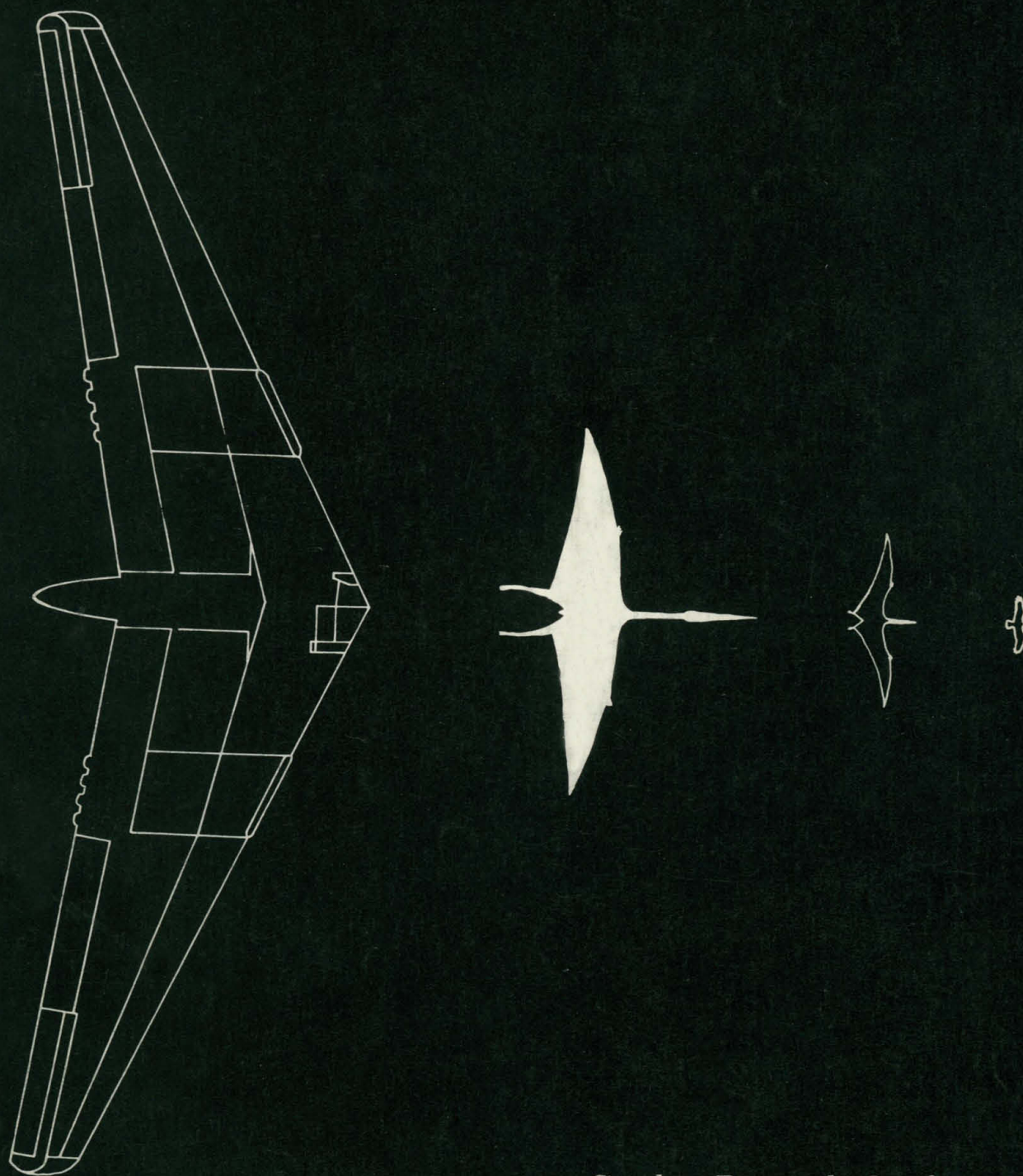


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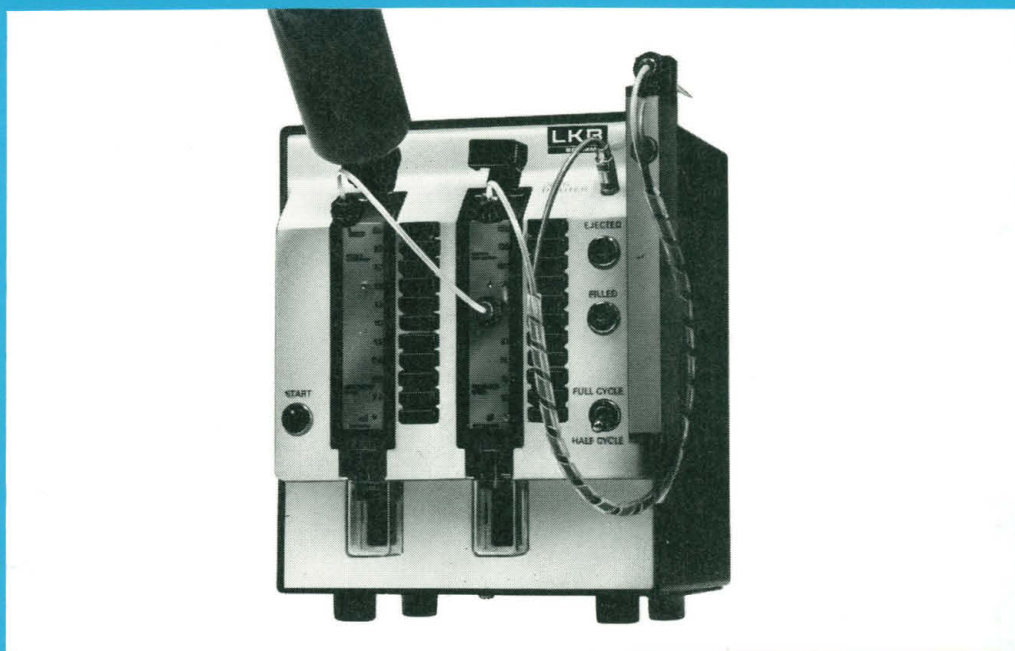
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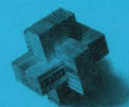
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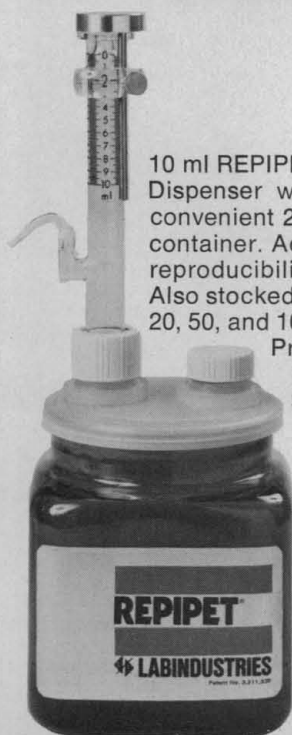


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COVER

Comparison of large Texas pterosaur with other familiar tailless aircraft. (Left to right) Northrop YB-49, "flying wing" (52.5 meters); Texas pterosaur (15.5 meters); Pteranodon (7.6 meters); and condor (2.75-meter wingspan). See page 947 [Douglas A. Lawson, University of California, Berkeley]

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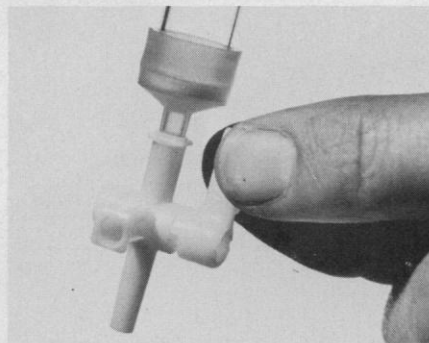
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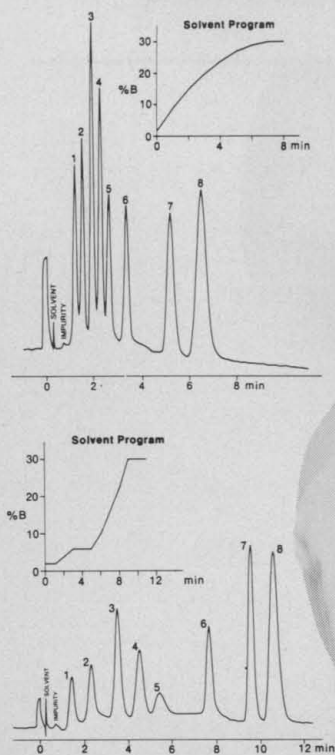
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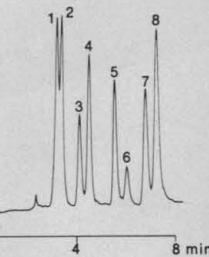
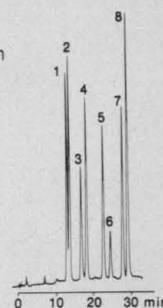
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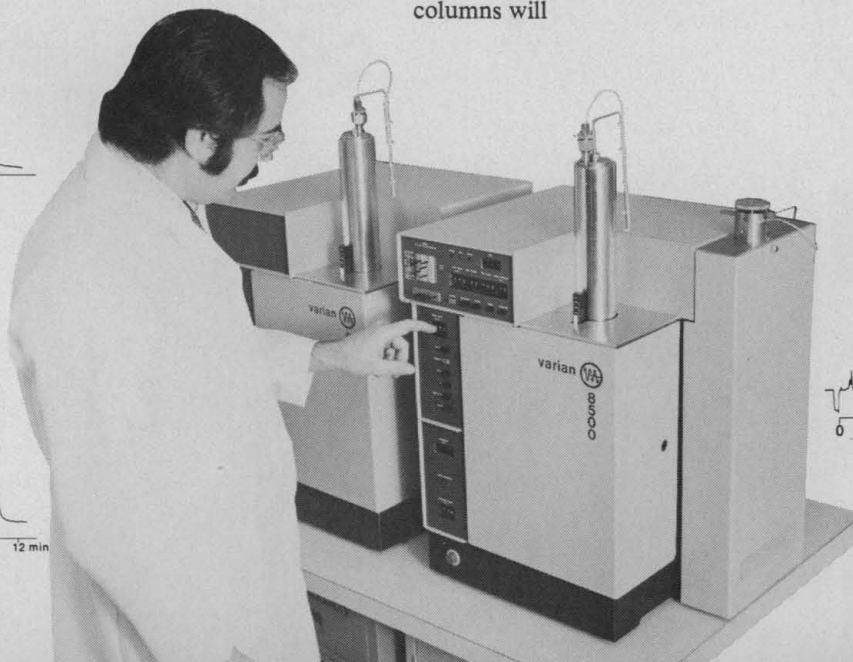
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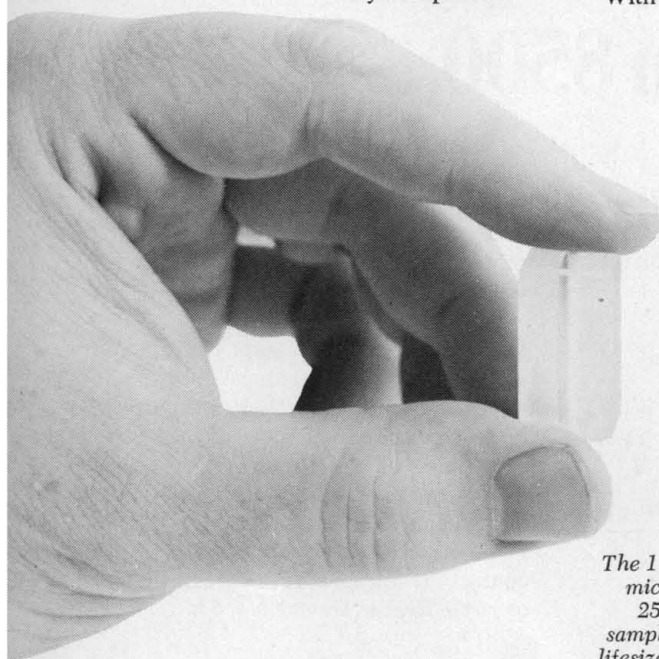
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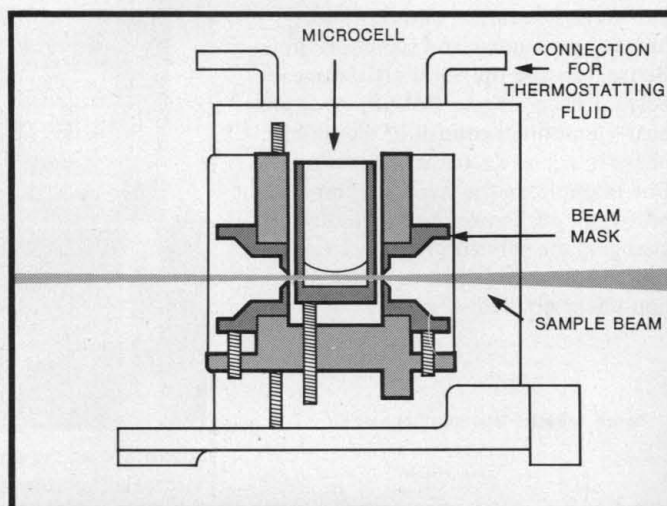
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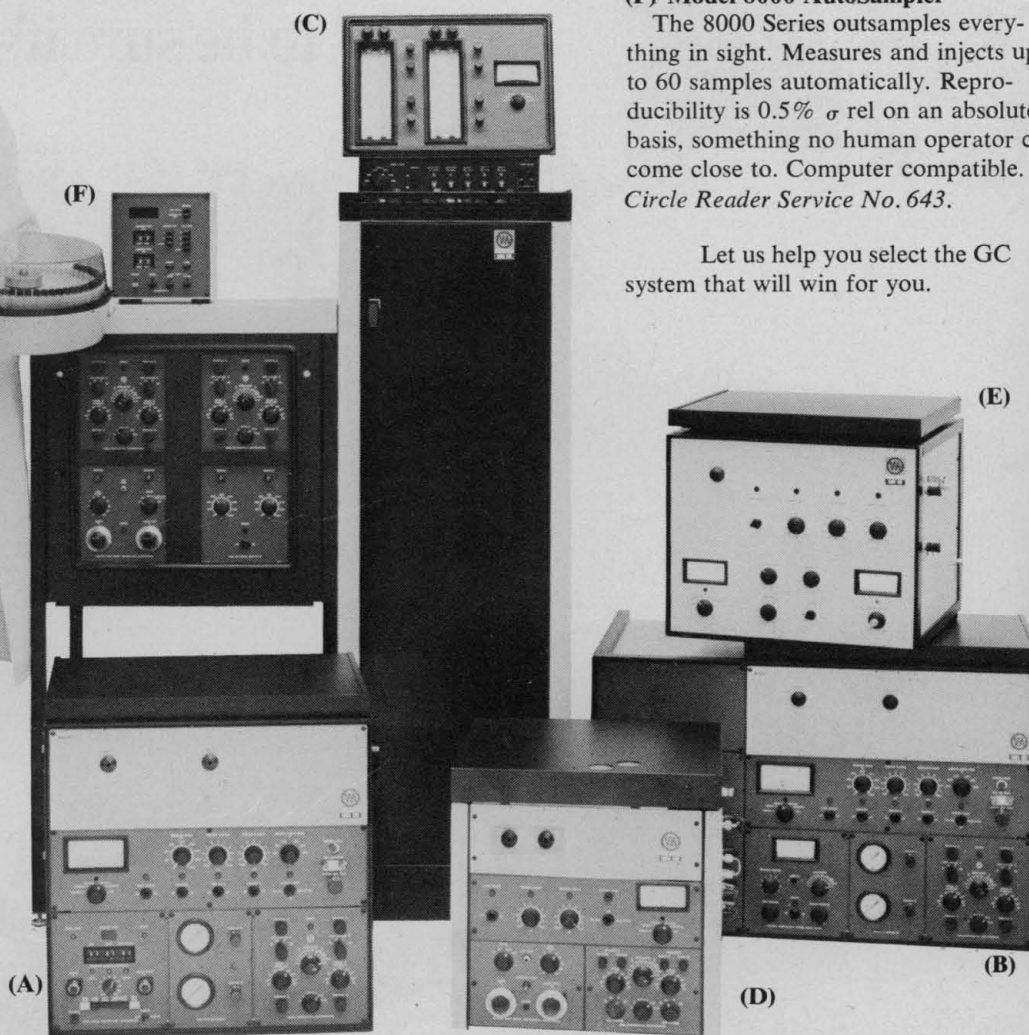
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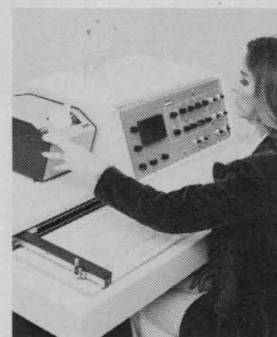
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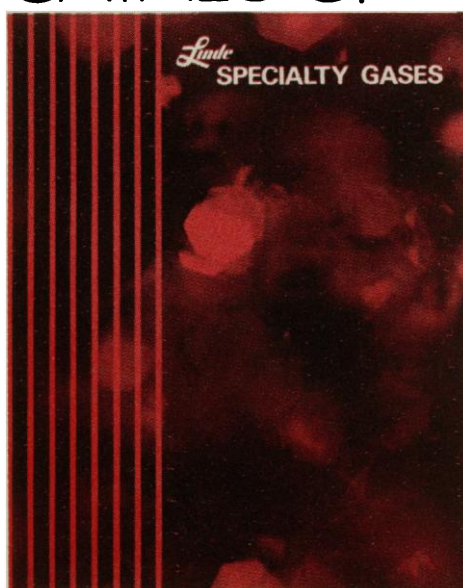
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Type 105 is a black-and-white film that gives you a superb print in 30 seconds and a superb negative in as little as three minutes. Without a darkroom.

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Type 105 film fits any Polaroid 3¼ x 4¼" film back or lab camera. Our Model 405 pack film back (shown above) also adapts most 4 x 5 cameras and instruments.

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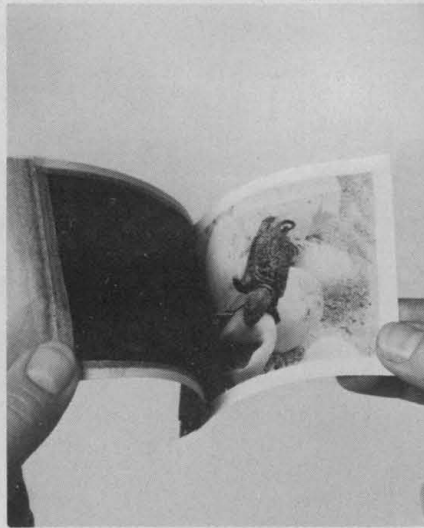
Polaroid lab cameras, and most 4 x 5 cameras and instruments (thanks to our Model 405 pack film back).

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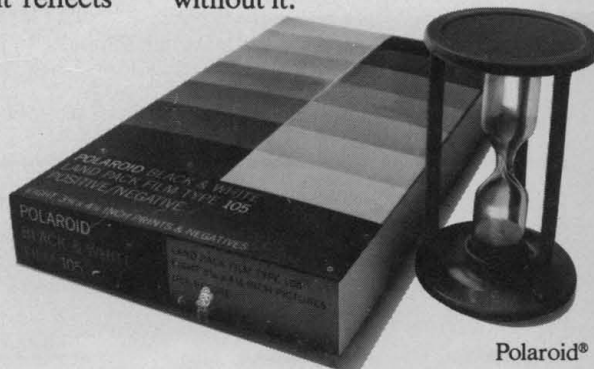
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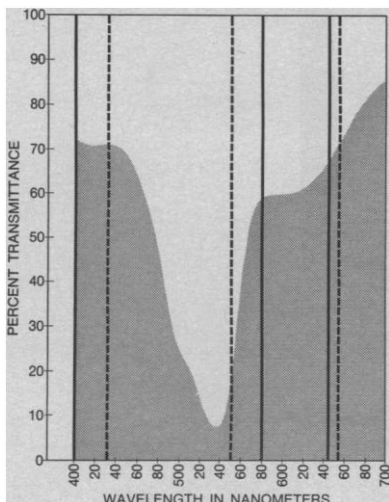
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Make better color photomicrographs with film that fits the job

Making photomicrographs is not like making pictures of landscapes or people. Different films are needed.

Conventional films are designed for pleasing the average "eye." But film which does well in rendering grass, sky, and flesh tones may not perform so admirably with the hues of hematoxylin and eosin. As the chart indicates, the green sensitivity



A comparison of film sensitivities with the spectral transmittance of H & E stain. Vertical lines are peak-wavelength sensitivities of KODACHROME II Professional Film (Type A [solid lines]) and KODAK Photomicrography Color Film 2483 (dashed lines). The spectral-absorption curve is from hematoxylin and eosin stain on a 5-micrometer section of adult guinea pig liver, read on a General Electric Recording Spectrophotometer.

of KODAK Photomicrography Color Film 2483 is a good match for the peak transmittance of the common H & E stain.

The contrast of ordinary color films is not really adequate for faintly stained or low-contrast subjects. In short, films excellent for routine photography may have serious shortcomings in the specialized world of photomicrography. But KODAK Photomicrography Color Film 2483 is a film fit for the job.

It has very high resolving power (200 lines per mm); extremely fine grain; green sensitivity which fits biological stains, as shown in the chart; and high contrast and color saturation.

Combined, these features make 2483 Film capable of producing sharp, brilliant, beautiful photomicrographs. It brings out fine detail and faint color with excellent clarity.

Of course, all high-contrast materials demand precise exposure. For best results with 2483 Film, uniform illumination of the field is essential, and bracketing exposures is recommended.

To try the film yourself, go to your usual source of Kodak products and ask for KODAK Photomicrography Color Film 2483. It comes in 36-exposure 135 magazines, 35 mm x 125-foot rolls, and 4 x 5-inch sheets. If your dealer doesn't have it on his shelf, he can order it.

He probably *will* have chemical kits for Process E-4, so you can process the film yourself in about an hour. Or you can have it done by any commercial processing laboratory that processes KODAK Ektachrome Films.

Or for prepaid processing, choose Kodak mailer PK36, and send the film to us.

More information? Just fill in and mail the coupon.

A better b/w film, too.

Excellent though color photomicrographs may be, there are times when black-and-white is preferable. The use of black-and-white may provide optimum visualization of detail via contrast control and color filtration. Or it may simply reflect the economics of reproduction in journals and monographs.

In such cases, the film of choice may well be KODAK Photomicrography Monochrome Film SO-410. To find out more, use the coupon.



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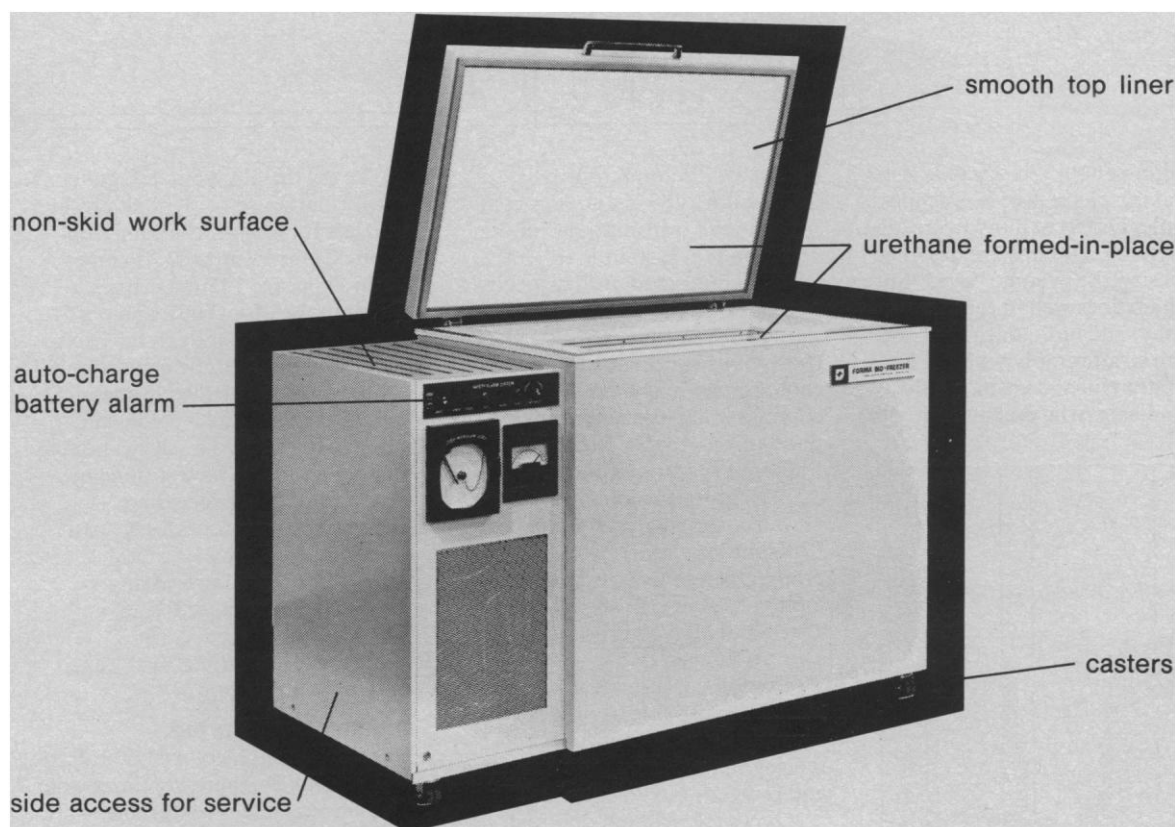
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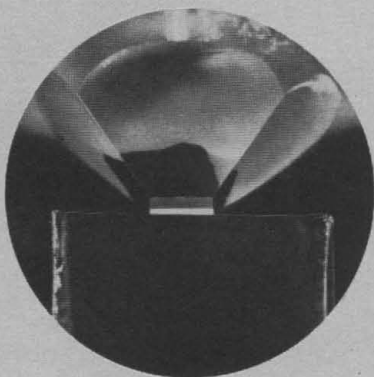
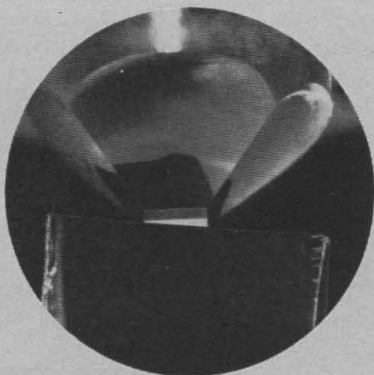
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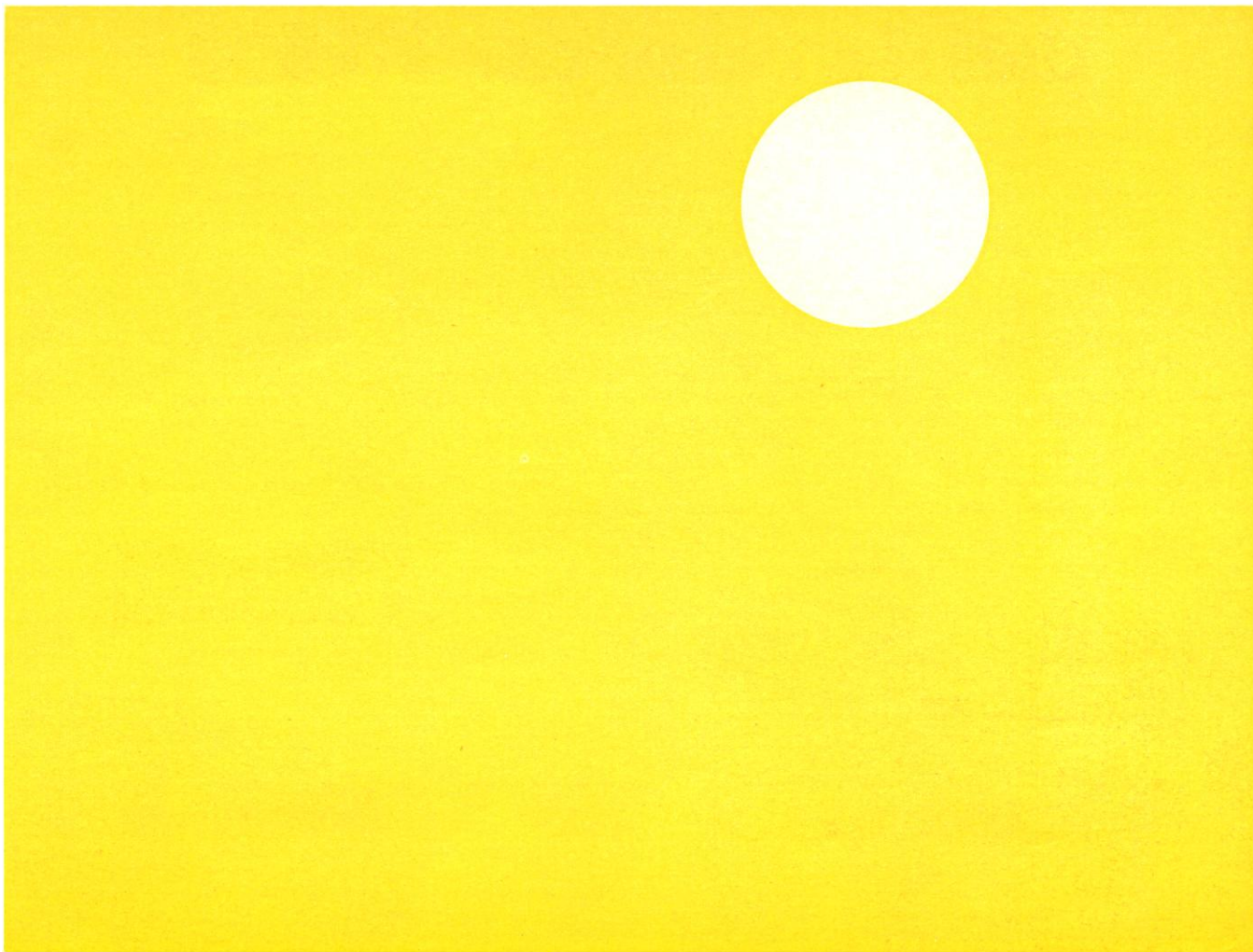
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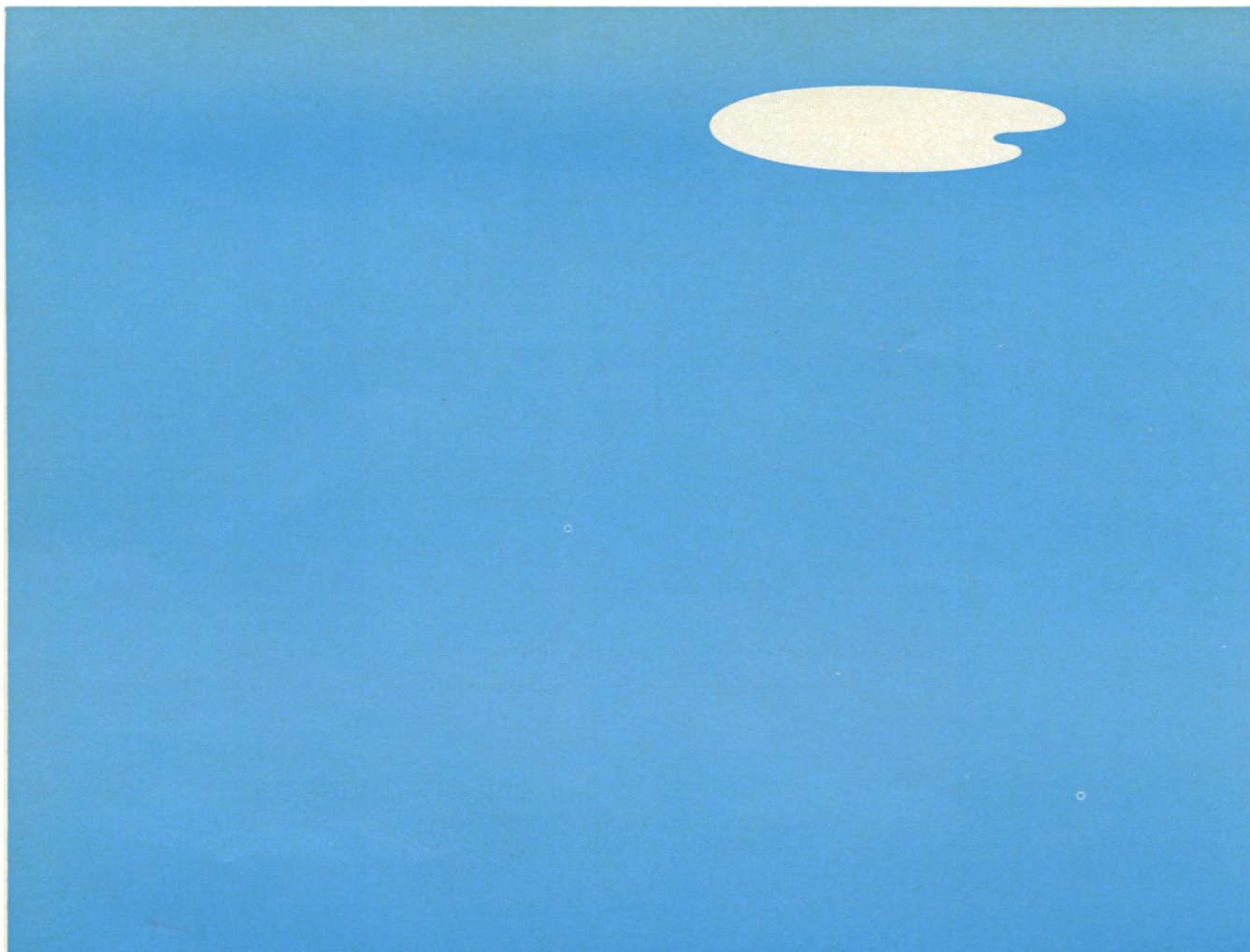
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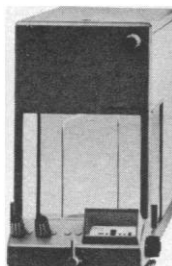
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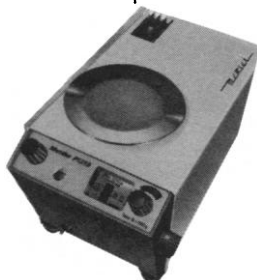
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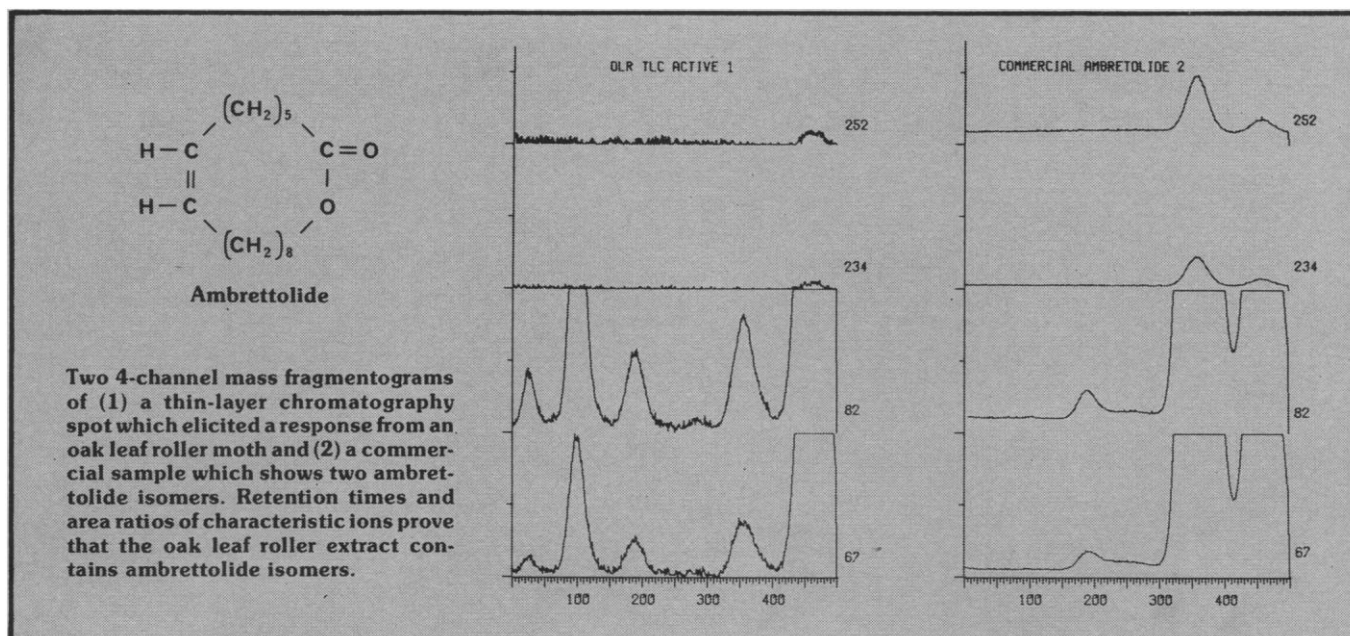
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Is It Spring That Turns A Young Man's Fancy? Or Is It Pheromones? Finnigan GC/MS May Provide The Answer



Pheromones — the chemicals that insects use as a sex attractant — are the subject of intensive research. A thorough knowledge of these complex substances could lead to an effective, ecologically sound means of controlling insect pests that now destroy billions of dollars worth of agricultural and forest products. Such a program would involve what might be called a negative approach — misdirecting the insects' attention and efforts. An interesting positive approach is suggested by recent findings that pheromones may also play a role in human sexual activity.

Ambrettolide

Dr. L. H. Hendry and his associates at The Pennsylvania State University have recently found that ambrettolide, a macrocyclic lactone, and similar isomers are part of the insect pheromone system, in which it acts as a sexual excitant. It is intriguing to note that ambrettolide and similar macrocycles have been associated with sexual activity in musk oxen, civet cats, muskrats, and black-tailed deer. Musk, with its naturally occurring ambrettolide, has long been used as a base for perfumes, and commercial ambrettolide is used in a number of perfumes which have the odor of musk. Perhaps science is about to prove what humans have known instinctively for centuries.

Pheromone Analysis Is Difficult

Sexual behavior in many insect species is mediated by specific mixtures and very low concentrations (10^{-7} to 10^{-10} g per insect) of pheromone components which are frequently positional and geometrical isomers. Analytical instrumentation capable of providing structural elucidation and quantitation of these chemicals is essential to continued progress in pheromone research.

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Finnigan GC/MS makes quantitative mass fragmentography a practical tool for difficult trace analyses (25 to 500 picograms range) like these because it provides extremely high sensitivity and the specificity to identify compounds even in unresolved GC peaks.

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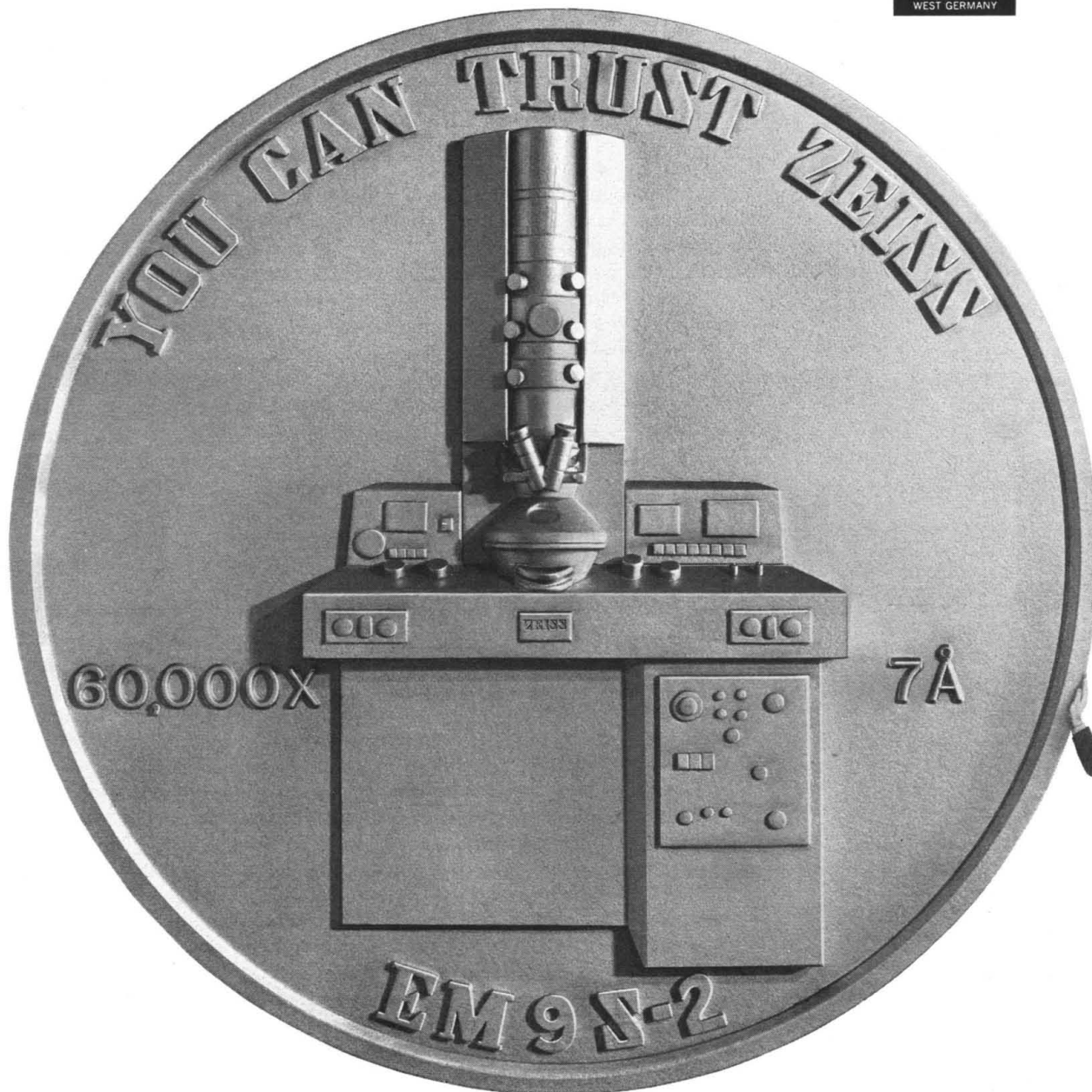
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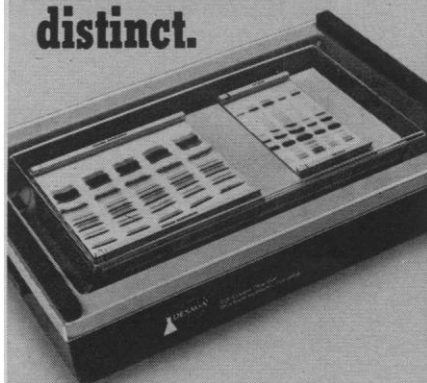
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LETTERS

Ilya Glezer's Struggle

The actions of diplomats tend to overshadow the countless individual struggles upon which they are based. One such struggle which should not be ignored or forgotten by the scientific community is that of the Soviet investigative neuropathologist, Ilya Glezer. He is coauthor, with Samuel Blinkov, of *The Human Brain in Figures and Tables* (Plenum, New York, 1968). On 7 February 1972, Glezer was arrested and convicted of anti-Soviet activity. His crime was the possession of Zionist literature. He was given a sentence of 3 years at hard labor to be followed by 3 years of banishment to Siberia. The hard labor portion of his sentence terminated on 8 February 1975, and it has been learned that, immediately thereafter, he was transferred to Krasnoyarsk for the banishment portion of his sentence.

Glezer, now 43, was in poor health when he began his sentence. He suffers from severe myopia with night blindness. Conditions at the camp have resulted in swelling of his legs with resulting physical disability. Some medical treatment that was previously available to him has now been withdrawn, and he has been denied contact with fellow prisoners of conscience. Thus, at a time when détente is preached at government levels, the treatment of Glezer and other political prisoners appears to be as bad or worse than ever.

Despite Soviet protestations about interference in its internal affairs, external pressure can bring relief to individual sufferers. Valery and Galina Panov, who are now free in the West, would still be detained if such pressure had not been applied. Now is the time to mount a campaign to relieve Glezer of the further torture of the prison camp and of three additional years of banishment to Siberia. We urge all readers to wire or write Leonid I. Brezhnev, General Secretary, Communist Party of the Soviet Union, The Kremlin, Moscow, U.S.S.R., asking him to grant a pardon to Ilya Glezer, to suspend the remainder of his sentence, and to permit him to join his mother who now lives in Israel. Copies should be sent to His Excellency Ambassador Anatoly F. Dobrynin, Ambassador of the U.S.S.R., 1125 16th Street, NW, Washington, D.C., and to your local senators and representatives urging further action by them. We also suggest that resolutions

demanding Glezer's release be proposed at all international scientific meetings. Inquiries about further details of the Glezer case are welcome and should be directed to the first signer.

ALBERT SATTIN

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IRWIN J. KOPIN

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*Department of Psychology,
Massachusetts Institute of Technology,
Cambridge 02139*

X-ray Crystallography Techniques

We would like to reply to the article by Thomas H. Maugh II, "X-ray crystallography: A refinement of technique" (Research News, 6 Dec. 1974, p. 913), because we believe it is likely to cause some misconceptions. The technological developments discussed may indeed prove useful, but they need to be put in the perspective of the whole process of obtaining a protein structure.

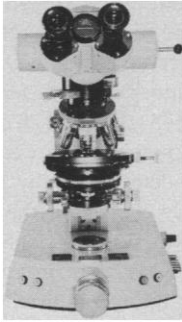
Briefly, the process of determining a protein structure consists of the following steps: (i) crystallization of the native protein and collection of diffraction data; (ii) obtaining and collecting diffraction data from heavy atom derivatives; (iii) obtaining phases for the native data; (iv) computing an electron density map from the data of steps (i), (ii), and (iii); (v) interpreting the electron density map; (vi) checking the structural model against the observed data and possibly refining the model to improve the fit to the data.

The misconceptions stem in part from minimizing the last two steps. Essentially, methods exist [MIR (multiple isomorphous replacement) phasing] for obtaining the phases necessary in step (iii). The techniques described in Maugh's article can be used primarily in a place between steps (iii) and (v): that is, it is apparently possible to take a poorly phased map or a map with limited resolution and improve it by extending the set of phases and by improving those on hand without imposing any interpretation on the electron density map. Inasmuch as protein crystallographic work in the past has been based only on interpretation of MIR-phased maps without

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Get the whole story by writing Carl Zeiss, Inc., 444 Fifth Ave., N.Y., N.Y. 10018. Or call (212) 736-6070.

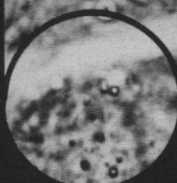
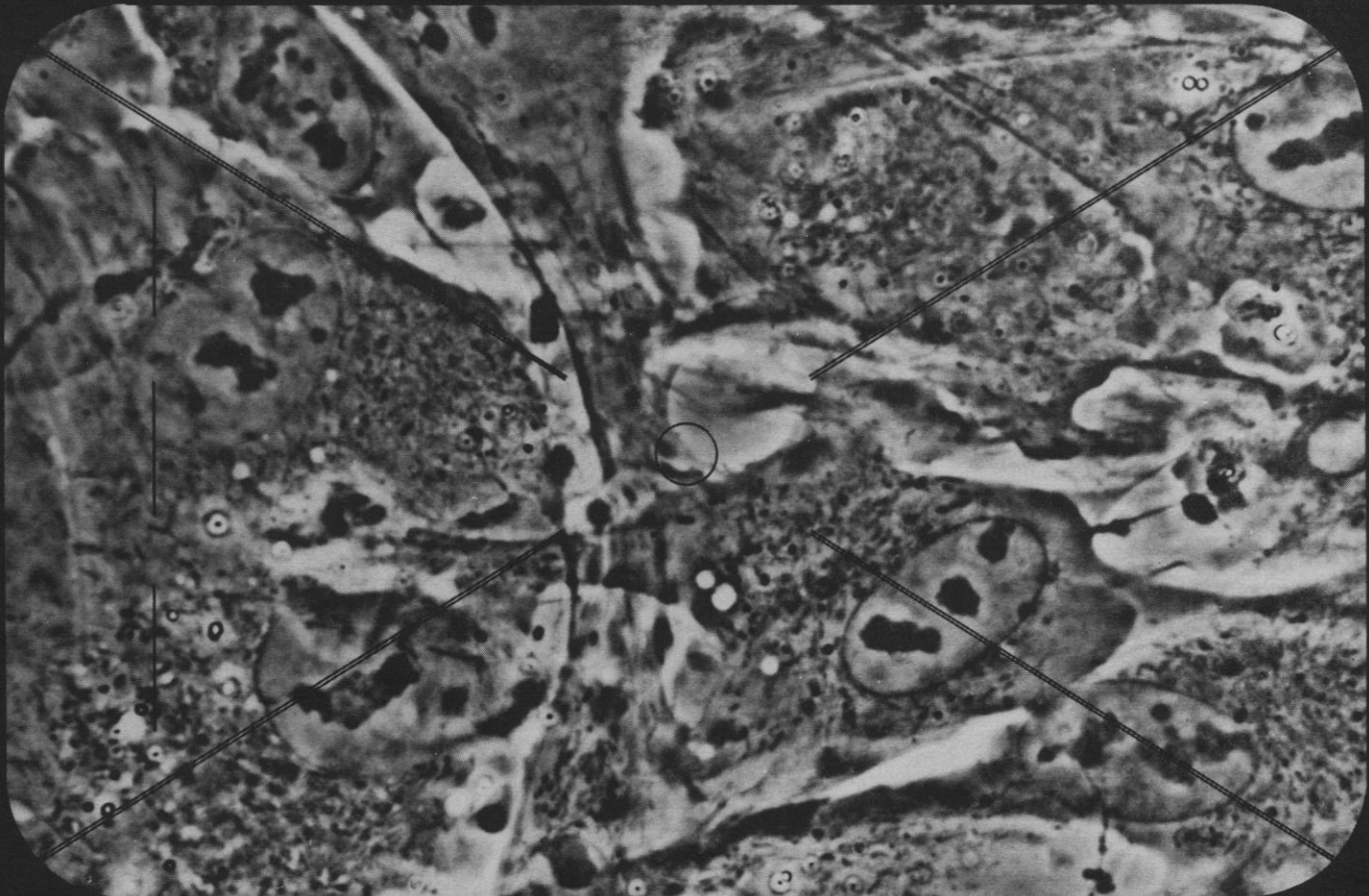
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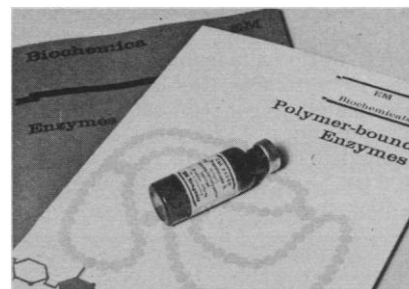
going on to step (vi), any improvements in these maps will be welcome. However, it has become clear in our own laboratory and in others that substantial improvement can be made in a trial model by refinement in the classic crystallographic sense: that is, once approximate atomic positions have been deduced from an electron density map, more accurate positions can be determined by minimizing the difference between the observed and calculated diffraction amplitudes.

We believe that, in an effort to make some complex material understandable to the general reader, Maugh has made some misstatements. Several examples follow. Maugh refers to refining the data, but this is by no means the case. Trial models are refined so that calculated amplitudes better fit the observed values; in the case of Sayre's technique, phases are refined to give a better fit for the equations he derived which constrain them.

It is true that substitution of heavy atoms may make the derivative crystals less isomorphous (that is, less like the native crystal), which may in turn result in both limited resolution and poorer phases for the data on hand. A loss of information occurs and maps may be less interpretable, but the native structures are not distorted. The techniques of Sayre and Collins may serve to add information, thereby improving interpretability of the maps, but in fact, since both their techniques and classical methods need a set of phases initially, they all start with the same distortions, and it is not yet clear how free of error the extended sets of phases really are.

Two statements in Maugh's article concerning our work at the University of Washington are erroneous. First, the assertion that Sayre's map correctly identified five amino acids that were obscure in our map is incorrect. The situation is essentially as stated in Sayre's paper (1). Second, Maugh states that our map could not have been obtained using 2.5-Å phase angles. All these methods of refinement, including classical methods, can be regarded as extending phases. In the *Cold Spring Harbor Symposium* (2) we state: "In retrospect, it is clear that the refinement [of rubredoxin] could almost equally well have been initiated with atomic coordinates from the 2.5-Å map rather than with those from the 2-Å map. Probably no more than one or two additional ΔF refinement cycles would have been re-

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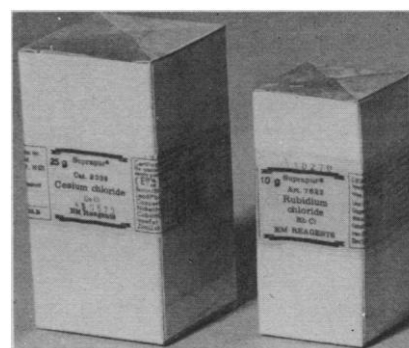
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quired." Our refinement of the ferredoxin model amply justifies this statement (3).

The point is not to quibble over techniques, but to judge which ones are useful at what stage of the game, and to be aware of the power and limitations of each. The techniques of Sayre and Collins are a welcome addition to the repertoire of possible approaches to obtaining reliable protein structures. The new methods contribute to improving the interpretability of the electron density maps while classical refinement results both in improved interpretability and in a set of atomic coordinates.

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2. K. D. Watenpaugh, L. C. Sieker, J. R. Herriott, L. H. Jensen, *Cold Spring Harbor Symp. Quant. Biol.* **36**, 366 (1971).
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Ethiopian Revolution

The article "Ethiopia: Did aid speed an inevitable upheaval?" by Constance Holden (News and Comment, 27 Dec. 1974, p. 1192) contains some absurdities and contradictions.

It is contradictory to call Ethiopia an "American client state," while at the same time pointing to Haile Selassie's "posture of nonalignment" which "welcomed aid from any and all foreigners" but "sought to prevent them from becoming too influential."

It is proposed in Holden's article that the United States may have helped shape the foundation for the events now being played out. Unfortunately, military dictatorships are frequent occurrences in many "less developed" countries, regardless of which country has been assisting with foreign aid. As for the Ethiopians themselves, the shock of the massacre of 23 November 1974, ordered by junior officers, will surely remind them of their own proverb:

A village ruled by young men,
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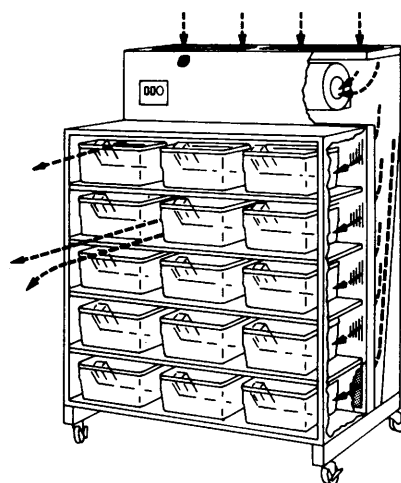


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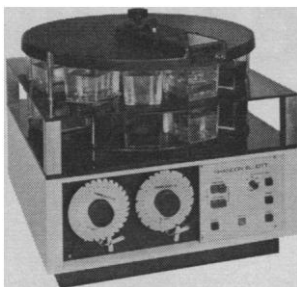
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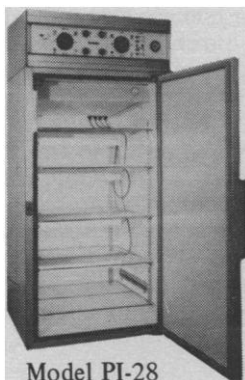


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the farms they have worked for so many centuries, not preachments by urban students now being sent to them in order to persuade them to exchange their old masters for new ones.

It is absurd to claim that the "unique identity of the ruling Amhara elite . . . has helped prevent the formation of culturally or ethnically based foreign alliances." Since when has an ethnic identity prevented alliances between societies holding very different values?

During the French Revolution, when Robespierre was led to his own execution, he predicted that the revolution would die after having killed its own participants. On 23 November 1974, the first victim in Ethiopia was rebel chief General Aman Michael Andom. It is likely that the outcome of the Ethiopian revolution will be another Napoleon, up from the ranks of the armed forces, or perhaps a more traditional Ethiopian-style emperor with sufficient security to practice more benign methods of government. Such a potential emperor already exists in the person of Prince Mengesha Seyyum of the rival, but linked, dynasty of the Province of Tigre. By personality and education he is the most qualified to retain Eritrea, prevent civil war, and promote the material development of Ethiopia.

SIMON D. MESSING*

*Department of Anthropology,
Southern Connecticut State College,
New Haven 06515*

* Formerly U.S. Agency for International Development officer in Ethiopia.

Standardized References

The Council of Biology Editors (CBE) promotes through the *CBE Style Manual* standardization of form and style in scientific papers in the biological sciences and allied fields. Many recommendations are based on standards that have been formally endorsed by national or international organizations. Progress already has been made in standardizing some aspects of bibliographic style, but the special needs of various disciplines often dictate more practical solutions than a single standard for all disciplines. Our comments below are numbered according to the issues raised by Stoldal and Gordon in their letter of 27 December 1974 (p. 1158).

1) CBE recognizes the desirability of citing references by number within

the text and numbering the references themselves seriatim according to the order of first mention. However, the kind of articles published by a journal sometimes requires a different standard; uniformity for all journals is not practical. For example, a journal publishing papers with numerous equations usually cites references by authors' names and year of publication, thus avoiding the confusion of a double set of numbers; the list of references is alphabetized by authors' names. Other publications containing review articles with numerous references prefer to arrange references alphabetically, with articles by the same author brought together; then the alphabetized references are numbered so citation within the text by number can be printed less expensively.

2) CBE recommends that references include full titles of articles and inclusive page numbers.

3) Standards for abbreviating journal names have already been endorsed by the American National Standards Institute (ANSI) and the International Organization for Standardization (ISO). These abbreviations may be found in the following publications: (i) *International List of Periodical Title Word Abbreviations* [Standards Committee Z39, American National Standards Institute, New York, 1970 (rules for abbreviating individual words accompanied by lists of abbreviated words)]; (ii) *BIOSIS, 1974, Lists of Serials with Coden, Title Abbreviations, New, Changed and Ceased Titles* [BioSciences Information Service of Biological Abstracts, Philadelphia, Pa., 1974 (title self-explanatory)]; and (iii) *Bibliographic Guide for Editors and Authors* [Chemical Abstracts Service and Engineering Index, Inc., Columbus, Ohio, 1974 (full journal names, abbreviated names, codens, and names of services that abstract each journal; no changed and ceased journal names)].

So far, no national or international standard for the order of components within a reference has been agreed upon, although the CBE Style Manual Committee hopes that such a standard currently in preparation by ANSI will have been adopted by the time the 4th edition of the *CBE Style Manual* goes to press in 1976.

MARGARET BROADBENT

EDWARD J. HUTH

Council of Biology Editors, Style Manual Committee, American Institute of Biological Sciences, 1401 Wilson Boulevard, Arlington, Virginia 22209

It is a bit discouraging that Stoldal and Gordon are unaware that the kind of standardized reference system they propose has been in effect for 5 years and observed by some 30 medical journals.

A letter from a medical academician's secretary, cited in a *New England Journal of Medicine* editorial (1), initiated this standardization and is worth reading. Unfortunately, a number of prominent publications, such as the *Journal of Clinical Investigation*—or even *Science* for that matter—have not seen fit to join the movement in spite of its patent advantages.

FRANZ J. INGELFINGER

New England Journal of Medicine, Boston, Massachusetts 02115

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Cultural Homology

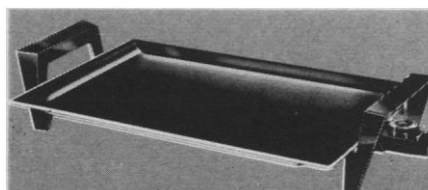
In his lecture "Analogy as a source of knowledge" (19 July 1974, p. 229), delivered on the occasion of his Nobel Prize in Physiology or Medicine, Konrad Z. Lorenz describes the usual morphological homologies and analogies, and, in his view, the equivalent cultural-behavioral homologies and analogies. Comparisons cover the spectrum of animals and man.

Morphological homology is demonstrated by the often-reproduced evidence of Cuvier showing similarities of vertebral anterior limbs, and cultural (technical) homology is demonstrated by the persistence of horse-drawn coach attributes in the development of railway cars. I submit that Lorenz's two homological demonstrations are not comparable.

Lorenz's morphological demonstration is based on the operation of classical Darwinian selection principles—gradual environmental changes, variation within species, and subsequent passage through the sieve of selection. Classical Darwinian theory specifically denies teleology (theological or man-made) in natural selection, that is, denies any plan, direction, or design. However, Lorenz's cultural demonstration is teleological. Those 19th-century mechanical engineers *designed* their railway cars, and, in our own time, they are called design engineers. They waited for no random variations and had no sieve of selection; on the contrary, they had preordained direction.

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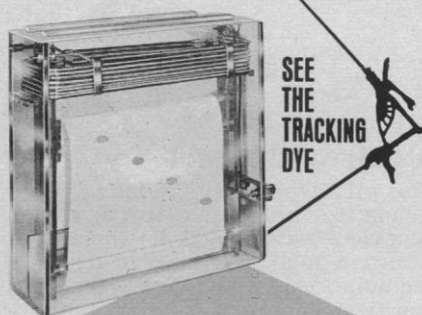
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So, in the first demonstration, Lorenz describes the operation of a fundamental law of nature as stated by classical evolutionists, and in the second case, Lorenz describes the operation of the contriving human mind. The resemblance between demonstrations is superficial.

The superficiality of this resemblance is further enhanced by consideration of the "evolution" of railway cars, as proposed by Lorenz. Railway cars, of course, are not species, and their "evolution" is a product of human endeavor. Their apparent homology, I am led to believe by Lorenz, reflects some sort of homological aspect of human thinking. I maintain that this progression of human thinking, for that is what it is, reflects naught but the learning process of a single species and is totally unrelated to the accepted definition of homology.

Lorenz also contends that morphological analogies are demonstrated by independently evolved shark and dolphin shapes and also by independently evolved octopus and human eyes. Behavioral analogy is demonstrated simply by similar behavior based on survival as defined by Lorenz. "We know for certain," Lorenz writes, "that it was more or less identical survival value which caused jealousy behavior to evolve in birds as well as in man." Lorenz insists that the origin of the behavior is unimportant—whether by a genetically fixed program in geese or by cultural tradition in man—just as long as the behavior is similar.

Surely Lorenz does not mean to imply that cultural tradition in man is transmitted genetically. The obverse is true, for cultural tradition is highly variable and flexible, easily changed within the same generation. The morphological demonstration rejects teleology; the homological demonstration acknowledges design. We cannot assume that the mechanism of the first is duplicated precisely by the teleology of the second. In the first case, selection has helped ensure survival in the environmental hostilities enumerated by Malthus, while in the second case, behavior is wrought by design internal to man and is often without survival value—a fact conceded by Lorenz. "In the complicated interaction of human social behavior," he writes, "there is much that does not have any survival value and never had any."

Long ago, Alfred Russel Wallace told us that man's organic evolution stopped in a single critical moment—

that singular time when mind or intelligence began. Man-made plans and designs entered man's behavior and, as he domesticated animals and plants according to his whims and fancies (and *not* for survival in the hostile environment as defined by classical Darwinism), so he domesticated himself to his whims and fancies. The answer to this 20th-century question of questions concerning man's behavior, I suggest, will not be assayed from an analysis of homologies and analogies. It will be found in those special rules that man has devised to evolve man in his own image, for these rules are also subject to scientific inquiry.

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Cohen has failed to grasp the inferences which are to be drawn from the fact that cultural homologies do indeed exist. He firmly believes in the unfounded doctrine that man's organic evolution came to a stop at a crucial point and that, from then on, his cultural history and his further genetical development have been exclusively governed by man-made plans and deliberate designs. I do not know whether A. R. Wallace has ever uttered this opinion in the unqualified form rendered by Cohen. However, Cohen takes it for gospel and refuses to take cognizance of all the evidence disproving it. The important point is that man, even while endeavoring to plan intelligently and flattering himself that he is a rational being, is still subject to unreflected motivations which force him to commit acts which are obsolete even at the time they are committed. In my book *Die Rückseite des Spiegels*, I contend that this retention of traditional patterns, irrespective of present unadaptiveness, is indispensable in order to render cultural information inheritable at all. Vestigial cultural patterns, such as attaching a whipholder to dashboards of early automobiles, are unadaptive side effects of a generally adaptive mechanism. Cohen's erroneous statement that any demonstration of homology implies teleological ways of thinking can be corrected by reading any modern book on comparative morphology or evolution.

KONRAD LORENZ

Institut für Vergleichende
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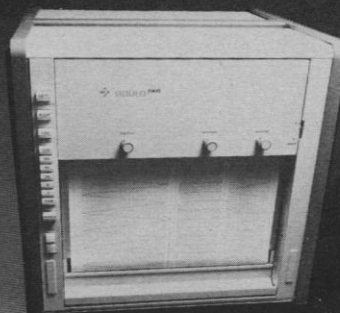
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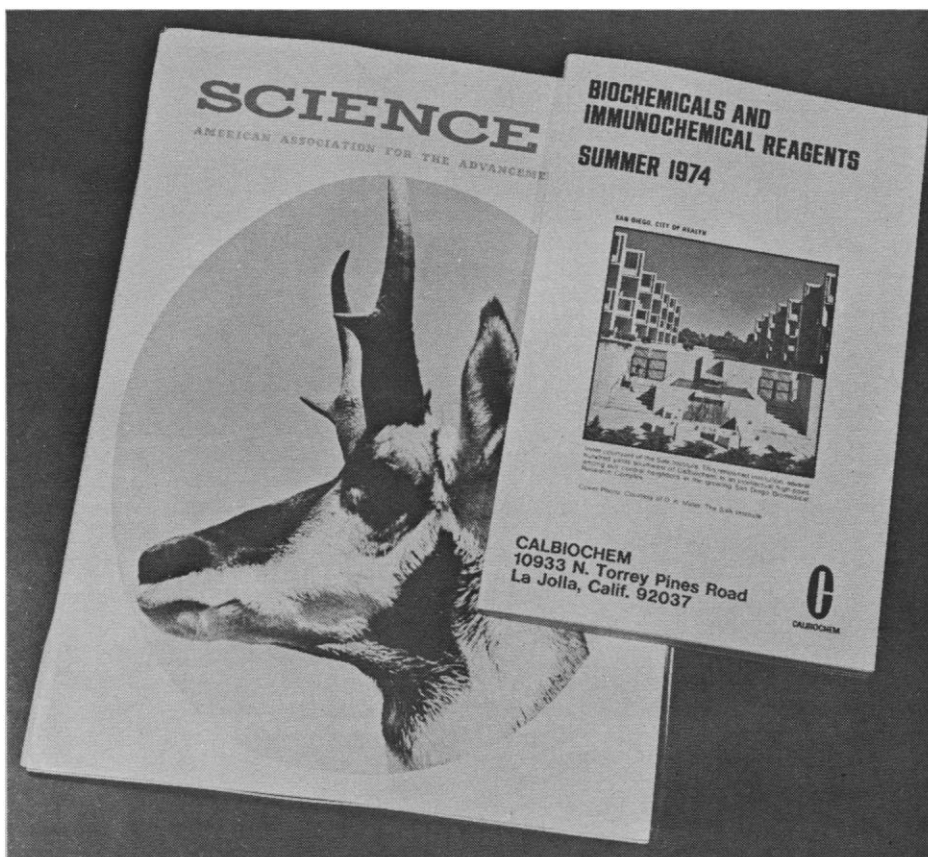


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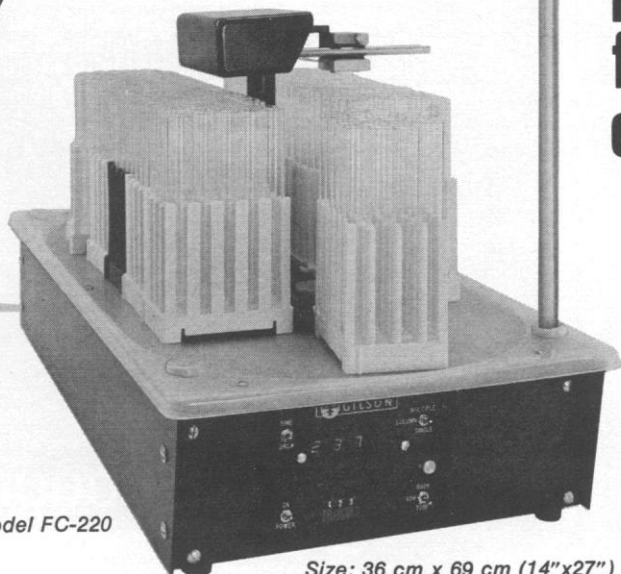
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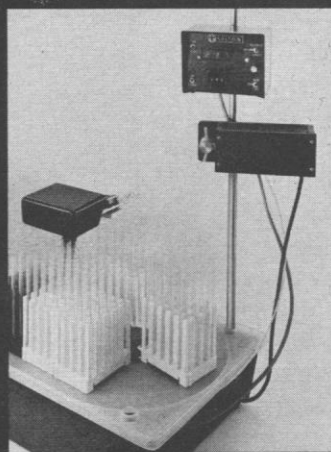
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Politics and Unesco

The vote at the 18th general conference of Unesco to prevent admission of Israel to its European regional group, and the approval of a resolution requesting the Director-General not to extend small financial assistance to Israel, are not the way an organization should operate that was established to wipe out ignorance and prejudice. These actions mark a sad incident in the history of Unesco, and they represent what Dag Hammarskjöld called "illusory voting victories." Those of us who served on the delegations to the conference know how hard certain people worked to avoid these unfortunate actions.

Since the conference many members of the U.S. Congress, several Nobel laureates, groups of college presidents, scientists, and others have condemned most things Unesco stands for because of these two actions. Some of these statements and actions may show even poorer judgment than that exercised by certain delegations at the conference. The post-conference critics should have taken more time to understand and assess the events that occurred during the Unesco conference, for they were not acting under the pressures of a multilingual 135-nation conference.

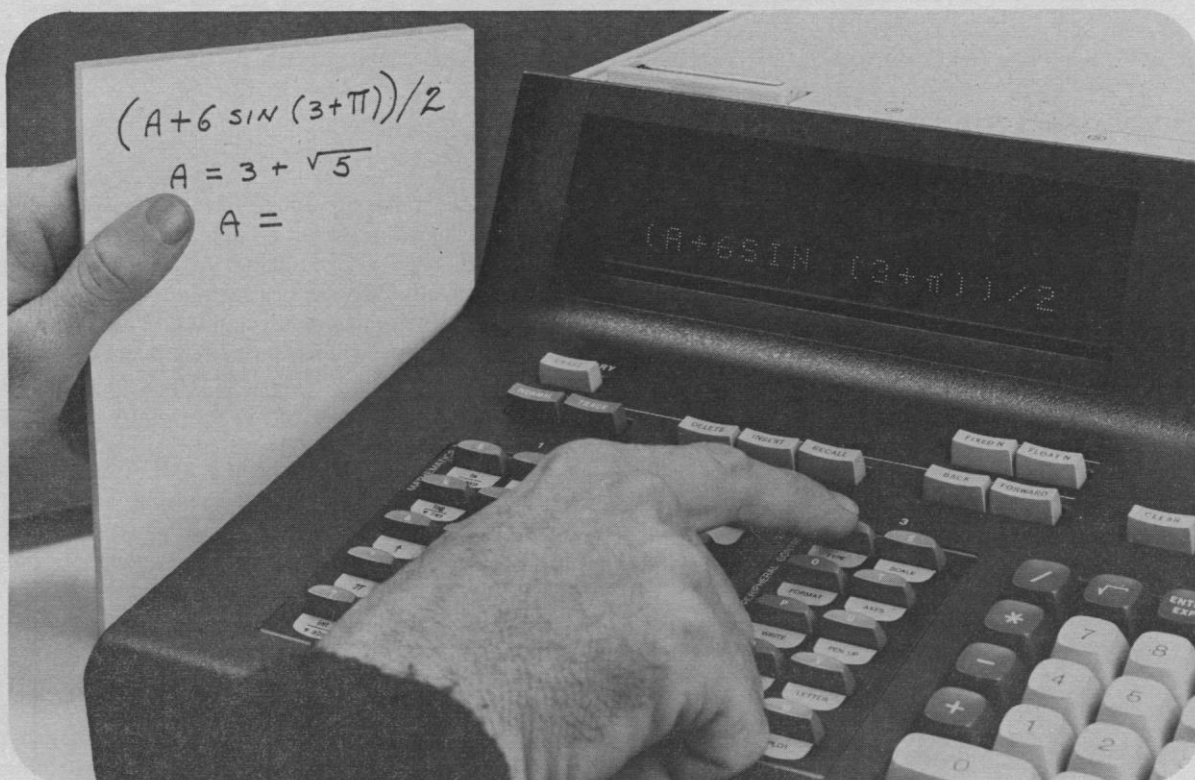
Until this 18th conference, Canada, Israel, and the United States had not been assigned to any Unesco regional group, because of geographic location, because they preferred independence, or because of other factors. At this last meeting Canada and the United States were admitted to the European group, but not without some heated debate. Although the vote favoring Israel was tied on the first ballot and close on the second, the motion lost. Israel, however, can still attend any regional group. We hope that at the next general conference understanding and wisdom will prevail so that Israel will be able to vote in a regional group.

Criticism of Israeli archeological work in the Holy City during the debates in the commission dealing with cultural heritage may or may not have been justified. It was fanned by the current passion associated with the problems of the Middle East. Had certain delegates exercised leadership and statesmanship during the Paris meeting this whole matter could have been delayed until the United Nations reaches a decision about the special status of Jerusalem.

Scientists can be pleased that little or no national politics entered the debates and actions of the commission on science; possibly this is because science by its very nature recognizes no national boundaries. Many important, far-reaching, and worthwhile activities and programs were approved by this commission, including: (i) the Man and Biosphere Program, (ii) the Geological Correlation Program, (iii) a World System for Transmitting Information in Science and Technology or UNISIST, (iv) a modest subvention to help nongovernmental scientific unions that are members of the International Council of Scientific Unions (ICSU), (v) support of the International Oceanographic Commission, and (vi) other highly significant programs concerned with teaching and research in the basic and applied areas of biology, chemistry, physics, and engineering as they relate to the ecological, earth, freshwater, and marine sciences.

The Unesco general conference elected a new Director-General who is dynamic, capable, and understanding of world problems. The science sector is in good and capable hands. Thus scientists are urged to give the secretariat a chance to demonstrate progress in handling the hundreds of worthwhile programs approved at the conference before condemning the entire organization because of two unfortunate actions at a general conference. If delegations learned even a little bit more about the meaning of international responsibility from their experience at this conference, perhaps at the next conference better reason, compassion, and cooperation will be demonstrated.—J. R. PORTER, *Department of Microbiology, University of Iowa, Iowa City 52242*

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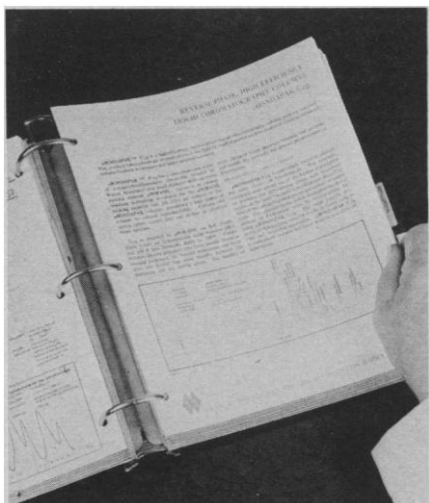
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and O. Sparre-Anderson. Ionic transport through hydrophilic pores (A. Finkelstein, discussion leader): J. Moreno, and M. Eisenberg. Membrane transport related to other biological processes (W. Hagins, discussion leader): P. Maloney, S. Yoshikami. Voltage-dependent gating (H. Lecar, discussion leader): J. Hall, F. Bezanilla, L. Goldman. Functional organization of epithelium (J. Diamond, discussion leader): E. Frömter, and S. Lewis. Reconstitution of function in membranes (T. Thompson, discussion leader): P. Hinkle, G. Kemp. Synaptic transmission (W. Van der Kloot, discussion leader): F. Sachs, and A. Clark. Membrane fluidity (S. McLaughlin, discussion leader): M. Edidin, and H. McConnell. Summary (G. Ehrenstein, discussion leader): P. Mueller.

Transport Phenomena in Synthetic and Biological Membranes

Colby Women's College

Norman N. Li, chairman; Christopher T. Rhodes, vice chairman.

14 July. William J. Ward, III, "Mass transfer through liquid and polymer membranes"; R. M. Minday, "Separations by selective transfer through liquid membranes." (Harold K. Lonsdale, discussion leader): H. K. Lonsdale and W. Pusch, "Donnan effects in synthetic membranes"; E. Drioli, H. K. Lonsdale and W. Pusch, "Dynamically formed and transient membranes."

15 July. (Christopher T. Rhodes, discussion leader): Victor Smolen, "Cooperative phenomena in solute interactions and transport"; G. N. Ling, "Applications of physical chemical theories to transport in biological membranes." (Donald R. Paul, discussion leader): C. E. Rogers, "Structure modification of polymer transport properties"; short presentations (invited or contributed by attendees. Attendees who would like to contribute should contact Prof. D. R. Paul of University of Texas, Austin, Texas 78712 before 15 June). A. A. Kozinski, "Separations with protein liquid crystals"; Joseph D. Henry, Jr., "Potential applications of dynamic membranes in biological transport systems."

16 July. H. B. Hopfenberg, "Relaxation-controlled transport processes in polymers"; William Eykamp, "Membrane processing of aqueous solutions." (Gilbert N. Ling, discussion leader): H. R. Kaback, "Molecular aspects of active transport in isolated bacterial

membrane vesicles"; Richard Jones, "Investigation of drug transport across the skin."

17 July. (James Danielli, discussion leader): David Trigg, "Calcium utilization and transport in smooth muscle"; Harden M. McConnell, "Molecular motion in biological membranes." (Clark K. Colton, discussion leader): Thomas M. S. Chang, "Microcapsule artificial kidney"; John A. Quinn, "Carbon dioxide transport through reactive membranes."

18 July. Harry P. Gregor, "Electrolytic water splitting"; Edwin N. Lightfoot, Jr., "Modeling membrane transport."

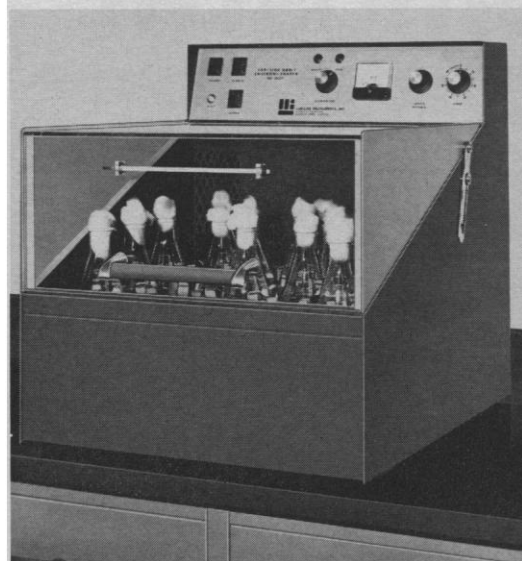
Toxicology and Safety Evaluations

Kimball Union Academy

Ralph C. Wands, chairman; Anne M. Wolven, vice chairwoman.

28 July. (L. D. Scheel, chairman): J. D. MacEwen, "Inhalation toxicology

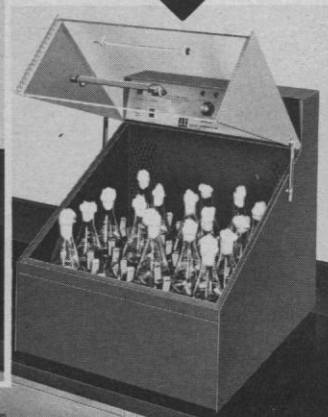
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of coal tar pitch volatiles"; C. F. Reinhardt, "Fire toxicology." (William E. MacDonald, chairman): P. N. Kaul, "Toxicology of marine natural products."

29 July. (Rolf Hartung, chairman): W. B. Buck, "Toxicology of copper and related metals"; Donald Barltrop, "Toxicology and absorption of lead in the young animals." (R. M. Hehir, chairman): W. K. Talley, "Research and development at EPA"; John Buckley, "Substitute pesticide development."

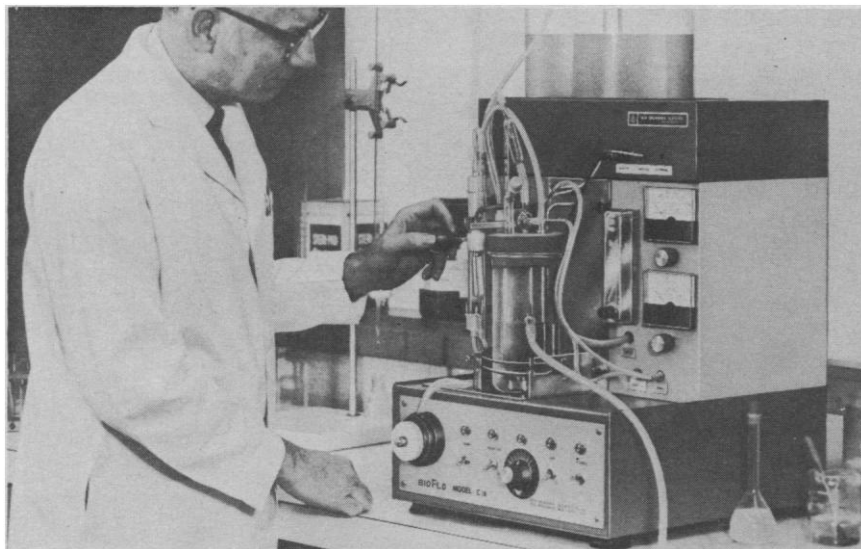
30 July. (W. G. Flamm, chairman):

J. F. Fraumeni, "Human genetics and cancer"; David Brusick, "Mutagenesis and carcinogenesis screening." (R. J. Weir, chairman): A. E. New, "Primate supply and breeding."

31 July. (E. V. Anderson, chairman): Panel discussion: Carcinogenesis data extrapolation. David Gaylor, R. E. Albert and C. S. Weil. (B. D. Dinman, chairman): W. W. Lowrance, "Safety evaluation and the public."

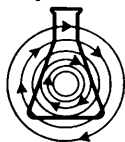
1 August. (A. M. Wolven, chairwoman): W. R. Muir, "Environmental decision making."

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Clifford A. Barnes. Washington Sea Grant Program, Seattle, 1974 (distributor, University of Washington Press, Seattle). xii, 236 pp. Paper, \$15.

Automata, Languages and Programing. Papers from a colloquium, Saarbrücken, Germany, July 1974. Jacques Loeckx, Ed. Springer-Verlag, New York, 1974. 620 pp., illus. Paper, \$19.70. Lecture Notes in Computer Science, vol. 14.

The Behavioral Treatment of Psychotic Illness. Advances in Theory and Technique. William J. DiScipio, Ed. Behavioral Publications, New York, 1974. 240 pp., illus. \$12.95.

Bilayer Lipid Membranes (BLM). Theory and Practice. H. Ti Tien. Dekker, New York, 1974. xii, 656 pp., illus. \$39.50.

Biochemical Reactors. B. Atkinson. Pion Limited, London, 1974 (U.S. distributor, Academic Press, New York). xiv, 268 pp., illus. \$13. Pion Advanced Biochemistry Series, 2.

The Biology of the Laboratory Rabbit. Steven H. Weisbroth, Ronald E. Flatt, and Alan L. Kraus, Eds. Academic Press, New York, 1974. xiv, 496 pp., illus. \$49.50.

Brain Dysfunction in Metabolic Disorders. Proceedings of a symposium, Dec. 1973. Fred Plum, Ed. Raven, New York, 1974. xii, 324 pp., illus. \$21.50. Association for Research in Nervous and Mental Disease Research Publications, vol. 53.

By Bread Alone. Lester R. Brown with Erik P. Eckholm. Published for the Overseas Development Council by Praeger, New York, 1974. xvi, 272 pp., illus. \$8.95.

Calkin Algebras and Algebras of Operators on Banach Spaces. S. R. Caradus, W. E. Pfaffenberger, and Bertram Yood. Dekker, New York, 1974. x, 146 pp. Paper, \$13.75. Lecture Notes in Pure and Applied Mathematics, vol. 9.

Capitalism in a Changing World. Rex Dye. Vantage, New York, 1974. x, 230 pp. \$9.50.

The Challenge of Facts. Selected Public Health Papers of Edgar Sydenstricker. Richard V. Kasius, Ed. Published for the Milbank Memorial Fund by PRODIST, New York, 1974. xii, 386 pp., illus. \$15.

Changing Perspectives in the History of Science. Essays in Honour of Joseph Needham. Mikuláš Teich and Robert Young, Eds. Reidel, Boston, 1973. xxii, 490 pp., illus. + plates. \$19.50.

Chemistry of the Carbonyl Group. A Programmed Approach to Organic Reaction Mechanism. Stuart Warren. Wiley, New York, 1974. Various pages. Paper, \$4.95.

Child-Centered Group Guidance of Parents. S. R. Slavson. International Universities Press, New York, 1974. x, 334 pp. Paper, \$3.95. Reprint of the 1958 edition.

Children in the Hospital. Thesi Bergmann in collaboration with Anna Freud. International Universities Press, New York, 1974. 162 pp. Paper, \$2.45. Reprint of the 1965 edition.

Chromosomes and Cancer. James German, Ed. Wiley, New York, 1974. xxviii, 756 pp., illus. \$35. Chromosomes Series, vol. 1. A Wiley Biomedical-Health Publication.

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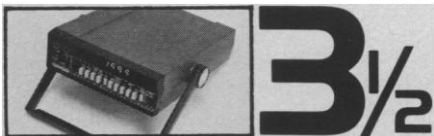
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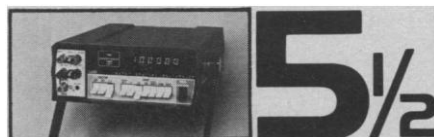
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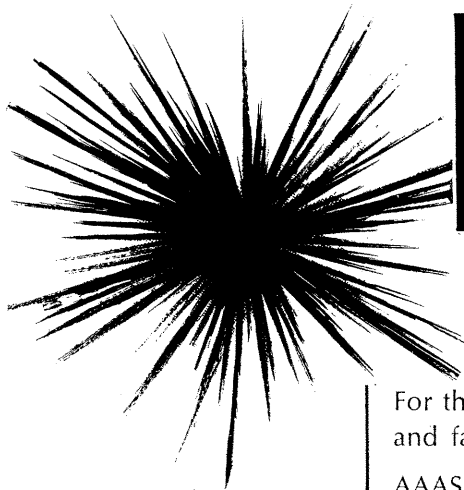
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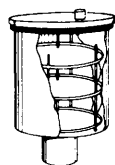
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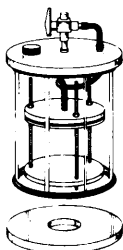
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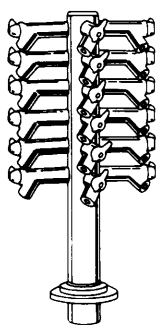
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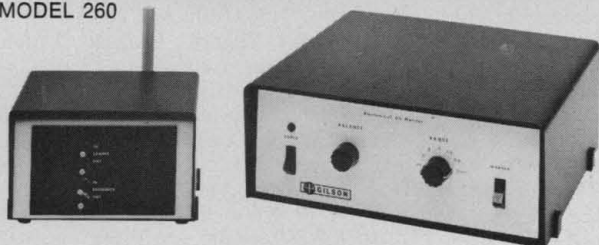
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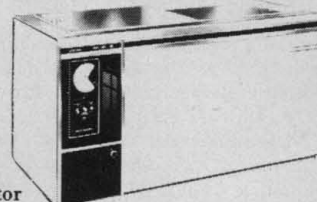
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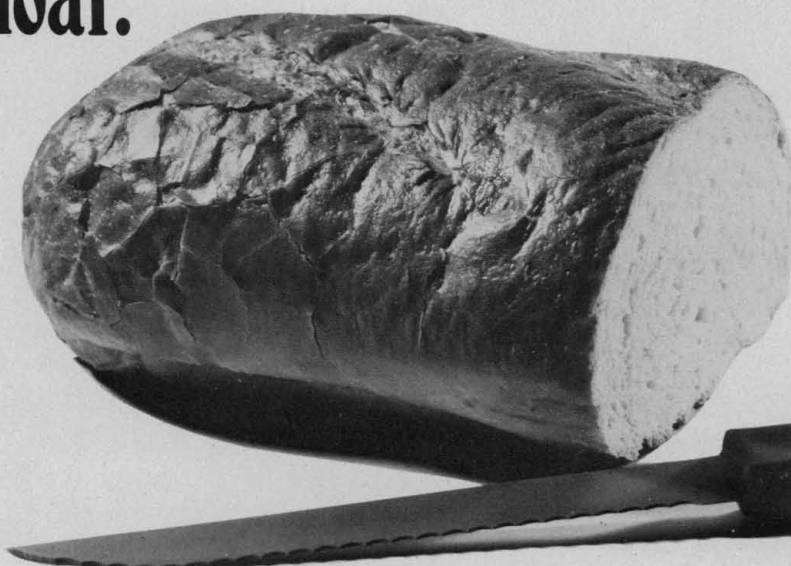
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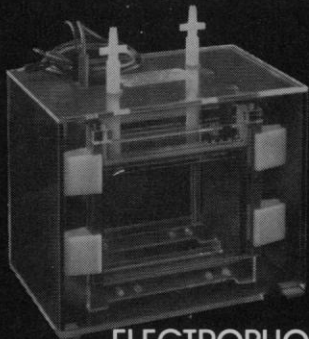
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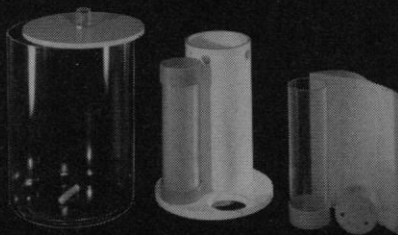
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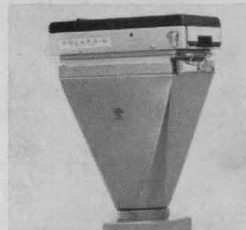
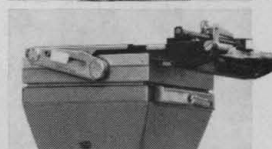
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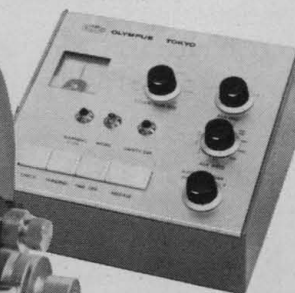
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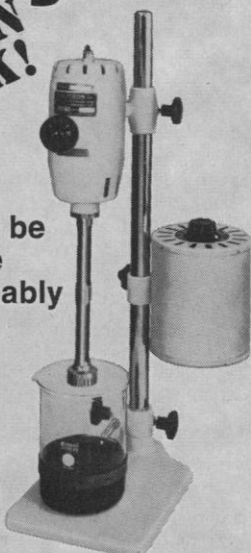
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