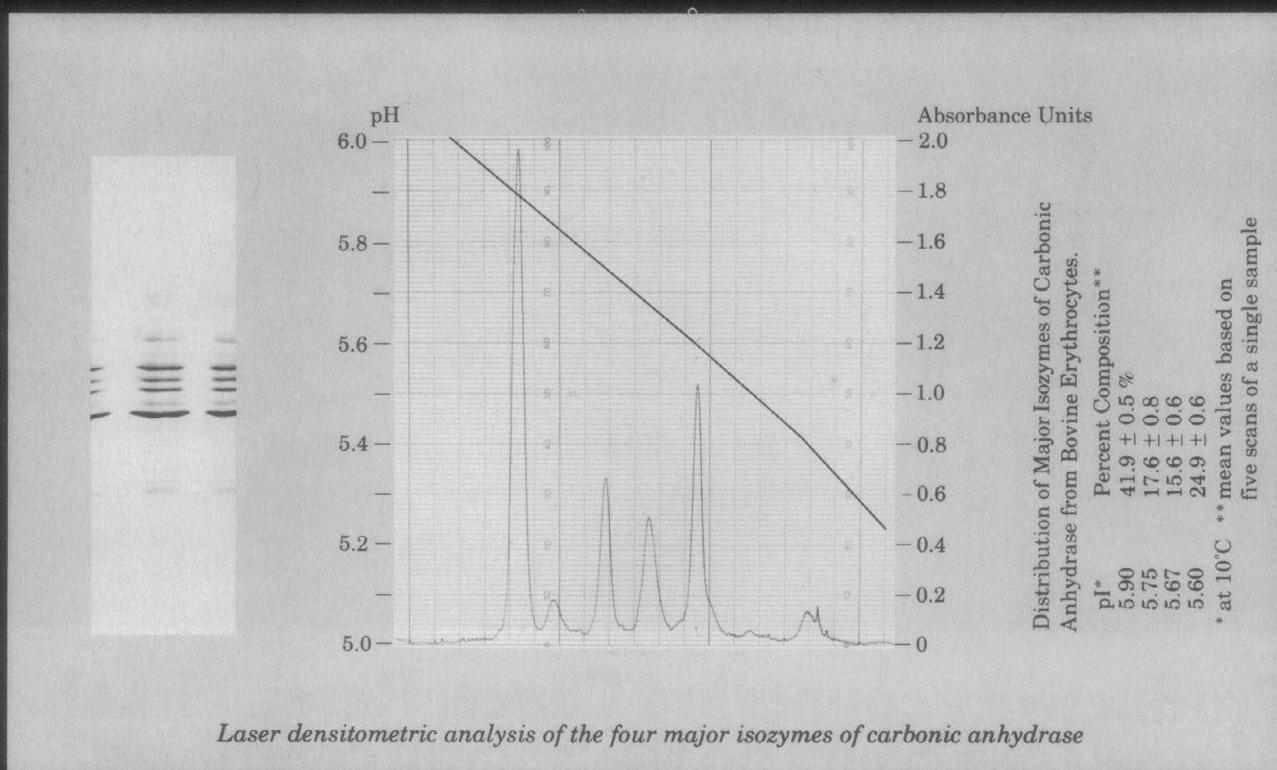
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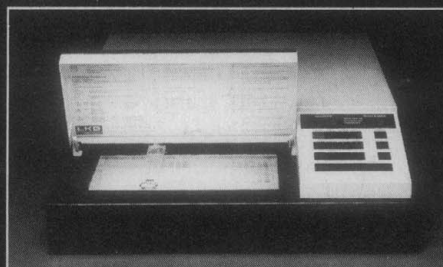
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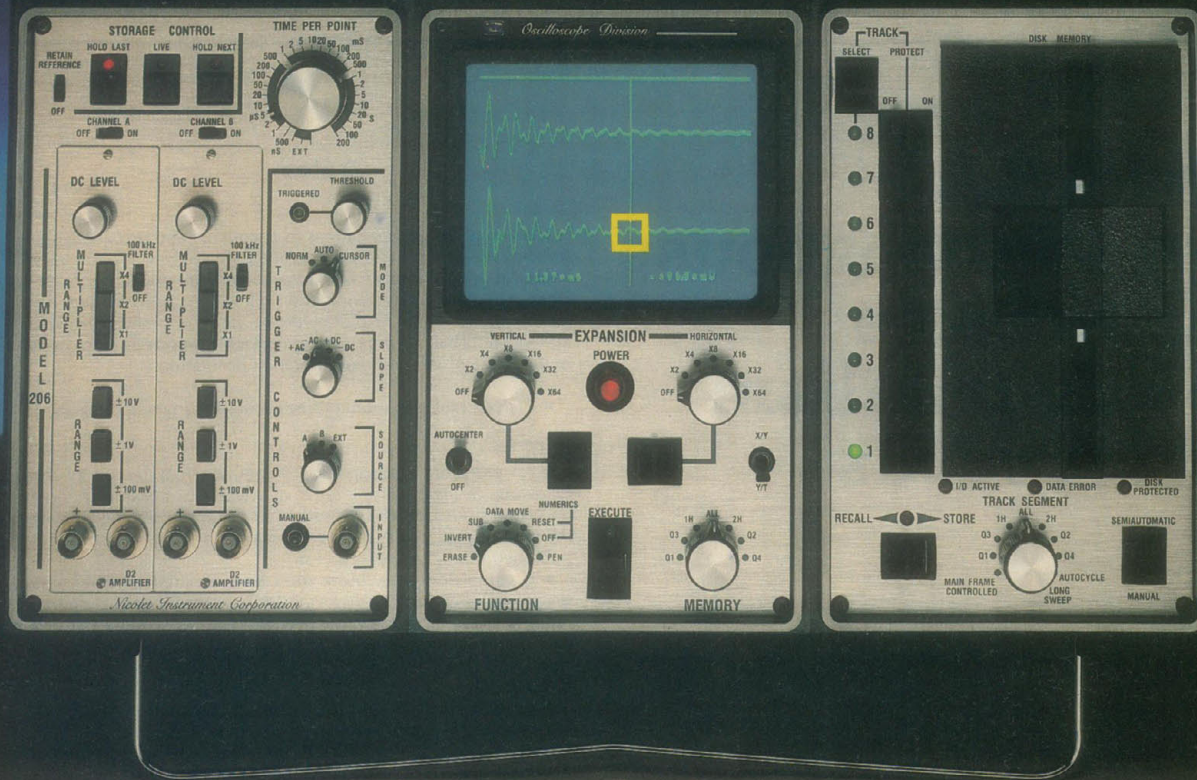
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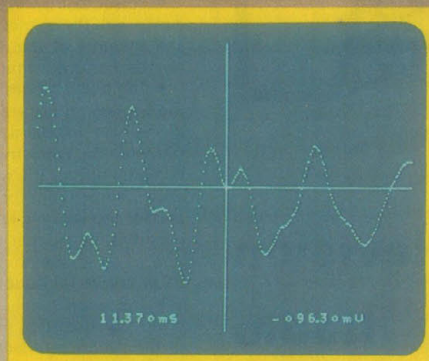
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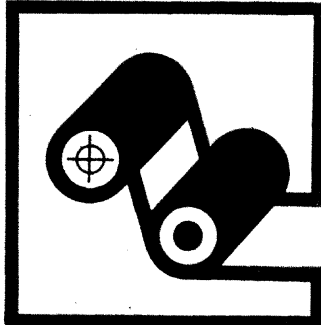
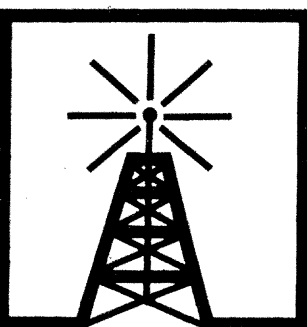
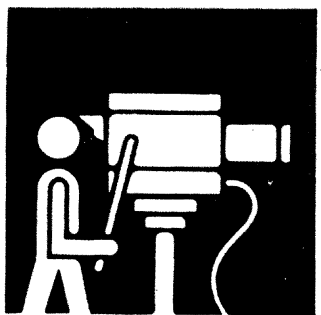
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- The awards will be presented at the dinner meeting of the National Association of Science Writers during the Annual Meeting of the American Association for the Advancement of Science in January 1982. Travel and hotel expenses of the awards winners will be paid. Entrants agree that, if they win, they will be present to receive their awards, unless prevented by circumstances beyond their control.

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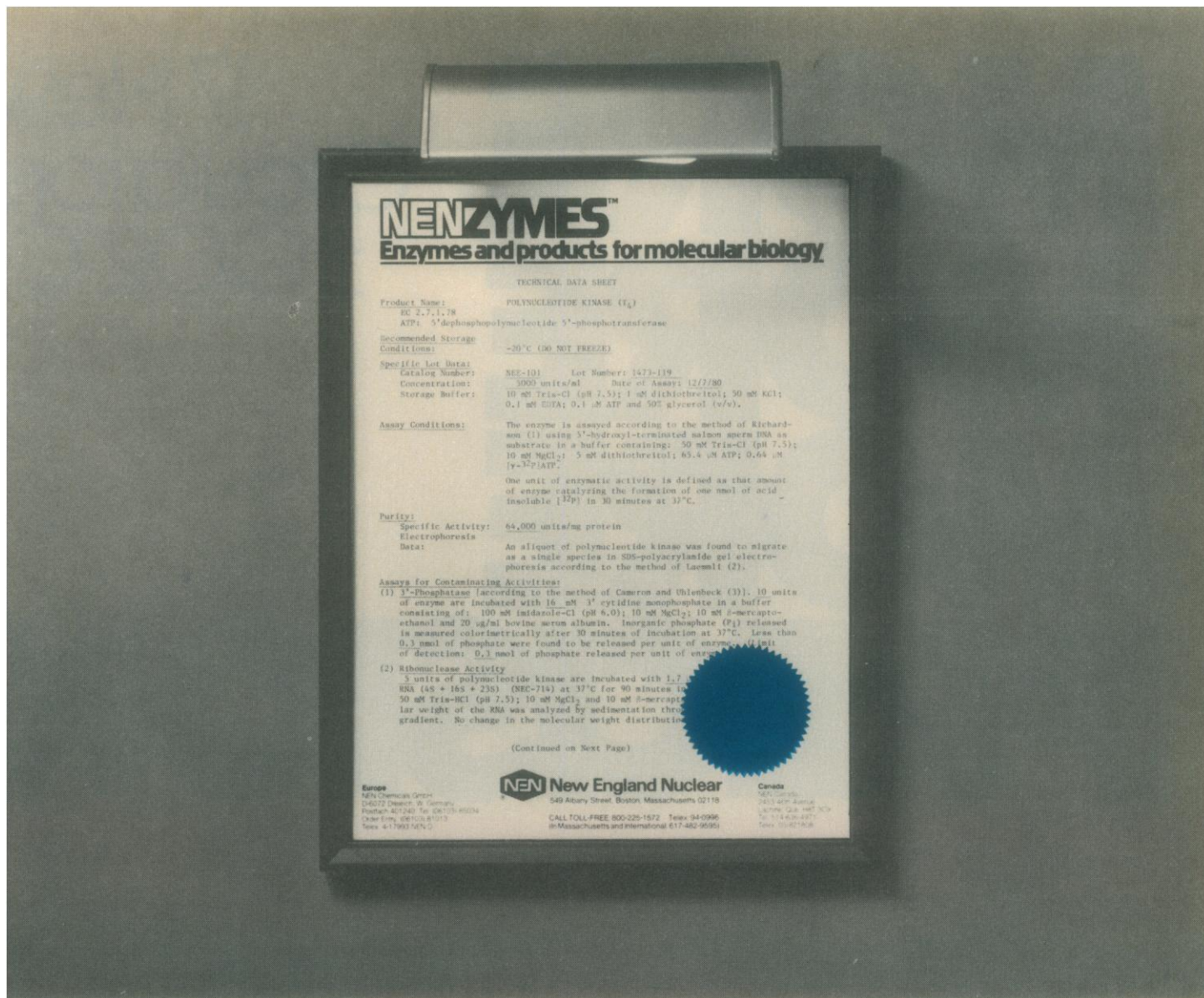
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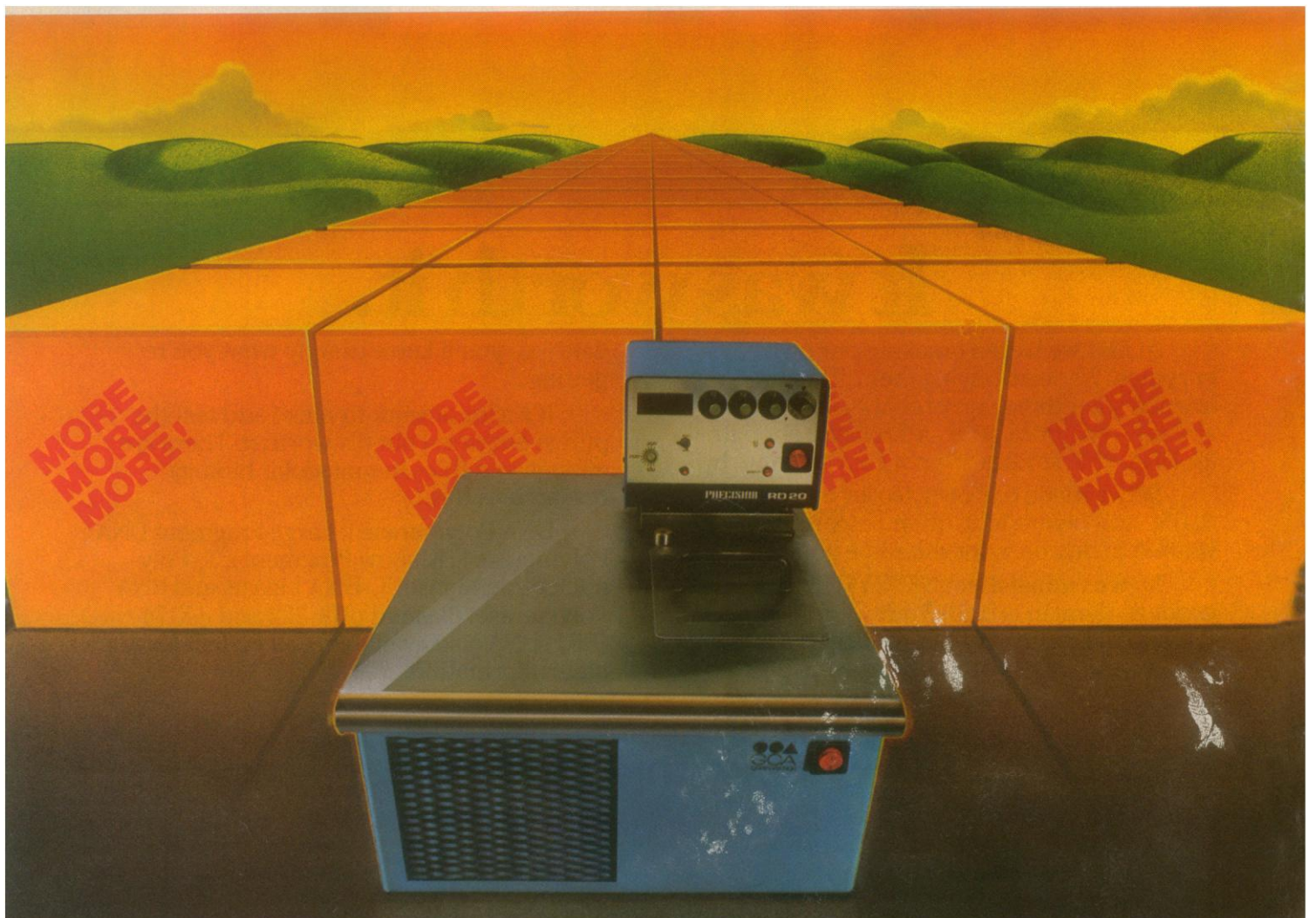
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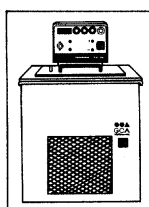


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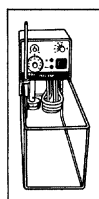
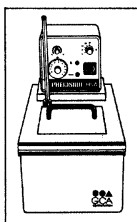
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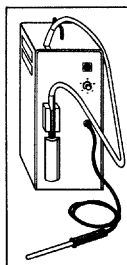
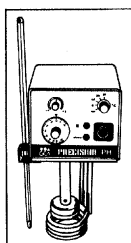
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LETTERS

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.

JON C. LIEBMAN

*Department of Civil Engineering,
University of Illinois,
Urbana 61801*

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.

RICHARD L. MCCREERY

*Department of Chemistry, Ohio State
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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-



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

MICHAEL W. BERNS

*Department of Developmental and
Cell Biology, University of
California, Irvine 92717*

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.

RUSTUM ROY

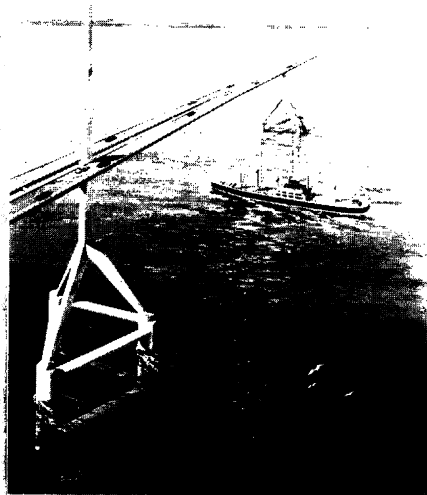
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Pennsylvania State University,
University Park 16802*

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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.

WILLARD BASCOM

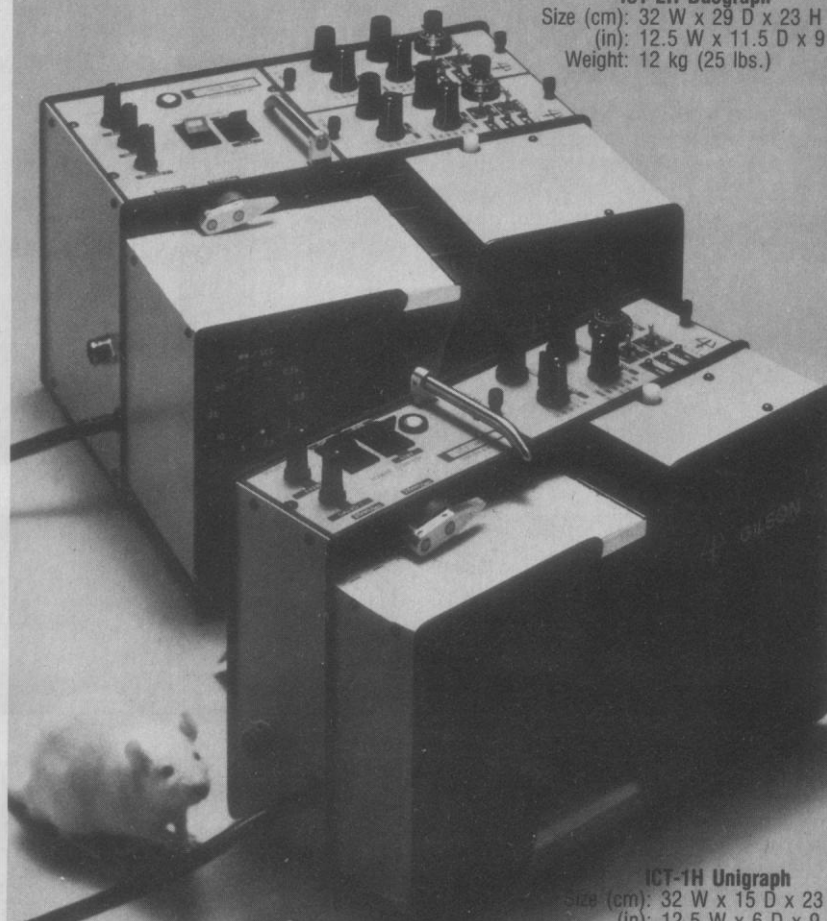
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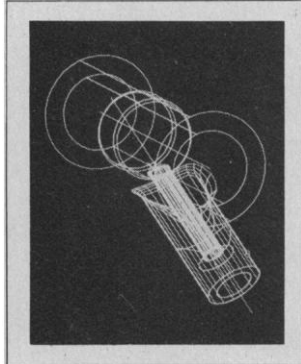
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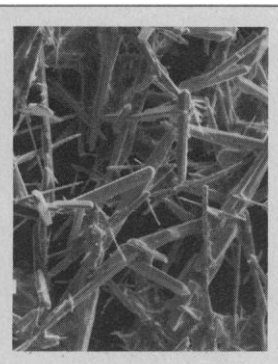
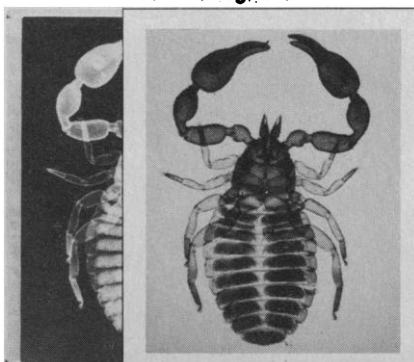
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Coal Research

Prepared under the auspices of the National Research Council, an authoritative book on coal has recently appeared that is the first comprehensive treatment of the subject in 18 years.* Publication now is particularly timely, for there is a growing consensus among energy experts that during the remainder of this century, coal will have the major role in replacing part of the energy and chemicals currently obtained from petroleum. About 75 percent of the coal used at present in the United States is burned by electrical utilities. Another 14 percent is used to make coke, and most of the remainder is burned for industrial heat. Heat energy from coal costs much less than that from petroleum. Large potential applications include gasification to produce methane, chemical feedstocks, and liquid fuels. Exports of coal are growing and are likely to become an important factor in global economics.

Research on coal has been conducted for more than 100 years. Through most of this century activity in fundamental research has been at a low level. In terms of constant dollars, support for coal research in the United States was about the same in 1970 as it was in 1910. From 1945 to 1973 the coal industry was barely able to exist in competition with petroleum and natural gas. Many of the studies that were conducted in the United States were performed by the Bureau of Mines. Some of that work as well as research elsewhere, notably in Germany, was devoted to efforts to establish general principles about coal, but these were not very successful. A principal reason is the great variability of the material.

Samples of coal derived from many horizons and localities are in some ways different from each other. The sulfur content of coals ranges from 0.2 percent to about 7 percent. The ash fraction varies from a few percent to 30 percent or more. Wide differences are encountered in the chemical nature of the inorganic constituents. Oxygen content ranges from less than 3 percent in an anthracite coal to more than 30 percent in a lignite. These differences reflect variations in the composition of the original organic material, in burial environment, in associated inorganic matter, and in burial history. With the passage of time innumerable chemical reactions occur within the organic matter, leading to complex substances of high molecular weight. Small wonder that most consumption of coal has been by burning it and advances in technology have usually been based on empiricism rather than theory. But in the future, as uses increase, determined efforts will be made to improve the characterization and understanding of coal. In such efforts workers will build on the body of existing knowledge. This knowledge has been compiled by experts in the new book.

This volume (2,395 pages, about 10,000 citations, and 1,000 tables or figures) is the third in a series bearing the title *Chemistry of Coal Utilization*. Its 31 chapters cover the science of coal and the many facets of technology involved in its use. For example, there are chapters on the geology, petrography, physics, and chemistry of coal, and on pyrolysis, combustion, gasification, and liquefaction. Environmental aspects are also treated. The book was prepared by 60 authors and coauthors, all experts in their fields.

As might be expected, the quality of the chapters is not uniform. Some are more tightly written and contain more new information than others. A notable example is a chapter entitled "Coal ash—its effect on combustion systems." A good comprehension of this topic could lead to enhanced reliability of steam boilers and substantial savings of operating and capital costs. A principal blemish of the book is that part of the material in it is outdated. Some authors delivered quickly but others were slow; careful review procedures were also time-consuming, and the large size of the book contributed to delays at the publisher. Nevertheless, this is a uniquely valuable book. It is not for the casual reader, but serious scholars in the field of energy will find it a must.—PHILIP H. ABELSON

**Chemistry of Coal Utilization*. Second supplementary volume. Martin A. Elliott, Ed. (Wiley-Interscience, New York, 1981). \$165.

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