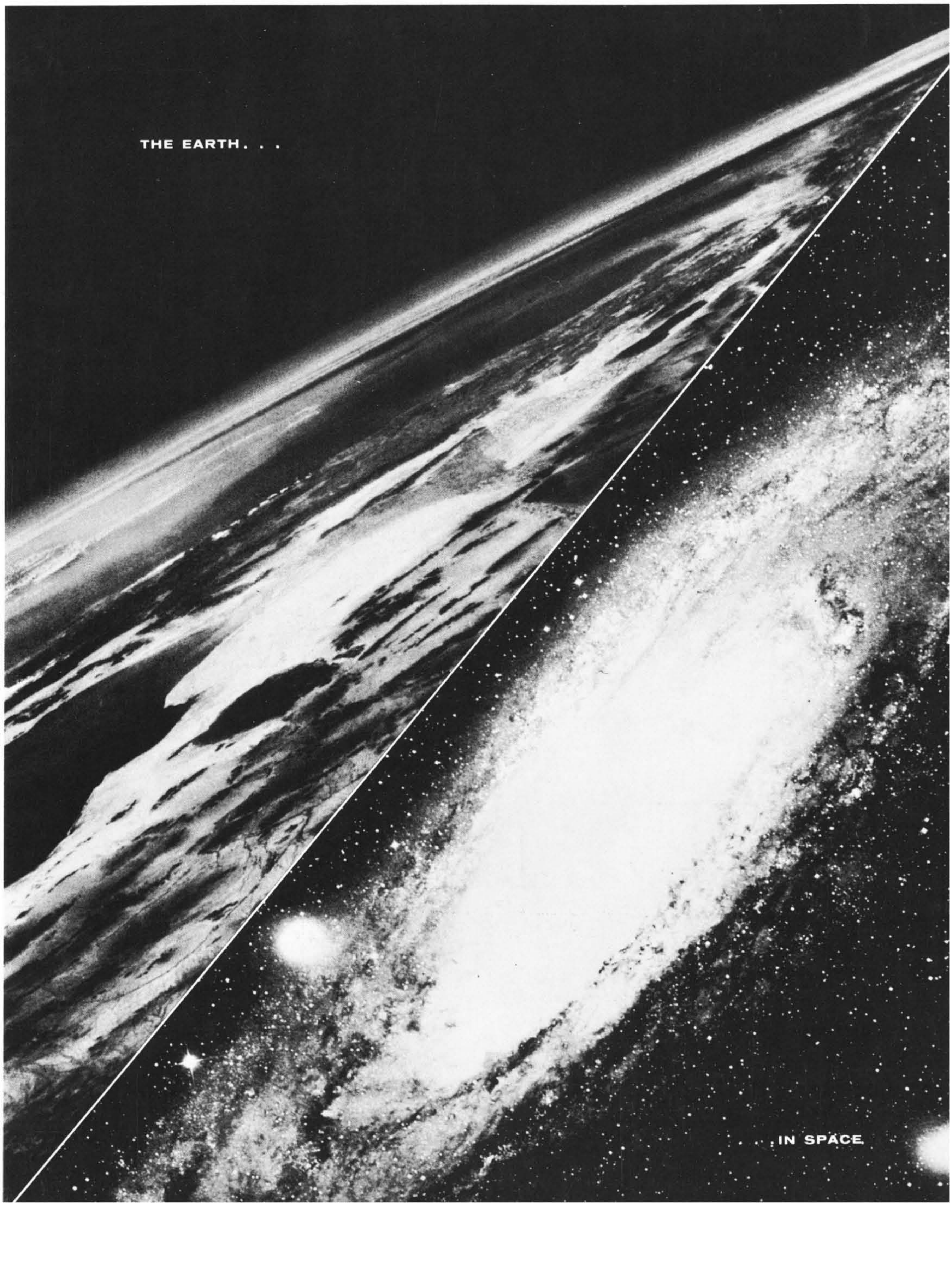
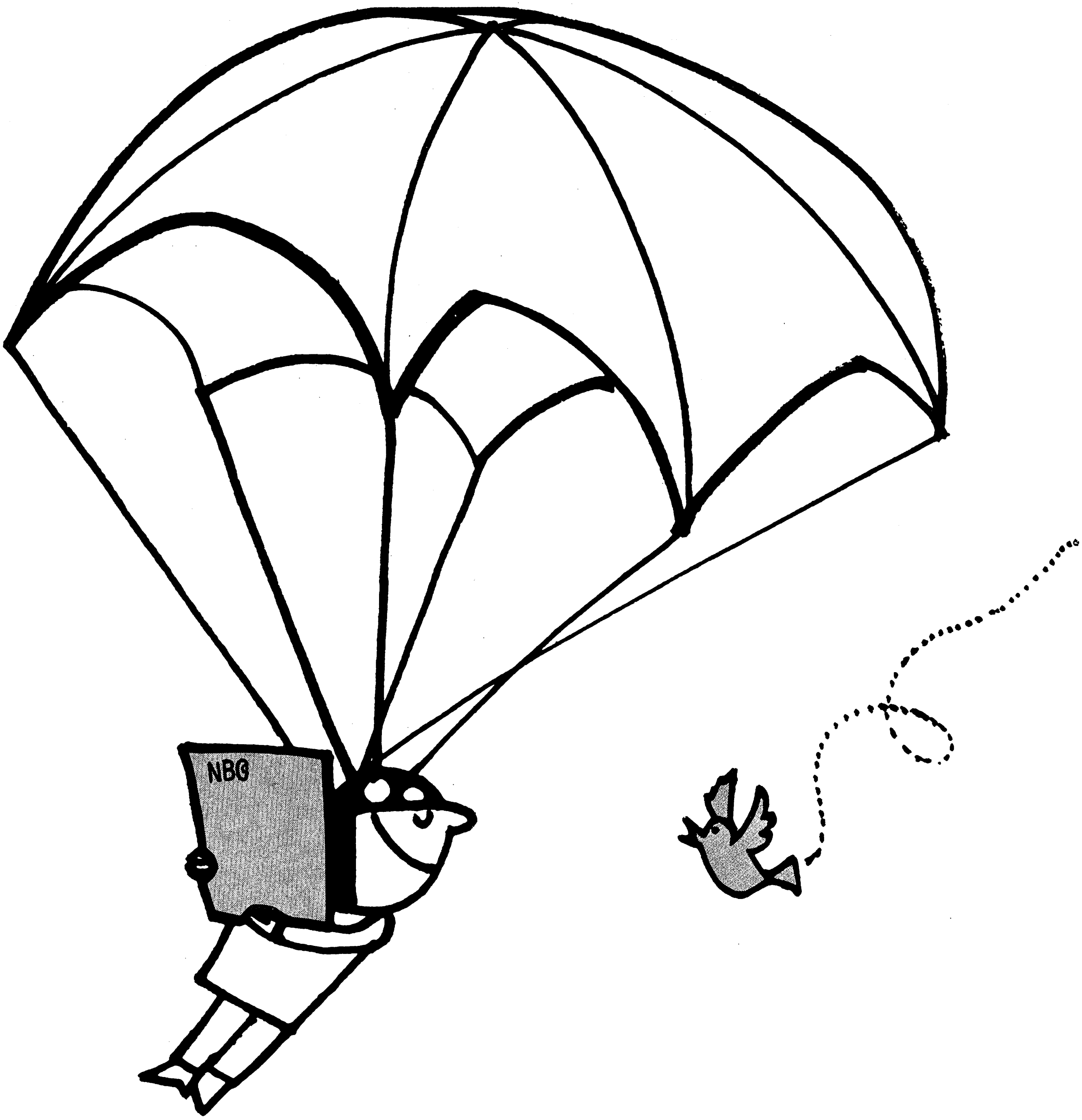
SCIENCE

9 November 1962

Vol. 138, No. 3541

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE





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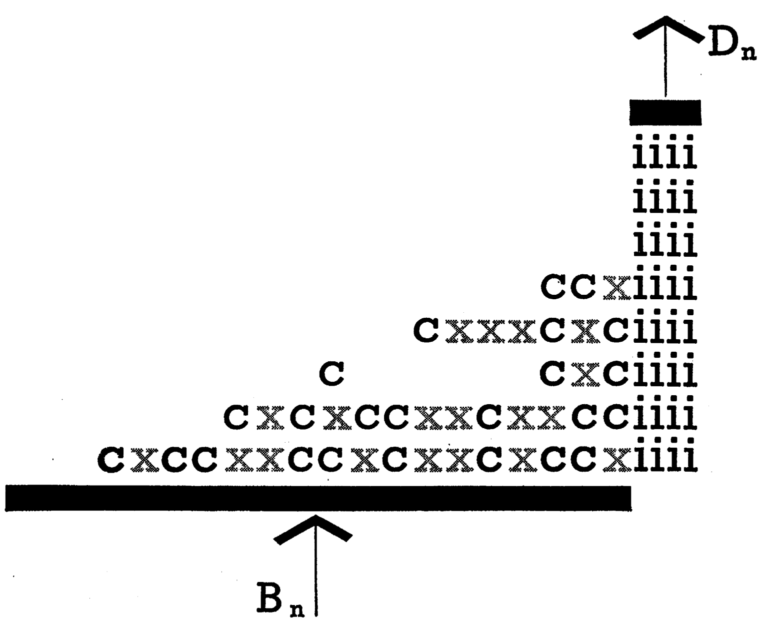
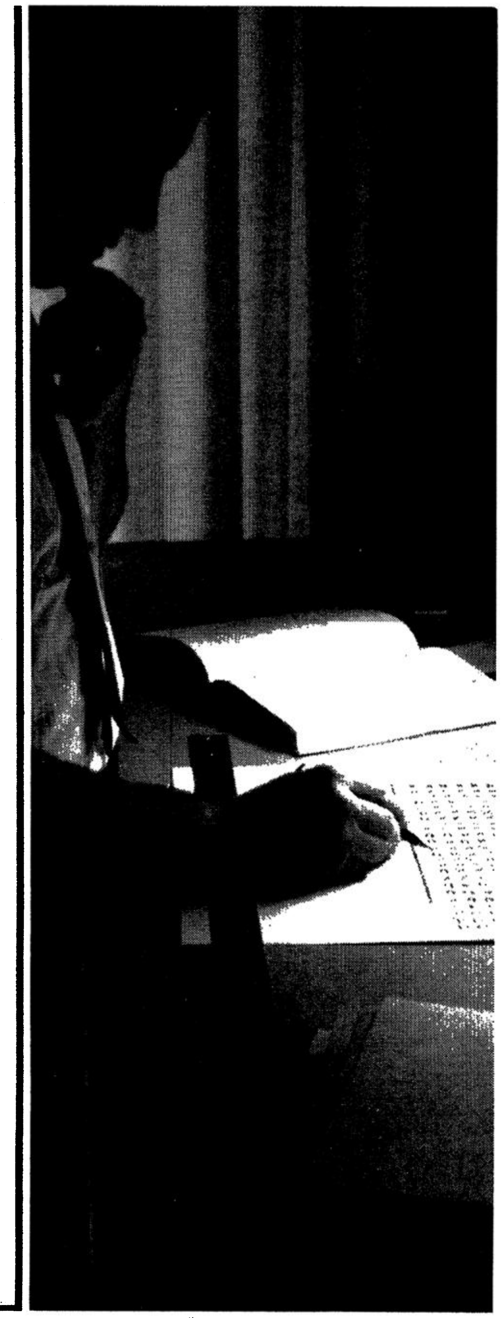
9 November 1962, Volume 138, Number 3541

SCIENCE

Cover *Left*, The earth, taken in infrared radiation, from a height of about 70 miles. The Pacific Ocean is on the horizon, with Los Angeles near the upper right. The largest dark area on the left is the Gulf of California. *Right*, Andromeda Nebula, the nearest large spiral nebula, distant about 9×10^{18} miles. See page 653.

design
DECISION





WHICH IS BEST... *for a 4096 channel analyzer:*

1 **A parallel, binary arithmetic system, which has the advantages of greater simplicity, better cathode ray tube display, fewer memory planes and arithmetic stages, faster multichannel scaling, and greater versatility?**

2 **A parallel, binary coded decimal arithmetic system which has the advantage of very high speeds of decimal information readout?**

The first thought is to use binary coded decimal arithmetic, of course, because in a 4096 channel analyzer, decimal information readout speed is a prime design consideration.

But Nuclear Data's new 4096 channel, two parameter analyzer uses parallel, binary arithmetic.

Why?

Because Nuclear Data has devised a new method for converting from binary to decimal which does the job orders of magnitude faster than the usual method. And does the reverse just as quickly, to allow fast read back of decimal information from punched paper tape.

As a by-product, the translator also decodes the binary address for each channel, to provide locked decimal channel identification. And it provides readout in the form of a running subtotal if desired by the operator.

We have all of the advantages of both number systems, in the new analyzer, and then some, which is no trivial accomplishment. But there are other little breakthroughs in the design of the instrument, of greater importance and about which you will be hearing a lot.

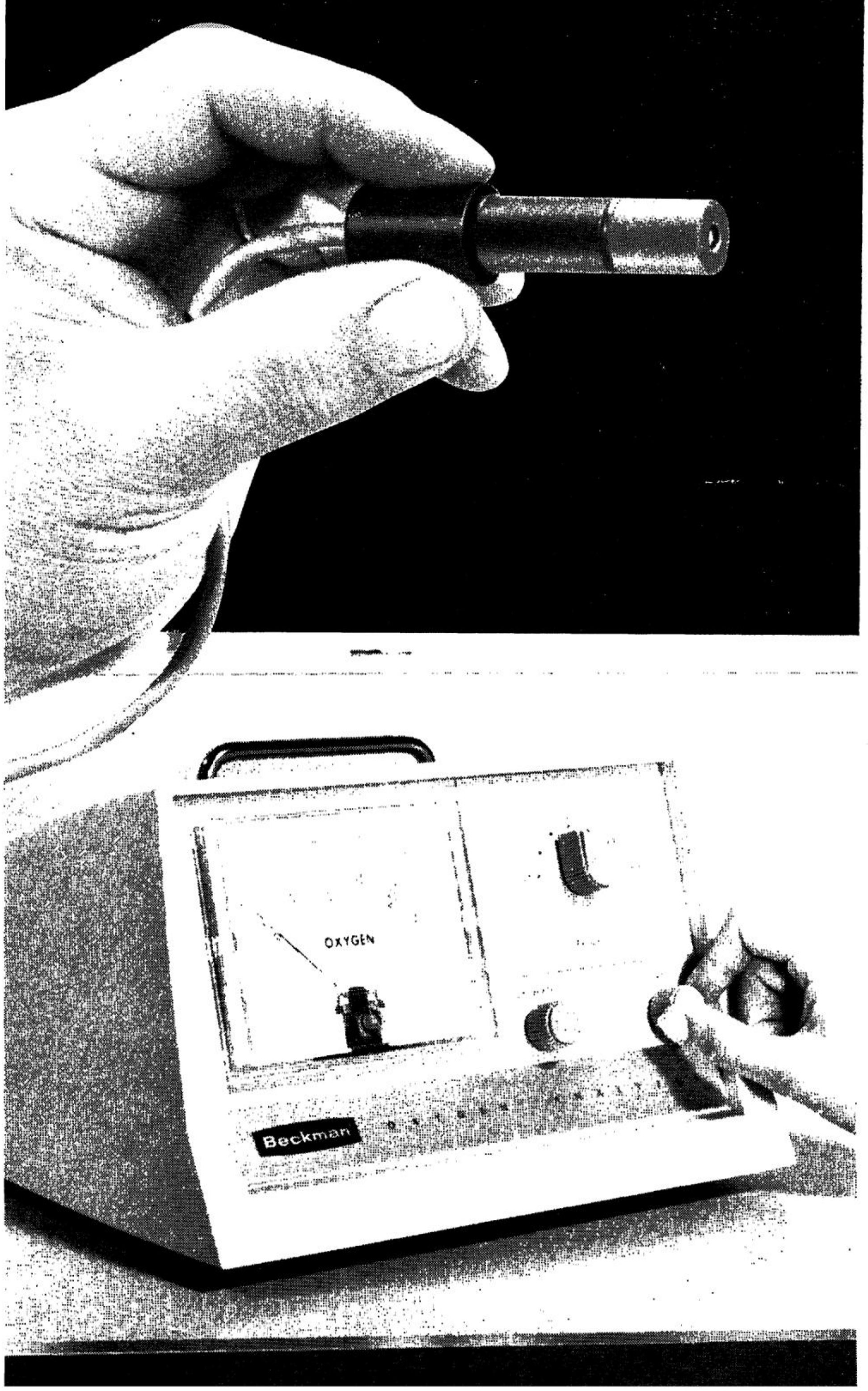
We don't plan to say a word about these for a month or two. But we will be delivering the analyzers on time, as usual, before winter is over, at a sensible price.

NUCLEAR
DATA
INC.

3833 West Beltline Hy.
Madison 13, Wisconsin

new Beckman system

measures gaseous O_2 and dissolved oxygen



This is the concept developed by Beckman for critical oxygen analyses in the Project Mercury space capsules. Now Beckman offers a commercial version of this rugged, highly successful system for laboratory measurement of gaseous oxygen *or* of dissolved oxygen in aqueous or non-aqueous solutions.

The New Beckman Oxygen Sensor is the key to the system. Working on a patented polarographic principle*, a silver anode and a gold cathode are separated from the sample by an oxygen-permeable membrane. Thus, the membrane prevents sensor contamination, yet permits oxygen to diffuse to the sensor for fast determinations–90% of response in less than ten seconds. A built-in thermistor compensates for temperature fluctuations. Readout is accomplished by one of two new Beckman devices:

Beckman Model 777 Oxygen Analyzer has a direct-reading meter plus recorder output, and translates sensor signal into convenient measurement units. Multiple ranges are expressed in per cent oxygen mm. partial pressure, per cent air saturation, and parts per million dissolved oxygen. Features a line-operated, solid-state A.C. amplifier for drift-free performance. Especially designed for the modern laboratory, the Model 777 is attractively housed in a light-weight, corrosion-resistant, polypropylene case.

Beckman Oxygen Adapter allows the Beckman Model 76 Expanded Scale pH Meter to function as an oxygen analyzer The Model 76 can then provide the same oxygen measurement ranges as the Model 777 with the same accuracy. The laboratory is thus equipped with a multi-purpose instrument readily convertible for either pH or oxygen measurements.

Your Beckman dealer has full particulars on these exciting new laboratory oxygen measuring devices. Remember too, that Beckman has comparable process oxygen equipment. Ask him, or write for data file LO-38-163.

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*U.S. PATENT NO. 2,913,386

MEETING • 26-30 DECEMBER

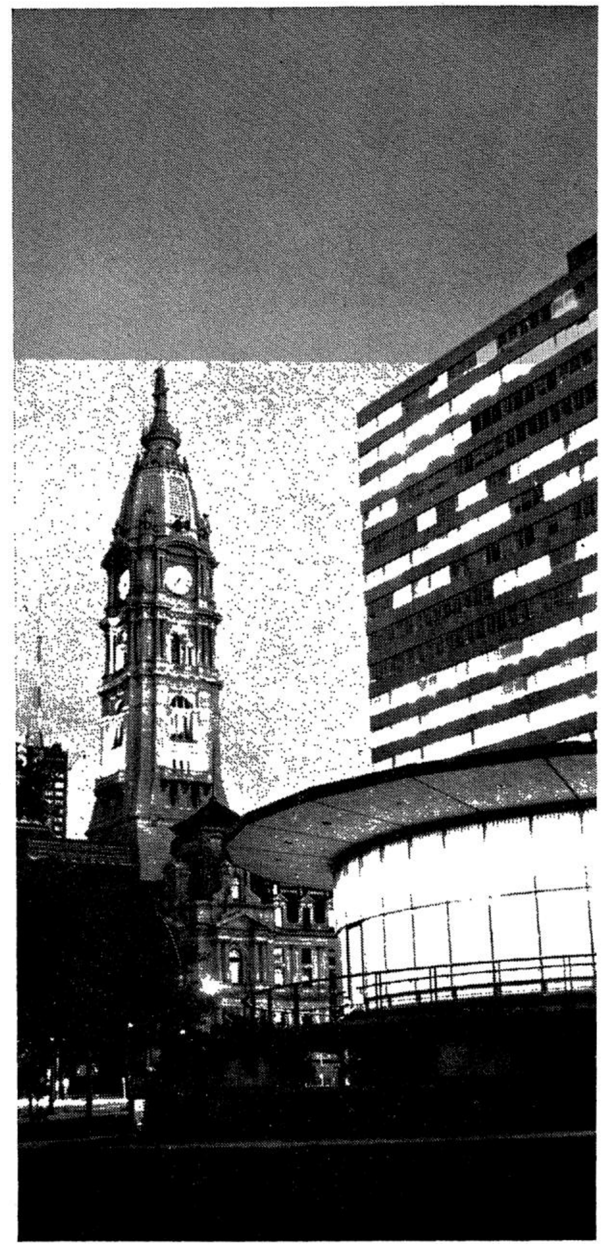
Reserve Your Hotel Room

Make sure you have the accommodations you prefer. A list of headquarters hotels of participating societies appears on page 235, SCIENCE, 20 July. The AAAS headquarters is the Sheraton.

The hotels for the AAAS Philadelphia meeting have established special, low flat rates and have reserved large blocks of rooms for the meeting.

Use the coupon below to make your hotel reservation in Philadelphia. Send your application to the AAAS Housing Bureau in Philadelphia, not to any hotel. Give a definite date and estimated hour of arrival, and also probable date of departure. The Housing Bureau will make the assignment and send you a confirmation in two weeks or less.

A rollaway bed can be added to any room at $3.00 per night. Mail your application now to secure your first choice of accommodations.



Pontiac Motor Division—*Safari Magazine*

HOTEL RATES*
AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

For a list of the headquarters of each participating society and section, see page 235, *Science,* 20 July.

Hotel	Single Bed	Double Bed	Twin Beds	Suites
Sheraton**	$8.50		$14.00	$35.00—$45.00
Bellevue Stratford**	8.50		14.00	32.00— 60.00
Warwick**	8.50		14.00	30.00— 65.00
Franklin Motor Inn	8.50		14.00	
Robert Morris	5.50—7.50	$9.00—$11.00	10.00—11.00	
Sylvania	7.50	11.00	12.00	
Adelphia	7.50	11.00	12.00	
Benjamin Franklin	8.50	12.00	14.00	32.00— 60.00

* All rooms are subject to a 4% Pennsylvania state sales tax.
** Hotels with sessions.

AAAS Housing Bureau, Hospitality Center.
16th Street and Pennsylvania Boulevard
Philadelphia 2, Pa.

Date of Application ....

Please reserve the following accommodations for the 129th Meeting of the AAAS in Philadelphia, 26—30 December, 1962

First Choice Hotel .... Second Choice Hotel .... Third Choice Hotel ....

Type of room: Single ☐ Double ☐ Double, twin beds ☐ Suite ☐ Rates: Desired .... Maximum ....

Number in party... Sharing this room will be: ....

(List name and address of each person, including yourself. Attach list if space is insufficient.)

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....

DATES: ARRIVAL .... A.M. ........P.M........ DEPARTURE ....
(These must be indicated—add approximate hour, A.M. or P.M.)

NAME ....
(Individual requesting reservation) (Please print or type)

ADDRESS ....
(Street) (City and Zone) (State)

Mail this coupon now to the Housing Bureau. Rooms will be assigned and confirmed in order of receipt of reservation.

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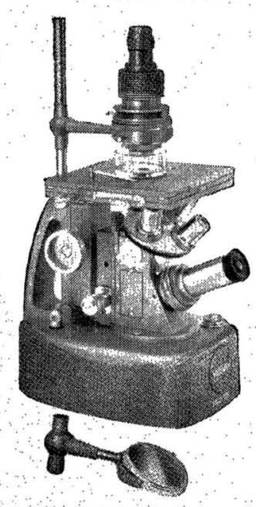
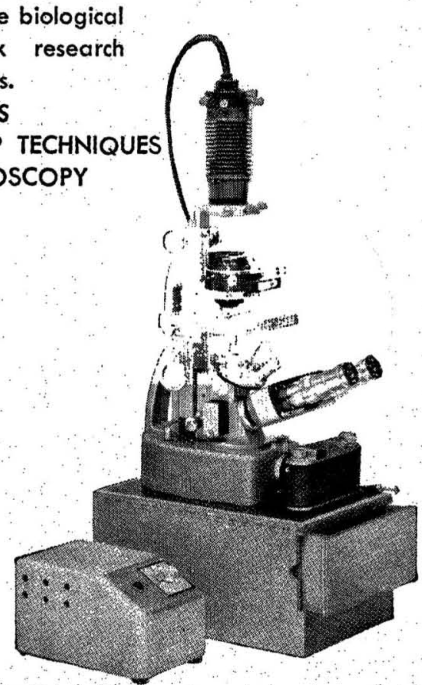
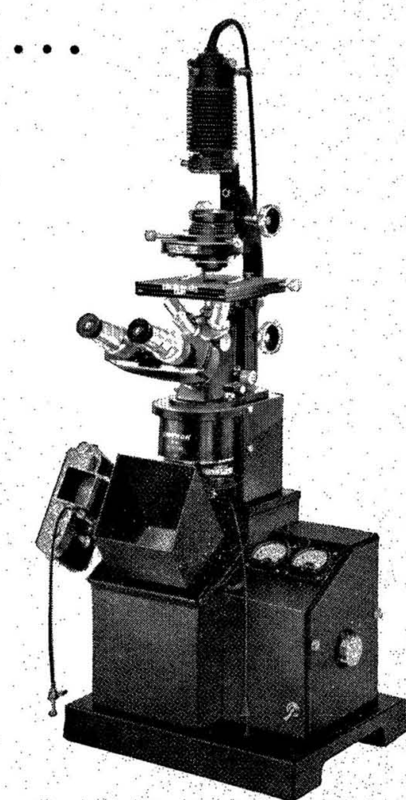


MODEL MIC



MODEL PH-BMIC



MODEL BU-13

INVERTED LABORATORY AND RESEARCH MODELS

Brightfield Laboratory Models:

MONOCULAR MODEL MIC. Four brightfield objectives 5X, 10X, 40X, 100X (oil); eyepieces 5X, 10X, 15X; ample height adjustment of condenser-illuminator for even large culture bottles; built-in base transformer. **$409.**

BINOCULAR MODEL BMIC. Binocular version of Model MIC, with camera mechanism. **$609.**

Brightfield Research Models:

MONOCULAR MODEL BR-MIC. Five brightfield objectives 5X, 10X, 20X, 40X, 100X (oil); eyepieces 5X, 10X, 15X; rack and pinion condenser mechanism with individual centering adjustments for condenser and illuminator; elevating compartment provides handy storage for accessories. **$545.**

BINOCULAR MODEL BR-BMIC. Binocular version of Model BR-MIC, with camera mechanism. **$745.**

Phase Research Models:

MONOCULAR MODEL PH-MIC. Eight phase objectives 10X, 20X, 40X, 100X (oil) in both bright and dark-medium contrast; eyepieces 5X, 10X, 15X; high intensity Koehler-type illuminator; five-choice intensity transformer; phase turret condenser with aperture for brightfield; elevating base. **$812.**

BINOCULAR MODEL PH-BMIC. Binocular version of Model PH-MIC plus built-in camera mechanism. **$1012.**

Prices include optics, cabinets, filters, special slides, petri dishes, and basic accessories. The built-in camera mechanism is standard with binocular models and available as an accessory for monoculars. Accommodates 35mm. camera back or Polaroid Land Camera Attachment. Both available at extra cost.

CAMERA MICROSCOPES

The all-purpose microscope for visual examination, screen viewing and photomicrography. Built-in 3¼" X 4¼" camera with four flat field photo-eyepieces on revolving turret. Accessory attachments for 35mm., Polaroid, and movie cameras. Low-power (5X–40X) accessories available. Needs only 9" x 12" table space.

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BINOCULAR MODEL BU-12. Binocular version of Model U-12. **$1379.**

Phase Research Models:

MONOCULAR MODEL U-13. Same phase objectives, turret condenser, and visual eyepieces as Model PH-MIC. **$1390.**

BINOCULAR MODEL BU-13. Binocular version of Model U-13. **$1580.**

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accommodates slides, wet mounts, special glassware, warming chambers and micro-manipulators ● unobstructed stage for easy access ● built-in, correct intensity illumination ● glare-free coated optics ● special petri dishes for observation of cultures even by highest power oil immersion objective ● graduated mechanical stage ● accessory camera attachments ● long working distance 40X objective and other accessories also available

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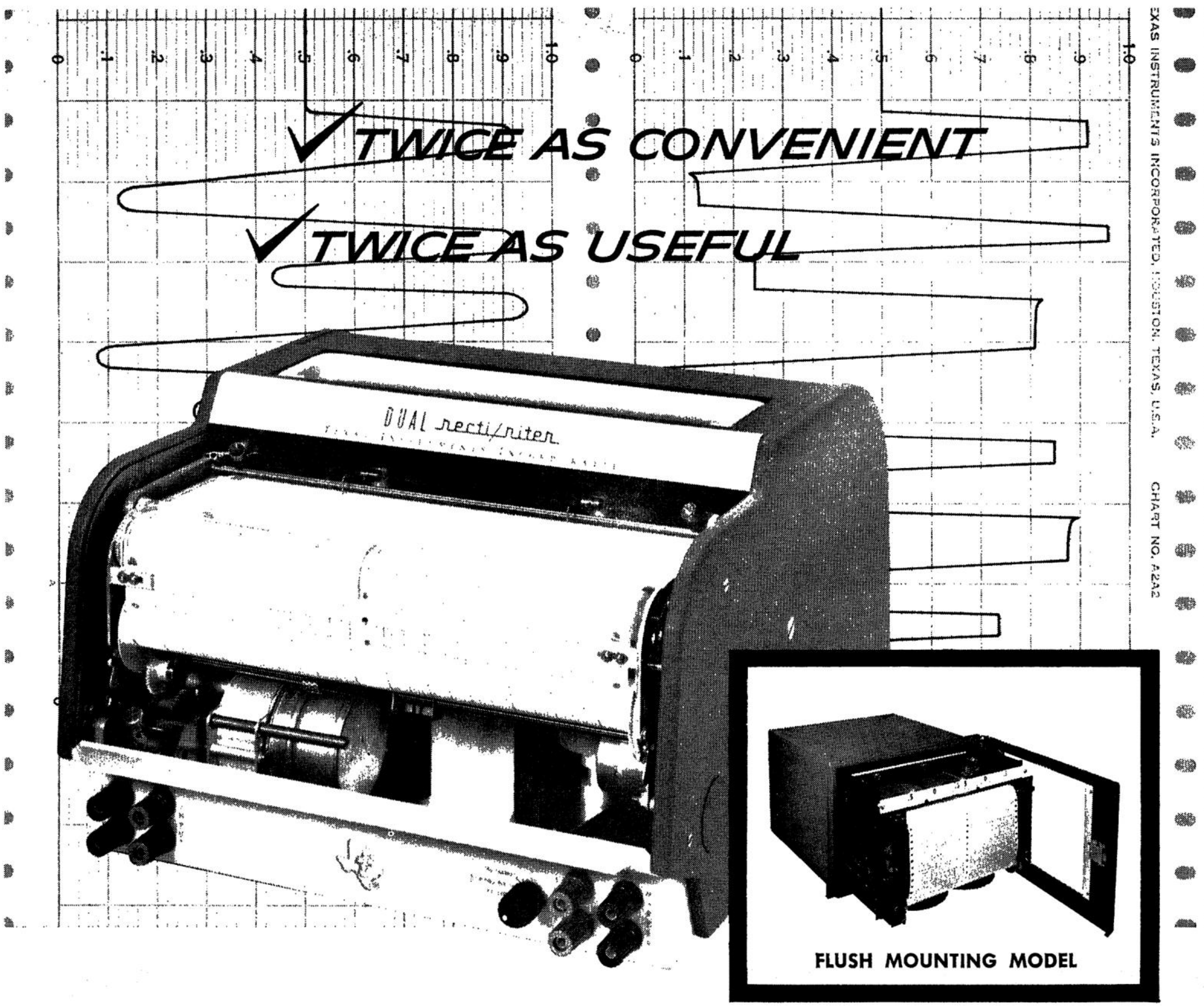

FLUSH MOUNTING MODEL

Record Two Variables Side by Side with the DUAL *recti/riter*® Recorder

The DUAL *recti/riter* recorder combines in one compact instrument two complete galvanometric recorders and a single chart drive . . . enabling you to record two variables simultaneously to a common time base. No chart drive synchronization problems, just one chart roll to handle and data can be interpreted in a glance. Only the DUAL *recti/riter* offers true rectilinear recording on two 4½-inch, side-by-side grids. Whether you use it primarily as a single channel instrument or for the hundreds of applications requiring the direct comparison of two variables, you'll find the DUAL *recti/riter* offers greater convenience and unmatched versatility. Available in either portable or flush-mounting models in the widest selection of standard ranges.

Two-Cycle Pen Response

d-c Milliampere Ranges.......................... ½ ma to 100 ma
a-c Ampere Ranges.......................... 0.25 amp to 25 amp
d-c Ampere Range.......................... 100 mv for use with standard shunts
Expanded Scale a-c Voltage Ranges.......................... 80-130 V, 160-260 V, 320-520 V
a-c and d-c Voltage Ranges.......................... 10 V to 1000 V
Frequency Ranges.......................... 50, 60, 400 cps

Five-Cycle Pen Response

d-c Milliampere Ranges.......................... 2.5 ma to 125 ma

Special options and accessories further expand the usefulness of *recti/riter* recorders. Also available in single channel models.

Write for complete information.

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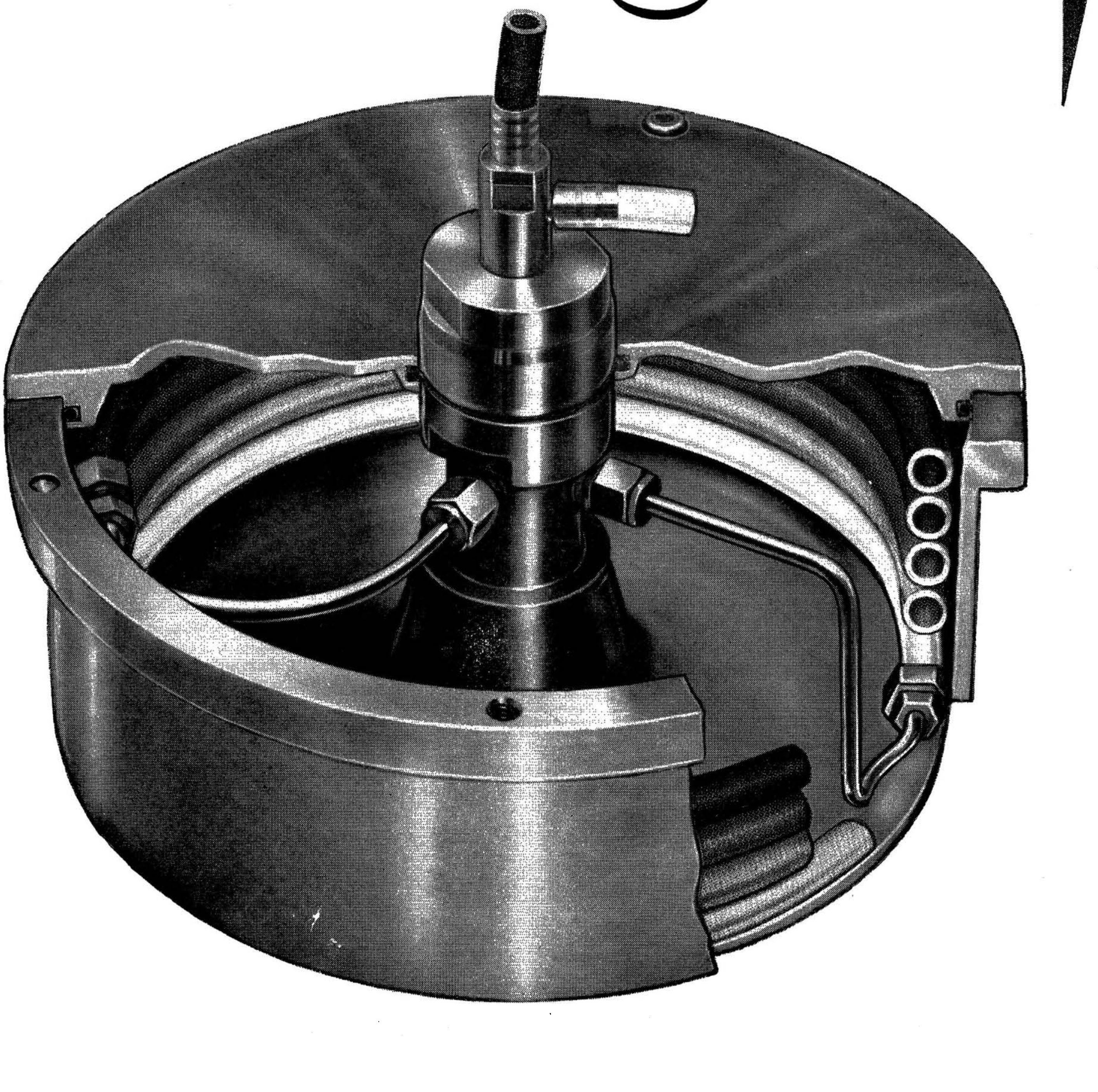
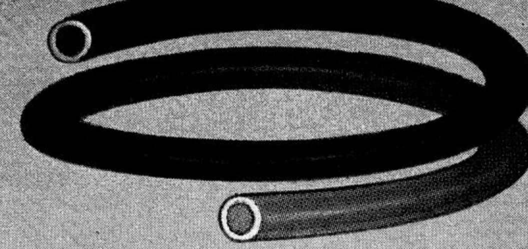
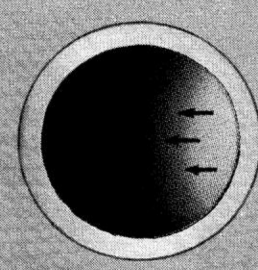




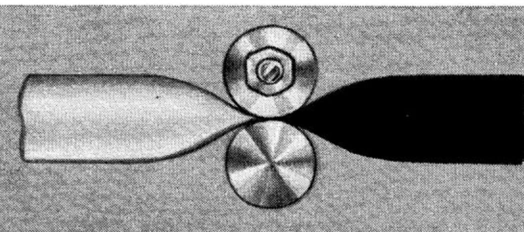
In the Helixtractor, separation takes place in a vertical coil (helix) of polyethylene or Tygon plastic tubing. Material to be centrifuged is introduced into the spinning helix through a central, stationary hub. Solids are collected along the wall of the tubing in the form of a paste. Because the tubing is transparent, different phases of the precipitate may be identified and isolated by simply cutting out a particular section of the tubing.

Helical flow continuous centrifugation brings you a new concept in separation efficiency . . .



All material passing through the tubing is subjected to the same G force for the same length of time. The flowing liquid is only a fraction of an inch in thickness; therefore the solid phase separates three to four times more rapidly than by any other method.

Now you can separate micro deposits from large volumes of liquids with better results and in a fraction of the time conventional methods require. Tests show that the Helixtractor increases separation efficiency by as much as 400%.



Precipitate is easily removed with the roller-type extractor supplied with each IEC Helixtractor unit or flushed with a syringe. Precipitates also can be preserved for future use simply by sealing the ends of the tubing. This forms an air-tight package which lasts indefinitely.

Before you study this significant new development, you may be pleased to know this: **the new Helixtractor fits all IEC PR-1 and PR-2 centrifuges now in use!** You may already have one ready for immediate adaptation to Helical Continuous Flow operation.

The Helixtractor is completely aerosol free. It is ideal for centrifuging infectious materials. You can autoclave the entire unit.

Can helical separation help you? A demonstration is easily arranged and complete literature is available. Write:

INTERNATIONAL IEC EQUIPMENT CO.

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The International Model PR-2 is easily the most versatile refrigerated centrifuge ever developed. It offers hundreds of accessories, 30 interchangeable heads, including horizontal, angle and now, **Helical Continuous Flow.**

WARING

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COSMETICS—Shampoos, nail polish, deodorants, powder, hand cream, hair dyes, aerosol hair spray and shaving lather
ELECTRONICS—Cathode ray tube coatings
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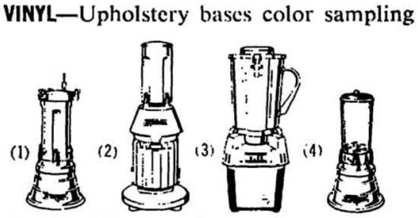
FOOD PROCESSING—Relishes, herb and condiment blending, dressings, dog foods
GENERAL LABORATORY USE—Experimental dispersions, emulsions, polymers, slurry and solutions
HOMOGENIZING TEMPLATE EMULSIONS—Sheet aluminum, stainless steel
INSECTICIDES—Repellents and killers
INSULATION—Rock wool-fiber glass, asbestos, ground glass beads
PAINT—Color sampling, water-base, rubber-base, waterproof, enamels
PAPER—Waste paper evaluation, pulping wood fiber, coatings
PETROLEUM—High octane fuel, additives, grease, detergent motor oils
PLASTICS—Experimental mixes, color testing, powder and resin blending
PRINTING INK—Color blending, waterproof inks, engraving fluids
RUBBER—Latex, sponge, synthetic
TEXTILES—Strength tests for synthetics, Nylon, Orlon, Dacron, wool and cotton
TOBACCO—Chemical analyses, homogenized, experimental blends, mixtures
VINYL—Upholstery bases color sampling



(1) 2-Speed High Torque Base Asceptic Dispersall
(2) Explosion Proof Base Stainless Steel Container
(3) 3-Speed Heavy Duty Base Stainless Container
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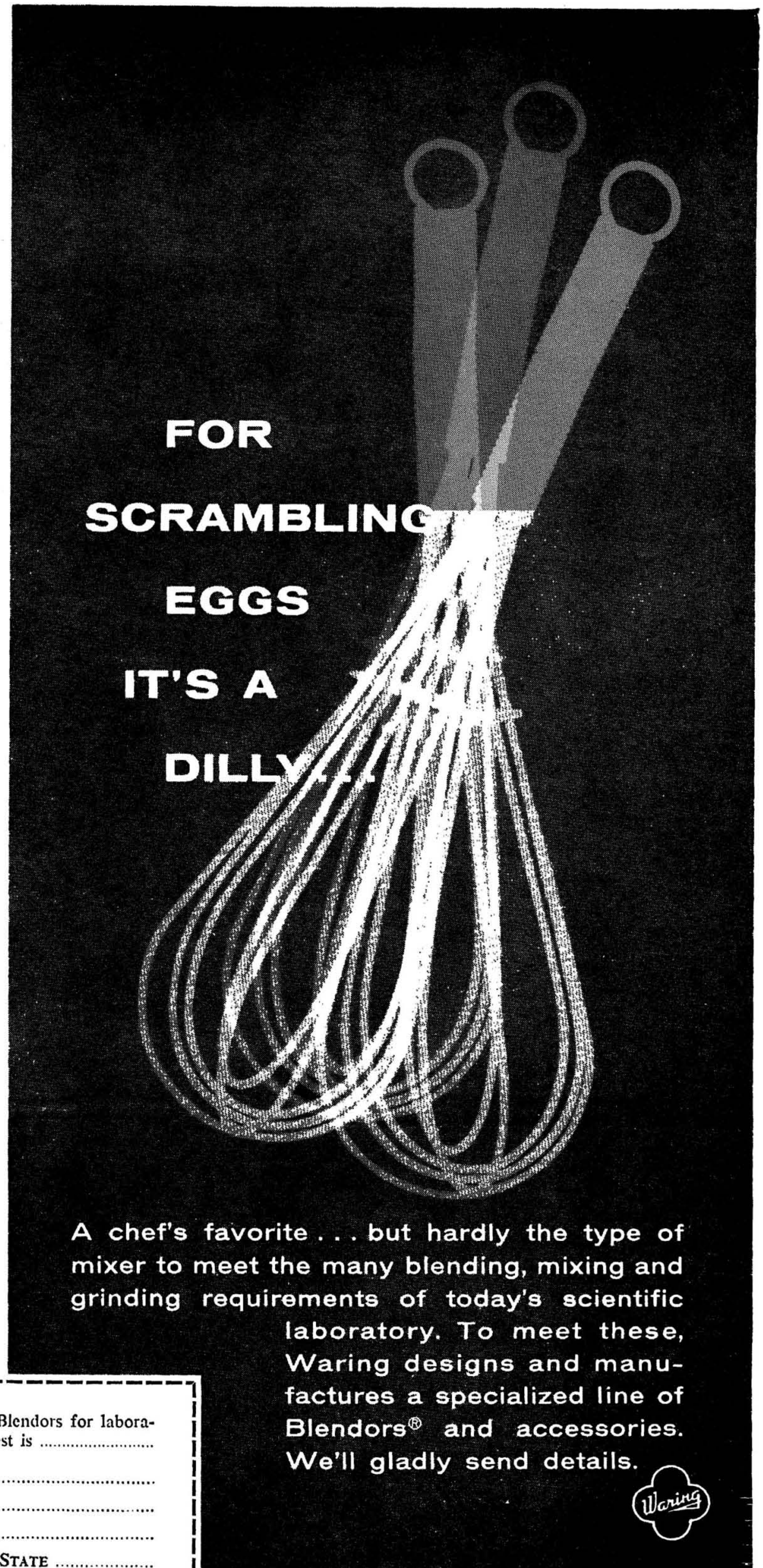
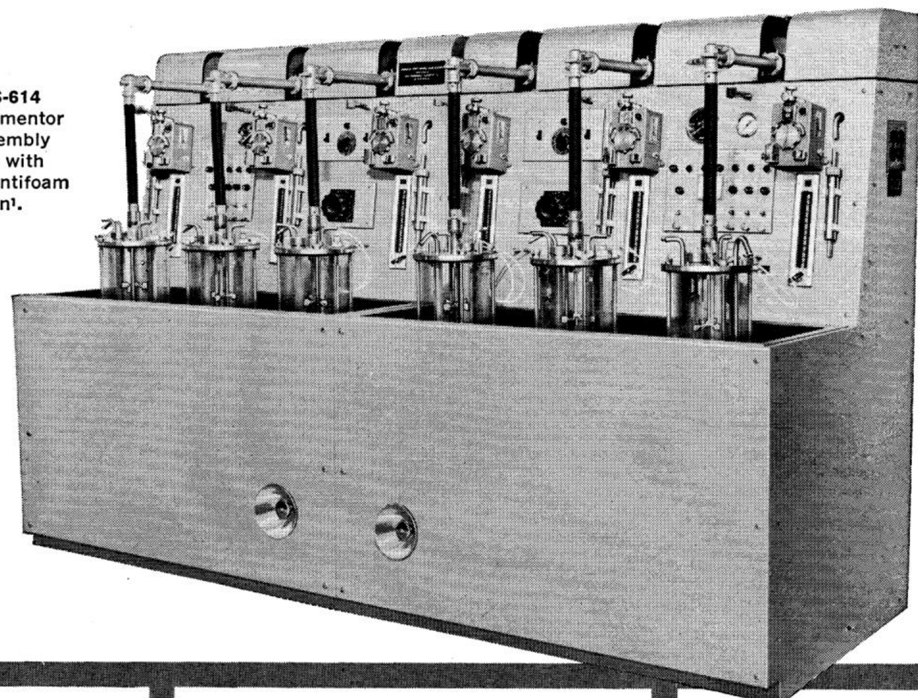
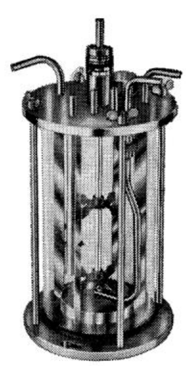
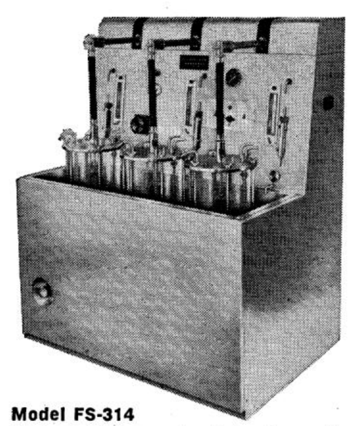


Model FS-614
Six-unit Fermentor
Drive Assembly
Equipped with
Automatic Antifoam
Addition[1].

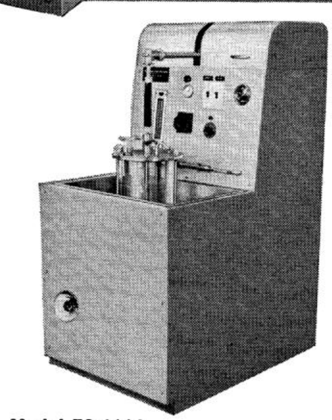




Model F-05
Stainless Steel Fermentor



Model FS-314
Three-unit Fermentor Drive Assembly



Model FS-1114
Single-unit Fermentor Drive Assembly

NBS FERMENTATION APPARATUS for the growth of Bacteria, Fungi, Yeasts, Tissue Cultures and Viruses

For Aerobic and Anaerobic Studies of Microbial and Chemical Processes at Laboratory and Pilot Plant Levels

The NBS Fermentor Drive Assemblies offer a convenient and efficient means for culturing microorganisms under well-defined conditions. Temperature, agitation, pH[1], air volume and pressure can be selected and rigidly controlled while sterility is maintained.

Stainless steel fermentors with Pyrex glass jars are available in 5, 7½ and 14 liter sizes. Impellers are driven through a non-freezing, stainless steel agitator housing which incorporates stainless steel ball bearings and leak-proof Teflon seals. The fermentors are easily and quickly disconnected from the drive and will sustain *repeated* autoclaving.

Water bath temperature is adjustable up to 60°C ± 0.5°C. A continuous-duty, variable speed motor enables impeller speed to be regulated between 150 and 750 rpm. Performance is quiet and dependable under continuous operation. All NBS apparatus is unconditionally warranted for one full year. **WRITE FOR CATALOG FSS/1192**

(1) Automatic pH Control and Antifoam Addition optional

NBS New Brunswick Scientific Co., Inc. 1130 Somerset St., New Brunswick, N.J.

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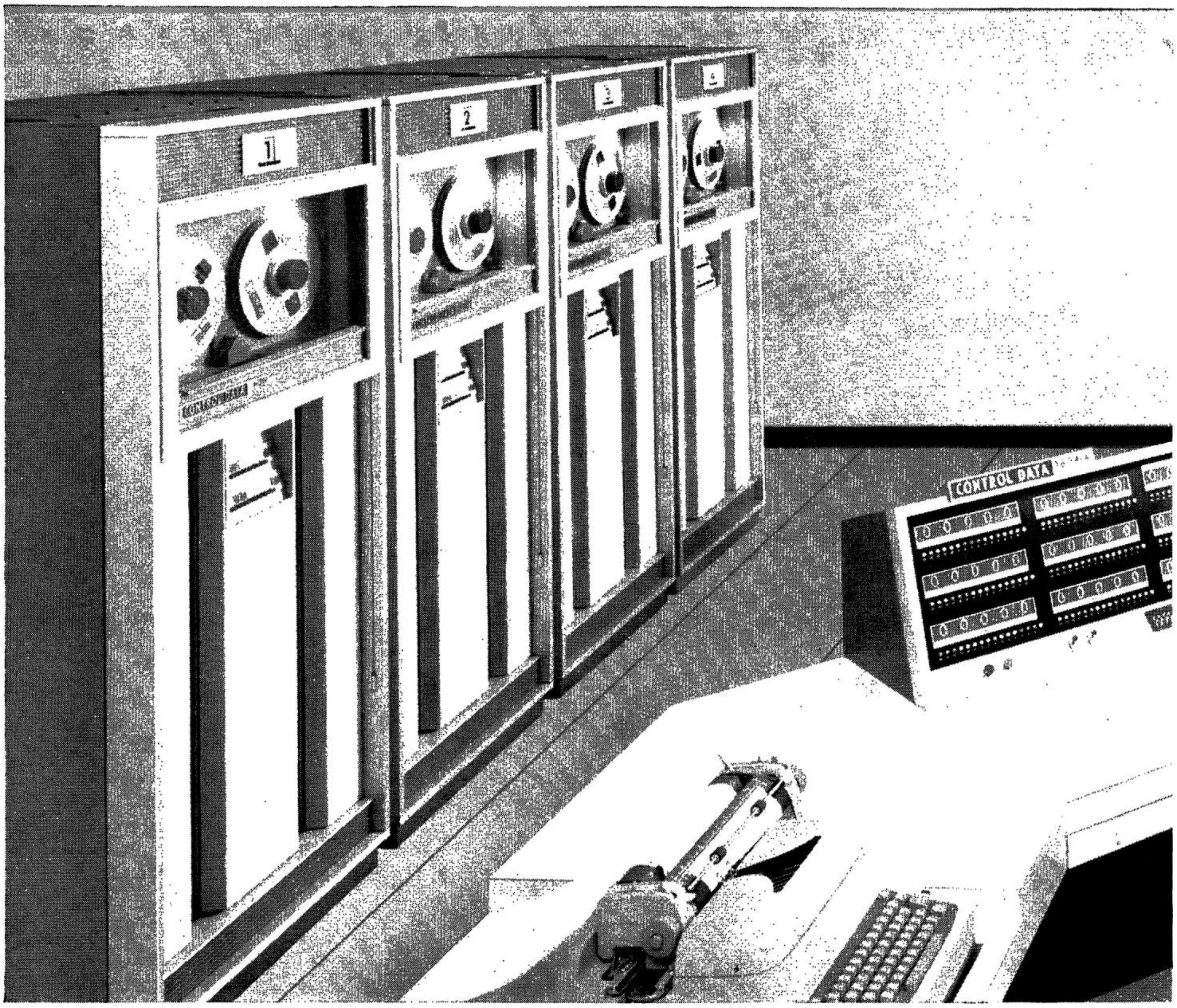

Why so many computer users prefer

...and five reasons why it will pay you to know!

The Control Data 1604/1604-A Computer, demonstrated a leader in a competitive and demanding field, continues to prove its ability to scientists and engineers as the computer to solve their problems, and to management as the computer to help maintain their profit. Here are the reasons why!

RELIABILITY: The 1604/1604-A continues to log an unusual history of "uptime." Why? The people from Control Data have been making computers for a long, long time. Their experience, their reputation are built into every Control Data computer. Your confidence has been their objective, your demands have been their guide. This is why the 1604/1604-A proven records set a high standard of performance.

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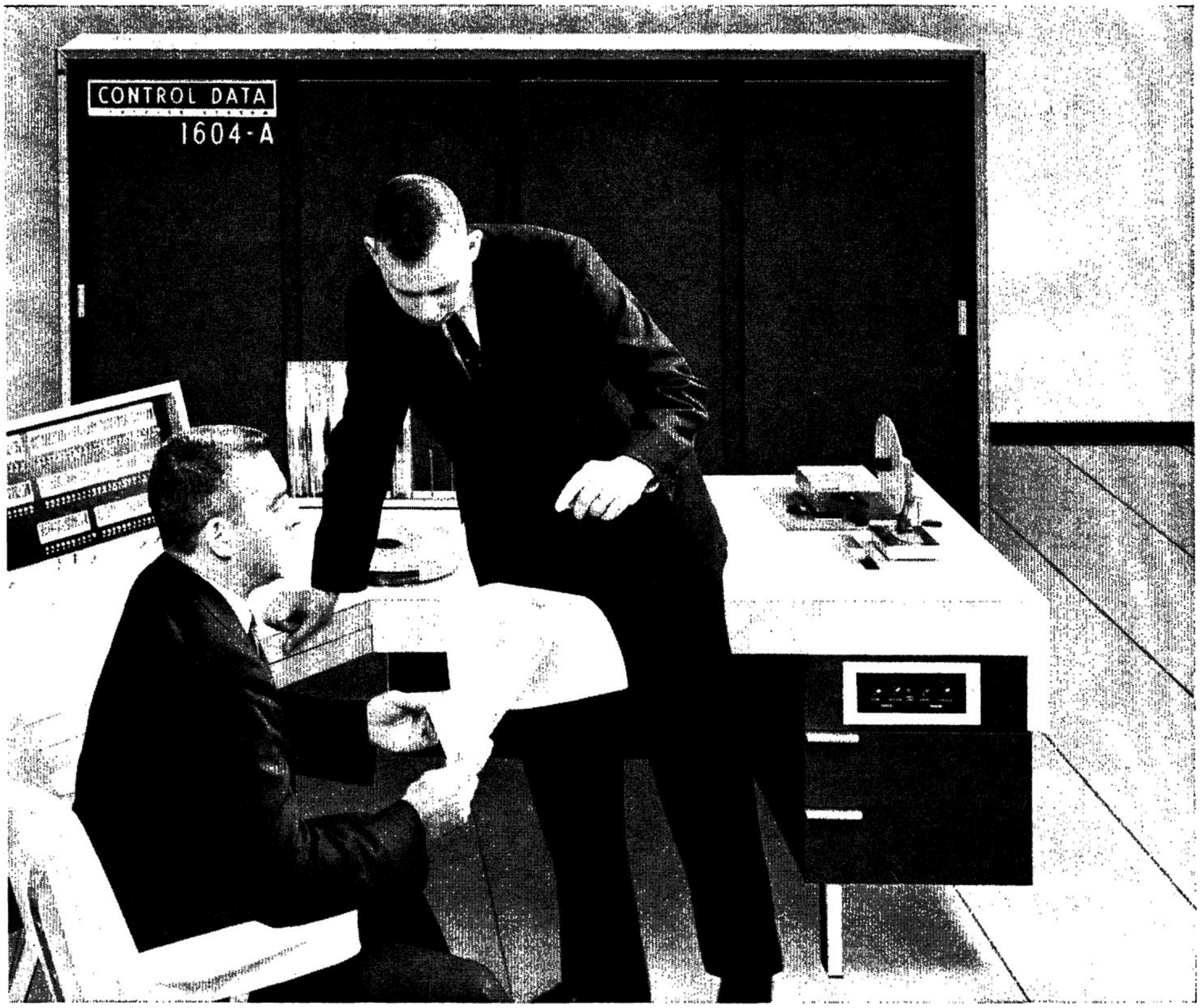
These systems will also be compatible with the 3600 Computer. Additional, proven programming systems (including JOVIAL) are available through the Co-op users organization.

SERVICES: A wide range of proven services come with the 1604/1604-A. These include Programming Assistance • Programmer and Operator Training • Programming Systems Improvement • Installation Check-out • Post Installation Assistance • System Analysis • On-site Customer Engineering.



the Control Data 1604/1604-A

APPLICATIONS: The power, versatility and usefulness of the Control Data 1604/1604-A Computer are demonstrated by the many users who rely upon its proven problem-solving capabilities. Real-time, on-line data reduction/data acquisition, large-scale problem-solving, large-scale data processing, biomedical and institutional research, weather prediction, oceanography, petroleum reservoir analysis, and flight simulation to name a few. Names of these satisfied users are available to you through the Control Data office near you.

LOW-COST: The Control Data 1604/1604-A is available today at an amazingly low cost. With its proven performance, programming systems and services, the Control Data 1604/1604-A offers the most computer for the least dollars spent.

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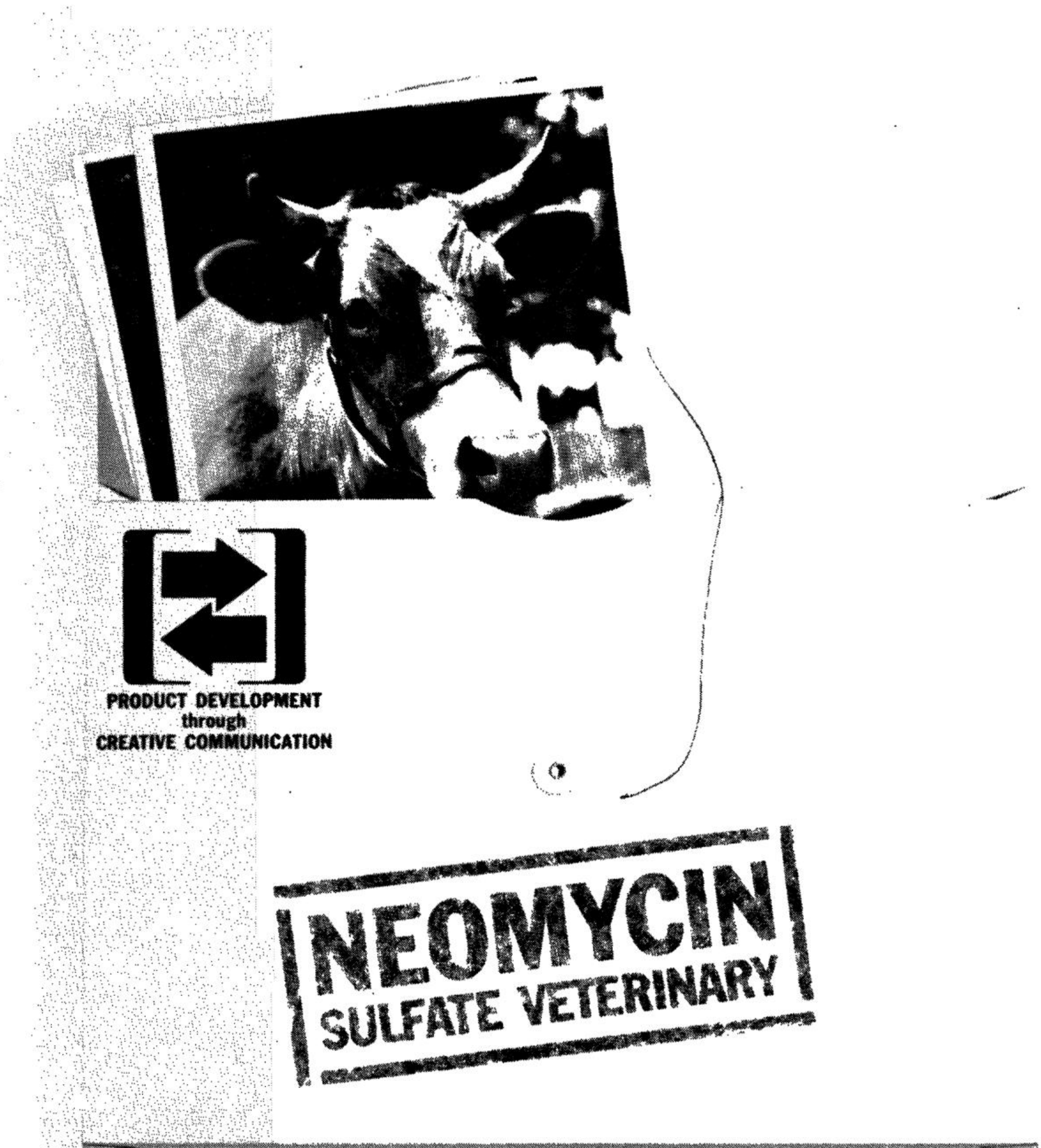
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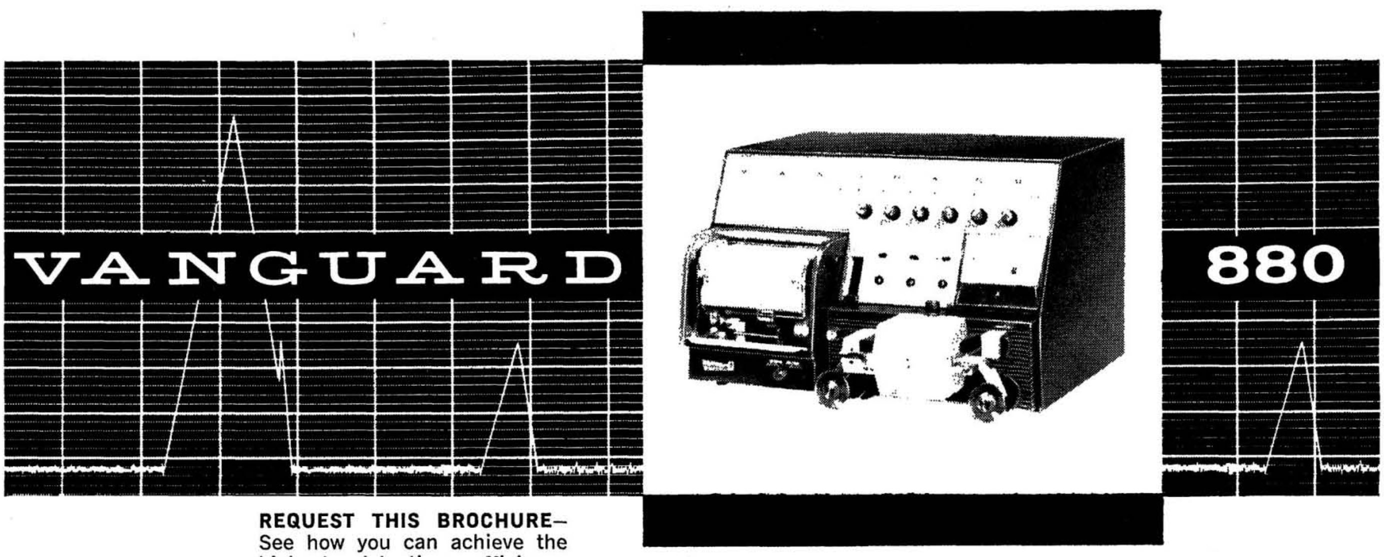
NEW STANDARD ACCESSORY, TOO! Also available is the new, exclusive Vanguard Model 880ADS, a completely automatic system for quantitative integration and digital presentation of radioactive zones. For complete details concerning either the Model 880 or Model 880ADS, please write or call.



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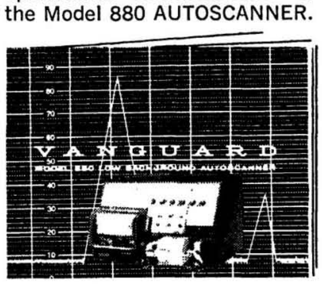

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Linde Cryobiology NEWS

REPORT NO. 2 FROM LINDE COMPANY, DIVISION OF UNION CARBIDE CORPORATION

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A PRACTICAL TECHNIQUE

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3) Storing at liquid nitrogen temperature (–196°C).

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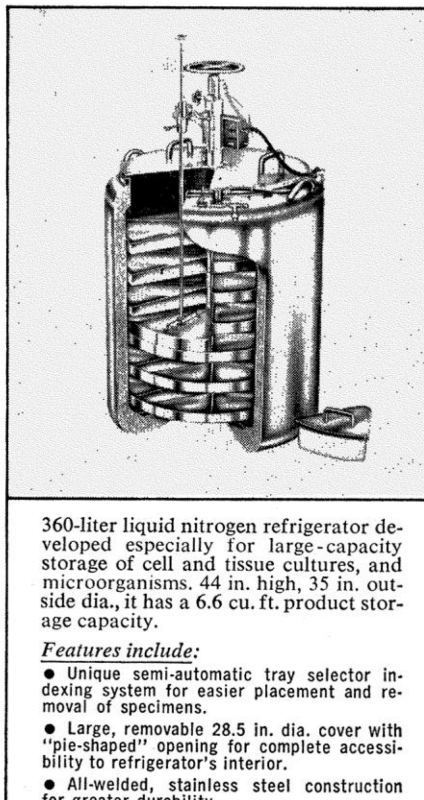
LINDE liquid nitrogen refrigerators come in a wide range of capacities. These include, for major projects, the new large-capacity LNR-640-C and sophisticated LNR-360 *(see photo)*. Also, there is the new high-accessibility LNR-250, the improved medium-capacity LNR-35, the standard 720-ampule capacity LNR-25 widely used by biologists for many years, and the all-new fully portable, highly compact LNR-10.

NEW LNR-360 REFRIGERATOR



360-liter liquid nitrogen refrigerator developed especially for large-capacity storage of cell and tissue cultures, and microorganisms. 44 in. high, 35 in. outside dia., it has a 6.6 cu. ft. product storage capacity.

Features include:

- Unique semi-automatic tray selector indexing system for easier placement and removal of specimens.
- Large, removable 28.5 in. dia. cover with "pie-shaped" opening for complete accessibility to refrigerator's interior.
- All-welded, stainless steel construction for greater durability.
- Low liquid nitrogen consumption—fully charged refrigerator has a holding time of more than 50 days.
- 6 tray levels containing 37 canisters for greater storage capacity.

Equipment includes two specially developed liquid nitrogen freezers which precisely control the cooling rates of individual specimens from 0.5°C./minute to 19°C./minute. The standard BF-3 holds up to 40 1.2 ml. ampules, has a total volume capacity of 110 cu. in. The new, larger BF-3-2 has a 1600-cu.-in. capacity.

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... from LINDE on advanced techniques in cryobiology is a comprehensive review by Dr. S. W. Moline of LINDE's Tonawanda Research Laboratories. Subjects under review (with numbered references to a bibliography of 59 reference works) cover preparation of cells for storing; cooling rates; the use of protective additives; storage at low temperatures; warming rates; and condition of cell or tumor strains after cooling, storage, and warming.

Detailed literature is available on request. Also, LINDE's "Cryobiology Report No. 1" which deals more generally with the all-new method of freezing and storing biologicals. For further information, check your area(s) of interest on the coupon below and send.

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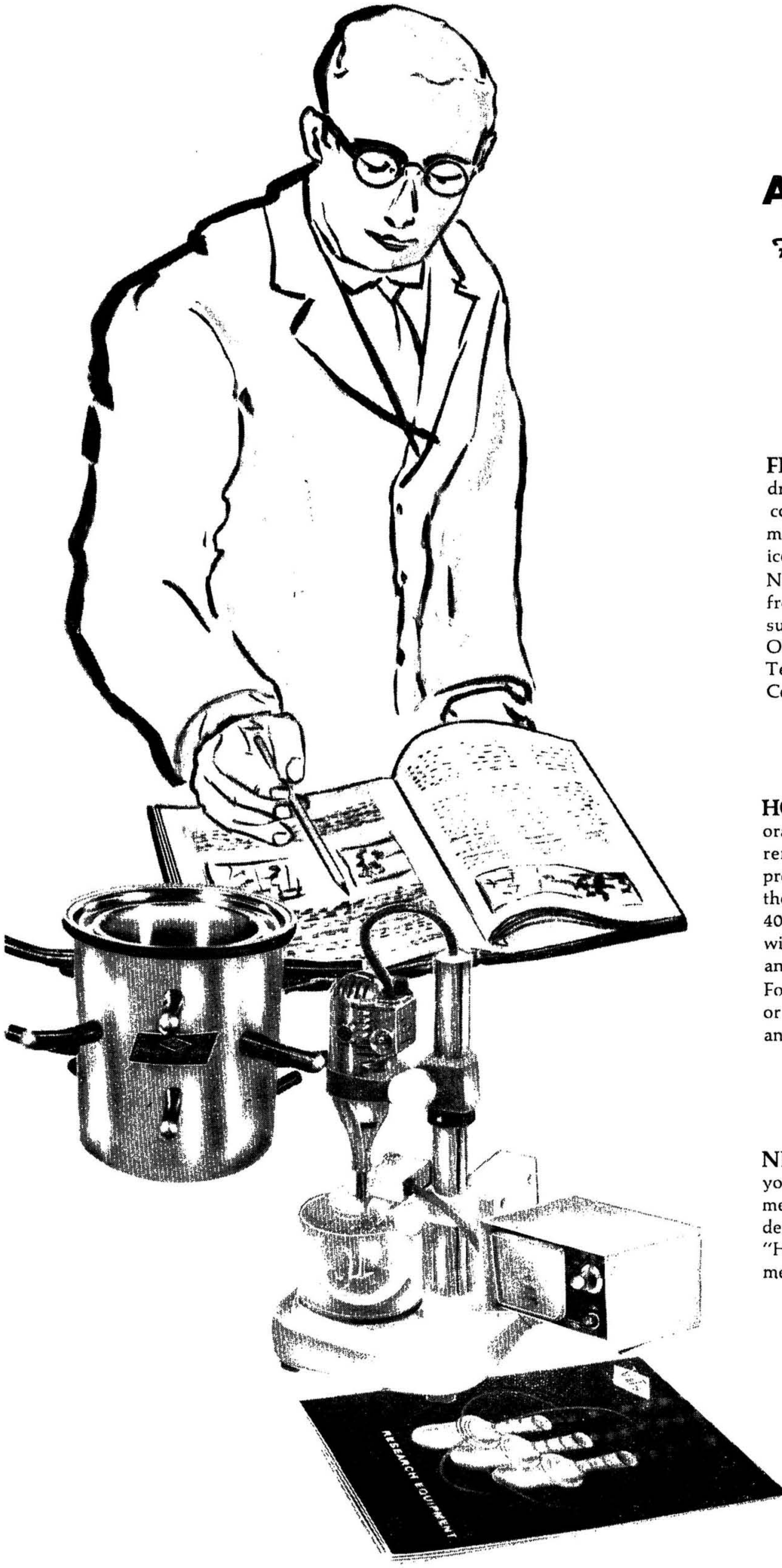
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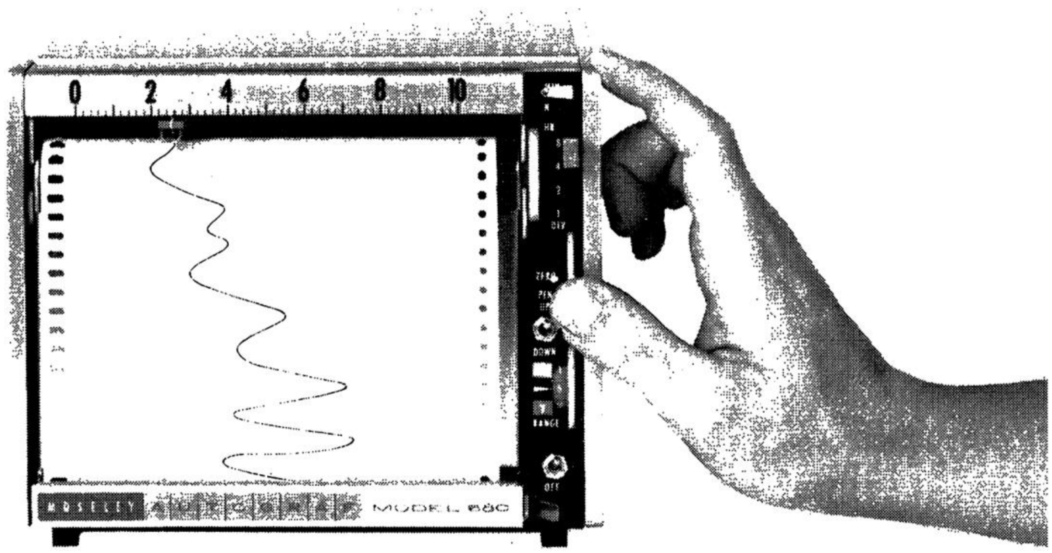

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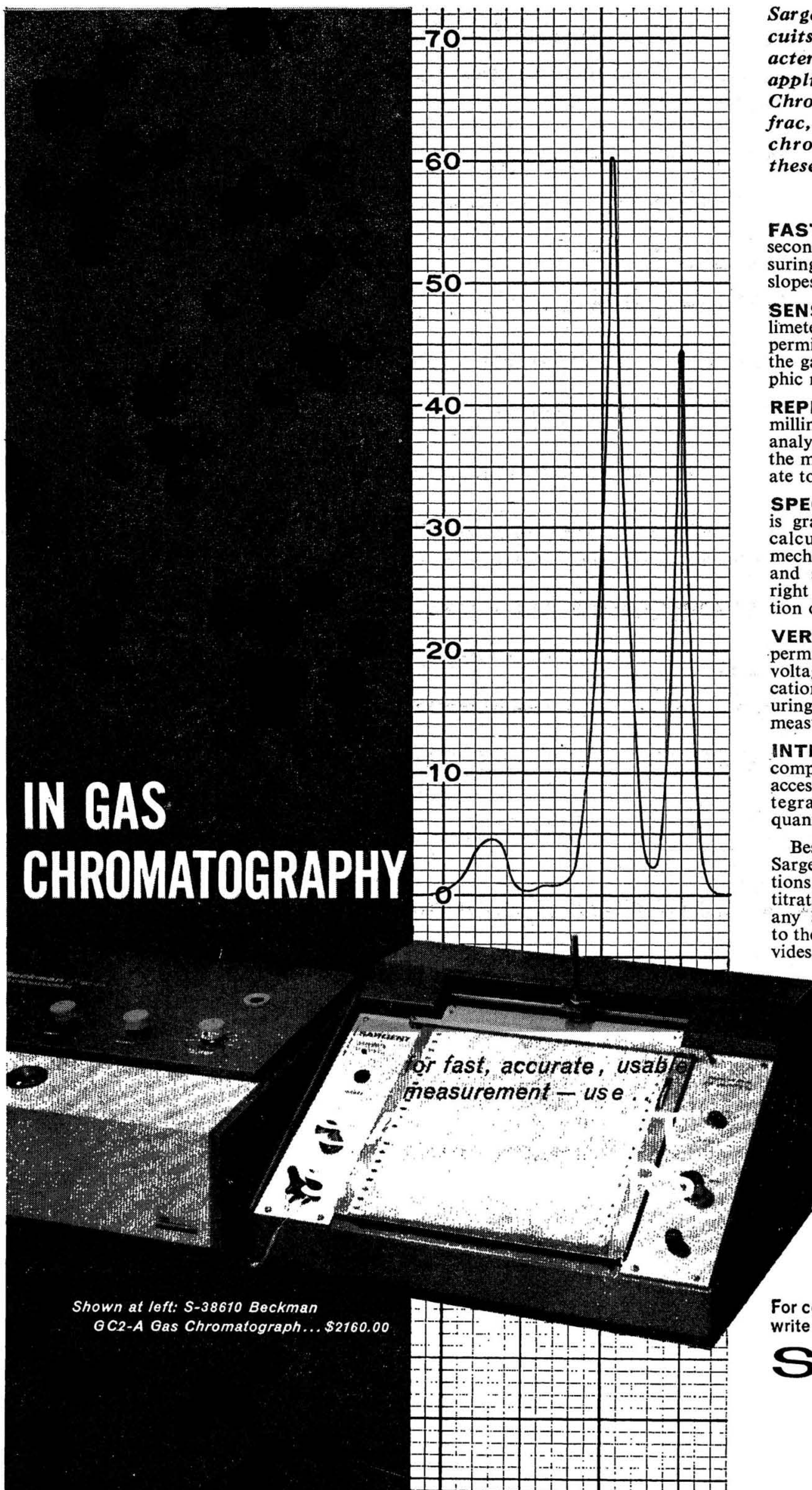

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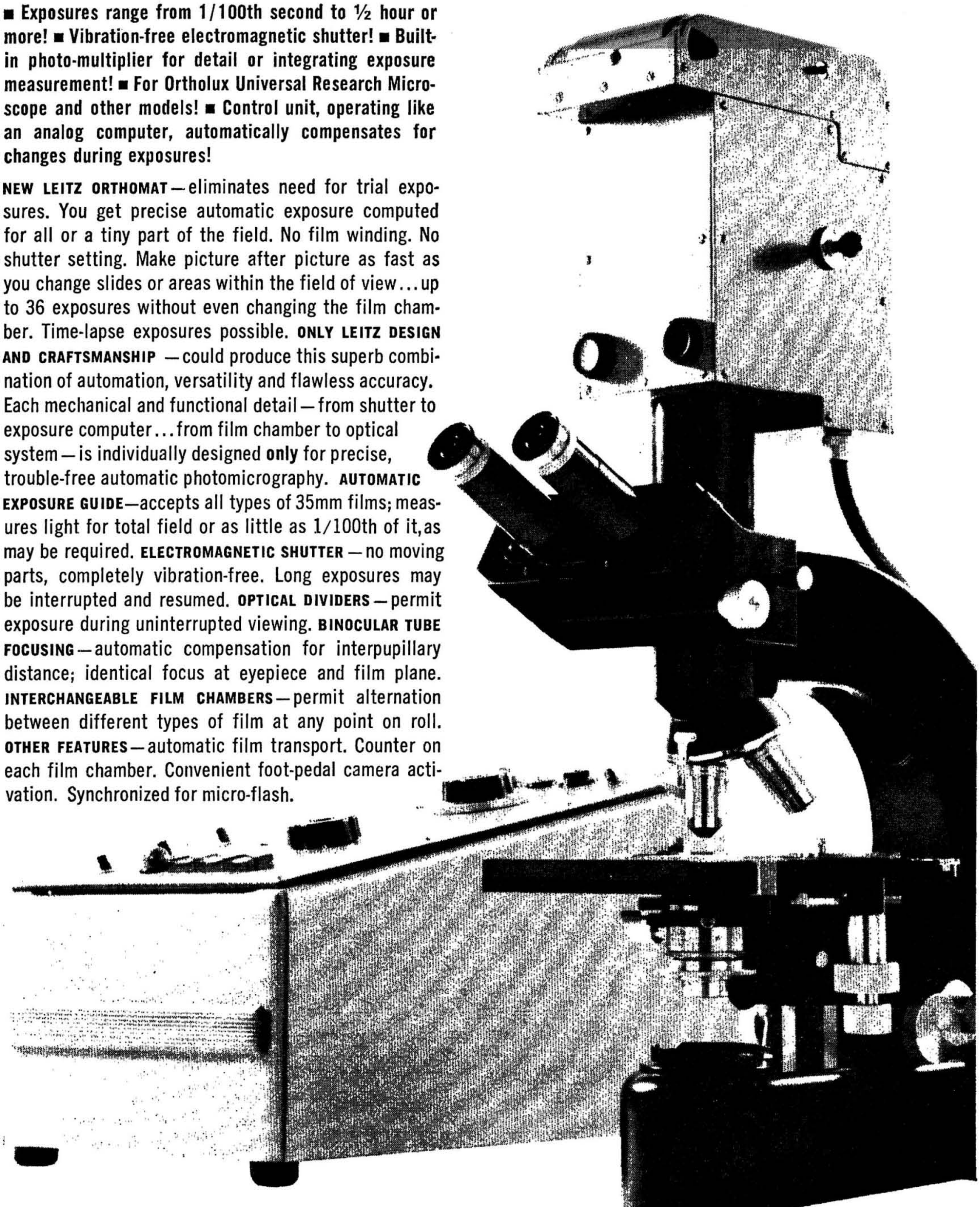

9 November 1962, Volume 138, Number 3541

SCIENCE

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Science and Society

The purpose of science in society is to enable us to react homeostatically to the vicissitudes of the future. This future is, however, not one which we can completely foresee beyond a certain very limited point, which moves ahead in time as our experience moves ahead. This being the case, we must always possess a much larger stock of information concerning the environment, physical, medical, and social, than we shall probably use in any particular course of history. It is of the utmost importance to our safety against the vicissitudes of the future that this stock of fundamental scientific information be kept extremely wide. It is of even greater importance that it be kept *potentially* extremely wide—that is, that the way for the internal development of science be kept open. It must not be at the mercy of historical predictions and prejudgments which belong primarily to one particular age, and may be proved false, incompletely justified, or irrelevant with the further development of history and the growth of our experience.

Thus the internal life of science must be preserved without a too direct dependence on the policies of the moment, or the official fashions of thought. This means that the scholar must retain for his own efficacy something—not too much—of the ivory tower attitude which it is the spirit of the times to decry.

It is well that we convince ourselves of the social usefulness of science before we go into it as a career. It is not well that we hold the test of social usefulness too immediately before us in the very difficult task of extending science.

The phenomenon that a human activity may be best pursued according to its internal logic, even if the general function of the activity should be considered most seriously in matters of the choice of a career, etc., is familiar to all of us. The man who becomes an officer in the army must be brave, but the man who asks during every military operation "Am I a brave man?" is not likely to make a good military officer. The surgeon should have convinced himself of a certain attitude of compassion before he is very far along in medical school, but the surgeon whose sense of compassion unnerves him in the performance of a cruel but necessary operation has chosen the wrong career.

Under these circumstances, we can see that it is possible for a scientist to be so socially minded that he does not find time or attention for the self-contained activity which forms a large part of the life of the working scientist. This fact has important consequences concerning the organization of scientific work. Certainly scientific work should be answerable for its value to the community—but at arm's length. If a man has no sense of social responsibility, don't appoint him, but if he is known to possess such a sense, for goodness' sake don't badger him with an unceasing inquiry as to his social responsibility while he is trying to perform the work that belongs to the fulfillment of his social responsibility. Science is a tender plant, which does not take kindly to a gardener who is in the habit of taking it up by the roots to see if it is growing properly.—NORBERT WIENER, Massachusetts Institute of Technology [excerpted from "Science and Society," which was originally published in *Voprosi Filosofii* and later in *Technology Review*, July 1961, pp. 49–52]

1 milligram per liter (G. E. Fogg, Great Britain). This is perhaps an equilibrium concentration, since the substance is reabsorbed by algae, under certain conditions, and by other organisms.

The activity of aquatic bacteria in natural waters was simulated in a steady-state continuous-flow system (H. W. Jannasch, Germany). Nutrients that limit growth were determined by increasing the growth rate of the organism in the chemostat until washout of the population occurred. The resulting maximum growth rate in the presence of the highest effective concentration of the nutrient being investigated was used for assaying the rates of decomposition under close approximation of natural conditions.

Oligotrophic lakes have a lower ratio of primary production to total biomass and a higher diversity of species at different trophic levels than eutrophic lakes (R. Margalef, Spain). Thus, they have to be considered more mature and more organized ecosystems than eutrophic lakes. (Eutrophy is mainly a consequence of external disturbance.)

Midge larvae (*Chironomus anthracinus*) grew very little during the summer in the anoxic bottom mud of Esrom Lake, Denmark, but their growth increased 600 percent in October, after the fall overturn (P. M. Jonasson, Denmark).

During the last 25 to 30 years the production of organic material in Lake Constance (central Europe) increased 20-fold because of domestic effluent, which boosted the growth of plankton; in turn, the white-fish population increased (W. Nümann, Germany). In 2 years the fish reached the length they normally reach in 4, and at the same time they showed 30 percent increase in weight. Catches increased almost 600 percent between 1910 and 1960 and another 50 percent in 1961.

In efforts made in recent months to eliminate the increasing pollution of the Oslo Fjord, the newly developed Elkem electrolytic sewage purification process was used. This removes sludge and appreciable amounts of phosphorus and nitrogen compounds from raw sewage and then sterilizes the effluent (E. Føyn, Norway).

A special session was devoted to discussion of the Amazon River, which is chemically rich but of low *p*H (H. Sioli, Germany). Unlike hard waters in other regions of the world, Amazon waters derive their main ions from alkali silicates (W. Ohle, Germany). Great seasonal differences in precipitation cause horizontal displacement of the Amazon's 200-kilometer mixing zone in the ocean (H. Schwassmann, U.S.A.). A mean discharge of 1850 cubic meters per second has been determined for the Rio Guamá, Brazil.

Whereas plant and animal communities on land vary greatly in number between tropic and temperate zones, some aquatic communities in tropical streams are very similar in number to aquatic communities in streams in the temperate United States (R. Patrick, U.S.A.).

J. W. G. Lund (Great Britain) delivered the traditional Baldi lecture. His theme was the ecology of plankton algae.

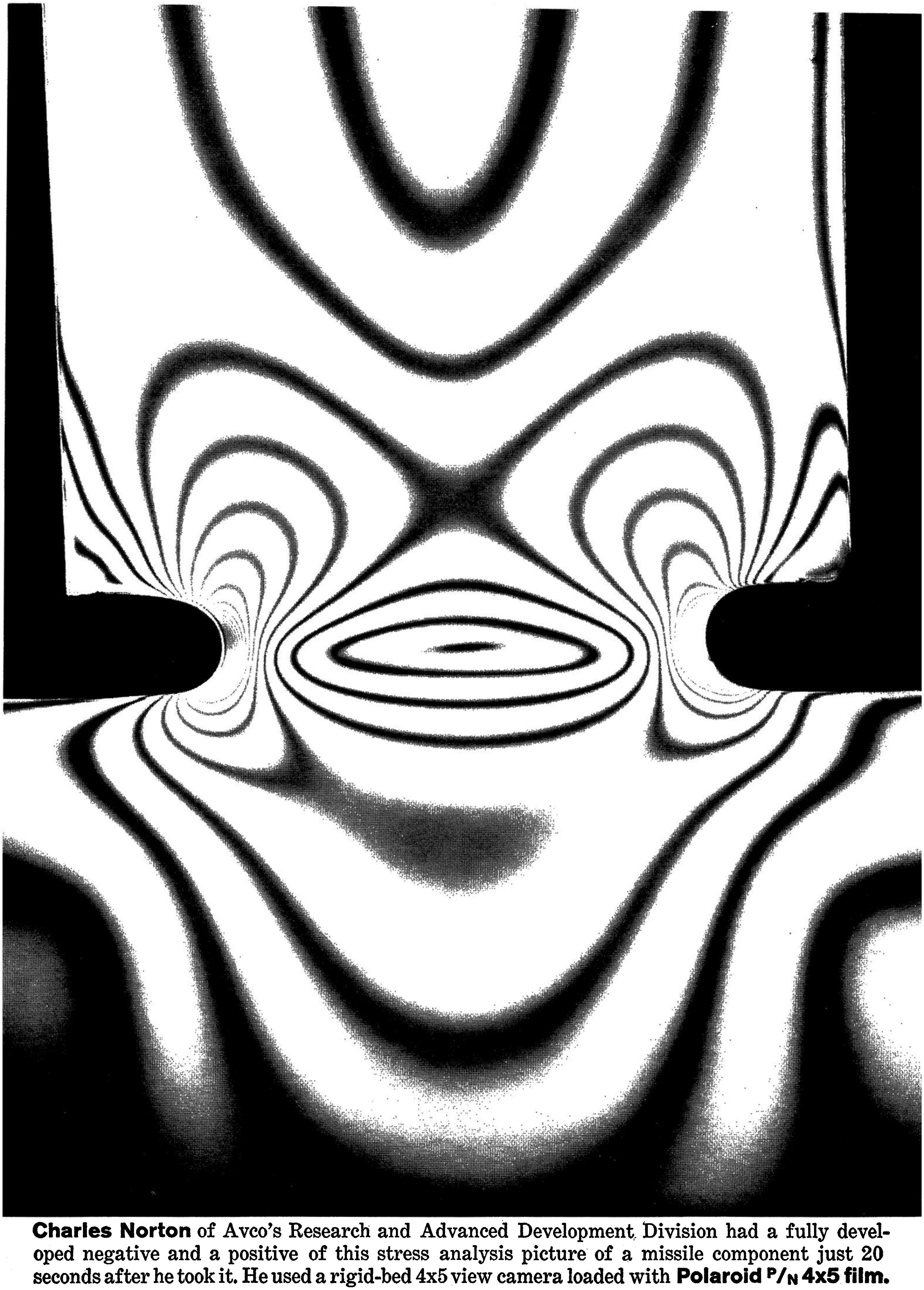
In a light moment the British representatives were chided for not having submitted a paper on the Loch Ness monster. Two British research expeditions, using the latest electronic detection devices, searched during the summer for this mythical creature. Certainly, had it been found, the congress should have been treated to plesiosaur steak.

During the week preceding the congress most of the 200 overseas visitors, representing 33 nations, participated in excursions to the alpine lakes of Colorado. In addition, five different tours were held after the congress: to the Ontario Lake District, the Great Lakes, the Finger Lakes, the Mississippi River, and the Ohio River Valley. Local tours to the lake district of northern Wisconsin and to Milwaukee and Lake Michigan, where Great Lakes research vessels were berthed, were held during a midweek recess.

Major financial sponsors of the congress were National Science Foundation, National Institutes of Health, Office of Naval Research, Atomic Energy Commission, and International Union of Biological Sciences.

Overseas visitors were assisted in two ways: 98 Europeans were awarded seats on a chartered airplane by an international selection committee. More than 50 other foreign limnologists received travel aid through symposiums and lecture honoraria from North American institutions. Each registrant received, with the program, a book of abstracts. The proceedings will be published in full in volume 15 of *Verhandlungen der Internationalen Vereinigung für Limnologie*.

The congress was organized under the aegis of the American Society of Limnology and Oceanography (ASLO) and the National Academy of Sciences–National Research Council on behalf of



the International Association of Limnology. J. C. Wright, limnologist at Montana State College, was granted leave for a year prior to the congress to serve as executive secretary, in Madison. The executive committee consisted of A. D. Hasler (chairman), D. G. Frey, K. D. Carlander, G. H. Lauff (treasurer), J. C. Wright (executive secretary), T. T. Macan (secretary ex officio), W. E. Ricker, and the late D. S. Rawson.

The committee acknowledges the assistance of many students and employees of the University of Wisconsin and of ASLO members who organized and conducted the excursions.

The United Nations was asked by the International Association of Limnology to set aside specified lakes, rivers, and ponds throughout the world for preservation and scientific study.

G. E. Hutchinson (Yale University) was elected president, to serve from 1962 to 1965; he is the first American to hold this post. It was resolved that the next congress will be held in Poland in August 1965.

ARTHUR D. HASLER
Hydrobiological Laboratory, University of Wisconsin, Madison

The Teaching of Science

The problems involved in the teaching of science are both international in scope and interdisciplinary in character. There is at present no international body whose primary function it is to discuss educational problems on such a broad and comprehensive basis. Recognizing the need for such a forum, the International Council of Scientific Unions (ICSU) convened representatives from its various scientific unions to consider the advisability of setting up an interunion commission on the teaching of science. This meeting was held in Paris early in May 1962. The following scientific unions were represented: the International Union of Biology, represented by P. Chouard (France); the International Union of Pure and Applied Chemistry, by E. L. Piret (U.S.); the International Union of Geography, by P. Pellissier (France); the International Union of Geology, by T. N. George (Great Britain); the International Union of the History and Philosophy of Science, by R. Taton (France); the International Union of Mathematics, by M. H. Stone (U.S.); and the International Union of Pure and Applied Physics, by S. C. Brown (U.S.).

The plan of constituting an interunion commission on the teaching of science was heartily endorsed, and M. H. Stone, of the University of Chicago, was elected president of the commission. P. Fleury, of the Institut d'Optique, Paris, agreed to fill the important post of secretary. The Interunion Commission on the Teaching of Science then focused its attention on a number of specific areas for action and planning.

The commission decided to enlist the aid of national correspondents in as many countries as possible. These correspondents would be asked to furnish information on the teaching of science in their respective countries and, in particular, to keep the commission informed on new experiments in education which would be of interest in the teaching of science. In return, the commission would undertake to keep the national correspondents informed as to the material the commission received and would ask the correspondents to be responsible for diffusing this information within their own countries insofar as this would be useful and practical.

The hope was expressed that the commission would be able to set up close liaison with official international organizations which are already active in the field of science education, or which plan to be active in the future. To explore this idea the commission met with Albert Baez, chief of the Division of Science Teaching of the Department of Exact and Natural Sciences, UNESCO, and with Ganeff and Trautmann of the Organization for Economic Co-operation and Development and found them all enthusiastic about such a liaison. The Interunion Commission hopes to formalize a liaison with each of these organizations and to establish similar arrangements with other international bodies.

Members of the commission felt that it was the particular responsibility of an interdisciplinary body to call attention to the role of science in general education and, in particular, to the role of the history and philosophy of science underlying all of the various individual disciplines.

In the interest of interdisciplinary science teaching it seemed important to stimulate the writing of books in borderline fields where essentially no interdisciplinary material now exists. Interdisciplinary sequences—such as physics, chemistry, biology; mathematics, physics, astronomy; social science, information theory, psychology; and physics, chemistry, geology—were some of the subject groupings suggested. Such books should give the science teacher sufficient background to develop particular course sequences within his own educational system. They should be written by recognized experts in the various fields, not as a collection of individual points of view but as a unit.

The assembled group expressed deep concern with the role of the teacher and a desire to strengthen this arm of the educational process in every way possible. The International Conference on Physics Education (*1*), held in Paris in 1960, had adopted a resolution in this regard which the ICSU Commission felt could justifiably be generalized to relate to all science teaching. Its modified version of the resolution read as follows.

We stress that efficient instruction in science requires specialized teachers who can keep abreast of developments in a rapidly growing subject. We are alarmed at the present shortage of such teachers, particularly in view of the growing demand for science education. The shortage is likely to become more acute in the years ahead. In our opinion, steps should be taken to improve both the efficiency and the attractiveness of science teaching as a profession. Insofar as the realization of these aims requires action by governments and universities, we recommend that these bodies should consider the following general conclusions:

a) In schools of secondary and higher levels, science should be taught by appropriately trained scientists, that is, by men and women who have received a professional training in the particular field of science in which they teach. Teachers must be encouraged to keep their professional experience up-to-date. The experimental nature of most sciences places an added burden on the teacher, and this must be recognized and adequately compensated by a reduction in his teaching hours and in other suitable ways.

b) To make teaching careers more attractive, improvements of salary and status are necessary in some cases, but most important are better conditions of work. For example, technical assistance and liberal provision of apparatus are vital, and facilities should be provided for all students at all levels to carry out experiments. Secondary-school teachers should have conditions in which they can feel that they form an integral part of the development of scientific knowledge.

c) Universities and comparable institutions should accept their responsibility to establish close relations with secondary-school teachers, to assist in solving the problems of instruction in schools, and to provide refresher courses. These courses would require extended periods of study leave for teachers.

The commission proposed to the International Council of Scientific Unions that an international conference on the teaching of sciences be held, possibly in 1964 or 1965, to bring together the various conclusions of the individual scientific unions on the following subjects: The place of sciences in general education; the place of mathematics in the teaching of science; the role of history in science teaching; new methods in teaching science; and recruitment and work conditions for professors and their pupils.

The Interunion Commission on the Teaching of Science wishes to publicize its existence on as wide a front as possible, so that the problems it works on will be truly representative of those faced by the international educational community and the methods of solution it suggests will have wide application.

SANBORN C. BROWN
Department of Physics, Massachusetts Institute of Technology, Cambridge

Reference

1. S. C. Brown and N. Clarke, *International Education in Physics* (Wiley, New York, 1960).

Forthcoming Events

December

4–5. Microbiological Problems in **Petroleum** Production, symp., Long Beach, Calif. (C. C. Wright, Oilwell Research, Inc., 1539 W. 16 St., Long Beach 13)

4–6. **Computers**, joint fall conf., Philadelphia, Pa. (J. W. Leas, Radio Corp. of America, Camden, N.J.)

4–7. American **Documentation** Inst., Hollywood-by-the-Sea, Fla. (J. B. Kaiser, 1718 N St., NW, Washington 6)

4–8. Controlled Field Trials of **Communicable Diseases**, conf., Geneva, Switzerland. (World Health Organization, Palais des Nations, Geneva)

4–13. Techniques of Surveys on **Epidemiology** of **Mental Disorders**, Manila, Philippines. (World Health Organization, Regional Office for the Western Pacific, P.O. Box 2932, Manila)

5–11. American Acad. of **Optometry**, Miami, Fla. (C. C. Koch, 1506-08 Foshay Tower, Minneapolis 2, Minn.)

6–8. **Mathematics**, annual conf., Santa Monica, Calif. (H. Couzins, Chaffey High School, Ontario, Calif.)

7–8. American **Rheumatism** Assoc., Richmond, Va. (J. A. Coss, 20 E. 76 St., New York 21)

7–8. Oklahoma Acad. of **Science**, Tulsa. (A. D. Buck, Northern Oklahoma Junior College, Tonkawa)

7–9. American **Psychoanalytic** Assoc., New York, N.Y. (H. Kohut, 664 N. Michigan Ave., Chicago 11, Ill.)

7–11. **Visual Communications**, intern. congr., Philadelphia, Pa. (Visual Communications Congr., 18465 James Couzens Hwy., Detroit 35, Mich.)

7–12. Environmental **Physiology and Psychology in Arid Conditions** symp., Lucknow, India. (UNESCO, Dept. of Natural Sciences, Arid Zone Unit, Place de Fontenoy, Paris 7e, France)

9–13. **Air Pollution,** intern. congr., Washington, D.C. (A. C. Stern, Div. of Air Pollution, Dept. of Health, Education, and Welfare, South Bldg., Washington 25)

10–14. **Neutron** Detection, Dosimetry, and Standardization, Harwell, England. (Intern. Atomic Energy Agency, 11 Kärntner Ring, Vienna 1, Austria)

10–15. Chronic Nonspecific **Pulmonary Maladies,** symp., Moscow, U.S.S.R. (World Health Organization, Regional Committee for Europe, 8 Scherfigsvej, Copenhagen Ø, Denmark)

10–15. Committee on **Biological Standardization,** Geneva, Switzerland. (World Health Organization, Palais des Nations, Geneva)

11–17. Committee on **Antibiotics,** Geneva, Switzerland. (World Health Organization, Palais des Nations, Geneva)

12–14. American Soc. of **Agricultural Engineers,** Chicago, Ill. (J. L. Butt, P.O. Box 229, St. Joseph, Mich.)

17–20. International **Arms Control,** symp., Ann Arbor, Mich. (IACS, P.O. Box 1106, Ann Arbor)

17–21. University **Physics Teaching** Curricula, Laboratory Experiments, and Equipment in UNESCO member states, comparative survey, Paris, France. (UNESCO, Place de Fontenoy, Paris 7e)

26–31. American Assoc. for the **Advancement of Science,** annual, Philadelphia, Pa. (R. L. Taylor, AAAS, 1515 Massachusetts Ave., NW, Washington 5)

The following 40 organizations will meet in conjunction with the AAAS annual meeting in Philadelphia:

Academy of **Psychoanalyisis.** (A. H. Rifkin, 125 E. 65 St., New York 21)

American Assoc. of **Clinical Chemists.** (P. Paubionsky, Abington Memorial Hospital, Abington, Pa.)

American **Astronautical** Soc. (J. G. Stephenson, Airborne Instruments Laboratory, Walt Whitman Rd., Melville, L.I., N.Y.)

American **Economic** Assoc. (H. F. Wiliamson, AEA, Northwestern Univ., Evanston, Ill.)

American **Geophysical** Union. (W. E. Smith, AGU, 1515 Massachusetts Ave., NW, Washington 5)

American **Meteorological** Soc. (F. Sergent, II, Dept. of Physiology, Univ. of Illinois, Urbana)

American **Nature Study** Soc. (J. A. Gustafson, Route #1, Homer, N.Y.)

American **Physiological** Soc. (R. E. Smith, School of Medicine, Univ. of California, Los Angeles)

American **Political Science** Assoc. (E. M. Kirkpatrick, APSA, 1726 Massachusetts Ave., NW, Washington, D.C.)

American **Psychiatric** Assoc. (M. Greenblatt, Massachusetts Mental Health Center, Boston)

American **Rocket** Soc. (B. Chifos, ARS, 500 Fifth Ave., New York 36)

American Soc. of **Criminology.** (J.

Chwast, New York Inst. of Criminology, 115–117 W. 42 St., New York 36)

American Soc. of **Naturalists.** (W. K. Baker, Dept. of Zoology, Univ. of Chicago, Chicago 37, Ill.)

American Soc. of **Zoologists.** (R. L. Watterson, Dept. of Zoology, Univ. of Illinois, Urbana)

American **Sociological** Assoc. (B. Clark, Center for the Study of Higher Education, 2747 Bancroft Way, Berkeley 4, Calif.)

Association of American **Geographers.** (B. Burton, 213-1500 Arlington Blvd., Arlington, Va.)

Association for **Computing** Machinery. (B. Gilchrist, IBM Research Laboratory, Post Office Box 218, Yorktown Heights, N.Y.)

Biomedical **Information-Processing** Organization. (R. S. Ledley, Natl. Biomedical Research Foundation, Silver Spring, Md.)

Biometric Soc., ENAR. (T. A. Bancroft, Iowa State Univ., Ames)

Ecological Soc. of America. (R. B. Platt, Dept. of Biology, Emory Univ., Atlanta 22, Ga.)

Herpetologists' League. (W. Fox, School of Medicine, Louisiana State Univ., 1542 Tulane Ave., New Orleans 12)

Institute of **Management Sciences.** (B. V. Dean, Dept. of Management, Case Inst. of Technology, Cleveland, Ohio)

Metric Assoc. (R. P. Fischelis, 1426 G St., NW, Washington, D.C.)

Mountain Lake **Biological** Station. (J. L. Riopel, Univ. of Virginia, Charlottesville)

National Assoc. of **Biology Teachers.** (P. R. Fordyce, Oak Park–River Forest High School, Oak Park, Ill.)

National Assoc. of **Science Writers.** (N. Haseltine, *Washington Post,* Washington, D.C.)

National **Geographic** Soc. (W. R. Gray, NGS, 16th and M Sts., Washington 6)

National **Science Teachers** Assoc. (R. H. Carleton, NSTA, 1201 16th St., NW, Washington 6)

National **Speleological** Soc. (J. V. Thrailkill, Dept. of Geology, Princeton Univ., Princeton, N.J.)

Philadelphia **Botanical** Club. (W. M. Benner, Acad. of Natural Sciences of Philadelphia, Philadelphia 3)

Scientific **Research** Soc. of America. (D. B. Prentice, RESA, 51 Prospect St., New Haven, Conn.)

Sigma Delta Epsilon. (D. Quiggle, 133 Chemical Engineering Bldg., Pennsylvania State Univ., University Park)

Society for General **Systems Research.** (R. L. Meier, Mental Health Research Inst., Ann Arbor, Mich.)

Society for the **History of Technology.** (T. P. Hughes, Dept. of History, Washington and Lee Univ., Lexington, Va.)

Society for Industrial and Applied **Mathematics.** (G. Kaskey, Remington Rand Univac, 1900 W. Allegheny Ave., Philadelphia)

Society for Industrial **Microbiology.** (T. C. Cordon, Agricultural Research Service, 600 E. Mermaid Lane, Philadelphia 18)

Society of the **Sigma Xi.** (T. T. Holme, 51 Prospect St., New Haven, Conn.)

Society of Systematic **Zoology.** (C. F. Lytle, 801 Ware St., SW, Vienna, Va.)

Tau Beta Pi Assoc. (R. H. Nagel, Tau Beta Pi Assoc., Univ. of Tennessee, Knoxville)

United Chapters of **Phi Beta Kappa.** (C. Billman, 1811 Q St., NW, Washington 9)

Special conferences to be held during the meeting include:

Academy Conference, natl. meeting of 47 state academies. (R. C. Miller, California Acad. of Sciences, San Francisco)

Conference on **Science Manuscripts.** (N. Reingold, Science and Technology Div., Library of Congress, Washington 25)

Conference on **Scientific Manpower.** (T. J. Mills, National Science Foundation, Washington 25)

27–29. American **Geophysical** Union, western natl. meeting, Stanford, Calif. (W. W. Kellogg, Rand Corp., 1700 Main St., Santa Monica, Calif.)

27–29. American **Philosophical** Assoc., Pacific Div., Berkeley, Calif. (L. E. Hahn, Washington Univ., St. Louis 30, Mo.)

27–29. Western Soc. of **Naturalists,** San Jose, Calif. (I. A. Abbott, Hopkins Marine Station, Pacific Grove, Calif.)

28–29. California Assoc. of **Chemistry Teachers,** Fresno. (R. H. Major, 1736 N. Sierra Bonita Ave., Hollywood 46, Calif.)

should like to emphasize certain of its constructive aspects.

First, there were several very useful presentations and discussions which were both stimulating and enlightening—for example, the paper by Gardner Murphy. Second, as at most large meetings, much was achieved in the halls and at the snack bars, outside of the formal sessions. It was here that scientists had the opportunity to meet with their colleagues and, under the stimulus of the meeting, exchange ideas pertinent to their individual interests and specialties.

The considerable confusion at the congress reflected in part the complexity of the problems to which the congress addressed itself and in part the diversity in background and orientation of the participants. This diversity led to a number of divergent assessments of the problems under consideration, and therefore to a number of conflicting images of the nature and intent of the congress. Some of those present felt that the problem of survival in a nuclear age is so complex that we haven't even begun to ask the right questions as yet, much less attempt to offer solutions. They believe that a necessary first step consists of getting together as many concerned scientists and scholars from various disciplines as possible, to talk to each other and to lay the groundwork for a continuing effort. Others, at the opposite end of the spectrum, felt much more certain of the nature of the problem and of the answers it entails. They consider it imperative that scientists make recommendations both to the public and to political leaders. Those who held this viewpoint considered it natural that a number of nonscientists should be included in the congress and felt that political leaders should be instructed as to what steps they ought to take. It was inevitable that these views would clash. But the clash, in itself, was not necessarily unhealthy. For it was only through the debates that ensued that some of the issues became clarified. Although such a process may be painful to all concerned, it is probably the necessary prerequisite to the birth of a new interdisciplinary effort.

Certainly there is need for an organization which can bring together the various data, concepts, and insights which apply to some of the major problems confronting our 20th-century nuclear-technological society. It is also clear that such an effort must rely heavily on the participation of social scientists.

An imperfect start on the part of a new group is better than no start at all. If there were shortcomings in this past congress, then let us all work for a more perfect one in the future.

TOM STONIER
Rockefeller Institute, New York

The failure of the New York "Science of Survival" meeting was unfortunate, but it might easily have been predicted by those who have read between the lines at similar, previous meetings, such as the AAAS meeting in Denver last December. Holding a town meeting under the guise of a scientific meeting is not the entire reason for chaos. Scientists, too, are at fault when in a strictly scientific meeting an individual, because of high stature in his field, suddenly becomes an "expert" in other scientific fields.

Perhaps some progress could be made if participants were required to limit their remarks to their area of special competence. Then we would not have prominent anthropologists expounding on fire storms; radiation biologists, with expertise in the effect of x-rays on *Drosophila*, expounding on the physics of fission-product decay; sociologists expounding on the characteristics of concrete failure under blast pressure; or physicists expounding on how to buy world peace with diversion of funds from other objectives. The public is understandably confused by conflicting statements from scientists whose eminence gives them large press coverage; the scientist is not much better off when it comes to judging the qualifications of another scientist, in a different field, to discuss something in still a third field. However, the scientist can help because he knows what it means to be qualified in one field, and if he would help shout down certain fellow scientists in scientific meetings, as well as hecklers in public meetings, we might make a nano-inch of progress.

NORMAN J. HOLTER
Helena, Montana

I would have been pleased to report that the Congress of Scientists on Survival was a success, but unfortunately it was a disaster. The possibilities open to me in describing the congress were thus limited, unpalatable as this may seem to those who feel that the congress's worthy goals compensated for its foolish behavior. I am pleased to learn that Eleanor Leacock found many of the workshop sessions "most productive." If they helped to enlighten her on the great problems of peace and disarmament, they served a useful purpose. It should be noted, however, that many people who are professionally involved with these problems were considerably less enthusiastic about the workshop results.

I think Stonier is too charitable to the congress's organizers when he writes that the confusion was attributable, in part, to the complexities of the problems under study. Chaos is not a necessary concomitant of attempts to deal with difficult problems. The congress did not break down because it could not find a quick solution to the world's sorry state; it broke down because it suffered from an overabundance of good intentions and a dearth of hard thought. Its guiding lights failed to recognize that if S.O.S. is ever to achieve any influence with policy makers, its recommendations must be based on more than a desire to do good. Anyone has a right to be against civil defense, but when an organization that purports to be "scientific" says it is against civil defense, it is not unreasonable to expect that it has looked into the matter, a step which S.O.S. neglected in its enthusiasm to get to work on the grave problems that afflict mankind.

—D. S. GREENBERG

Hybrid Enzymes and Isozymes

The publication of two articles on hybrid enzymes in the same week, one by Cahn, Kaplan, Levine, and Zwilling (*1*) and the other by Drew Schwarz (*2*), emphasizes the complexity of the problem of multiple molecular forms of enzymes as well as the differences in viewpoint. The thesis of Cahn and his associates is that the five electrophoretically distinct types of lactic dehydrogenase (LDH) arise as a result of combinations of two different subunits (M and H) to form tetramers, three of these types being "hybrid" enzymes. On the basis of "indirect" genetic methods—that is, comparison of species differences in lactic dehydrogenases, these authors conclude that M and H are under the control of "different" genes, presumably at different loci. The associations of M and H that give rise to the hybrids are presumed to occur at random.

The hybrid enzymes of Schwarz

occur in maize heterozygous for alleles at the *E* (esterase) locus (*2*). A heterozygote produces three electrophoretically distinct types of esterases: two parental types and a hybrid type that lies midway between the parental types. A comparison of cells with different gene dosages suggested that each type occurs as a dimer. These esterases are bound to particles obtained by centrifugation for 2 hours at 105,000*g* in cells that are actively synthesizing enzyme. Experiments with mixtures of cell fractions from different genotypes showed that the hybrid enzyme appears only in the heterozygote and that it is bound to the particles. It does not arise as a result of adsorption of "formed" enzyme. Schwarz concludes that the hybrid esterase arises *during* protein synthesis under the joint influence of ribonucleic messengers from the two alleles.

These studies point up the difficulties of trying to "classify" macromolecules. On the one hand, a hybrid enzyme is produced under the influence of genes at different loci (*1*); in the other case, a hybrid enzyme results from interaction of alleles at a single locus (*2*). As the evidence accumulates, it may be possible one day to devise a "taxonomy" for macromolecules. Until then, it should be pointed out that a particular type of macromolecule, such as a hybrid enzyme, may arise through various routes of synthesis.

Cahn *et al.* (*1*) make the distinction between a hybrid enzyme and an isozyme. They suggest that an isozyme is more restricted in its composition. In the strictest sense, isozymes are the products of a single gene (*3, 4*). Recently my co-workers and I have found evidence for the formation of both isozymes and hybrid acid phosphatases under the control of alleles at a single locus in variety 1 of *Tetrahymena pyriformis* (*5*). One of the homozygotes produces three isozymes; the other homozygote produces only one. The heterozygote ($P\text{-}1^A/P\text{-}1^B$) forms hybrid phosphatases intermediate in electrophoretic position to the parental phosphatases. Five electrophoretically distinct types are produced, in various proportions, in different cell lineages of the heterozygote. In this case, the five types of acid phosphatases are the joint products of alleles at a single locus.

Cahn *et al.* (*1*) suggest that the M and H lactic dehydrogenases may operate under different metabolic conditions. These functional differences appear to have developmental and evolutionary consequences. Differences in localization of the lactic dehydrogenases within a single cell (*6*) might also be stressed. Thus, the tertiary structure of a macromolecule may be modified by its incorporation into the three-dimensional framework of the cell. Such localization to specific cell sites could restrict the randomness of the system. In *Tetrahymena* the esterases and acid phosphatases are "particulate" bound (*3, 5*). In the case of the esterase isozymes a differential localization of members of the isozymic set occurs. Mutation may even affect an isozyme by shifting its position in the cell (*7*).

SALLY LYMAN ALLEN
Department of Zoology,
University of Michigan, Ann Arbor

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The Individual in History and in Cultural Evolution

The recent article by Thomas Kuhn, "Historical structure of scientific discovery" [*Science* **136**, 760 (1962)], has some significance for cultural evolution as well as for history, particularly in its discussion of the importance of the individual in discovery. There is an established tendency in historiography to segment the past arbitrarily into units that coincide with the lives and accomplishments of great men. Besides being a useful means of ordering the narrative, this tendency reflects the well-inculcated feeling that the individual is of prime importance in "shaping" the course of events.

For defining and describing specific things and events of the past—in their structural aspects—the historian finds this diachronic segmentation not only convenient but advisable. The past must be tied to those who lived it. It is with this realm of description that Kuhn is concerned, and it is on the formal difficulty of matching a discovery to a single discoverer that he focuses his attention.

The scholar who is concerned with the *functional* ordering and explaining of events of the past, whether he calls himself historian or cultural evolutionist, can ill afford this emphasis on the individual, for he is dealing with a process that transcends the individual. Kuhn was not concerned with the evolutionist approach, but his admonitions nevertheless concern the cultural evolutionist.

The things and events of the past can be described—and this we usually think of as the historian's task—or they can be explained through analysis of the network of antecedent events. Such an explanation, whether it deals with specific historical facts or general trends, constitutes an approach to the past that is qualitatively different from description. It is not in content but in context that the two approaches differ. And in both it is dangerous to attribute a discovery entirely to the genius of the individual who brought it to scientific light. For the historian describing or defining a historic situation, Kuhn's objections are well taken; for the student concerned with the developmental pattern—with cultural evolution—the danger is the anthropocentric fallacy of citing the individual as the moulder of culture. In this context the individual is the instrument of the culture. One should not attribute cultural events to his instrumentation. The discoverer or inventor, however enlightened, does not explain the development.

It is a fact of history, for example, that James Watt developed and patented the first true "steam engine" in 1769. Here the descriptive historian has little difficulty. But this innovation did not occur simply because Watt was there to give it birth, as a mere statement of the event might imply. It can be attributed only to the cumulative and directional flow of the process of cultural evolution. These attributes of the process—cumulation and direction—remove it from the sphere of fortuitous occurrence and give it scientific meaning. Barzun (*1*) illustrates both in his treatise on the evolution of evolutionary thought. Darwin, he shows, was the point of articulation of the cultural process, not its instigator. The process had reached a consummation in time (*1*, p. 33): "Earlier . . . a new scientific theory would have reached but a small circle of readers. Later, it would have been lost amid the clatter of front-page headlines." White (*2*) has made the relationship of the discoverer to the process of discovery explicit: the discovery or invention is explained (*2*, p. 170) "in terms of a growing and interactive culture process; the individual inventors or discoverers are merely the loci and vehicles of this process." And Kroeber examined the difference long ago (*3*): "the content of the invention or discovery springs in no way from the makeup of the great man . . . but is a product purely of the civilization. . . ." There are always many other enlightened individuals on hand to bear the torch of discovery if the Watts and Darwins are not available.

Despite Kuhn's emphasis on the individual vis à vis the discovery in descriptive history, he said much for the student of cultural evolution regarding the individual vis à vis his cultural milieu. Slanted emphasis in either case can easily spotlight the personality and obscure the process.

RONALD K. WETHERINGTON
7047 Larchmont Drive,
North Highlands, California

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Since I had intended my article to be read as a plea for what Wetherington calls the "evolutionist approach," I can only gratefully agree with most of his comments. In one respect, however, I find his letter troublesome. Let me here restrict my attention to it.

Wetherington seems to imply that there are two distinct approaches to the analysis of historical development—one structural, the other functional—each with its own strength and dangers. Applied to the problem of discovery, the structural approach is the one that attributes a discovery to the man who made it, while the functional approach suppresses the role of the individual in favor of the cultural milieu. Thus, to say that "James Watt developed and patented the first true 'steam engine' in 1769" is for Wetherington an example of the structural approach. In the functional approach one would study the evolution of the steam engine from the background of social needs and craft techniques supplied by the Industrial Revolution.

But are there really two approaches? Only by defining Watt's engine as the first "true" one can one transform Wetherington's structural statement into "a fact of history." Even with that definition, one would have difficulty justifying the date 1769 as the year of the engine's development. The sort of statement which Wetherington employs to illustrate the structural approach is, more often than not, simply a mistake. On the other hand, to say that discoveries normally evolve with time is not to say that individuals have a negligible role in their evolution. Nor is it to say that the evolution of each and every discovery must be studied against the entire cultural ambiance of the day.

Given any particular discovery, it is a historical problem—perhaps *the* historical problem—to discover whether and how the individual interacts with his milieu, and with which parts of the milieu he interacts, in the evolution of novelty. There is no single answer. Studying different discoveries, the historian will find many different sorts and degrees of dependence. But it is the discovery that varies, not the method of study. Call that structural, functional, neither, or both, the historian can neglect the individual or his milieu only after research has shown that one or the other played a negligible role in the development of the episode that concerns him. Structure and function will emerge together from that research. They are not to be had separately.

THOMAS S. KUHN
University of California, Berkeley

Interpretation of Cation-Exchange Mechanisms

In their report, "Course of cation absorption by plant tissue" [*Science* **136**, 1051 (1962)], E. Epstein, D. W. Rains, and W. E. Schmid discuss in terms of an "enzyme-kinetic" model experimental data of constant absorption rates depending exponentially on temperature. The authors conclude: (i) the overall mechanism of cation absorption (not further defined) possesses a "high degree of irreversibility"; (ii) absorption is metabolically mediated; (iii) the cation-exchange capacity (labeled general, nonselective) is largely satisfied by calcium under physiological conditions; (iv) the time course (the rate?) of the absorption of monovalent cations under these conditions is precisely like that of anions; (v) "overt, nonselective cation exchange" plays no role; and (vi) there is no evidence of "measurable instantaneous absorption" (zero time intercepts) ostensibly reflecting specific ion-carrying sites.

I shall pass over the fact that clarification of some highly individual expressions used by the authors is needed, to point out that it is difficult to see the logic of drawing such sweeping generalizations from the 12 data points (from two absorption experiments, at 4.5° and 30°C) presented. On a strictly thermodynamical basis the only result seems to be establishment of constant diffusion rates for rubidium from outside solution to interior phase of the tissue, varying with temperature.

In particular, no irreversible nature of the absorption process (conclusion i) is proved by constant reaction rates when a rate-limiting diffusion process may certainly be present. It should be valuable to follow the process up to the inevitable diminution of absorption rate and final saturation; infinite capacity of the interior phase cannot be postulated. The "enzyme-kinetic" model referred to is thermodynamically and kinetically identical to the mechanism of ion transport across an ion-exchange membrane; therefore, the possibility that this is simple nonmetabolic transport cannot be excluded until conclusion ii is supported by actual correlation with metabolic measurements. The analogy with enzyme mechanics depends also on proof of conclusions i and ii. Conclusion iii follows from the laws of ion exchange and mass action, while no evidence is presented in the report itself concerning conclusions iv and v. There is no need to fight "instantaneous absorption" (conclusion vi), which does not exist in chemistry. This should not be equated with zero time intercepts drawn on paper, which will result in all cases of initial higher absorption rate (a phenomenon quite usual in ion exchange and physical absorption and reflecting not so much differences in concentration as differences in environment and reactivity of exchange sites).

Diffusion, selective ion exchange, and aging processes are not even mentioned in the discussion of these experiments on the diffusion of a rather special ion in biologic material, though these factors combined may actually confer a "degree of irreversibility" to the process.

I wish to enter a strong plea against the use of well-defined terms like *irreversibility* and *equilibration* without close regard to their actual meaning; it should be completely clear that an irreversible process can never reach equilibrium.

PAUL S. ANTAL
Institute of Marine Science,
University of Miami, Miami, Florida

With respect to conclusions i and ii, the enzyme-kinetic model of ion transport by carriers is not thermodynamically and kinetically identical with the mechanism of transport across an ion-exchange membrane. Both rubidium and chloride are transported, at about equal rates, and accumulate as free ions in an interior aqueous phase at concentrations far in excess of their external concentrations. These well-documented facts imply that metabolic energy is expended, and indeed transport is inhibited by factors that interfere with normal metabolism, including low temperatures. Simple nonmetabolic transport is therefore excluded.

Diffusion and even isotopic exchange across the osmotic barrier are extremely sluggish as compared with net rates

of absorption. Simple diffusion and ion-exchange mechanisms are inadequate for this and other reasons, and a carrier mechanism analogous to enzyme mechanisms has been proposed not by way of "proof" of anything, but as a working hypothesis or conceptual model. According to this model, the rate of absorption would be expected to decline as the accumulation ratio rises with time if we were dealing with a readily reversible process. Instead, the rate remains constant.

As for conclusions iii, iv, and v, the contrast between the time course of the absorption of cations and that of anions has been known since the 1920's; in the absorption of cations there is a marked initial, rapid phase of uptake representing cation exchange, while anion exchange is negligible. In our experiment, the simple cation-exchange uptake of rubidium was abolished by the presence of calcium, as evidenced by the absence of any measurable rapid initial phase of uptake, and the time course of rubidium absorption was like that of anion absorption.

As for conclusion vi, saturation of the active transport mechanism (carriers) occurs without a measurable time lag after immersion of the tissue in a solution: the process is "instantaneous" (Webster defines *instantaneous* as "occurring . . . without any perceptible duration of time"). In our experiment, a perceptible lag would show up as a negative intercept on the *Y* axis.

We are not acquainted with any theory of biological ion transport combining "diffusion, selective ion exchange, and aging processes." Selectivity was not discussed in our report because the experiment did not deal with it. However, selective ion binding is an integral part of the enzyme-kinetic model of ion transport by carriers, and much evidence on selectivity has been published.

Rubidium is indeed a "rather special ion" in biological material, but is Antal unaware of its extensive use in studies of biological ion transport? This and other well-known facts mentioned here were not elaborated upon in our report, which, like other short technical reports, dealt with certain specific points and was not meant to be a primer on the entire subject.

EMANUEL EPSTEIN
D. W. RAINS
WALTER E. SCHMID
Department of Soils and Plant Nutrition, University of California, Davis

Terminology for the Anterior Pituitary Hormones

Josephine Stewart and C. H. Li [*Science* **137**, 336 (1962)] have made what they apparently consider an appeal to reason for the acceptance of *-tropin* and *-tropic* endings in connection with anterior pituitary hormone terminology. It must be pointed out that this particular coin does have another side and that an equally good, if not better, case can be made for the acceptance of the *-trophin* and *-trophic* endings. Preference, according to Stewart and Li, has a geographic or personal basis, and they imply that a multitude of biochemists and endocrinologists have no knowledge or awareness of the derivation and meaning of the suffixes *-tropin* and *-trophin*. I would like to suggest that the opposite is actually true; that *because of* the continued use of both endings, practically everyone who is aware of this duality of terminology is well versed in the etymologic basis for both usages and is cognizant of the equivocal nature of the problem. The somewhat facetious, tongue-in-cheek statement of the editors of *Hormones in Blood* would seem merely to emphasize this point.

While I agree wholeheartedly with Stewart and Li in regard to the *desirability* of universal acceptance of one or the other type suffix, I do not agree with their contention that *-tropin* and *-tropic* should be adopted on the basis of "signification." They are willing to take a few linguistic liberties in proving the signification of *-tropin* but are really puristic in what they consider the verb *nourish* implies. In their report they state, "Thus, terminology with this suffix [*-tropic*] implies the stimulating effect of the hormone on its target organ which is characteristic of all these adenohypophyseal hormones." This statement would seem to involve an undue extension of pituitary-gonadal reciprocity, since the term *gonadotropic* ought to imply that the gonad, not the "tropic" hormone, is the stimulating agent. What this terminology actually indicates, and properly so, is the fact that the adenohypophyseal hormones turn toward or are directed toward, or have affinity for, their target organs. On the other hand, it is held by many that the pituitary "trophic" hormones do, in fact, *nourish* the target glands, if not in the restricted sense of supplying carbohydrates, fats, and proteins, certainly in the general sense of supplying "hormonal nourishment" which somehow fosters, supports, and stimulates the subordinate glands. Furthermore, one of the significant effects of the anterior pituitary hormones is the production of an increased blood flow through the target glands, and this must also make for better nutrition (in the restricted sense of the word) of the glandular cells, indirectly at least.

Finally, I would suggest that, in addition to legitimacy and signification, priority and volume of usage be considered in the eventual resolution of this question. The terms *gonadotroph* and *thyrotroph* were suggested as suitable functional designations for the particular basophils which produce the gonadotrophic and thyrotrophic hormones, respectively. Although originally used by H. D. Purves and W. E. Griesbach of New Zealand [*Endocrinology* **49**, 224 (1951)], these terms have found almost universal acceptance among the cytologists of the world, and, in fact, cytologists in the United States have since added the following names for other specific cell types: "corticotroph" for a cell type thought to be the producer of ACTH [M. G. Farquhar, *Anat. Record* **127**, 291 (1957)] and "luteotroph" for the acidophil which produces Luteotrophin (E. G. Rennels, *Endrocrinology*, in press). Obviously, acceptance of the *-tropin* and *-tropic* endings would demand also a change to the suffix *-trope* for the various cell types. Such a universal change is not likely to be dictated by the wishes, whims, or preferences of any one laboratory or any one journal. Any eventual agreement and unification of usage must be the result of an arbitrary decision among either the leading endocrinologists of the world or the several journals which publish endocrinologic papers. I would favor acceptance of the endings *-trophic*, *-trophin*, and *-troph*.

EDWARD G. RENNELS

Department of Anatomy, University of Texas, Galveston

Thousands of scientists and teachers have wrestled more or less seriously with the etymo-biological problem stated by Stewart and Li in their article concerning the use of *-tropin* or *-trophin* in reference to the adenohypophyseal hormones. From long perusal of the endocrinological literature I note that some investigators have avoided making a firm personal decision in this matter, possibly being influenced by coauthors or other colleagues to use one or the other form at various times. (Perhaps the rare cases in which both terminologies have appeared in one paper can be ascribed to typographical error.)

In my own case, I took a firm stand in favor of *-trophin* and *-trophic* some 12 years ago—not in response to any endogenous conviction but as a result of reading a compelling argument published several years previously by G. W. Corner [*Endocrinology* **33**, 405 (1943)]. The amazing thing is that Stewart and Li have used a similar approach to reach a conclusion exactly opposite to that reached by Corner, who agreed with J. H. Burns that a *thyrotropic* hormone, for example, would mean one which is attracted *to* the thyroid by some stimulus exerted *by* the thyroid. Said Corner: "The use of -tropic as in gonadotropic therefore reverses and confuses a clear, practical pre-established usage in the broad field of biology. To use it is to countenance the warping and blunting of our scientific terminology, which should be a precision tool."

Anyone for *-trophin*?

William J. Mellen
College of Agriculture,
University of Massachusetts, Amherst

In connection with the hormones of the anterior pituitary, Stewart and Li have proposed that the suffixes *-tropin* and *-tropic* be generally adopted. We believe, however, that a better case can be made for the use of *-trophin* and *-trophic*, both on the basis of current usage and on the basis of etymology.

Widespread use of *-tropic* is relatively recent in this country, and it is now almost wholly restricted to the United States. When I (J.A.R.) was a student at the University of California in the early 1930's, I first became acquainted with the pituitary hormones as "trophic" substances, and in our time at Yale from 1938 to 1950, "trophic" was the preferred usage. *Endocrinology* for 1934 carries only *gonadotrophin* in its index. By 1936 only *gonadotropin* appears, but in later years *Endrocrinology*, like many other journals in this country, has allowed either word according to the usage of the authors. In Europe, *-tropic* seems to have been used first in several languages. However, the *Biochemical Journal* began to use *-trophic* consistently in 1937, and the *Journal of Endocrinology* has used only *-trophic* since its first volume in 1939. Except for those in the United States, publications in English throughout the world—in Britain, Scandinavia, Canada, and Australia—have now for many years used *-trophic* almost exclusively. In Latin America, only *trophic* seems to be used, with *f* replacing the *ph* in Spanish. In France both forms are used, but that related to *-trophic* is the more common in scientific journals. Thus, those who prefer the early European usage of *-tropic* are now in a minority in the world literature.

Like Stewart and Li, we believe that terminology is closely related to concept and that precision in meaning should be considered more important than popular usage in determining scientific language. We differ from these authors, however, in the meanings we associate with the words in question. Here are some summary definitions from a standard Greek lexicon (Liddell and Hart, ed. 8):

tropē: a turn or turning; solstice; turning about of the enemy, putting to flight, a rout; of wine, turning sour; a turn of speech.
tropikos: of the solstice (i.e. of the apparent turning of the sun, hence modern Tropic of Cancer, (tropical); in rhetoric, figurative.
tropos: a turn, direction, way; manner, guise; of persons, habit, temper.
tropheus: one who rears or brings up; foster father, nurse; breeder, rearer.
trophē: nourishment, food, provisions; livelihood, that which procures sustenance; nurture, rearing, bringing up, education; rearing or keeping of animals.
trophicos: nursing, tending.
trophimos: nourishing; one who finds board, master of the house; foster child; of bodies, healthy, strong; of plants, flourishing.
trophos: feeder, rearer, nurse.

It is evident that the original meanings of the two root words differ not only from each other but from the meanings attributed to them by Stewart and Li. *Tropē* or *tropos* refers literally to a physical turn or turning away, figuratively only to way or manner or turn of speech. *Trophē* refers, not to nutrition in the sense of food per se, but to nurture in the sense of providing sustenance and care, of fostering or making to grow and flourish.

These meanings have been preserved closely in modern usage. In several dictionaries (standard English, medical) *-tropic* is defined as referring to turning or movement toward or away from a focus of heat, light, or other stimulus. No type of "response" other than turning is indicated. A tropometer is an instrument for measuring angle of torsion; the examples given by Stewart and Li, such as *heliotropic* or *isotropic*, all refer to turning movement or to physical direction. *Trophic* is defined as meaning concerned with, or regulating, nutrition of tissues, often with the example "as in *trophic* nerves." Surely the latter usage implies not that nerves furnish food to the muscles they serve, as Stewart and Li appear to believe, but rather that the nerves foster growth and function. Even those who describe the adenohypophyseal hormones as "tropins" admit that in the absence of these substances the target organs become "atrophic" and that excess of the hormones induces "hypertrophy."

From these definitions and uses we conclude that *-tropic* is not well applied in this context but that *-trophic* is apposite. For example, *somatotropic* suggests only turning the body—something to do with orthopedics or gymnastics perhaps—whereas *somatotrophin* can be understood readily as concerned with the nutrition of the body and with its nurture and growth in a broad sense. Our understanding of the meanings of *gonadotropin* and *gonadotrophin* are illustrated in the older fashion by the following cautionary tale.

There once was an Interstitial Cell residing in a Non-American testis. It was a plump and peaceful cell, rich in lipid and productive of testosterone. This benign supporter of the pleasure principle depended for its *nurture* and its *nourishment* on the gentle constant attendance of a swarm of companions from a distant gland, who called themselves gonado*trophins*, or sometimes, playfully, Interstitial-Cell-Stimulating Hormones. Once, during a terrible phase of the late war, the possessor of this Non-American testis nearly starved in a concentration camp. The amiable companions of the Interstitial Cell nearly disappeared, and the cell knew for a time a lonesome dwindling which it recognized as *atrophy*. Happily, the postwar period brought prosperity and good health to the possessor of the Interstitial Cell. All was as before, until, as may befall any man, the owner came to dwell in the United States. For a few months (one may presume a period of cultural lag) all was well, but then the Interstitial Cell perceived that the gentle nurture of its companions was supplanted by rude jostling. Frequent tremors shook its Golgi apparatus, wavelets crisscrossed its endoplasmic reticulum, and the cell, fumbling, and embarrassed, began to produce rather more etiocholanolone than it ought. Then one morning, horribly, its nucleus began to revolve counterclockwise. The cell cried to its erstwhile gentle companions: "What awful change has come upon you?" The companions in brute chorus replied: "We are *gonadotropins* now, members of the American Adenohypophyseal Turnverein." Said the cell: "I see that you are already much coarsened; you have begun to drop your aitches." "Never mind that," said the hormones, "we are true *tropins*, and you must respond in kind." The poor cell replied: "But I am unaccustomed; I hardly know which way to turn." "It matters not," roared the grinning tropins. "You're in the U.S.A. now. Twist, brother, twist!"

JANE A. RUSSELL
ALFRED E. WILHELMI
Department of Biochemistry, Emory University, Georgia

As Mellen notes, the eminent biologist George W. Corner published an article some 20 years ago on the problem of the suffixes in the terminology of the pituitary hormones, presenting another side of the argument. We have also read with interest Mellen's own arguments and those of Rennels and of Russell and Wilhelmi in favor of the *-trophin* suffix, and since these arguments seem to rest primarily on linguistic grounds, we want to add a linguistic footnote here, at the same time reiterating that the chief concern in all these arguments should be to establish a terminology where the *meaning* is congruent with our best present knowledge of the mechanism of action of the pituitary hormones.

The crux of the arguments on the other side (first stated by J. H. Burn in 1937 and developed fully by Corner) is that a term like *phototropic*, for example, refers to an organism that turns toward the light, or that has light as its orienting stimulus. In this usage, the first or noun portion of the compound designates the stimulus, not the object of the stimulus. On this basis, the argument continues, a term like *gonadotropic* would imply that the gonads act upon the hormone, rather than the reverse; on the other hand, by analogy with our usage, a heliotrope would be a flower that turns the sun!

This argument is an intriguing one, but let us examine it more closely. In the Greek, these compounds consisting of a noun element and a verbal element, as well as the compound adjectives formed from them, are legitimately operative in both directions; that is to say, the verbal element can bear with equal legitimacy either a passive or an active meaning, can imply either "acting" or "acted upon," depending upon the context. To take an example that contains one of the suffixes in question, *kourotrophos* can mean either "nourishing youths" or "nourished by youths." [To be entirely accurate, one should mention that the Greeks usually (although not invariably) made the distinction between the two by the placing of the accent. If the verbal portion of the compound bore an active meaning, the accent was on the antepenult—thus, *kourótrophos*—and if passive, on the penult, *kourotróphos*. When foreign words came into English through the Latin, however, the very strongly regular Latin accent was imposed; thus, any difference in accentuation disappears in the English language]. By the same token, the meaning of *-tropos* could be either "turned upon" or "turning"; and even in the case of the oft-marshaled example *phototropism*, the interpretation "light-turning" (where *light* is the object of the action), rather than "turned by or toward the light" would be perfectly designative in the proper context—say in optics, where an agent could be referred to as bending a light beam. Thus, there is no linguistic precedent in the original Greek that would favor one cause-and-effect order over the other in connection with these compounds.

As to the point that our usage is contrary to a familiar precedent in the field of biology, it is true that there stands established in biological usage a group of words where the noun portion of the compound represents the orienting stimulus and not the thing stimulated. But Corner himself quotes other instances where the reverse is true: *chromotrope*, meaning a dye which changes color under chemical action, or *bacteriotropic*, meaning directed toward bacteria or affecting them in a specific way.

Moreover, if words like *electrotropic*, *phototropic*, and *thermotropic* can be cited, in which the electricity, the light, or the heat is the stimulating agent, the same holds true for the other suffix. Consequently, we can argue that if *phototrophic* means nourished by light and *neurotrophic* means nourished by or through the nerves, then *gonadotrophic* means nourished by the gonads or *thyrotrophic* means nourished by the thyroid. Yet we are certain that no one would insist, except for the sake of argument, that any serious confusion has actually arisen on this ac-

count with either of the suffixes. And furthermore, surely in 1962, nearly three decades after the usage was incorporated into *Webster*, there can be no valid objection to extending the sense of "tropic" from the purely physical sense of "pertaining to a turning," to the figurative "tending to turn or change, esp. . . . in response to a (specified) stimulus." So what we are left with is a difference of opinion about which terminology reflects most accurately the actual mechanism involved in the action of the pituitary hormones on their target organs.

It is interesting that one of the most convincing arguments for the *-trophic* suffix, made by Russell and Wilhelmi, also depends upon a similar extension from the literal to the figurative, from "to nourish" to "fostering growth and development." Their argument that the target organ atrophies if it is not stimulated, and that therefore the hormone fosters growth and development is a good one. We, however, still adhere to our objection to *-trophin* in either of these senses. We feel that the hormone must be viewed, not as an agent which per se fosters the growth and development of the target organ, but rather as an agent that triggers a mechanism in the target organ whereby that gland or organ flourishes and grows. Or it may be seen as a key which *turns* a lock, like an ignition key in a motor car, setting the mechanism into motion. What the mechanism is, we do not know at present. In the next decades we will begin to find out what it is and how it operates, since most of the pituitary hormones have been isolated in pure form and some of them have been synthesized. The mechanism may involve nucleic acids, or it may be something like an interaction between the protein hormone and some subcellular entity or entities in the target gland. At any rate, Corner himself saw that the *-trophin* suffix was "not perfectly apt," but he considered it the lesser of two evils. We strongly believe that, in view of the present concern about hormonal mechanism, the sense of nurture is no longer "sufficiently noncommittal" to render the suffix *-trophin* free from confusion (*1*).

JOSEPHINE STEWART
CHOH HAO LI

Hormone Research Laboratory, University of California, Berkeley

Note

1. We wish to thank Professors J. Fontenrose and L. Bundy, classics department, University of California, for helpful discussions.

Tidal Forces and Widespread Precipitation

In recent reports in *Science* by Bradley, Woodbury, and Brier (*1*) and by Adderley and Bowen (*2*), statistical evidence was presented to show that precipitation maxima tend to occur much more frequently in the lunar intervals new moon to first quarter and full moon to third quarter than they do in the intervals first quarter to full moon and third quarter to new moon. In the former report the authors suggest that the mechanisms responsible for the effect are not known. In the latter report, Adderley and Bowen indicate that the phenomenon "is clearly not incompatible with the meteor hypothesis."

I should like to point out that widespread precipitation, including many maxima, has often been associated with hurricanes. About 9 years ago I suggested (*3*) that there was good reason to suspect that tidal forces play an important role in the formation of hurricanes, and I offered statistical evidence in support of the suggestion. I look upon the evidence presented in the two reports cited as supporting my thesis.

An examination of the distribution within the solar year of the dates used in the statistical work of the two reports would indicate whether or not the maxima considered there may be related, at least in part, to hurricanes. A division of the dates into two groups, those nearer a time of lunar perigee and those nearer a time of lunar apogee, might help to show that the phenomenon is tide-related, if not tide-induced. A division of the dates according to decimal fractions of the tidal period, apogee to apogee, may well show that the work of Rodés (*4*) is sound and significant. I regret that this paper is not available to me at present.

If tidal forces have an important effect in the formation of large storms, one would expect some lag between the time of formation of a storm and the time of maximum precipitation attributable to it. The apparent difference in phase between the graphs for the Northern and Southern Hemispheres presented in the two reports (*1, 2*) may result from differences in the average distance of travel of large storms between the area of formation and the various points of precipitation.

If, as Adderley and Bowen suggest, the phenomenon is related to meteoric material diverted by the moon, one would expect to find the far side of the moon much less pitted with craters than the near side. The argument is that if the moon tends to focus meteor streams on the earth, the earth must, to a much greater extent, focus meteor streams on the face of the moon that is toward us. This seems likely to be true; in any case, we shall have better evidence on the roughness of the far side of the moon in the near future.

One regrets that Adderley and Bowen decided not to publish their data at once. However, the reasons for their decision are clear.

CHARLES H. SMILEY

Ladd Observatory, Brown University, Providence, Rhode Island

References and Notes

1. D. A. Bradley, M. A. Woodbury, G. W. Brier, *Science* **137**, 748 (1962).
2. E. E. Adderley and E. G. Bowen, *ibid.* **137**, 749 (1962).
3. C. H. Smiley, *Nature* **173**, 397 (1954).
4. L. Rodés, *Memorias del Observatorio del Ebro No. 7* (Tortosa, Spain, 1937).

Effect of 1953 Fallout in Troy, New York, upon Milk and Children's Thyroids

The report of Ralph Lapp on the fallout from Nevada testing in Troy, New York, on 26 April 1953 [*Science* **137**, 756 (7 Sept. 1962)] has led to publicity in newspapers and has aroused public concern in the Troy-Albany-Schenectady area. Lapp estimated the level of iodine-131 contamination in Troy and calculated that individuals drinking milk from this area could have received a total dose up to 30 rad. He suggests a thyroid survey of children in this area who were under 2 years of age in 1953.

The records of the New York State Department of Agriculture and Markets for 1953 reveal that, on the average, cattle were first turned out to pasture on 12 May. While some farmers may put their cattle on pasture earlier than the average date, the variation tends to be small, for too early pasturing leads to damage to the turf if the ground is so soft that the hooves of the animals sink into it. Thus, the iodine-131 had 17 days, or more than two half-lives to decay to something less than 25 pecent of the activity deposited on 26 April.

Moreover, the period from 26 April to 12 May was exceptionally rainy that year, even for spring. A total of 5.36 inches of rain fell during this period, in which there were four storms of more than ½ inch each. Weathering may be many times more effective than decay in reducing the amount of fallout activity. Certainly the weather in that period was most favorable to such reduction.

The New York State Department of Health receives reports of cancer cases from physicians. A recent review of these records for residents of counties of Albany, Schenectady, and Rensselaer (where Troy is situated) who were under 2 years of age in 1953 revealed no cases of thyroid cancer. There have been no deaths from thyroid cancer in children from these areas since 1953. Taken together, case reports and death certificates identify approximately 90 percent of the cancer cases occurring in New York State. A study of the files of a surgeon (John C. McClintock) who does most of the thyroidectomies in the Albany-Troy-Schenectady area revealed one thyroid carcinoma in a Troy child who was 20 months of age in April 1953. This is, as Lapp points out, the number to be expected in the population at risk.

While a continuing surveillance of this age group will be maintained by this department, it seems most unlikely that an event which has resulted in no increase of thyroid carcinoma during the ensuing 9 years will lead to such an effect in the future.

JAMES H. LADE

New York State Department of Health, Albany

Population Biology

Ehrlich and Holm in their article "Patterns and populations" [*Science* **137**, 652 (1962)], finding life as a whole to be too complex to be conveniently programmed for a computer, conclude that the only biological unit worth considering is the individual organism, and at one moment of its existence. Even so, there is some doubt as to whether head and tail of the same individual form a congruent whole!

It is easier to get exact quantitative data from a dead animal than from a live one, so we need only take the final step and declare life itself to be a useless concept. This inability to nail life down, once and for all, in a statistical formulation is perhaps analogous to the problem behind the "uncertainty principle" in physics.

Since human culture began it has been obvious to scientist and layman alike that the vast majority of living things fall into well-defined units, for which the generally accepted name is "species." Darwin showed that these species have evolved and are evolving. It then became clear why many species vary to some extent from place to place or, as seen in the fossil record, from time to time. Given the conditions on this planet and the genetic properties of living organisms, thousands, and indeed millions, of these essentially discrete and genetically self-contained units have evolved. We may as well continue to call them species, for to deny their existence is as futile as it would have been for Thoreau to deny the existence of Walden Pond because he found it not exactly the same at all times and places.

DEAN AMADON
American Museum of Natural History, New York, New York

Amadon's approach to problems of population biology emphasizes the need for unemotional evaluations of the concepts in this field.

PAUL R. EHRLICH
RICHARD W. HOLM
Division of Systematic Biology, Stanford University, Stanford, California

Overtrimmed Cover Picture

This note may clear up some confusion which may arise from a perusal of the cover illustration on the 13 July 1962 issue of *Science*. The drawing shows a portion of the follicular epithelium which surrounds a developing oocyte of *Drosophila melanogaster*. A corner of a follicle cell nucleus is shown, as are various cytoplasmic organelles including mitochondria and arrays of endoplasmic reticulum (enclosing lipoidal droplets). To the right of the basement membrane of the follicle cell lies a segment of the epithelial sheath (characterized by its smooth muscle fibrils) which encloses the follicle. To the right of this lies a longitudinally sectioned trachea. The magnification is 27,000. The confusion with respect to the caption arises from the fact that only the right-hand half of a larger drawing was used for the cover illustration.

R. C. KING
Northwestern University, Evanston, Illinois

Yardsticks

The editorial "Which yardsick?" [*Science* **136**, 397 (1962)] deserves commendation for turning attention to the budgetary problems connected with the financing of research—particularly publicly supported research. It is most certainly correct to urge that greater attention be given to describing quantitatively the costs of research proposals and the most likely benefits to be gained from the undertaking.

The notion that there are benefits and costs of research which ought to be accounted for in deciding where to allocate research funds is the type of assertion which an economist interested in the problems of public decision-making can support. I am puzzled, however, by the rejection of certain economic concepts as a means of reaching the conclusion that we need to possess information on the costs and returns of research. I wish both to question the arguments used against economic comparisons and to point out a variance in the implication of the editorial.

In the first place, it is necessary to distinguish more than one level of budgetary choice. The highest level of allocation decision is between the broad categories of public goods and private goods, or—quote—between the speaker's chosen field and beer or popcorn. It is right and proper that we should debate the issue of allocating our resources between these two categories, and I would defend any scientist who wishes to argue that we should review our expenditures for "research" on the one hand and consumption on the other. Insofar as research is a publicly supported activity whose benefits accrue widely over space and time without much relation between sponsor and beneficiary, there is a good deal of relevance to consumption-research comparisons. I urge that this kind of debate be given its proper perspective, and that it not be implied that all discussions of this sort related to research budgets are irrelevant.

In the second place, the only good argument against dollar comparisons of alternative expenditures is that the benefits and costs of the alternatives cannot be quantified. The editorial assumes that the quantifications can be made, but argues that to make the ensuing comparisons is irrelevant. This position fails to recognize the universal attribute of the price mechanism in reducing unlike things to the same common denominator. The paradox that the price system places a lower value on the life-giving air we breathe than on hair tonic is not a cogent criticism of dollar-value comparisons. All that can be claimed for money prices is that they order values at the margin of choice. Because oxygen is in such copious supply we do value the marginal units at less than we value our last bottle of much scarcer hair tonic. But take some of our oxygen away and the price of oxygen will soon reflect its superior use value. (This paradox of value in use and value in exchange puzzled several generations of early economists.)

At the level of budgetary decisions to which the editorial remarks are confined—that is, decisions among choices open to the administrative head of a research organization or of an action agency with related research activities—the kind of cost and return information to be collected has, contrary to the assertion, unlimited usefulness in comparing alternatives. To focus only on "its own nature, needs, and opportunities" in deciding on the right amount of money for a function can be highly misleading and result in a patently irrational system for accomplishing those things we value most highly. The concept of "optimal support" for all desirable purposes appears to mean that each purpose would receive the amount of support believed ideal by those in charge of it. If we are to follow what on the one hand is rational advice and consider "the characteristics and needs of the work to be done," then marginal comparisons of alternatives is unavoidable in all rational allocation decisions involving perennially limited public funds.

I find this schism between a concern for rationally gathered information and a rejection of the economic tools for making rational use of that information most disconcerting. I hope this topic will be carried further in a future editorial.

ROBERT K. DAVIS
1775 Massachusetts Avenue, NW, Washington, D.C.

Book Reviewermanship

From time to time the readers of *Science* are treated to splendid demonstrations of book reviewermanship, definable as the art of discussing a book in such a way that the reader quickly forgets it in his admiration for the reviewer. But earlier exercises in this subtle art have been far surpassed by Lloyd Cabot Briggs in his masterful review of—well, I can't remember the name of the book, and I doubt if anybody else can who got as far as the sentence, "I am writing this review in the heart of the Sahara, under conditions which make it not entirely certain that I will get home alive."

I can hardly wait for his next book review to find out whether he *did* get out alive.

ERWIN KLINGSBERG
1257 Cedar Avenue, Mountainside, New Jersey

I must say this is very flattering, although I'm not so sure that a reviewer can properly be recommended for making his readers forget the title of the book he is reviewing! [Kirk-Greene's *Barth's Travels in Nigeria, Science* **137**, 31 (6 July 1962).]

L. CABOT BRIGGS
Hancock, New Hampshire