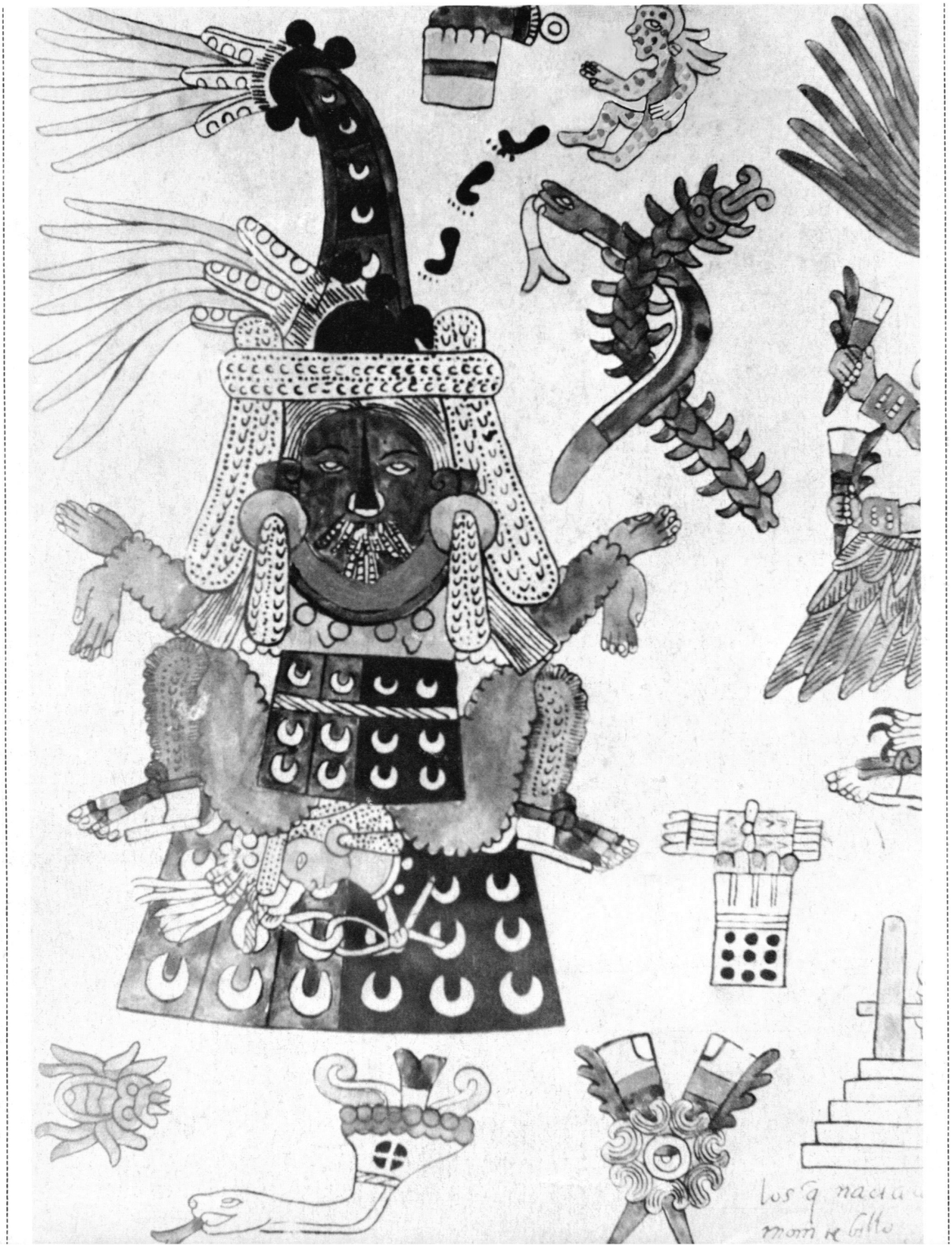
SCIENCE

18 April 1975

Vol. 188, No. 4185

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE



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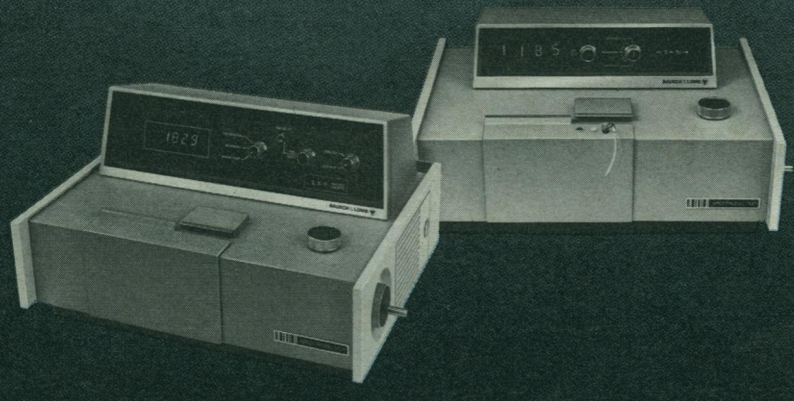

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SCIENCE

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COVER

Goddess Tlazolteotl, the goddess of childbirth, midwives, and medicine. Drawing was taken from the Codex Borbonicus, a pre-Columbian Aztec codex, Folio 13. See page 215. [Gary Stroebel, University of Utah]

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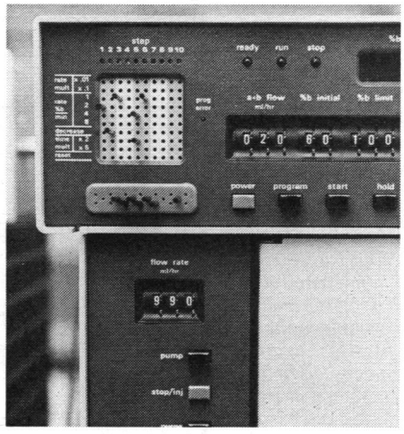

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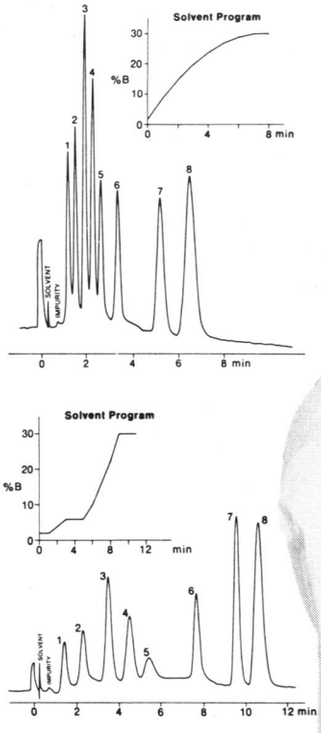
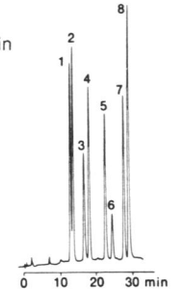
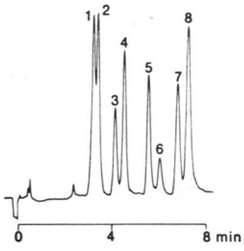


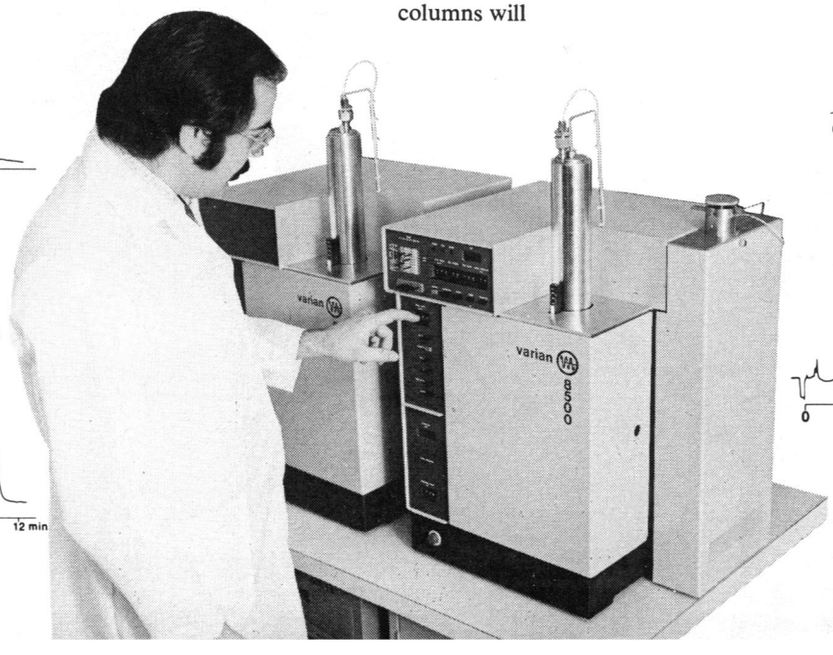
Figure 2.
Faster PTH Amino Acids Separation

30 minutes
Flow Rate: 2.0 ml/min
Pressure: 1700 psi





8 minutes
Flow Rate: 8.0 ml/min
Pressure: 8000 psi



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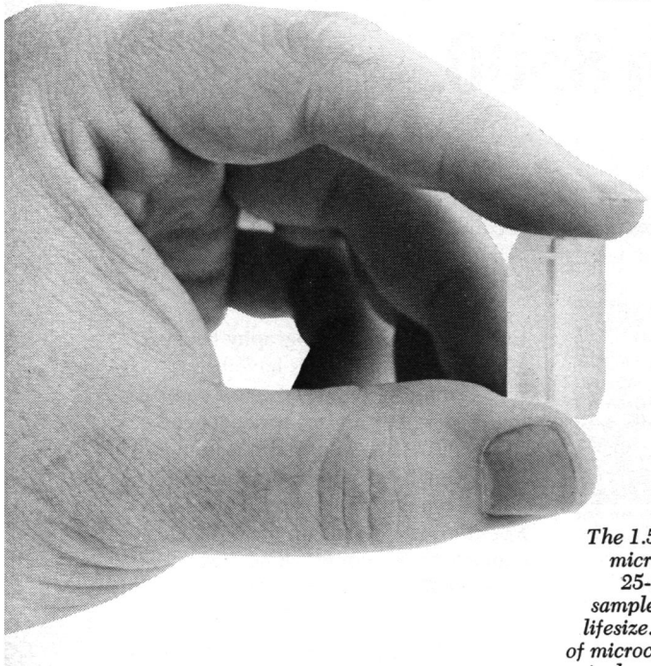
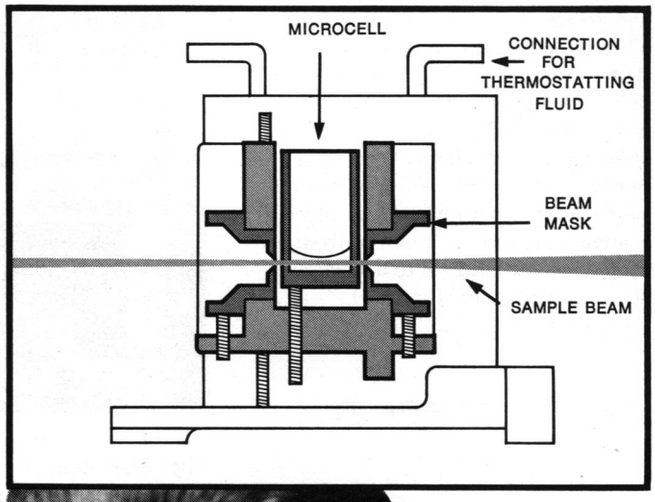
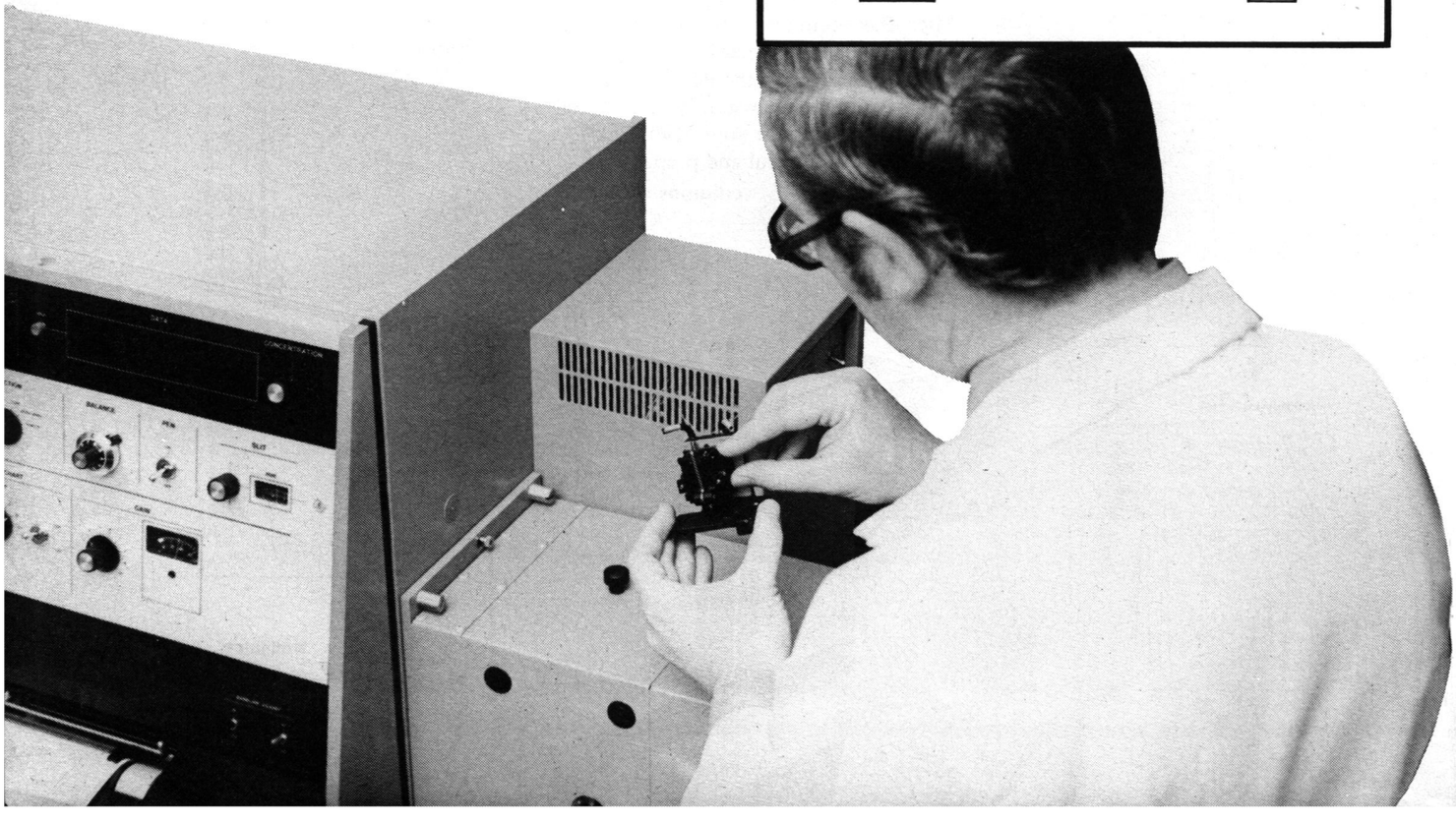


The 1.5 x 10 mm microcell with 25-microliter sample is shown lifesize. Diagram of microcell holder is shown at right.





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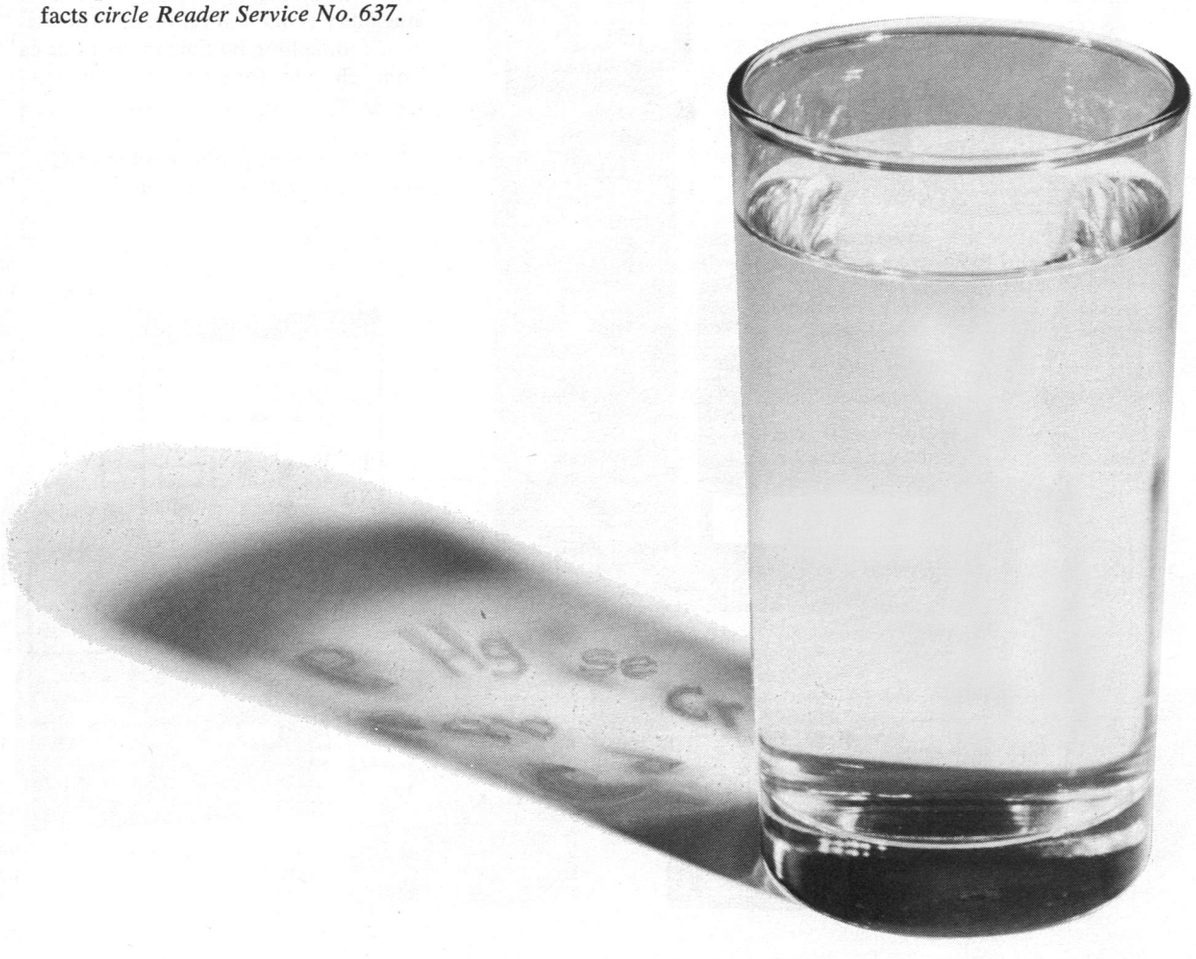

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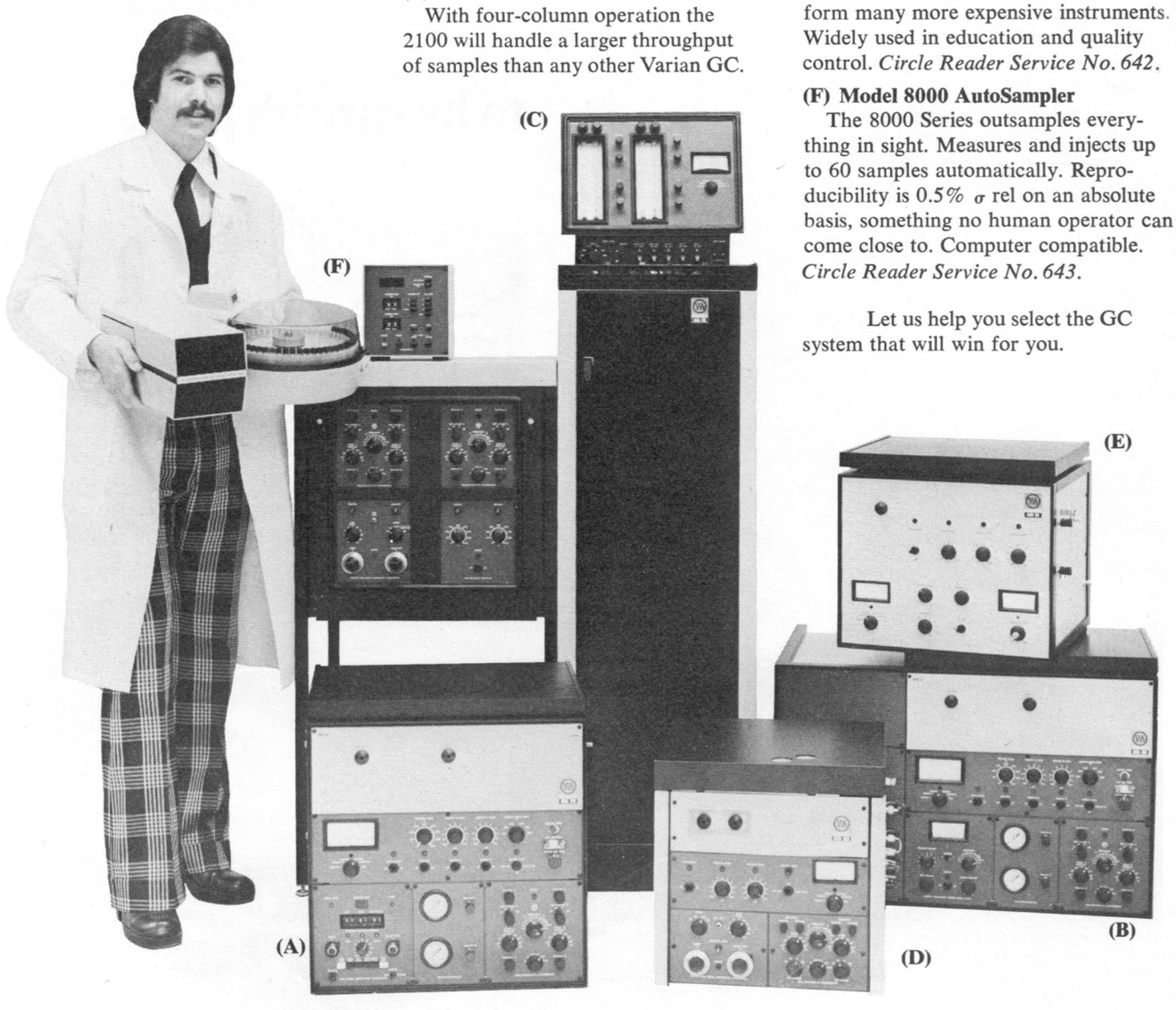

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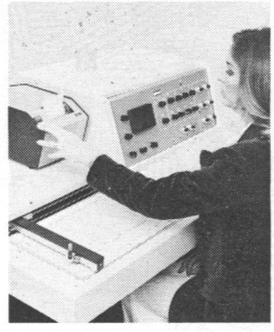

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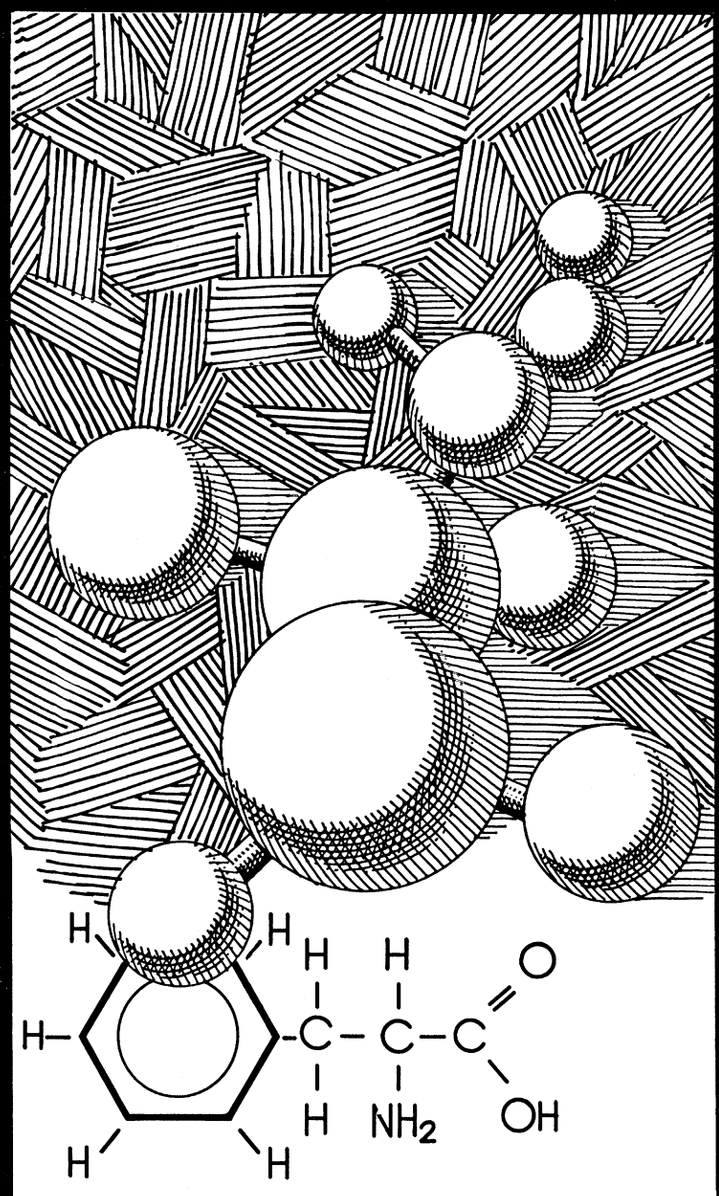

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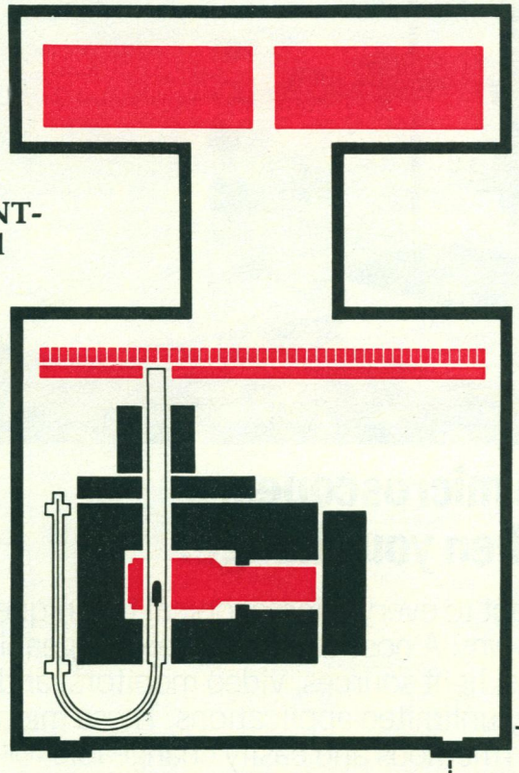

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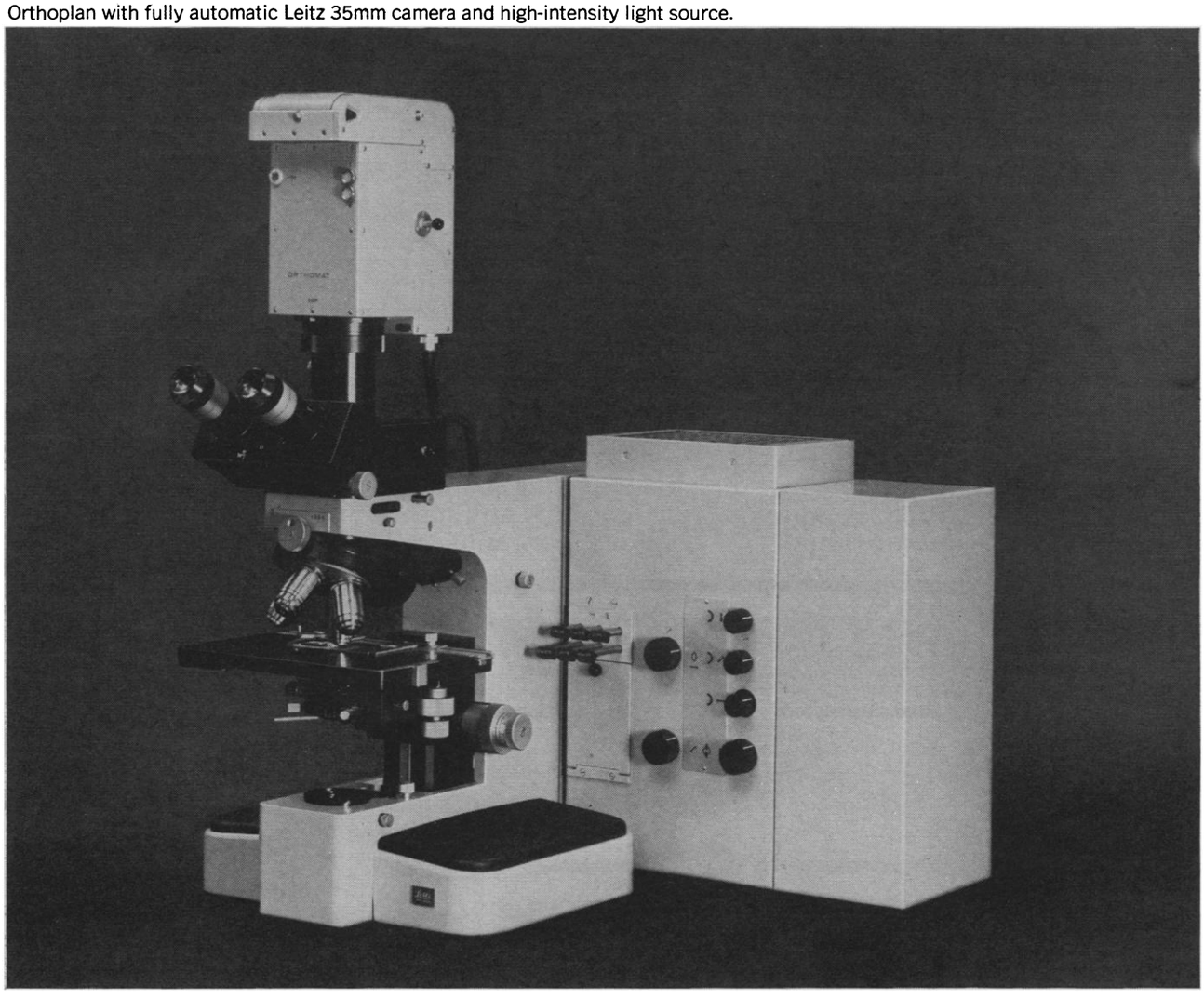

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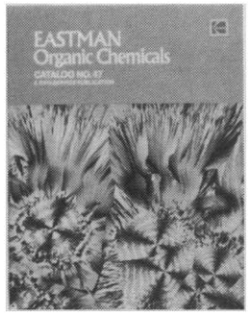

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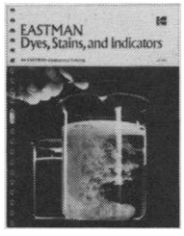

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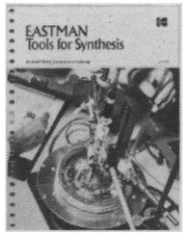

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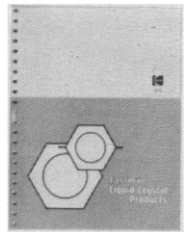

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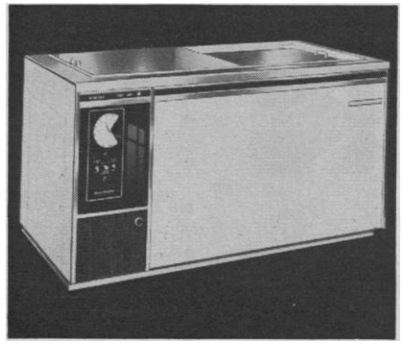

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to survive at least 6 years (*4*). Mice, being short-lived, should have cells with less than the human doubling capacity of 50. However, labeling studies of mouse tongue epithelium showed a minimum of 146 and an average of around 565 doublings over the life-span, with no significant difference in cell division betweeen 3-, 13-, and 19-month-old animals (*5*). In tissues showing decreased cell proliferation with age, there is no dying out or loss of cells (*5*). Martin, Sprague, and Epstein cultured human cells, and although their report is frequently cited as supporting the Hayflick model, inspection of the data indicates there is no significant difference in doubling capacity of cells obtained from donors 20 to 90 years of age (*6*).

Available evidence overwhelmingly supports the view that in mammals there is no generalized age-associated loss of stem cell ability to divide, followed by the dying out of these cells. Reported alterations in numbers of proliferating cells with age could result from comparing growing animals with older ones, from changes in cellular environment, or from changes elsewhere in the body, particularly in stimuli to growth or cell turnover, and not represent intrinsic capacity for cell division. Before committing resources and effort to the study of a model, it should be ascertained that the model bears some relationship to the phenomenon in question.

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Cost of Fuel

Before the energy crisis hit last year, the University of Massachusetts was paying $4 per barrel of oil and $16 per ton of coal. Our latest costs are $13 per barrel of oil and $70 per ton of coal. Thus the cost of coal has increased by a factor of 5.4 while the cost of oil has increased by a factor of 3.25. At current prices, coal costs $2.60 per million Btu's (British thermal units), while oil costs $2.06 per million Btu's. It does not appear that the increase in the cost of coal could possibly result from increased labor prices or from a concern that our coal supplies—which are much more extensive than all of the oil in the Middle East—could be depleted, but rather from companies making great profits from the crisis that is facing our nation. We are essentially unable to influence the price of imported oil, but we could place controls on the price of domestically produced coal. In my opinion this should be an immediate goal of our federal energy policy.

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Peer Review: Distribution of Reviewers

The peer review system of evaluating grant applications has been criticized by both scientists and nonscientists. Much of the criticism, especially from within the scientific community, has been based on the fear that the system is operated by a self-perpetuating oligarchy. Few facts about the actual operation of the system have been available. The Public Policy Committee, Division of Biological Chemistry of the American Chemical Society, has examined the composition of 11 National Institutes of Health (NIH) review groups from 1964 to 1973 by listing the names and institutions of the members as published in the 1964, 1968, 1972, and 1973 editions of *NIH Public Advisory Groups*. Since members serve 4-year terms, this list should include essentially all those who served during this period. Because of the interests of our division, we chose those study sections most likely to review basic research grant applications in biochemistry. The sections we considered were: Allergy and Immunology, Arthritis and Metabolic Diseases (Program Project Committee), Biochemistry, Biophysics and Biophysical Chemistry (A and B), Endocrinology, Medicinal Chemistry (A and B), Microbial Chemistry, Molecular Biology, and Physiological Chemistry.

During the stated period, 366 individuals served on the 11 study sections; only 7 of these served twice. They were affiliated with 113 institutions that were distributed geographically as follows.

Group 1. New England, plus Delaware, New Jersey, New York, Ohio, and Pennsylvania. Of the total number of reviewers, 38 percent, or 140 individuals, came from 48 institutions. Thirteen of the institutions contributed five or more individuals; of these, only the University of Pennsylvania contributed nine or more.

Group 2. Alabama, Florida, Georgia, Kentucky, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, and West Virginia. Of the total, 17 percent, or 61 individuals, came from 25 institutions. Duke, Johns Hopkins, National Institutes of Health, and Vanderbilt contributed five or more; none contributed nine or more.

Group 3. Colorado, Illinois, Indiana, Iowa, Kansas, Michigan, Minnesota, Missouri, Nebraska, and Wisconsin. Of the total, 22 percent, or 81 individuals, came from 20 institutions. Eight of the institutions contributed five or more and, of these, the University of Illinois and Washington University, St. Louis, contributed nine or more.

Group 4. Arizona, California, New Mexico, Oregon, and Washington. Of the total, 23 percent, or 84 individuals, came from 20 institutions, six of which contributed five or more individuals. Of these, Stanford, University of California at Los Angeles, and the University of Washington contributed nine or more.

In order to get some rough idea of the pool from which these reviewers were drawn, we counted the number of current grants reviewed by these same study sections that have been continuously funded for six or more years. This number should approximately reflect the number of projects that have successfully competed for funding more than once and may give some indication of the number of scientists who are qualified by experience alone to sit on review panels. There are 1167 such grants currently funded. If this number approximates even within a factor of 2 the number of scientifically mature individuals in these areas of science, it appears that a substantial fraction of such scientists serve on a peer review panel at some time in their career.

These data do not prove that the study sections are free from bias; for example, we did not look at the replacement sequence in particular study sections to see if "scientific lineage" is a major determinant in the composition of the study sections. Nevertheless, the data do show a wide representation, they show that no small group of scientists has served repeatedly on any of these review groups, and they show that certain prestigious departments or schools have not had a disproportionate voice in the funding process. We feel that the data are consistent with the confidence that the scientific community has generally had in the NIH peer review system.

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Notes

We thank Solomon Eskenazi of the Division of Research Grants, National Institutes of Health, for supplying the computer printout from which we obtained the number of grant holders.

Insect Control

In discussing future means of controlling insect populations, Djerassi, Shih-Coleman, and Diekman (15 Nov. 1974, p. 596) fail to emphasize a method of biological control that may deserve more attention than it has received since its discovery (*1*), namely, introduction of alleles causing sex ratio distortions. In 1969, Hamilton (*2*) proposed control by locating "driving" sex chromosomes, isolating the relevant alleles from epistatic modifiers, and introducing those alleles into (outbreeding) populations lacking the modifiers. He noted that completely driving alleles, in the absence of modifiers, should cause extinction. Incompletely driving alleles, on the other hand, would cause an immediate, rapid decline but would not extinguish the population completely (*3*). Apparently no further attention has been paid to Hamilton's idea, despite his intriguing speculation that the general inertness of the "sex determining" chromosome suggests a prevalence of such loci (they are more effective on sex chromosomes, and more effective on the Y than on the X in insects and in man). If genes for such effects could somehow be manipulated to thwart the spread of modifiers, perhaps by suc-

cessive introductions of different sex chromosomes bearing genes at different loci producing similar effects, populations might be collapsed to levels at which supplemental means of maintaining control or effecting local exterminations might be feasible. In their review of genetic means of controlling insect populations, Smith and von Borstel (*4*) mention meiotic drive, but mainly in connection with the introduction of dominant lethal mutations. Perhaps the paradox of natural selection favoring the spread of an allele whose net effect is to reduce a population has hindered research on this apparently important phenomenon.

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In our article we did not attempt to list all, or even many, methods of biological insect control. We cited some general leading references, but our main purpose was to illustrate the crucial policy questions associated with future insect control methods by selecting a few specific examples. Therefore, our omission of specific mention of sex ratio distortion should not at all be interpreted as denying its potential; we share Sherman and Alexander's view that this as well as many other biological methods merit more intensive work. However, as we pointed out in our article, the economic realities of research funding and of the market place as well as current government policy offer more lip service than real incentives for fundamentally novel approaches to insect control.

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Weather Modification: Possible Effects

Kellogg and Schneider (27 Dec. 1974, p. 1163) discuss the future and problems of climate stabilization and propose a type of "climate disaster insurance." I wish to point out an extremely important and quite neglected possible consequence of both climate stabilization and weather modification.

Engineering works for the storage and transmission of water within and between river basins are designed on the basis of streamflow records that can be statistically evaluated for reasonable design criteria. For example, flood control works are generally built to contain the "100-year flood" or that flood with a 1 percent probability of occurrence in a given year. If 100 years of streamflow records are available, the water stage and discharge statistics are highly reliable, but if only 50, 20 fewer years of records are available, then the statistics are less reliable. In such cases, flood control structures must be overdesigned, not because of the possibility of large floods, but because of the uncertainty of the design criteria.

Weather modification and climate stabilization have the potential of changing rainfall and runoff patterns to a largely unknown degree. If such programs are successful, the result could be the invalidation of previous streamflow records as a statistical basis for the design of new water engineering works. New structures would have to be overdesigned, and older engineering works might well need to be changed or supplemented with new works, all at a tremendous cost.

This potential effect of climate stabilization and weather modification should be considered in the planning and before implementation of any local, regional, or worldwide weather and climate modification program.

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The article by Kellogg and Schneider poses questions, not of man's scientific ingenuity, which the authors demonstrate so well, but of man's political intelligence, which still lurks in the shadows of uncertainty.

However, what we call political "science" has wrestled with the solution of the types of problems to which the article refers. In regard to water rights, there are numerous precedents. In the

days of the League of Nations, the World Court decided a dispute between Belgium and the Netherlands on the diversion of water from the river Meuse. A notable recent case, decided by a specially appointed international tribunal, concerned a dispute between France and Spain about the alleged diversion of the waters of Lake Lanoux. Problems of the border waters between the United States and Canada are handled by a special body set up by treaty and called the International Joint Commission. Kellogg and Schneider mention our negotiations with Mexico; the problem of the salinity of the Colorado River has now been settled by agreement. Many other cases could be cited, most of them pointing to settlement by negotiation rather than by decision of an international court. The India-Pakistan agreement on the Indus River is a notable example. Barros and Johnston have recently published a documentary volume (*1*) on *The International Law of Pollution*.

There are fewer precedents for international litigation of atmospheric pollution, but on 20 December 1974, the International Court of Justice at The Hague (commonly called the World Court) handed down its decisions in the cases brought by Australia and New Zealand against France because of French nuclear tests in the atmosphere in the South Pacific area. The court did not hold that France was liable for actual or potential injury in the plaintiff countries because it found—by 9 votes to 6—that France has now bound itself to discontinue such atmospheric tests; the court therefore denied the plaintiffs an opportunity to prove that a state is liable if it deposits nuclear fallout on other states or on oceans where fisheries may be affected. Since I was counsel to Australia, it would not be seemly to comment on the court's judgment. The plaintiffs could invoke a much-cited decision of a special international tribunal set up by agreement of the United States and Canada, which found Canada liable for damages inflicted on livestock and forest on the U.S. side of the border by fumes emitted by the Trail Smelter located on the Canadian side. Had the Nuclear Test cases been argued on the merits, the World Court might have had its attention called to litigation in the courts of the United States, especially the case of the Reserve Mining Co., which is alleged to have discharged materials into the air and into the waters of Lake Superior, entailing risk of cancer and other health injuries. The case is still involved in procedural difficulties, but at a stage when it was before the Supreme Court, Mr. Justice Douglas, in a dissenting opinion, said:

> If, as the Court of Appeals indicates, there is doubt, it should be resolved in favor of humanity, lest in the end our judicial system be part and parcel of a regime that makes people, the sovereign power in this Nation, the victims of the great God Progress which is behind the stay permitting this vast pollution of Lake Superior and its environs.

International courts have fewer opportunities to decide such questions, since they can decide cases only where both parties consent. There is more hope for the success of international negotiation, as in the recent agreement of 16 coastal states to protect the Mediterranean against the growing threat of pollution. UNEP (United Nations Environment Program) with the aid of UNSCEAR (United Nations Scientific Committee on the Effects of Atomic Radiation), GEMS (Global Environmental Monitoring System), and other international organizations may provide the basis for the resolution of disputes, despite the reluctance of governments to allow their sovereignty to be questioned. Agreements already reached on outer space and on Antarctica are encouraging.

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1. J. Barros and D. M. Johnston, *The International Law of Pollution* (Free Press, New York, text ed., 1974).

* The author was a Judge of the International Court of Justice from 1961 to 1970.

Methanol-Gasoline Fuels

While E. E. Wigg (29 Nov. 1974, p. 785) presents much of interest relative to the use of methanol-gasoline fuels in various models of automobiles, his conclusions are flawed by the narrow interpretations he affords them.

Wigg's claim that the presence of methanol in gasoline would result in vehicle carburetion beyond the lean limit for satisfactory performance, since automobiles equipped with emission controls are in many instances at that limit, ignores the fact that a methanol-gasoline mixture produces fewer emissions than does gasoline alone. If the greater flammability limits of methanol and the lower carbon

monoxide and hydrocarbon emissions of a methanol-gasoline mixture are taken into consideration, it seems likely that carburetion problems would be less severe with such a hybrid fuel than they are today.

Wigg's arguments that newer autos run more efficiently than older cars and that the addition of methanol to the fuel of newer autos will not result in fuel economies ignores, it seems to me, an important point—the addition of 15 percent methanol, by volume, to gasoline reduces by that amount our dependence on gasoline, other factors being relatively constant. The importance of such a saving in petroleum seems obvious.

Wigg makes the additional point that only older autos exhibit a significant reduction in hydrocarbon emissions when fueled with methanol-gasoline mixtures. Newer autos equipped with exhaust catalysts show no further significant reductions in hydrocarbon emissions beyond those attained with the catalytic converter. He does not address the possibility that methanol-gasoline fuels may eliminate the need for, and the expense of, such catalytic converters on new autos.

Finally, the test fuel used in Wigg's experiments contained 15 percent methanol, by volume. That amount of methanol approaches the limit that is soluble in gasoline, and at low temperatures, exceeds it. It would have been enlightening if the author had investigated fuels containing 5 and 10 percent methanol, as well as similar percentages of methyl-fuel (*1*).

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Mullen's broadened interpretation of the methanol in gasoline study does not stand up when examined in the light of the data. He suggests that the greater flammability limits of methanol should result in better driveability for methanol-gasoline blends. This is not the case, however, because the leaning effect of the methanol more than offsets the relatively small benefits due to greater flammability limits. This is borne out by the observations in my article, as well as by data generated in a subsequent study (*1*). It is not clear why Mullen relates lowered carbon monoxide and hydrocarbon emissions

to improved vehicle performance. Optimum vehicle performance occurs during slightly fuel-rich operation with resultant high carbon monoxide and hydrocarbon emissions.

Mullen also claims that my article ignored the basic advantage of gasoline supply extension via methanol use. However, he overlooks the fact that methanol could be used in other applications in which petroleum products are currently used. The thrust of my article was to quantify the specific advantages of methanol-gasoline blends to determine if this would be the preferred use of methanol. As I pointed out, the potential problems of methanol in gasoline far outweigh the advantages, making alternate uses, such as for gas turbines, preferred. The advantage of using methanol, if it does become available in large quantities, to extend our liquid fuel supplies was tacitly assumed. Mullen makes the common mistake of equating methanol to gasoline on a volumetric basis. In fact, due to its lower energy content, 15 percent methanol is equivalent to only 7.5 percent gasoline.

The use of methanol could not be considered as a substitute for advanced emission control systems such as catalytic devices. The basic finding of the study was that methanol's effect on emissions is primarily due to carburetion leaning. Thus, the effect of methanol could be approximated through carburetion adjustment. However, the lean carburetion approach to emission control is not suitable for the future stringent standards. For example, the lowest carbon monoxide and hydrocarbon emissions achieved with methanol-gasoline blends, using a lean-carbureted 1973 car, were about a factor of 3 higher than future standards will allow.

In reference to Mullen's last point, 15 percent methanol, by volume, was chosen for our studies because this is the upper practical blending concentration for most gasolines. The absence of significant benefits at 15 percent would rule out benefits at lower concentrations.

E. E. WIGG

Fuels Research Laboratory, Products Research Division, Exxon Research and Engineering Company, Linden, New Jersey 07036

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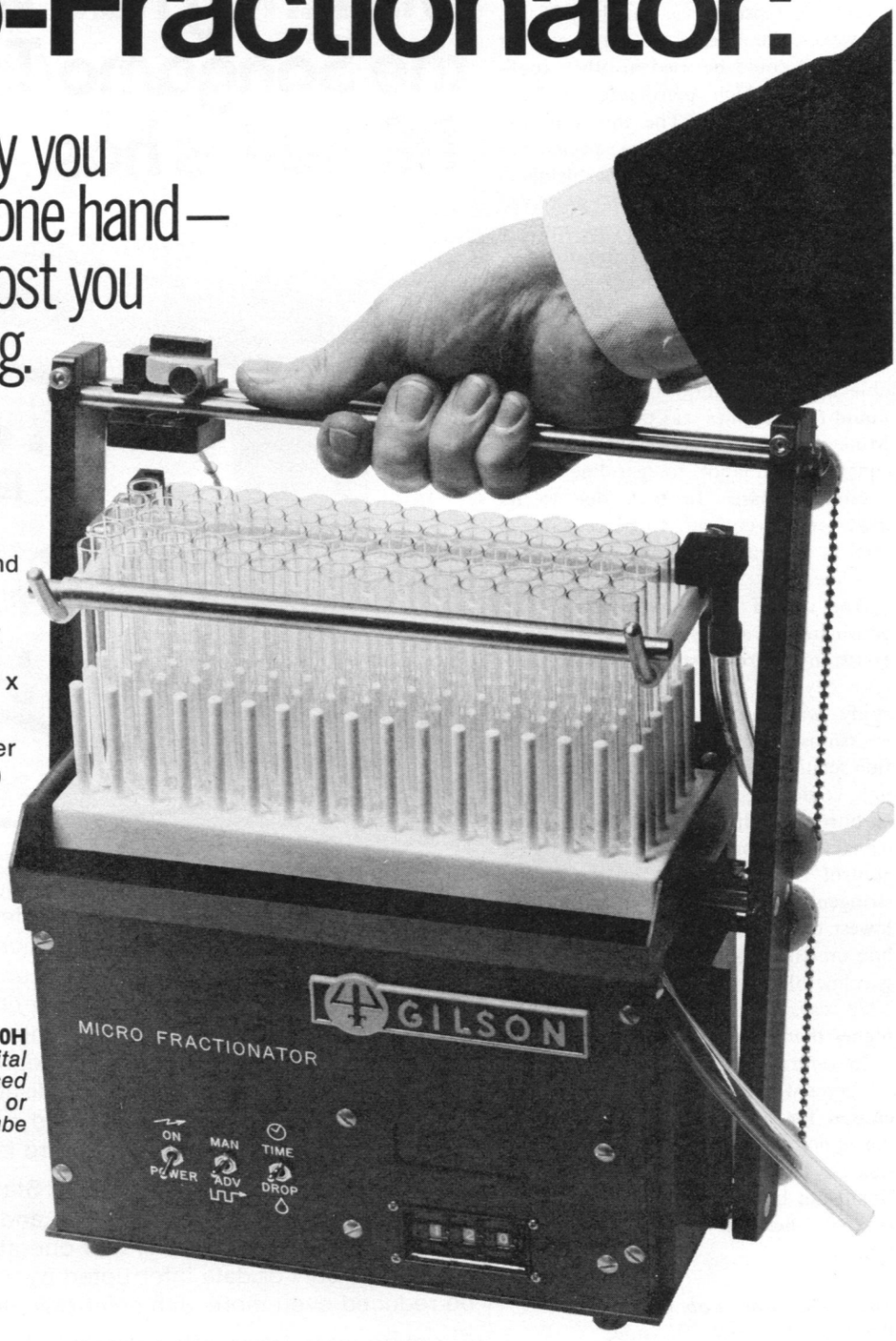

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18 April 1975, Volume 188, Number 4185

SCIENCE

Preventive Technology: A Cure for Scientific Ills

Scientists want very much to spend more of their time on science and less on its defense. They are making it apparent that they want to talk more and listen more to science at scientific meetings. At the annual AAAS meeting in January 1975, sessions on specific scientific topics were more popular than sessions devoted to policies, effects, and other themes peripheral to traditional science.

At the same time, there is no lessening of concern over unanticipated, unwanted fallouts from scientific advances. The public has associated nuclear physics with the hydrogen bomb and damaging radiations from nuclear power plants, computer science with an undesirable "data bank" society, and the development of synthetic plastics with environmental pollution. The public is correctly concerned. But public and scientific intelligence both do poorly in deciding which is worse—the disease or the cure, the problem or the solution.

The generic problem is simple. Scientists should advance science. Technologists should continue to apply scientific advances to change the products, services, and processes that support society. All harmful effects should be prevented through combined public, governmental, and scientific agreement. And above all, scientific advances should not take the public by surprise.

The solution to the problem is not in successive cures to successive science-caused problems; it is in their prevention. Unfortunately, cures for scientific ills are generally more interesting to scientists than is the prevention of those ills. We have the unhappy history of the medical community to show us the difficulties associated with trying to establish preventive medicine as a specialty.

Scientists probably had more fun developing scientific defenses against nuclear weapons (that is, cures) than they would have had practicing preventive nuclear science during the development of the atomic bomb. Computer scientists find it more attractive to develop technological safeguards, after the fact, to prevent invasions of privacy associated with computer data banks than to develop good information practices along with the computer systems.

However, it now seems quite clear that public patience with the cure always following after the ill has worn thin. The public wants to see some preventive measures taken. Indeed, individuals have taken what can be called preventive technology into their own hands. We have seen the public in action in this way in its handling of the supersonic transport issue and its reaction toward siting of nuclear power plants. This is the reactive mode of practicing preventive technology, and it hinges on public recognition that technology is fallible. But it is important in practicing preventive technology to also recognize that science has been the primary cause of beneficial change throughout man's history.

It is now time for the formalization of preventive technology as a scientific specialty. This new field must be populated with economists, lawyers, technologists, and scientists. It will be practiced during the entire cycle of research, innovation, application, diffusion, and impact of technology. It will make possible both more science and more public peace of mind and may already have more focus than technology assessment or science policy. It is safe to predict that delays in setting up preventive technology as a scientific specialty bode ill both for science and for future beneficial changes for society.—RUTH M. DAVIS, *Director, Institute for Computer Sciences and Technology, National Bureau of Standards, U.S. Department of Commerce, Washington, D.C. 20234*

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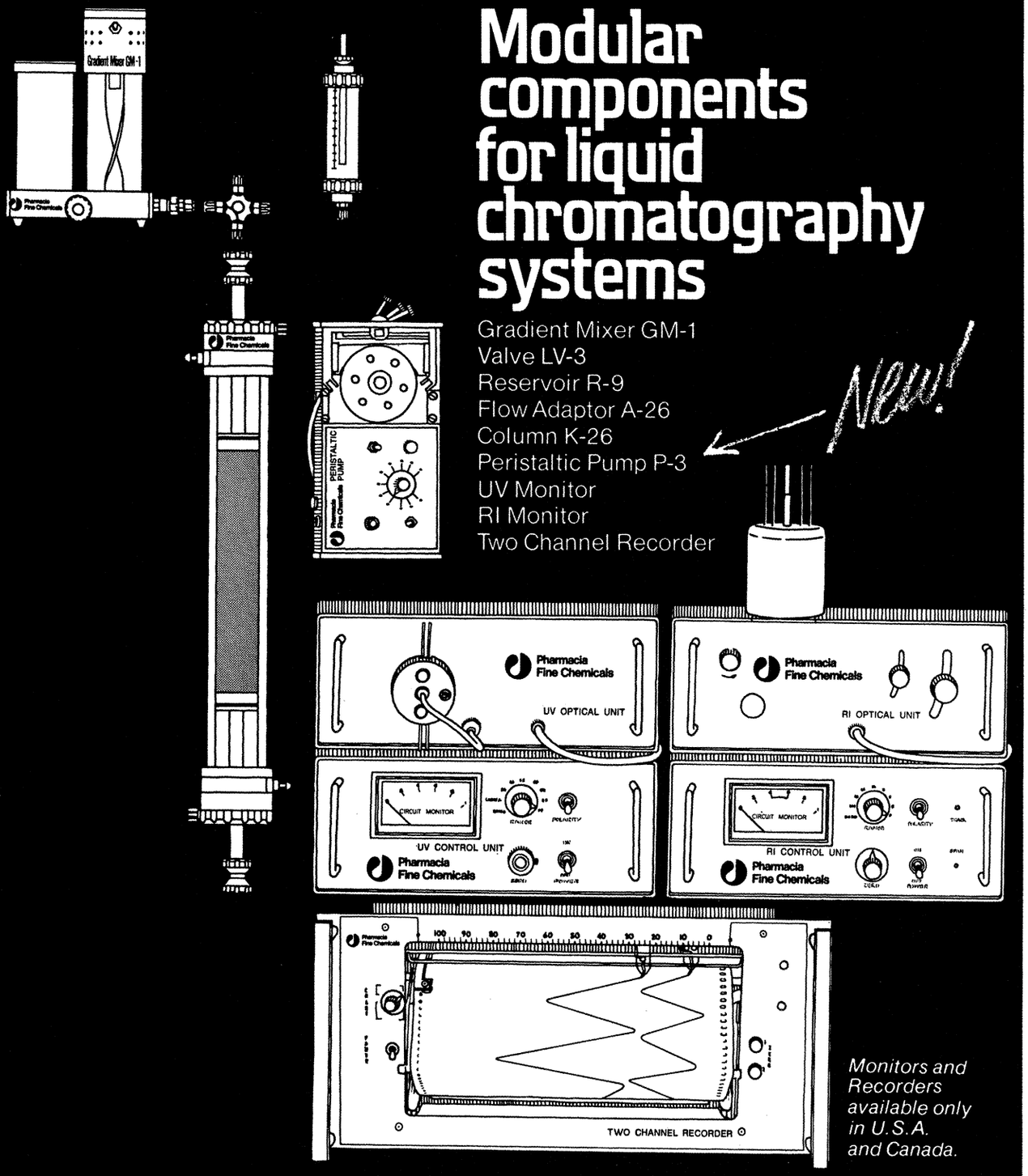

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Experimental Principles and Methods Below 1 K. O. V. Lounasmaa. Academic Press, New York, 1974. xii, 316 pp., illus. $20.25.

Feast or Famine. Food, Farming, and Farm Politics in America. Ed Edwin. Charterhouse, New York, 1974. xvi, 366 pp. $10.95.

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NEWS AND COMMENT

(Continued from page 244)

Why the "good soldier" is now being reassigned to Commerce, albeit retaining his portfolio as chairman of the ERC, at least for the time being, is not entirely clear. The official story is that the President wants Morton to "galvanize the resources of the industrial community to help solve the energy problem." Another possible explanation, which the secretary's press spokesman says is unfounded, is that Morton wants a less demanding job (in 1973, he underwent treatment for cancer of the prostate; his recovery is said to have been complete). Still another explanation is that he is being moved into a position where, in 1976, he can galvanize the business community to help in Ford's reelection campaign.

At least as recently as last fall Morton was still championing the idea of a Department of Energy and Natural Resources (DENR) that was put forward by the Nixon White House in 1973 but allowed to languish. And it now happens that this idea never yet really fully formed is still alive.

Charles F. Bingman, the Office of Management and Budget's deputy associate director for organization and special studies, told *Science* that OMB has recently asked all interested agencies for comment on the DENR concept and other alternatives. A decision must be made within the year because the law creating FEA expires 30 June 1976, with the FEA functions then to be returned to Interior or any successor agency.

What the White House and the Congress must decide is whether the life of FEA is to be extended or whether its functions should be given to a DENR. And, if there is to be a DENR, should ERDA be part of it? In Bingman's view, a DENR consisting of—to put the matter roughly—the FEA functions, the Interior agencies, and NOAA would be a viable department, although this is not to say that OMB has concluded what should be done.

Before any decision as to a DENR and the future of Interior and other agencies has been made, there will surely be some bureaucratic infighting. Morton proved a loser at the bureaucratic game, and, at this point, one can only speculate whether his successor, Stanley Hathaway, will be any better at it. If Interior loses out in any more executive reorganizations, all will see that it is a department in name only.

—LUTHER J. CARTER