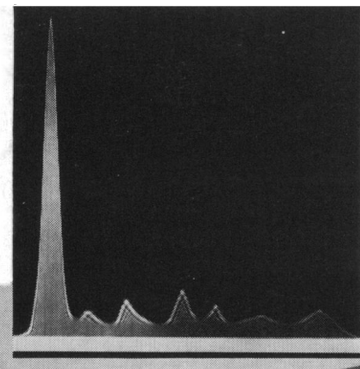
SCIENCE

24 March 1961

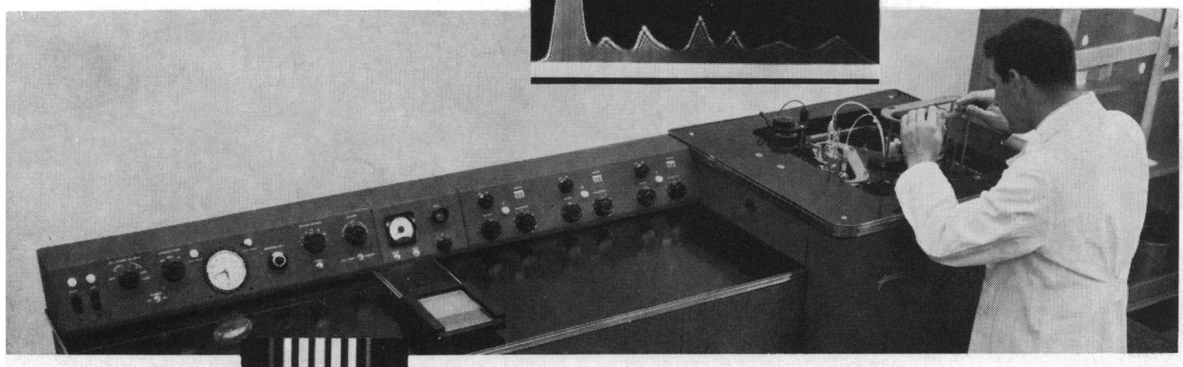
Vol. 133, No. 3456

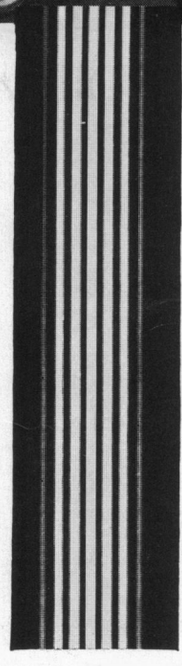
AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

Gordon Research Conferences



Electrophoresis of human plasma diluted 1:6; ascending boundaries. Inclined knife-edge schlieren.





Portion of typical reference fringe pattern obtained from standard production model, magnified to show straightness and definition of entire pattern.

ELECTROPHORESIS AND DIFFUSION

in one precision instrument

As protein research progresses, biochemists rely more and more upon instruments of high precision for diffusion and electrophoresis studies. Especially critical are the optical measurements needed to obtain accurate diffusion coefficients, absolute electrophoretic mobilities, and information on purity.

An exceptional optical system is one of the outstanding features which have made the Spinco Model H invaluable for exacting work in both electrophoresis and diffusion. Light passes through each operating cell twice, giving double sensitivity. Patterns are sharply defined and peak positions can be precisely determined. Reproducible measurements may be made to better than 1/25 of a fringe, which corresponds to approximately .00025 percent protein.

The optical system is flexible, too. It permits measurements by five different methods — ordinary and cylindrical lens schlieren, Rayleigh and Gouy fringes, and mechanical scanning.

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We'd like to tell you more about the Model H and how it can fit the requirements of your research program. For complete details, please write Spinco Division, Beckman Instruments, Inc., Stanford Industrial Park, Palo Alto, California, for information File H-5.

Sales and service facilities on the Model H are available on the same basis as for Spinco Ultracentrifuges, assuring prompt, efficient service for users here and abroad.

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S-53

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Sulfur Tetrafluoride-Boron Fluoride Complex. ($SF_4 \cdot BF_3$)
N, N-Dimethyl Sulfamyl Chloride. [$(CH_3)_2NSO_2Cl$]
Sodium-2-Chloroethane Sulfonate. ($ClCH_2CH_2SO_3Na$)
Hydroxylamine-O-Sulfonic Acid. (H_2NOSO_3H)
Duroyl Propionic Acid. ($C_{10}H_{13}COCH_2CH_2COOH$)
2, 2-Dichlorobicyclo- (4,1,0) Heptane. ($C_7H_{10}Cl_2$)
2-Methylmercaptophenol. ($CH_3SC_6H_4OH$)
4-Methylmercaptophenol. ($CH_3SC_6H_4OH$)
Sulfamide. ($NH_2SO_2NH_2$)
Sulfuryl Fluoride. (SO_2F_2)
Sulfur Trioxide-Pyridine Complex. ($SO_3 \cdot$ Pyridine)
Methylene Sulfate. $(CH_2SO_4)_2$

Frankly, as yet we know very little about the uses for these new chemicals. We do have limited technical data on each which we'll be glad to send you. We can also provide small laboratory quantities of most of them for your investigation.

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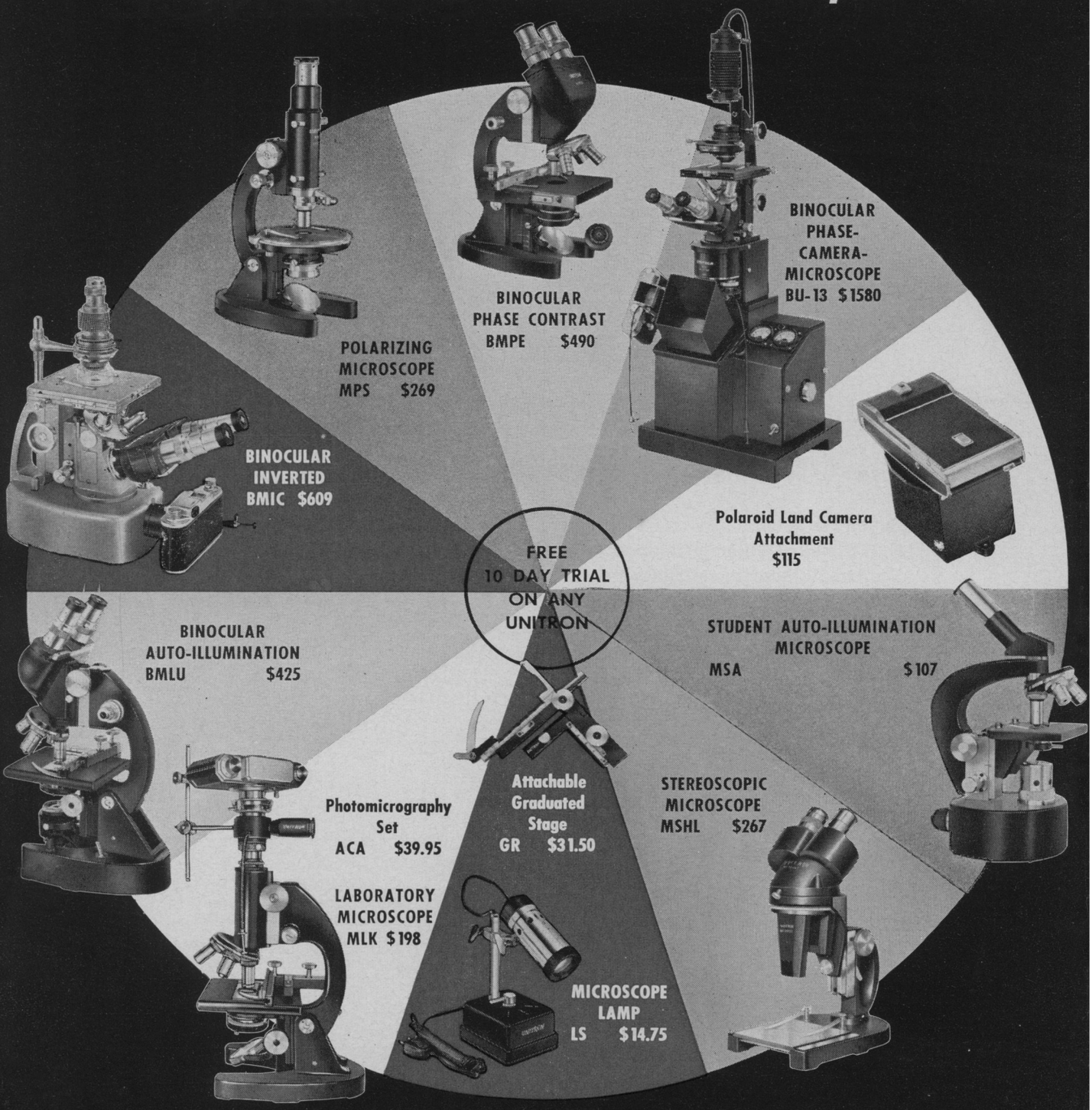

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24 March 1961, Volume 133, Number 3456

SCIENCE

Cover

Oblique aerial view of the Wabar meteor crater, about 300 feet in diameter and 40 feet deep, near Al Hadida, in east-central Saudi Arabia. See page 882. [Arabian-American Oil Company]

measure the volume of a pulse beat . . . the weight of a breath . . . the vacuum of outer space
measure gyro rotor unbalance . . . liquid level in a remote tank . . . the thickness of a continuous sheet of hot metal

Convert any variable into a change in capacitance and there's a Delta unit available to measure, record, or control that variable more accurately and more economically than was ever before possible.

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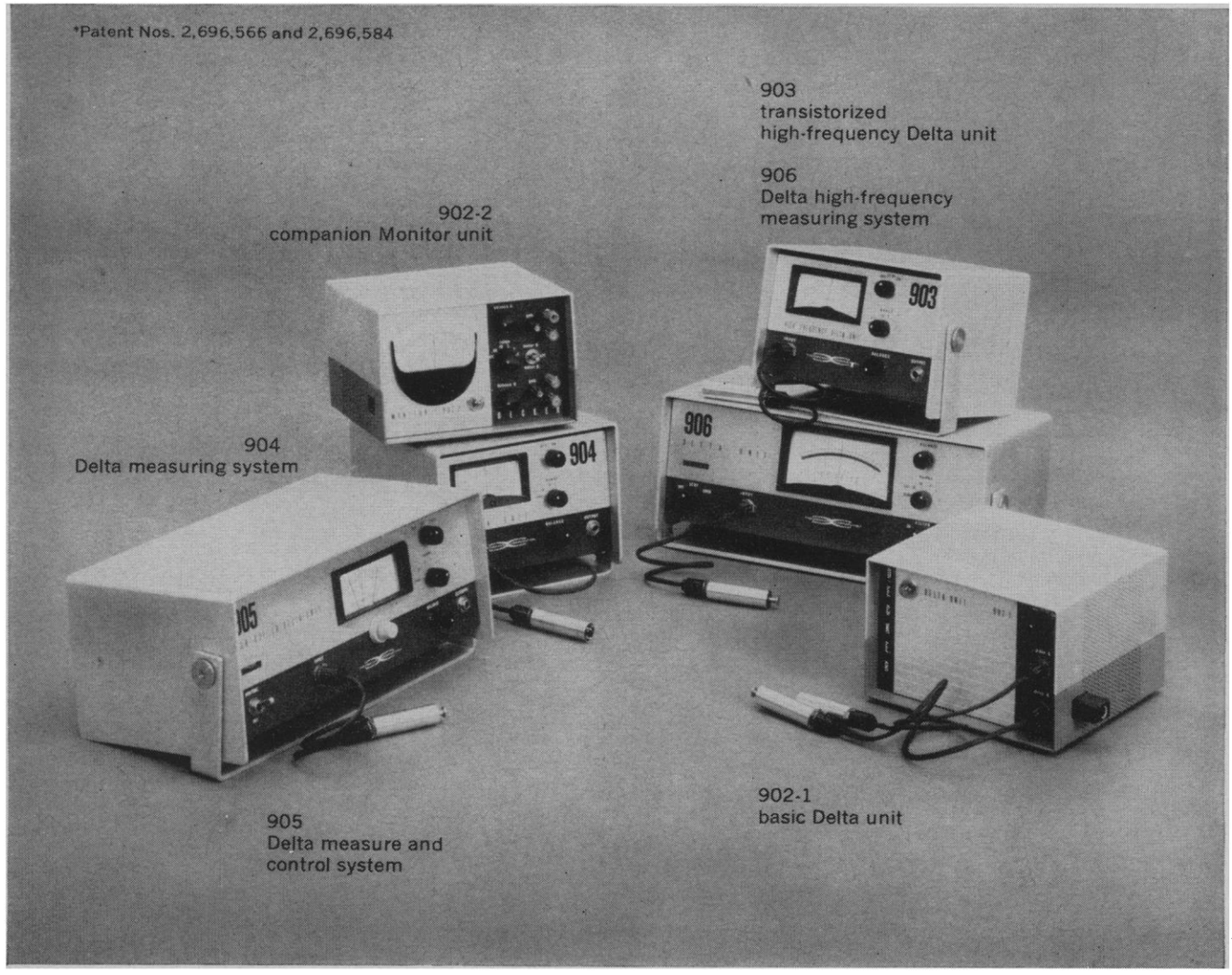

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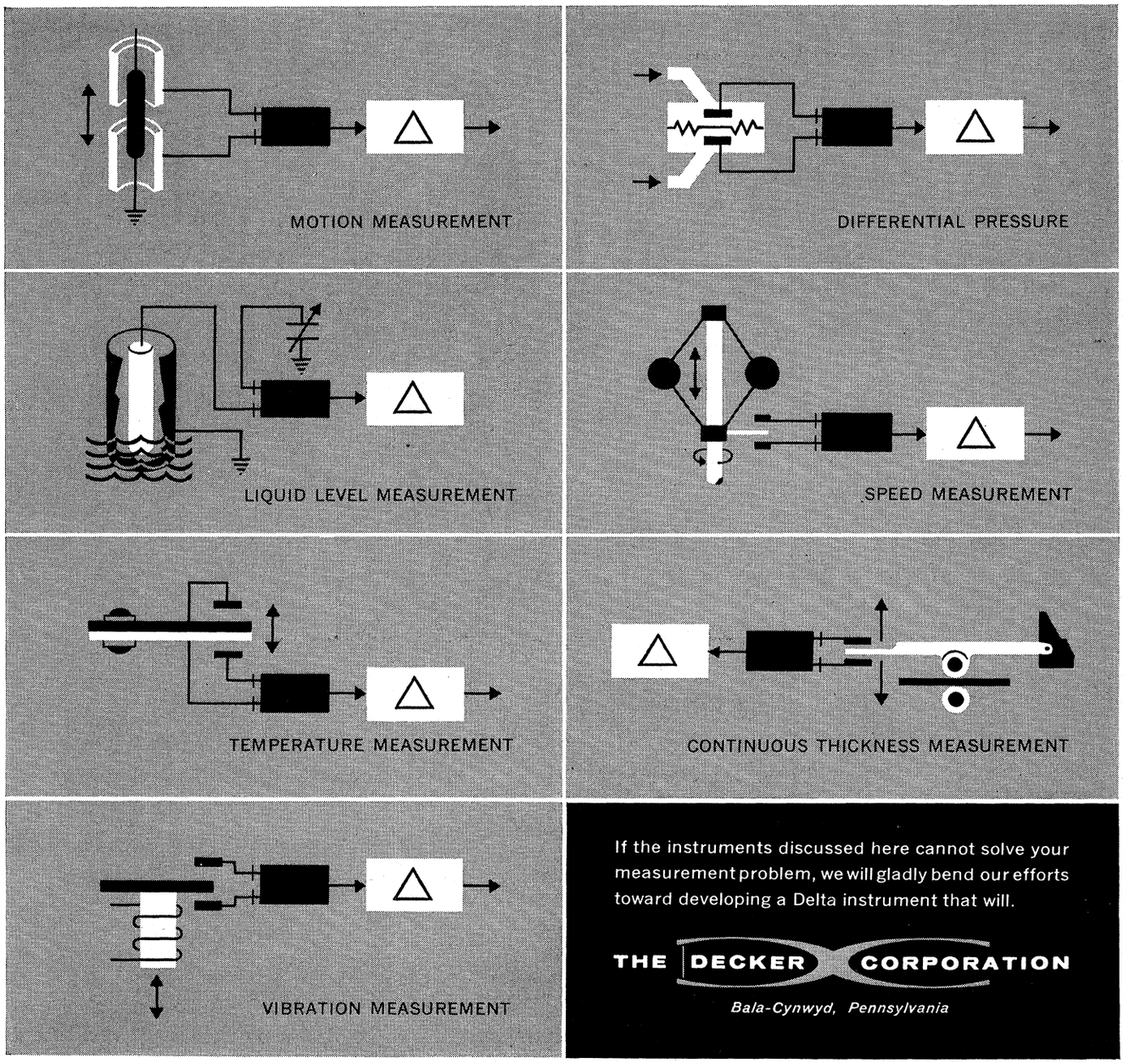

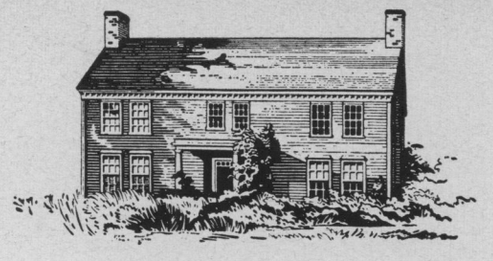

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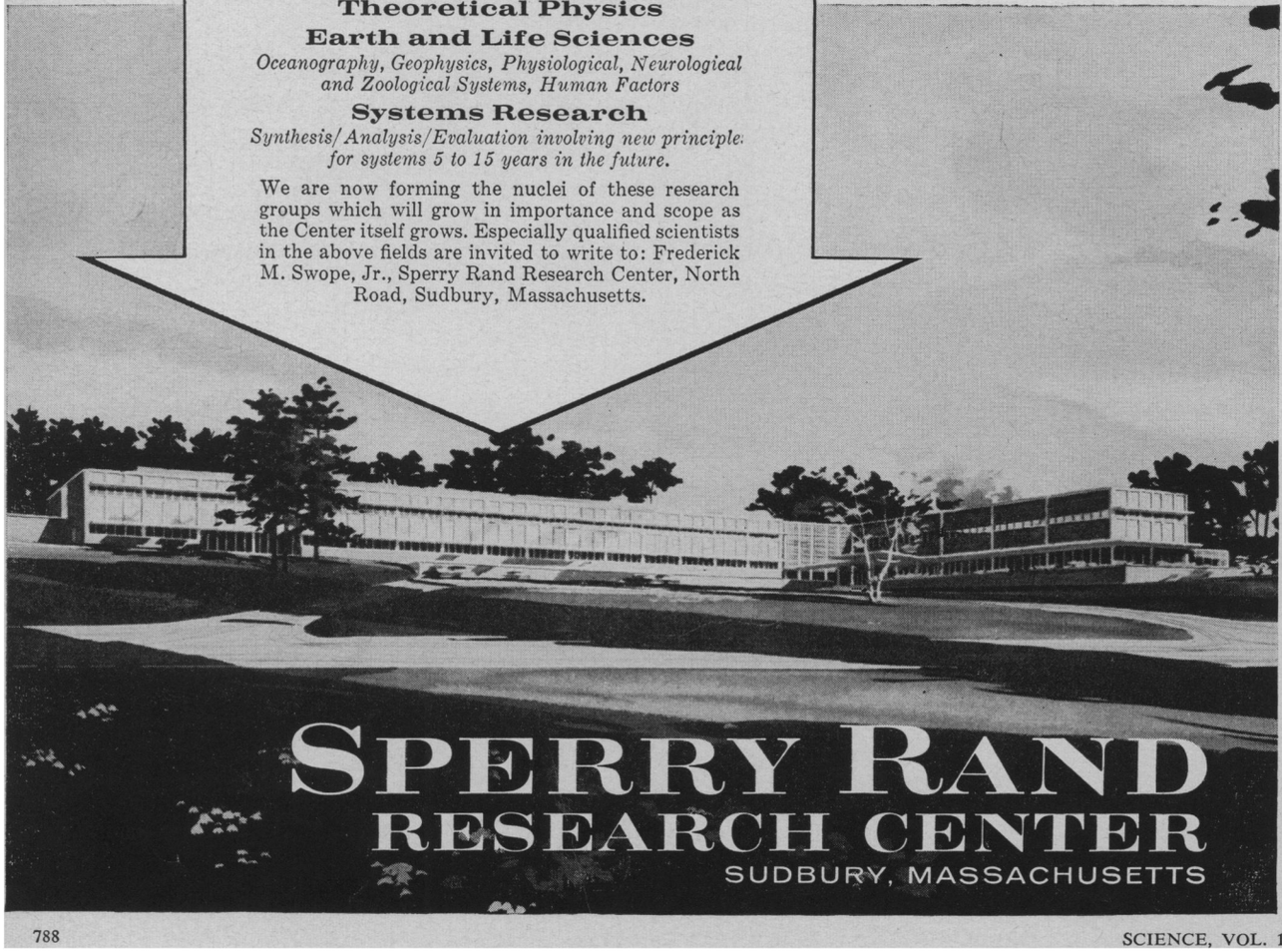

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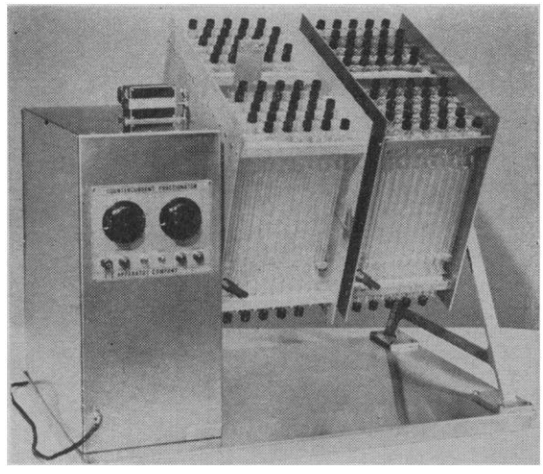

EC520 COUNTERCURRENT FRACTIONATOR with two racks of 100 20-ml tubes on base 24" x 34".

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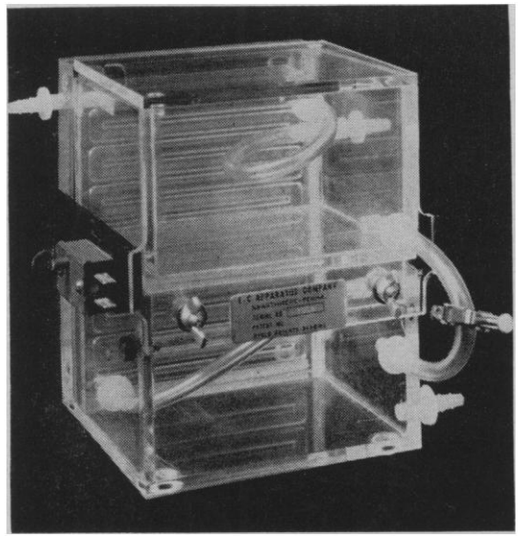
VERTICAL GEL ELECTROPHORESIS



EC470 VERTICAL GEL ELECTROPHORESIS APPARATUS.

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ANALYTICAL and *PREPARATIVE METHODS* of *SEPARATION. GEL ELECTROPHORESIS*

Electrophoresis on gel supporting media affords much improved resolution in comparison with paper-strip or free-solution electrophoresis. As many as 24 distinctly-separated components can be obtained from serum samples. Altho equally suitable for micro quantities, gel electrophoresis also accepts much larger quantities making it a practical method for preparative separations on a semi-micro scale; up to 200 mg. of protein mixture can be separated on one gel slab. If direct-contact cooling of the gel slab is employed, complete serum protein patterns can be obtained in less than two hours, showing better resolution than a 20-hour pattern on paper electrophoresis.

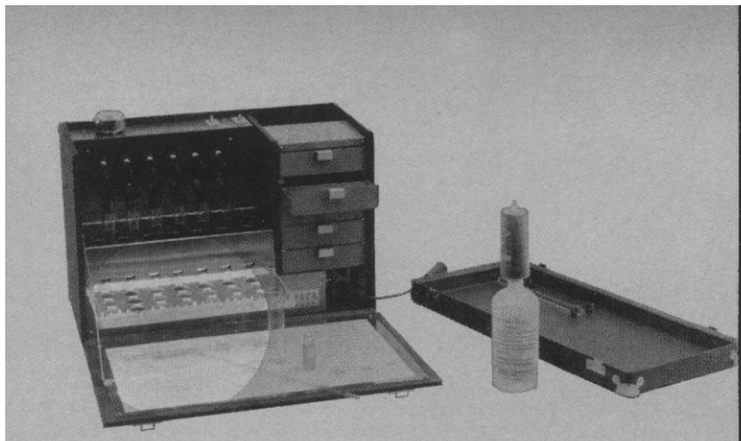
The gel material used as supporting medium may be hydrolyzed starch, agar, or the synthetic polyacrylamide Cyanogum (Reg. American Cyanamid Co.). Each of these media gives a different type of resolution, and in many problems it will be advantageous to employ all three concurrently or successively; the procedure then becomes analogous to two-dimensional chromatography. Migration thru a gel slab in the horizontal position introduces undesirable gravitational-convection effects in the migrating zone; therefore, a vertical gel slab is preferable, providing that a proper supporting apparatus is available.

COUNTERCURRENT DISTRIBUTION

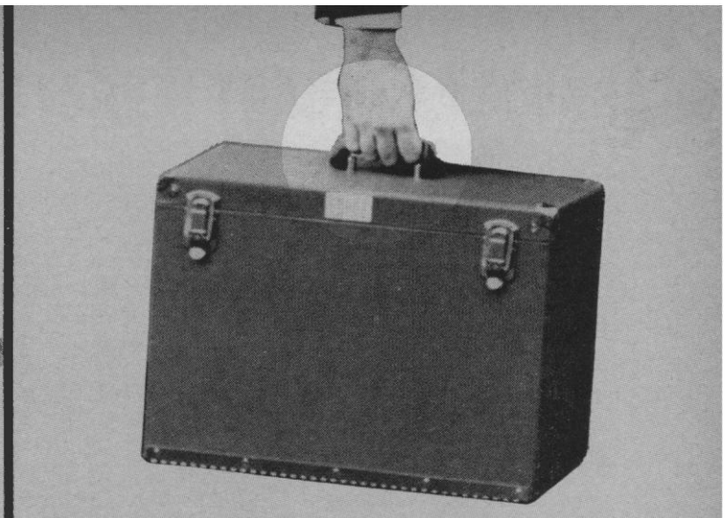
Counter-current extraction is a method of analyzing or separating the components of a mixture by dissolving it in one phase of a two-phase solvent system, and extracting it with the other phase. This effects a relative separation of the components according to the distribution co-efficients of each. Repeating the extraction many times in a systemic manner multiplies the effects of even small differences in distribution co-efficients, so that closely similar substances can be separated. The separated volumes may be analyzed by any applicable method, preferably one which gives the total quantity of solute in each volume. Thus the total volume of solvent may be titrated when dealing with a mixture of acids. The most generally applicable, when non-volatile solutes are concerned, is the determination of the weight of solute in each fraction. It should be noted, however, that macro quantities of material are not required. Extremely low concentrations may be used, which may be far below those required for weight determinations. Necessary, of course, are sensitive analytical methods, such as color reactions or ultraviolet absorption. With such methods, counter-current extraction can analyze quantities as small as those in any other analysis. Results using low concentrations may be even more accurate than those with high concentrations owing to a closer approach to the laws of ideal solutions from which the following equations are derived. Counter-current extraction follows very exactly the Distribution Law

$$Ki = \frac{(i)1}{(i)2} \qquad (1)$$

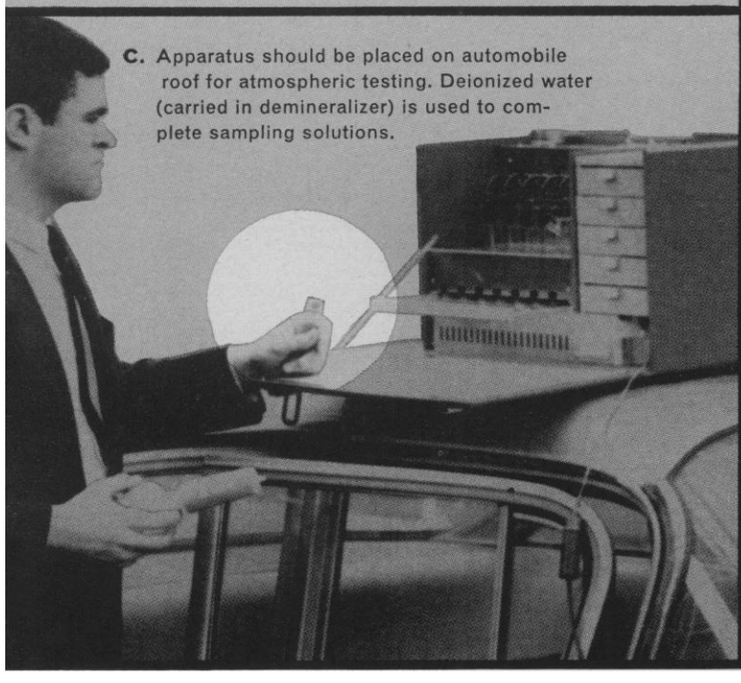
where K_i is a constant characteristic of the compound i; $(i)_1$, and $(i)_2$ are the concentrations of compound i at equilibrium in phase 1 and phase 2 of the solvent system. This fact allows a very mathematical analysis of the quantitative results obtained in counter-current extraction, and permits the direct comparison of experimental and theoretical results.



A. Air Pollution Apparatus ready for survey. Illuminated filter slide permits direct, quantitative determination of gaseous pollutants.



B. Lightweight, compact design makes it easy to transport to test site. Even untrained personnel can make initial studies with the Cenco Test Apparatus.



C. Apparatus should be placed on automobile roof for atmospheric testing. Deionized water (carried in demineralizer) is used to complete sampling solutions.

D. Colorimeter is used to estimate gaseou pollutants. Cenco Field Test Apparatus car test over 18 major pollutants.

NEW AIR POLLUTION FIELD TEST APPARATUS

You can make on-the-spot surveys of 18 major pollutants in just minutes. This new Air Pollution Test Apparatus is ideal for detection where fast accurate information is needed. It can also be used to make initial studies of previously unsurveyed areas prior to the setting up of a monitoring program. Trained personnel are not required for operation. The Field Test Apparatus is portable, lightweight and easy to set up and use. It was developed by Kem-Tech Laboratories, Inc. of Baton Rouge, La., for the U. S. Department of Health, Education and Welfare. For details, write for Booklet 313.

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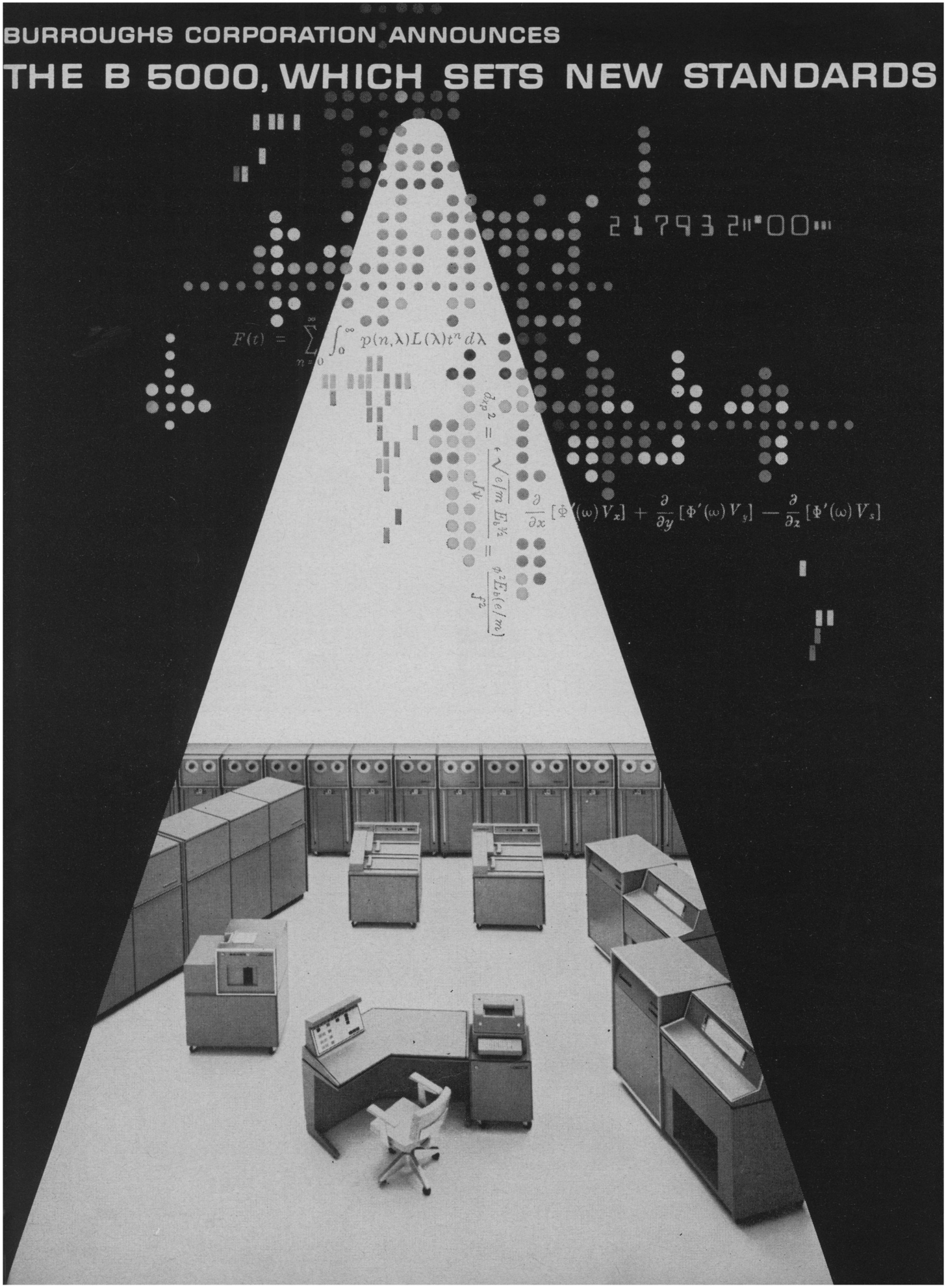
BURROUGHS CORPORATION ANNOUNCES

THE B 5000, WHICH SETS NEW STANDARDS



IN PROBLEM SOLVING & DATA PROCESSING

The new Burroughs B 5000 Information Processing System is a decided departure from conventional computer concepts. It is a problem-oriented system. Its markedly different logic and language are in large part dictated by the characteristics of ALGOL and COBOL. And it incorporates a complete set of operating, monitoring and service routines.

Additional operational features include an average add execution time of three microseconds, and a memory cycle time of six microseconds. Both character- and word-oriented, the B 5000 operates in binary *and* alphanumeric modes; a single set of arithmetic commands operates interchangeably on both fixed-point and floating-point numbers.

More important than these features is the fact that they combine with compiler-oriented logic and language to provide a new concept in computing—an integrated hardware-software system which sets:

NEW STANDARDS OF PROGRAMMING EFFICIENCY

Incorporating logic and language designed to take advantage of modern compiler techniques, the B 5000 permits straightforward, efficient translation of common-language source programs. And it brings a new high in compilation speeds—20 to 50 times faster than those possible on conventional computer systems.

NEW STANDARDS OF AUTOMATIC OPERATION

A Master Control Program, incorporating the automatic operating, monitoring and service routines, is pre-stored on a fast-access drum. It automatically schedules work according to pre-assigned priorities; allocates memory and input/output assignments; and maintains maximum-efficiency use of all components through a comprehensive interrupt system. As a result, human intervention is minimized, system efficiency maximized.

NEW STANDARDS OF PROGRAM-INDEPENDENT MODULARITY

Availability of multiple, functionally independent modules provides the B 5000 with excellent system flexibility and expansibility. The system may include one or two independent processors; up to eight core memory modules with a total capacity of 32,768 48-bit words; and one or two fast-access bulk storage drums, each with a capacity of 32,768 words. Up to four independent input/output channels control a maximum of 26 input/output units, including up to 16 standard-format magnetic tape units. Additional input/output units include card punch and reader, two types of printer, plotter and keyboard.

NEW STANDARDS OF EFFECTIVE MULTI- AND PARALLEL PROCESSING

The Program Independent Modularity of the B 5000, combined with the automatic scheduling and control features of the Master Control Program, permits multi-processing—the B 5000's normal mode of operation. The addition of a second functionally independent processor provides true parallel processing ability.

NEW STANDARDS OF SYSTEM COMMUNICATION

The new B 5000 permits simultaneous on-line/off-line operation. It features completely flexible communication among all of its units. A central processor communicates with all memory units. Any input/output channel communicates with any peripheral equipment and any memory module.

NEW STANDARDS OF THROUGH-PUT PER DOLLAR

All of these B 5000 features combine to provide an important new standard of through-put—the maximum amount of work in the shortest possible time, using the fewest possible components. The result is large-scale performance in the medium-price range.

For details in depth on the B 5000, call our nearby office. Or write for a copy of "The B 5000 Concept" to Data Processing Division, Burroughs Corporation, Detroit 32, Michigan.

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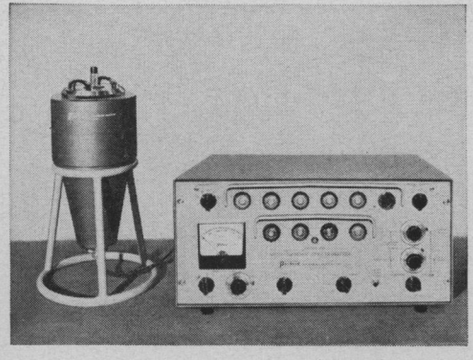

For complete information write for Bulletin 400.

The Auto-Gamma Spectrometer System counts and records data from as many as 100 test tube samples. Operation can be maintained on an around-the-clock basis.

The energy spectrum of an isotope can be plotted with the Auto-Gamma Spectrometer by means of the precise Narrow Window setting. Ordinarily, the photopeak is then counted within the Wide Window of the pulse height analyzer to minimize background. This use of the spectrometer optimizes the count-to-background ratio and permits shorter counting periods or lower tracer levels.

Obviously, automatic sample counting is desirable when large numbers of samples are to be counted. It is just as useful, however, for counting small numbers of low activity samples. Blanks and standards can be arranged among the samples for background checks and calibration. The complete series of tubes can then be counted automatically as many times as required to give desired statistical accuracy.

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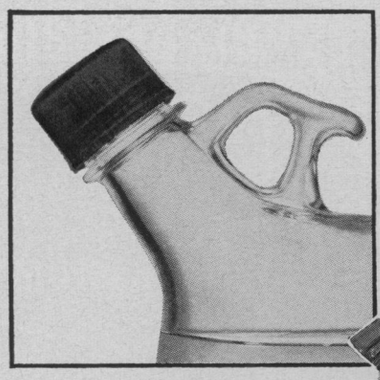

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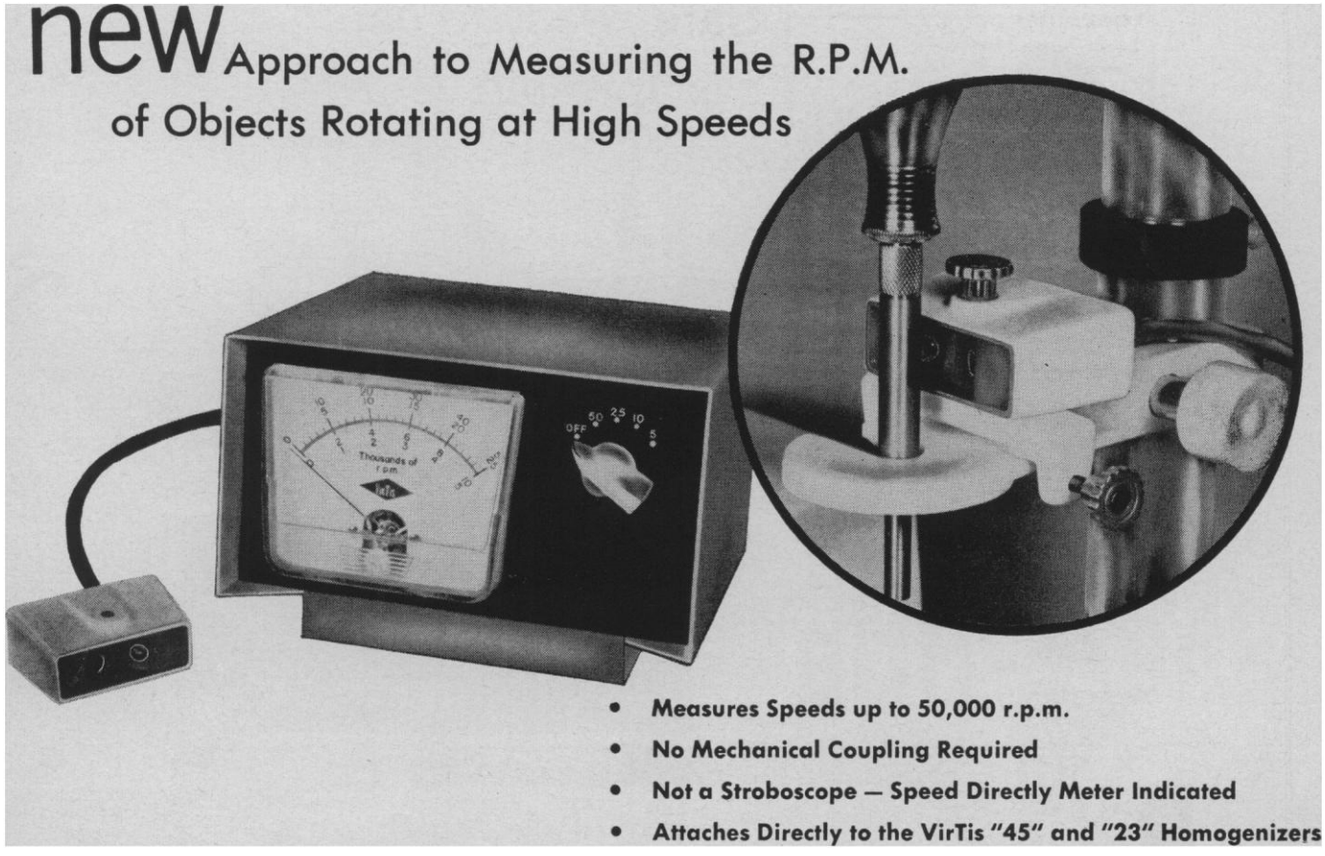
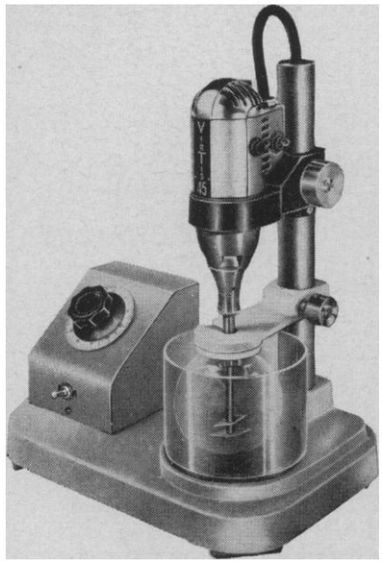
VIRTIS PHOTOCELL TACHOMETER



The VirTis Photocell Tachometer is particularly useful for precise studies with the VirTis "45" and "23" Homogenizers. The compact pick up head clamps directly onto the flask holding clamp on either model. For measuring the speed of other objects in the laboratory, the pick up head can be used as a portable r.p.m. monitor or can be clamped into any position with routine laboratory holders and stands.



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Now a standard in the laboratory, the VirTis "45" provides remarkable efficiency for difficult homogenizing problems. The top speed of 45,000 r.p.m. rapidly reduces samples to a fine particle size. Glass beads and a special chrome plated impeller are available for grinding bacteria. Volumes between 0.2 ml. and 400 ml. are all easily processed in the one instrument.

"23" Homogenizer

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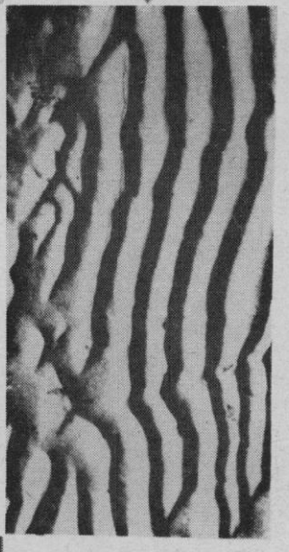

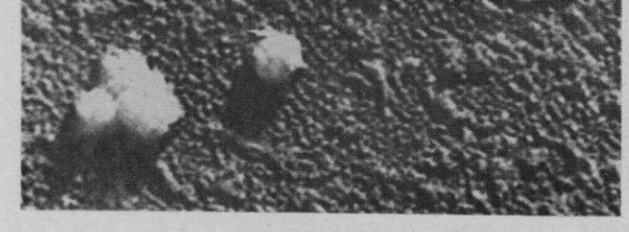

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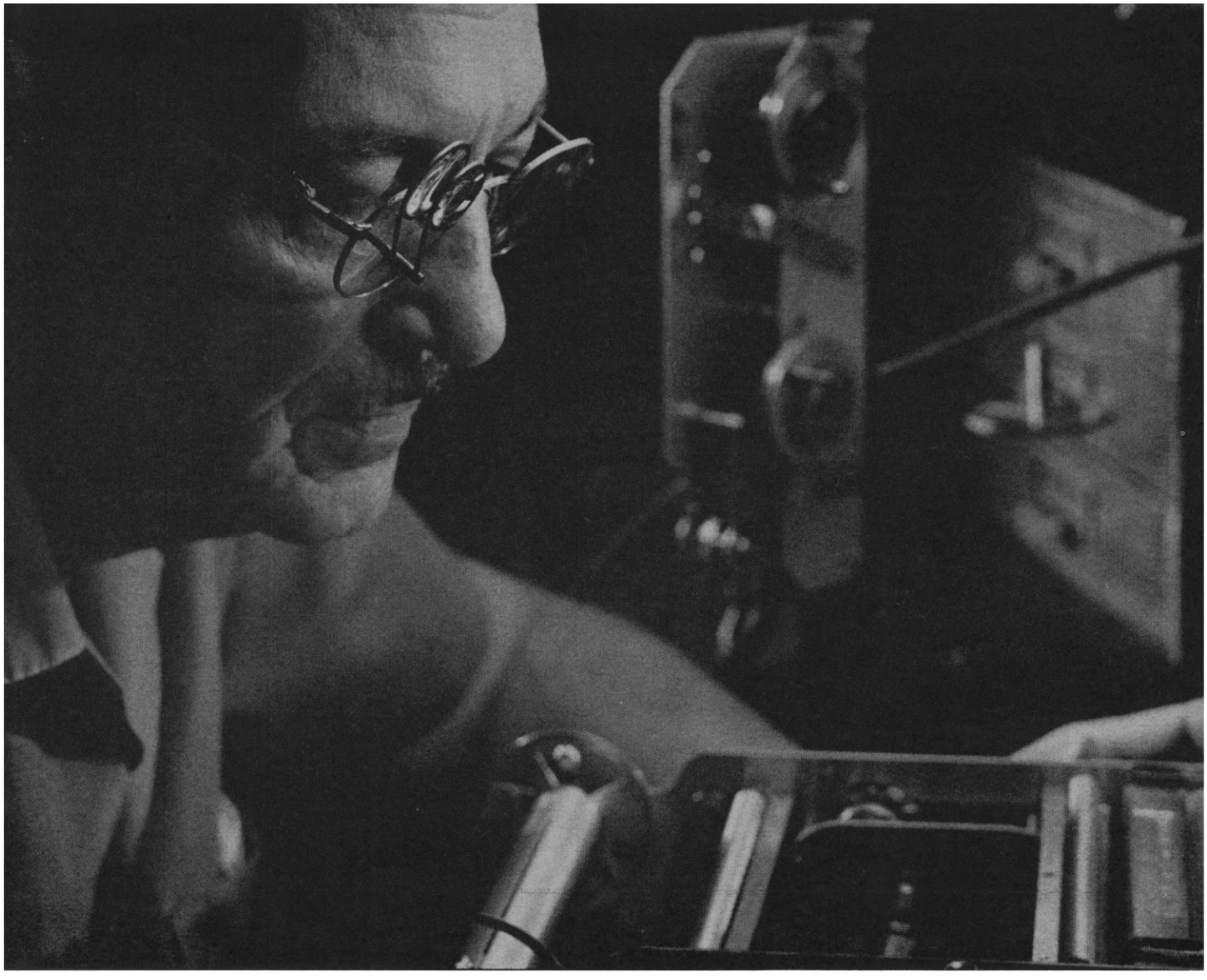

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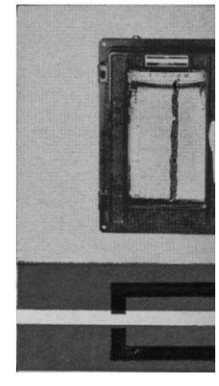

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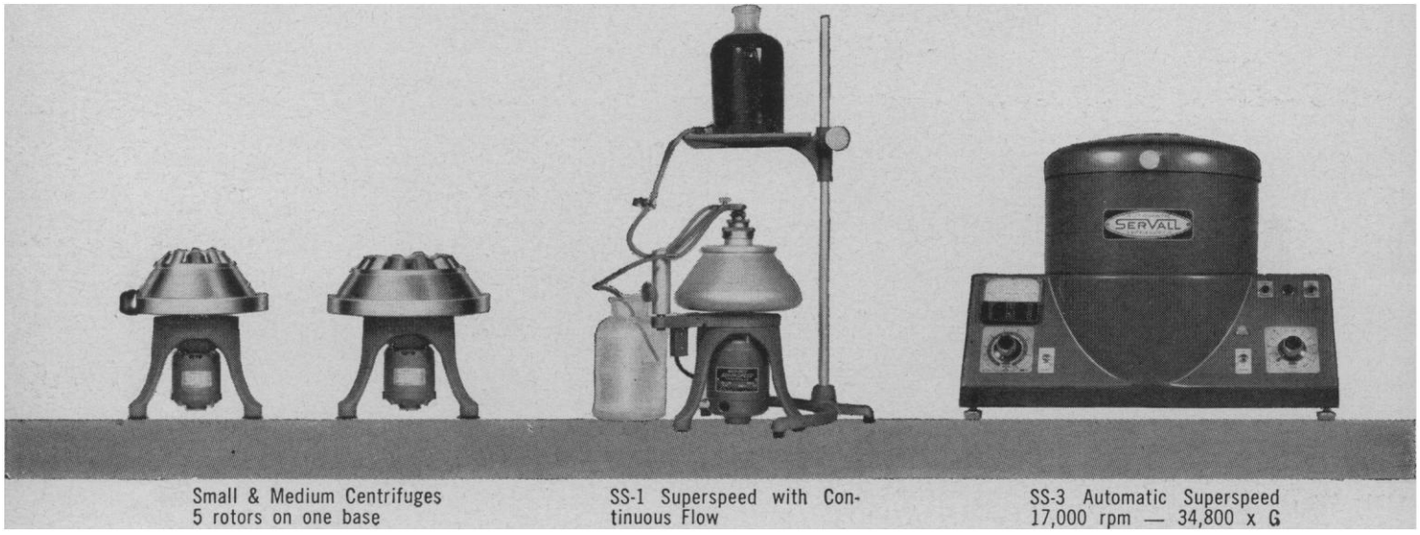

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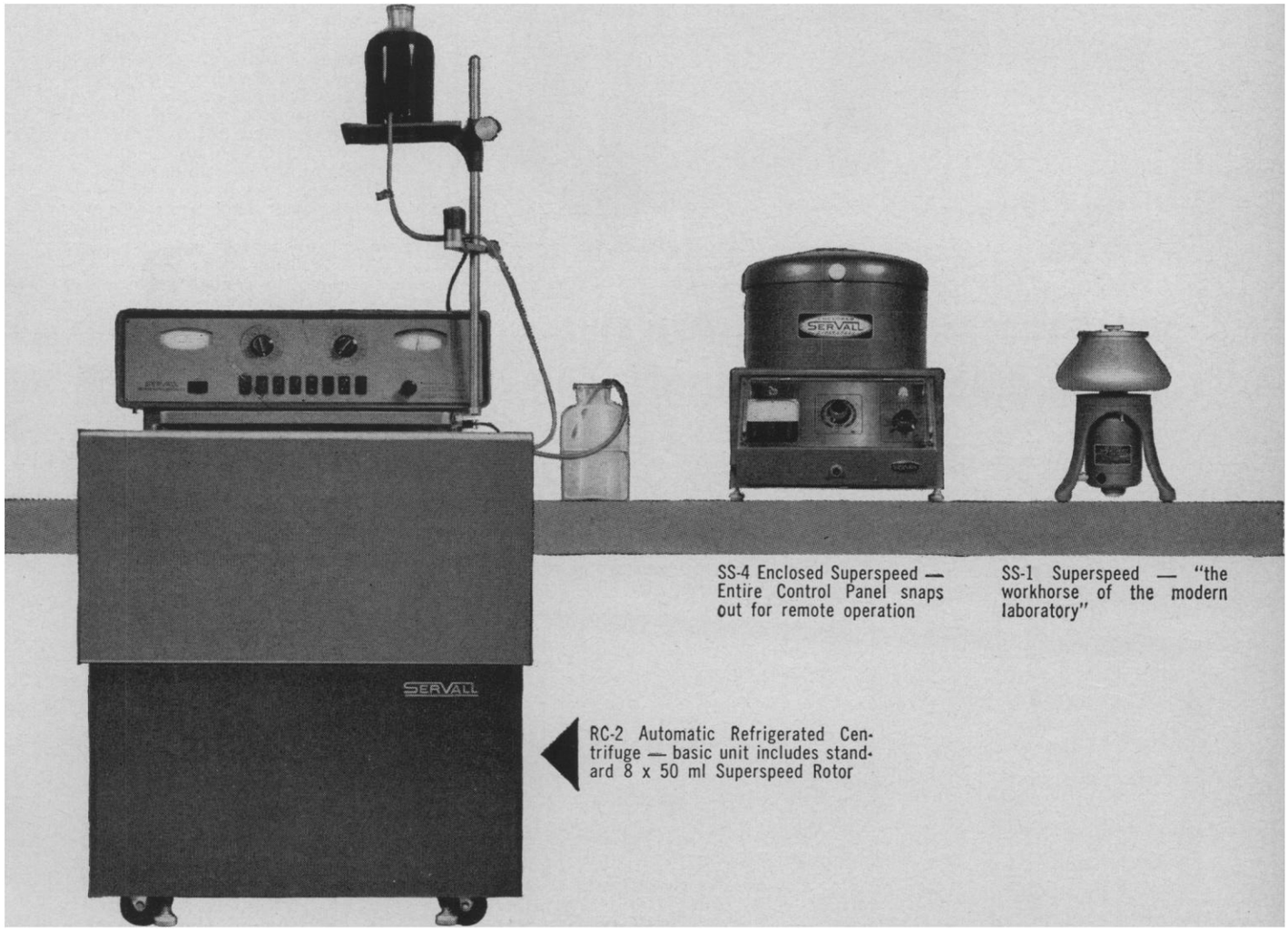

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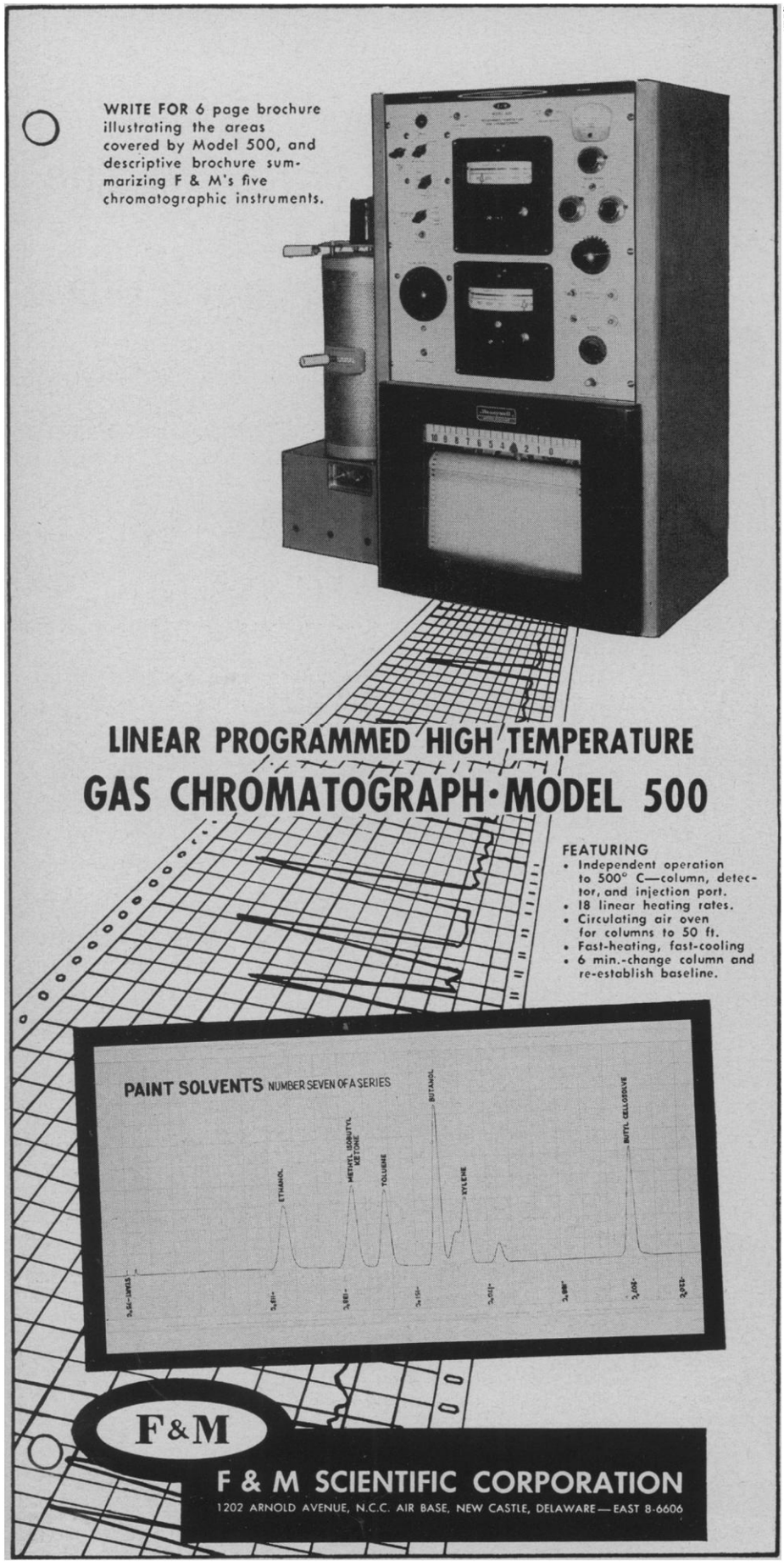

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LITERATURE CITED

(1.) Altschuller, L. W., and Shreve, O. D., "Techniques and Procedures in the Application of Gas Chromatography to Paint Solvent Analysis," presented at Symposium on Gas Chromatography, Third Delaware Valley Regional Meeting, Philadelpbia, February 1960.

(2.) Crippen, R. C., and Emmerling, J., *Am. Paint J. Convention Daily*, pp. 37-41, November 1, 1960.

(3.) Durrett, L. R., *Anal. Chem.*, **31**, 1824-1825 (1959).

(4.) *Martin, A. J., Bennett, C. E., and Martinez, F. W., Jr., "Linear Programmed Temperature Gas Chromatography to 500°C," presented at the Cleveland ACS Meeting, April 1960.

(5.) Scherzinger, R. A., *Off. Digest*, Fed. Soc. for Paint Tech., September 1960.

(6.) Technical Bulletin, SC:60-137, Shell Chemical Company, "The Application of Gas Liquid Chromatography in Surface Coating Research," October 1960.

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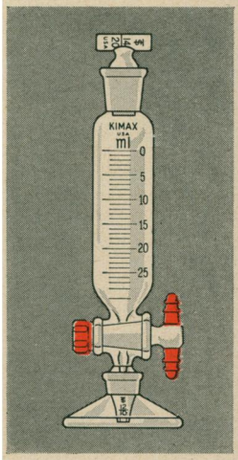

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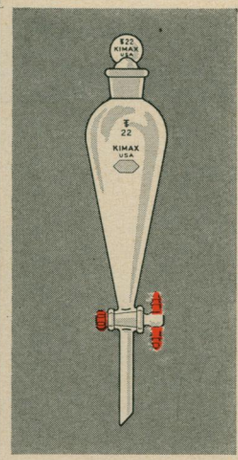

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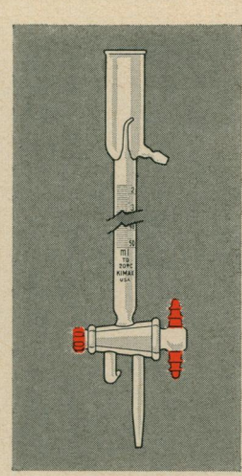

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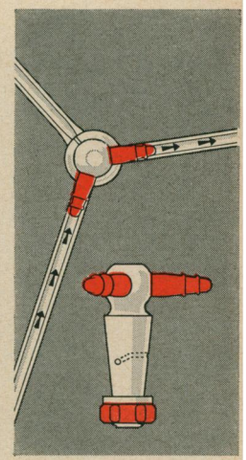

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EXAMPLE

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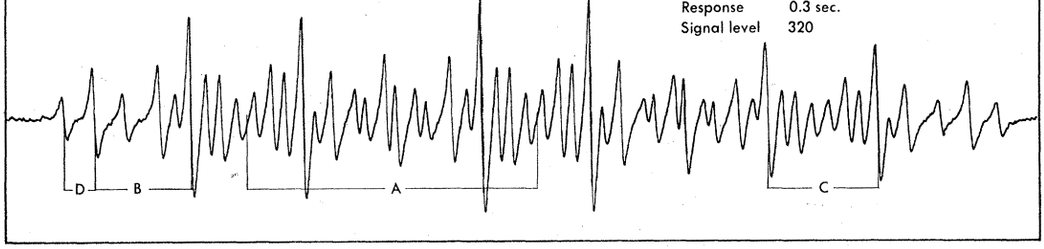

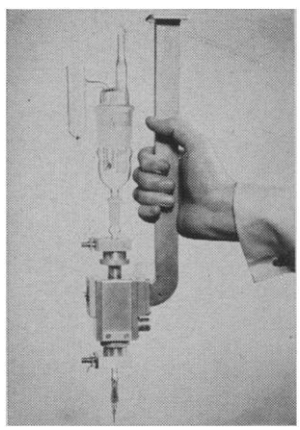

Figure 2 Electrochemical cell as used with the spectrometer

Recently Maki and Geske[1] reported a radically new and important application of EPR. They showed that it was now possible to observe directly the one electron transfer process in the electrolytic reduction of nitrobenzene to the negative ion. They prepared the negative ion by constant potential electrolysis of nitrobenzene in a solution of acetonitrile with tetra-n-propylammonium perchlorate as supporting electrolyte. Such methods of production of negative ions are preferable to the metal reduced systems in that the EPR spectrum can be interpreted completely without complication of interaction by the metal.

Figure 1 shows the spectrum of the nitrobenzene negative ion when generated from a solution of benzonitrile and tetra-n-propylammonium perchlorate[2]. Forming the ion in benzonitrile seems to improve the resolution of the spectrum obtained. The predicted 54 lines are easily observed.

Splitting (A) represents the nitrogen coupling constant which is 10.3 gauss. Splitting (B), (C) and (D) correspond to the coupling constants of the ortho, para and meta hydrogens. The electrochemical cell used in the generation of the negative ions is illustrated in Figure 2 and is a modification of the V-4548 aqueous sample cell accessory.

[1] *JACS 82, 2671 (1960).*
[2] *Sample donated by Dr. R. Adams, University of Kansas.*

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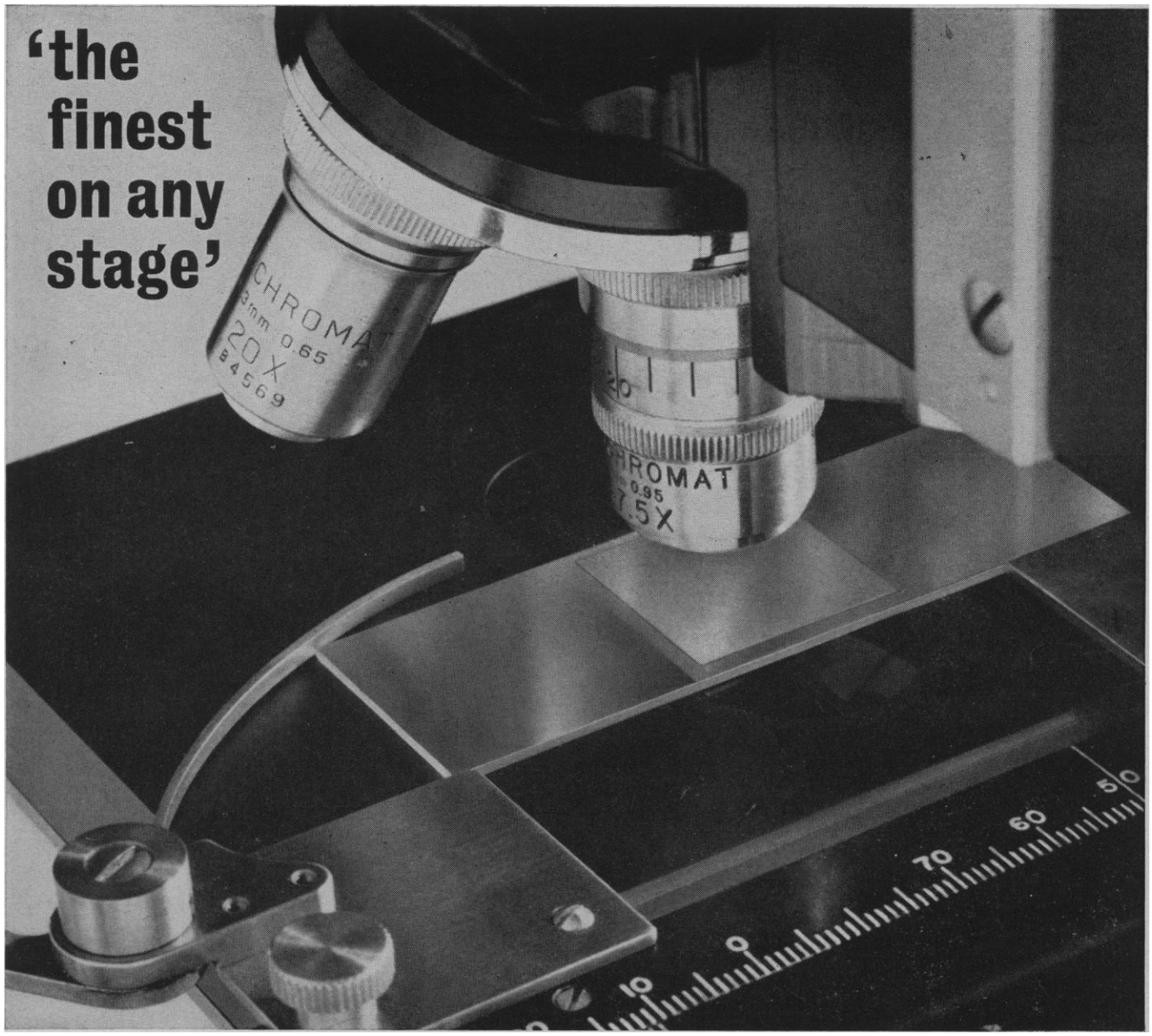

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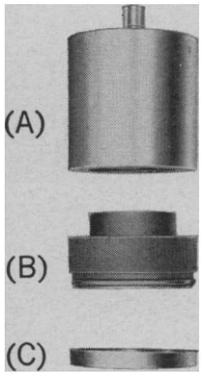

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FD-1	Prop.	G-2	α 10 mv	2200V ± 500	α < 6 c.p.h.	1000V	<2%
			β 1 mv	2300V ± 200	β <12 c.p.m.	400V	<2%
FD-1	Prop.	Pure Methane	α 10 mv	3500V ± 500	α < 6 c.p.h.	1000V	<1%
			β 10 mv	4500V ± 500	β <12 c.p.m.	1000V	<1%
FD-1	Prop.	Natural Gas	α 10 mv	4000V ± 500	α < 6 c.p.h.	1000V	<2%
			β 10 mv	4700V ± 300	β <12 c.p.m.	400V	<2%
FD-2	Prop.	G-2	α 10 mv	2200V ± 500	α <12 c.p.h.	1000V	<2%
			β 1 mv	2300V ± 200	β <30 c.p.m.	400V	<2%
FD-2	Prop.	Pure Methane	α 10 mv	3000V ± 500	α <12 c.p.h.	1000V	<1%
			β 10 mv	4500V ± 500	β <30 c.p.m.	1000V	<1%
FD-2	Prop.	Natural Gas	α 10 mv	4000V ± 500	α <12 c.p.h.	1000V	<2%
			β 10 mv	4700V ± 300	β <30 c.p.m.	600V	<2%

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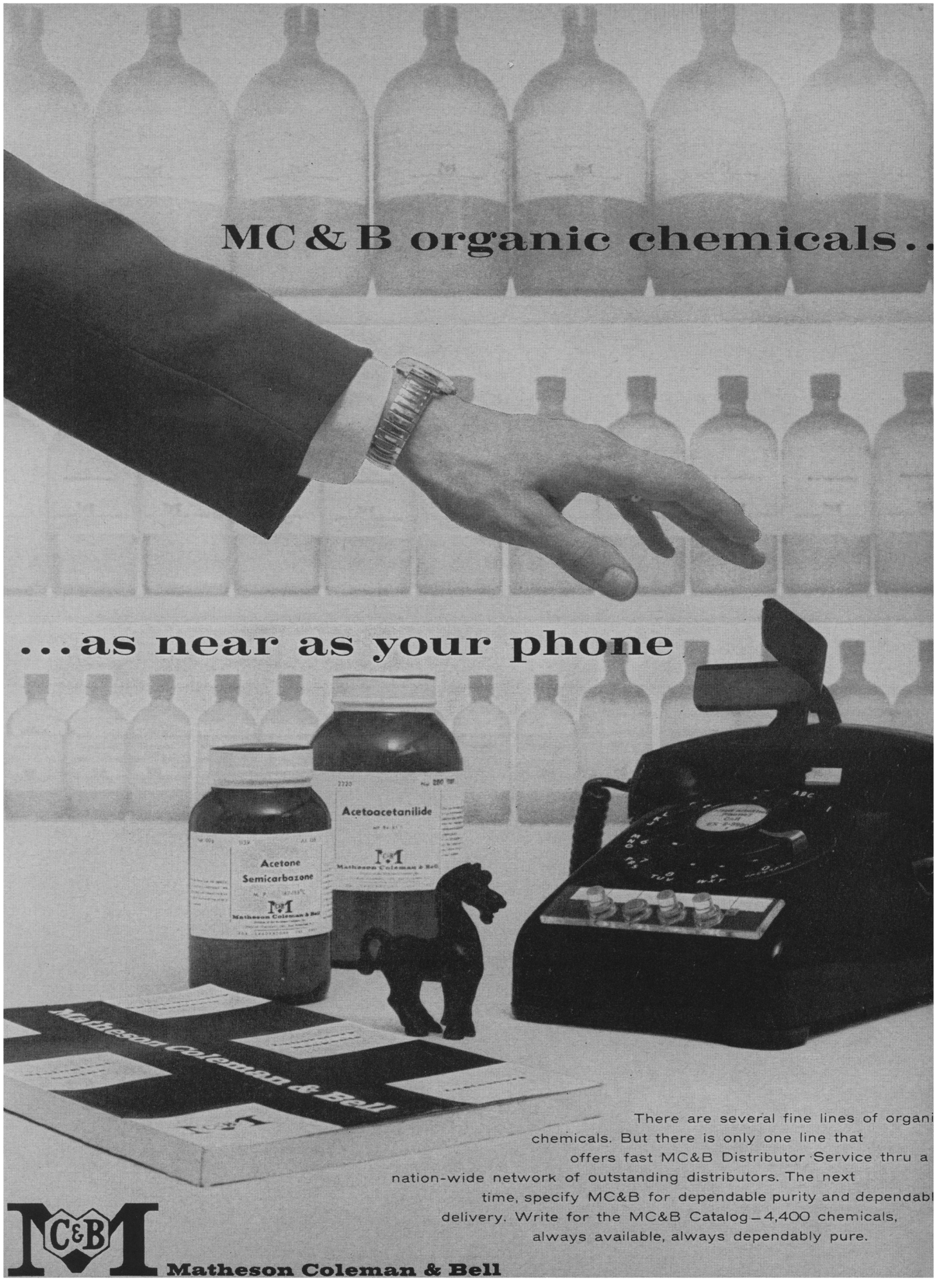
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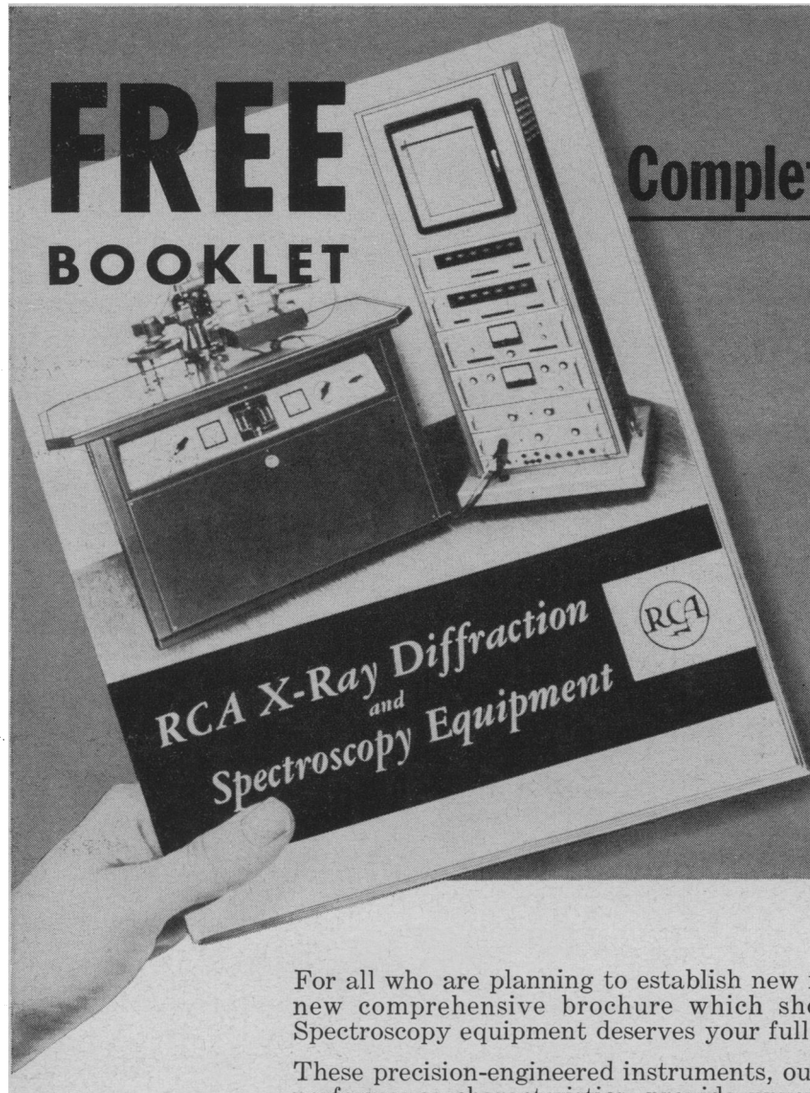

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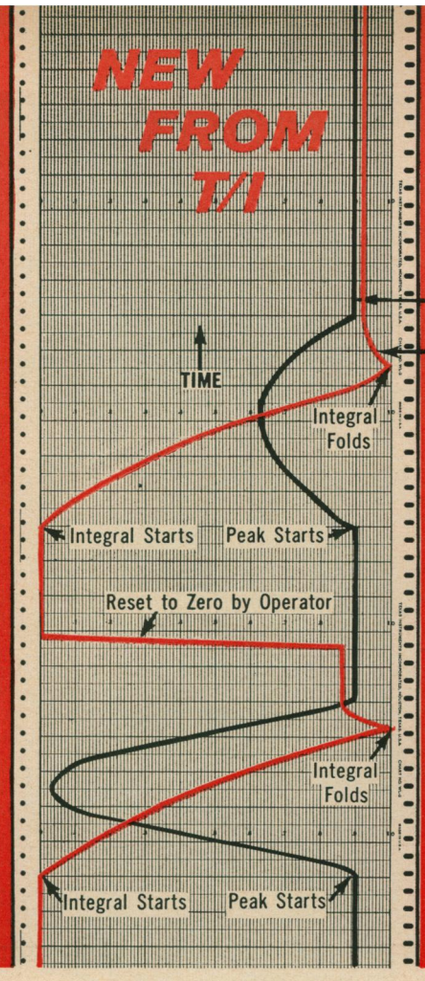

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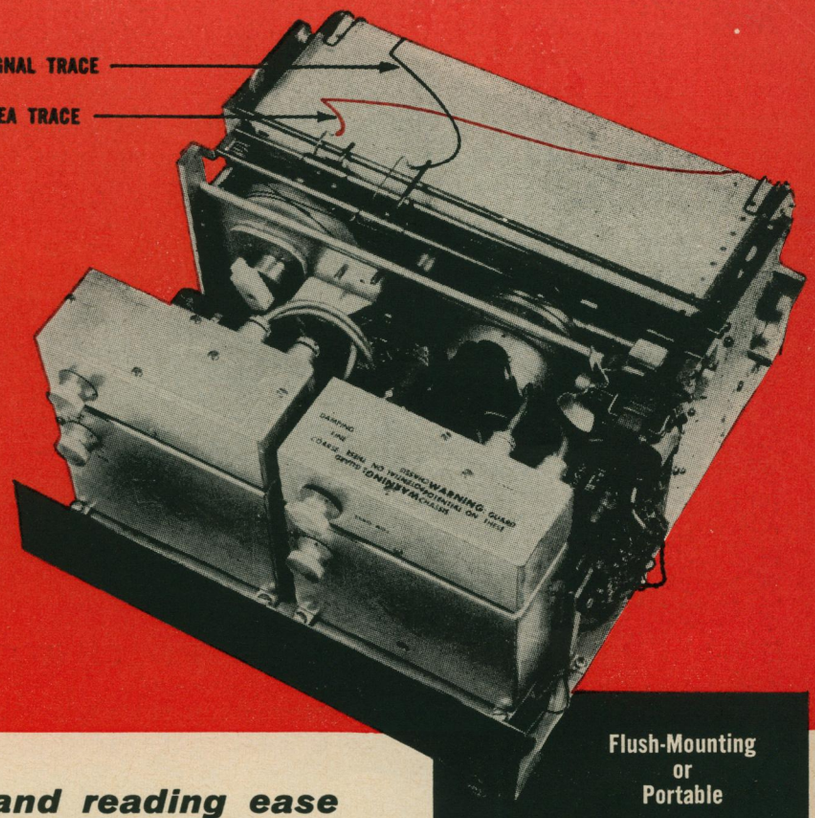

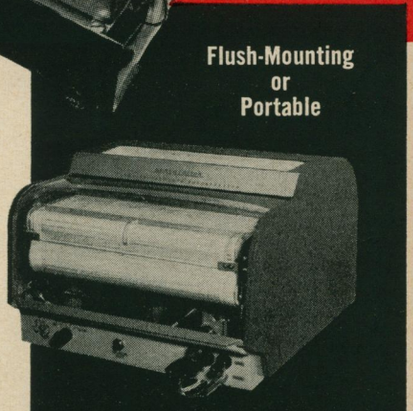

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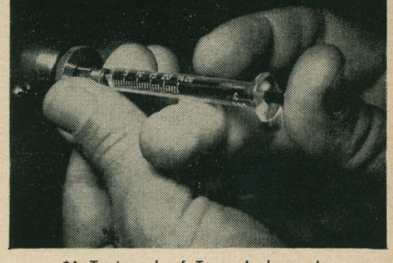

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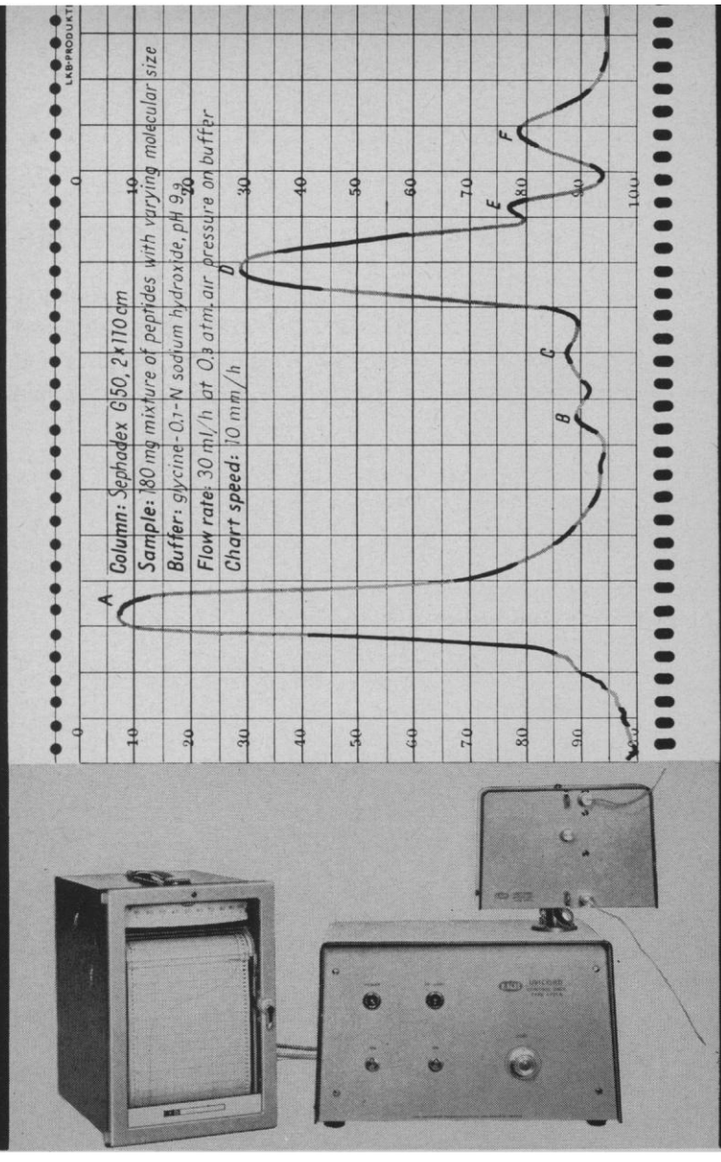

THIS UVICORD DIAGRAM SHOWS:

Peptides released from α-casein through enzymatic degradation by action of Lactobacillus lactis. Six peaks are separated by molecular sieving through a column of SEPHADEX*) G50. Change of test tube in the associated fraction collector appears as a colour change of trace from blue to red or vice versa (grey and black in this reproduction).

Peak	Approx. amount of substance (mg)	Molecular weight
A	50	1.4×10^6
B	3	—
C	4	—
D	39	2.7×10^4
E	12	—
F	11	—

Note:
Molecular weights are calculated from the coefficients of diffusion. Peaks B, C, E and F are too small to permit accurate determination of diffusion coefficients.

**) SEPHADEX is manufactured by AB Pharmacia, Uppsala, Sweden.*

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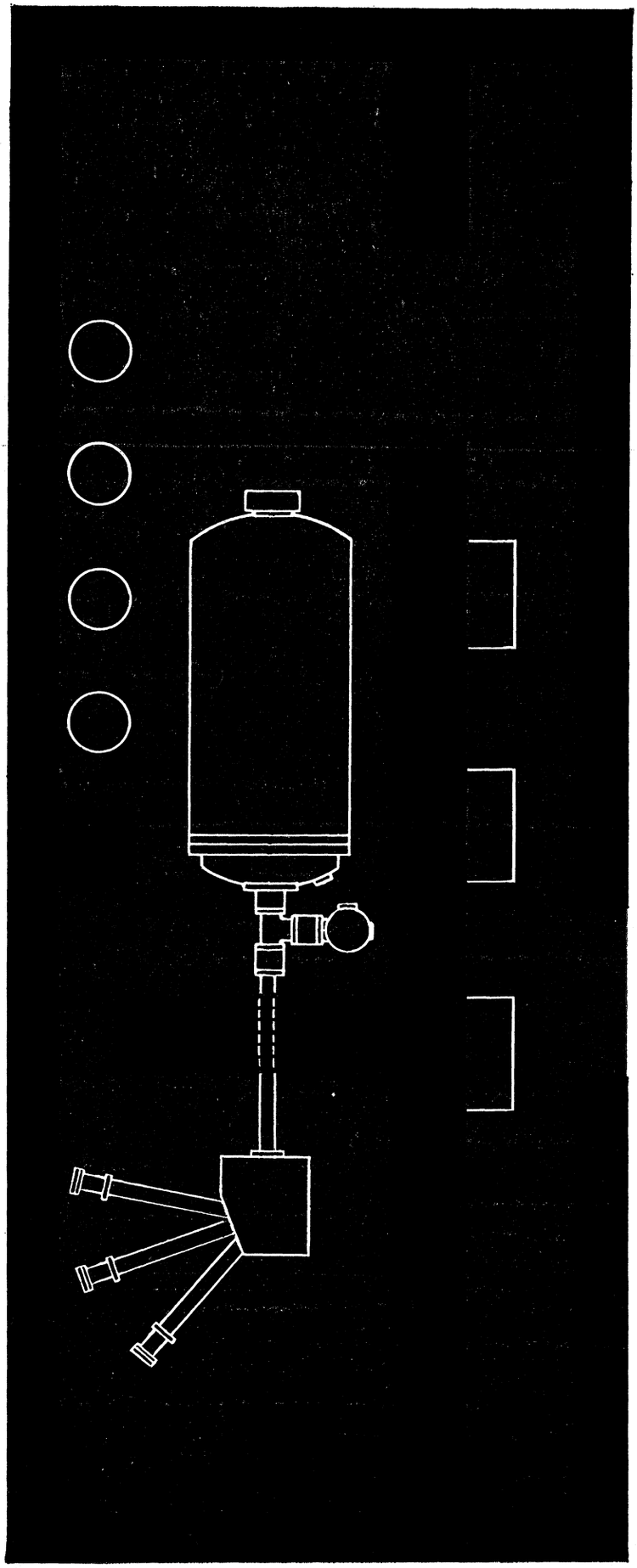

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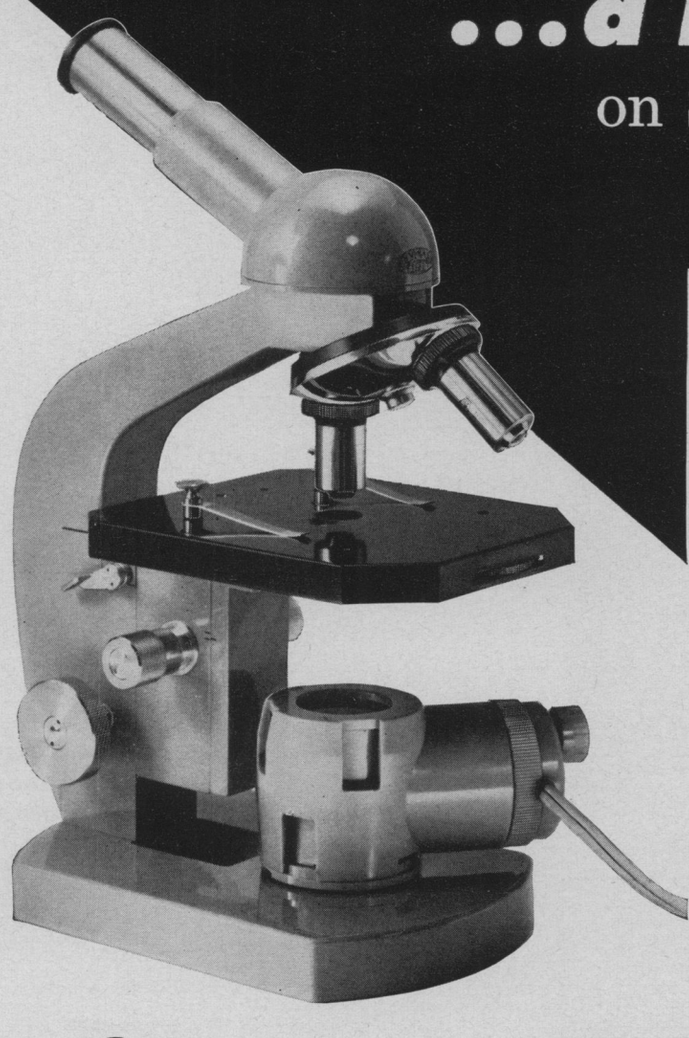

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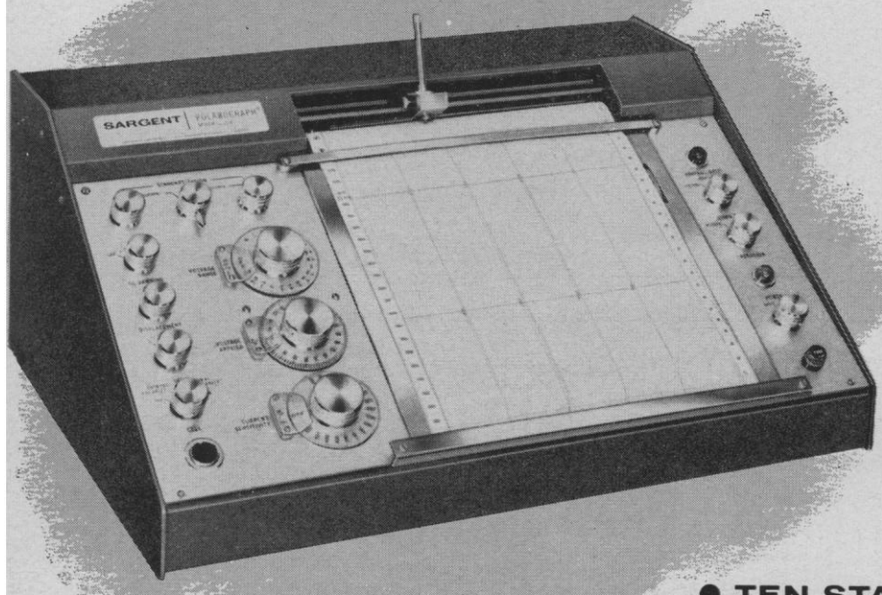

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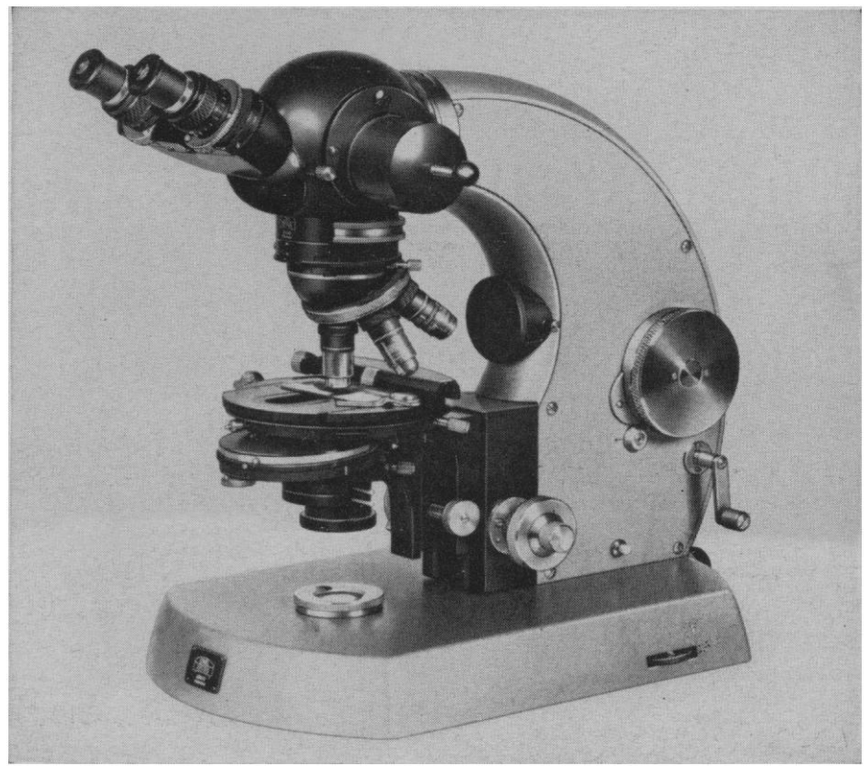

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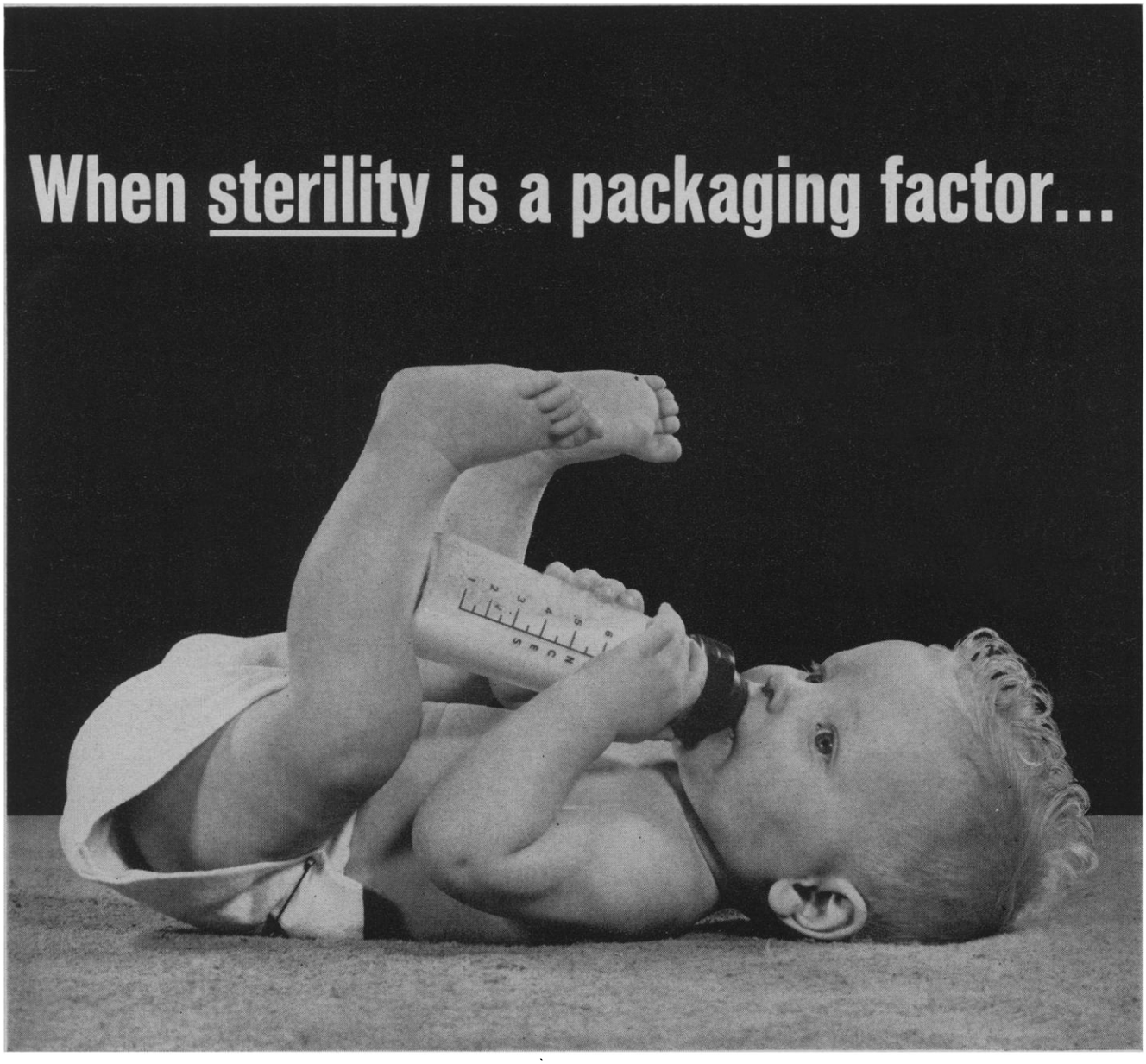

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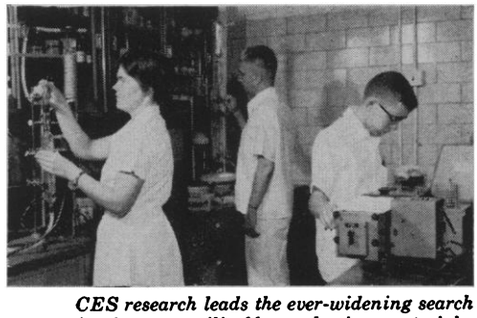

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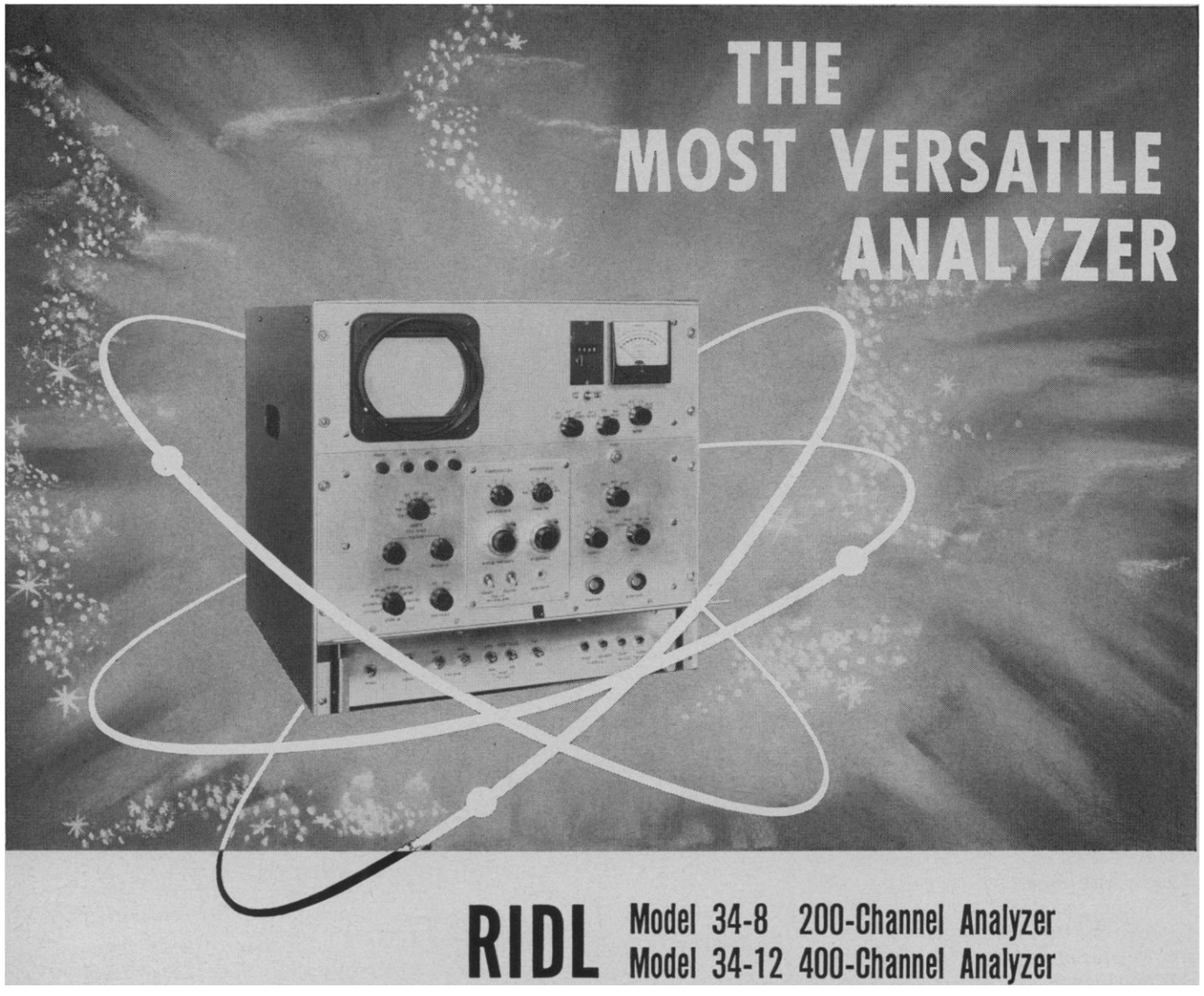

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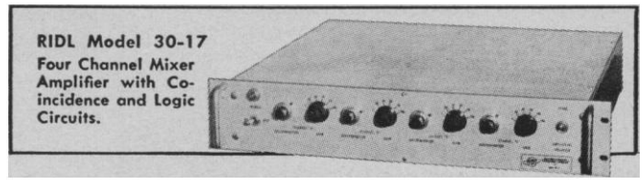
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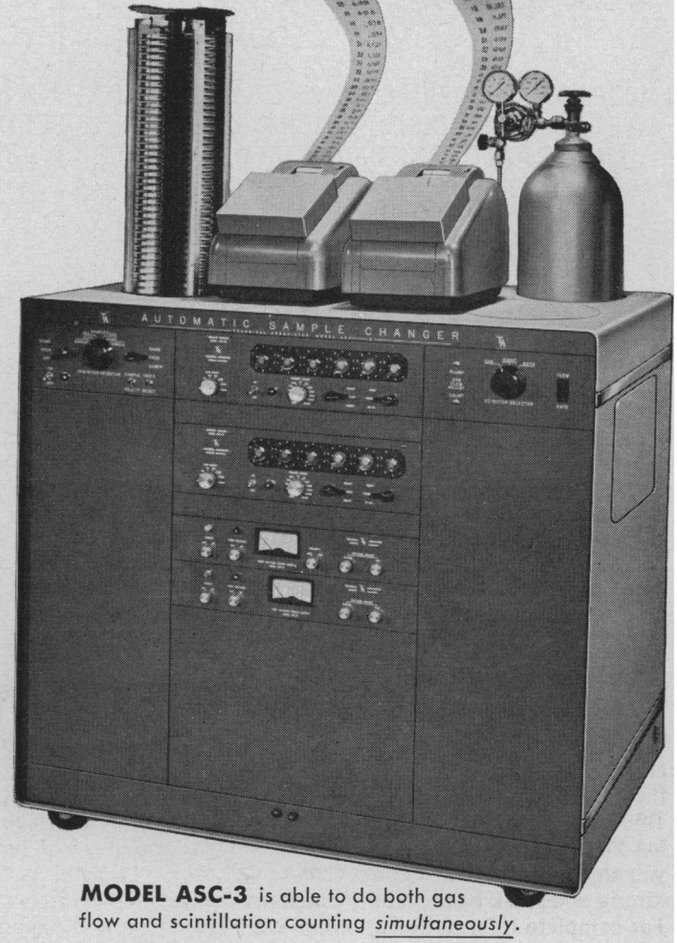

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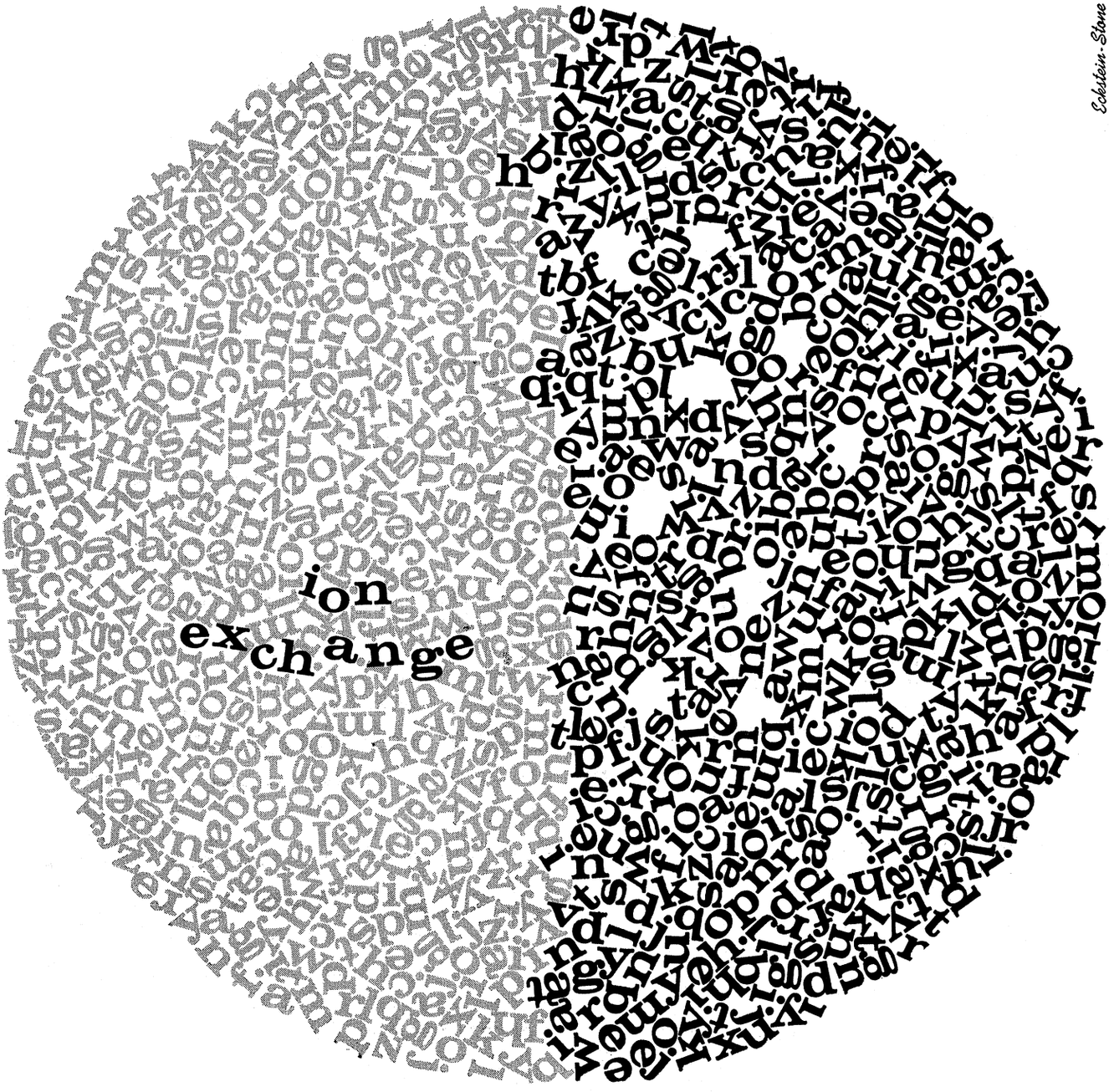

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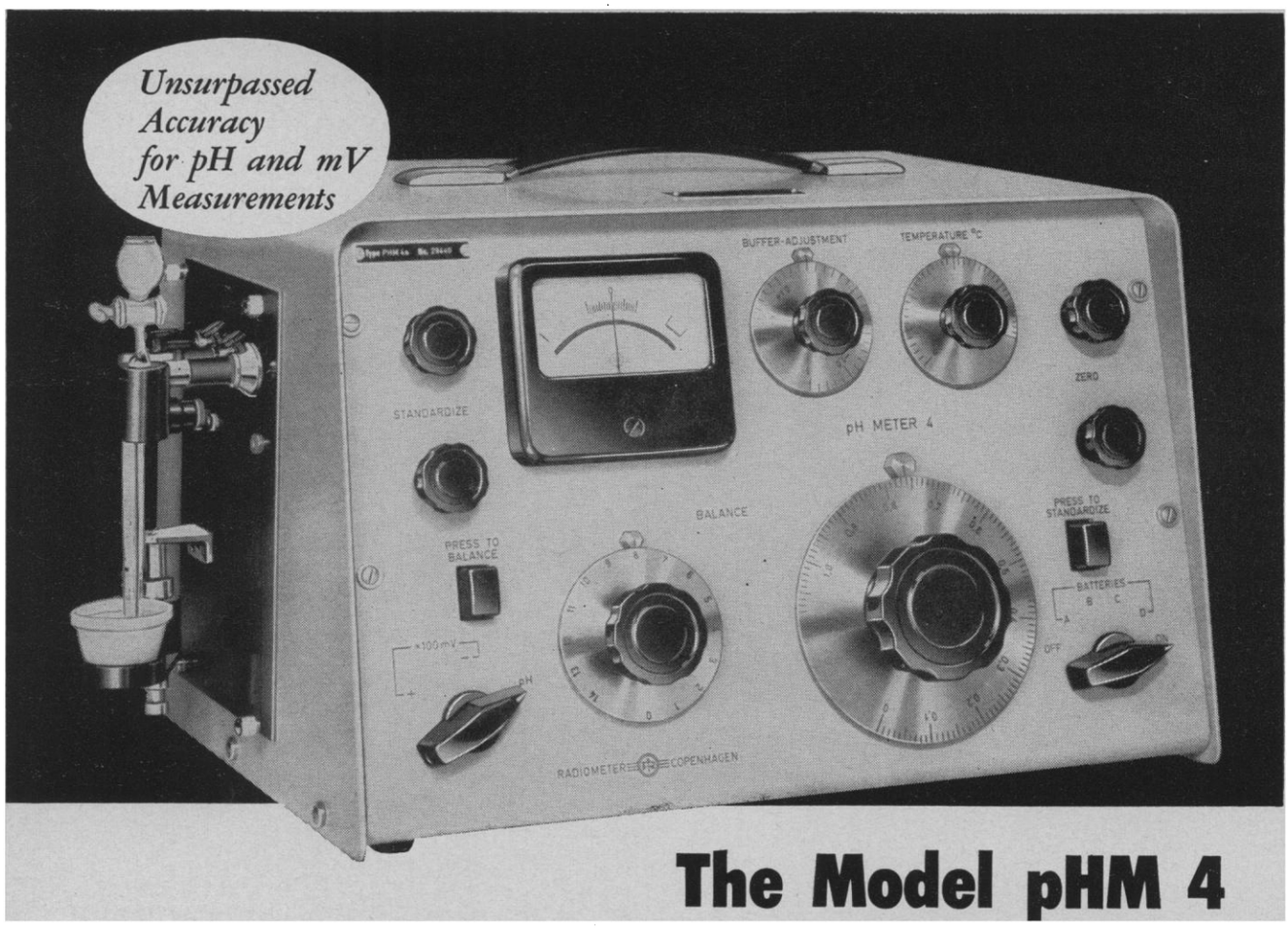

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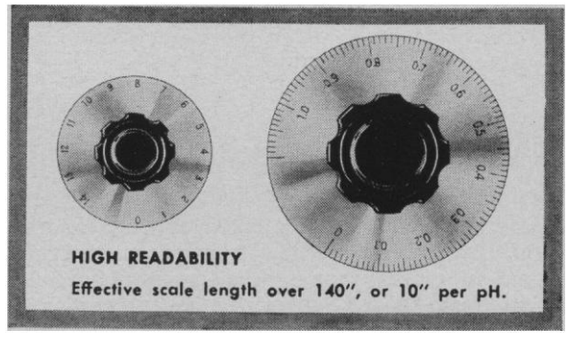

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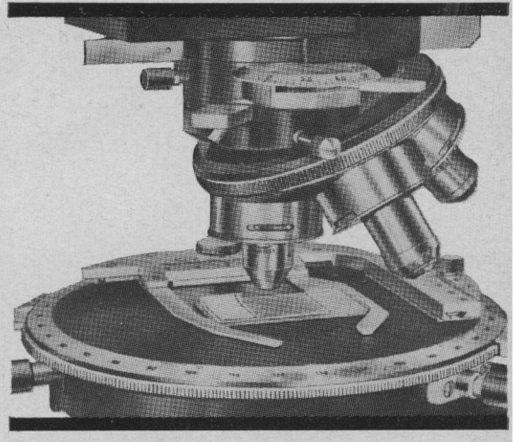

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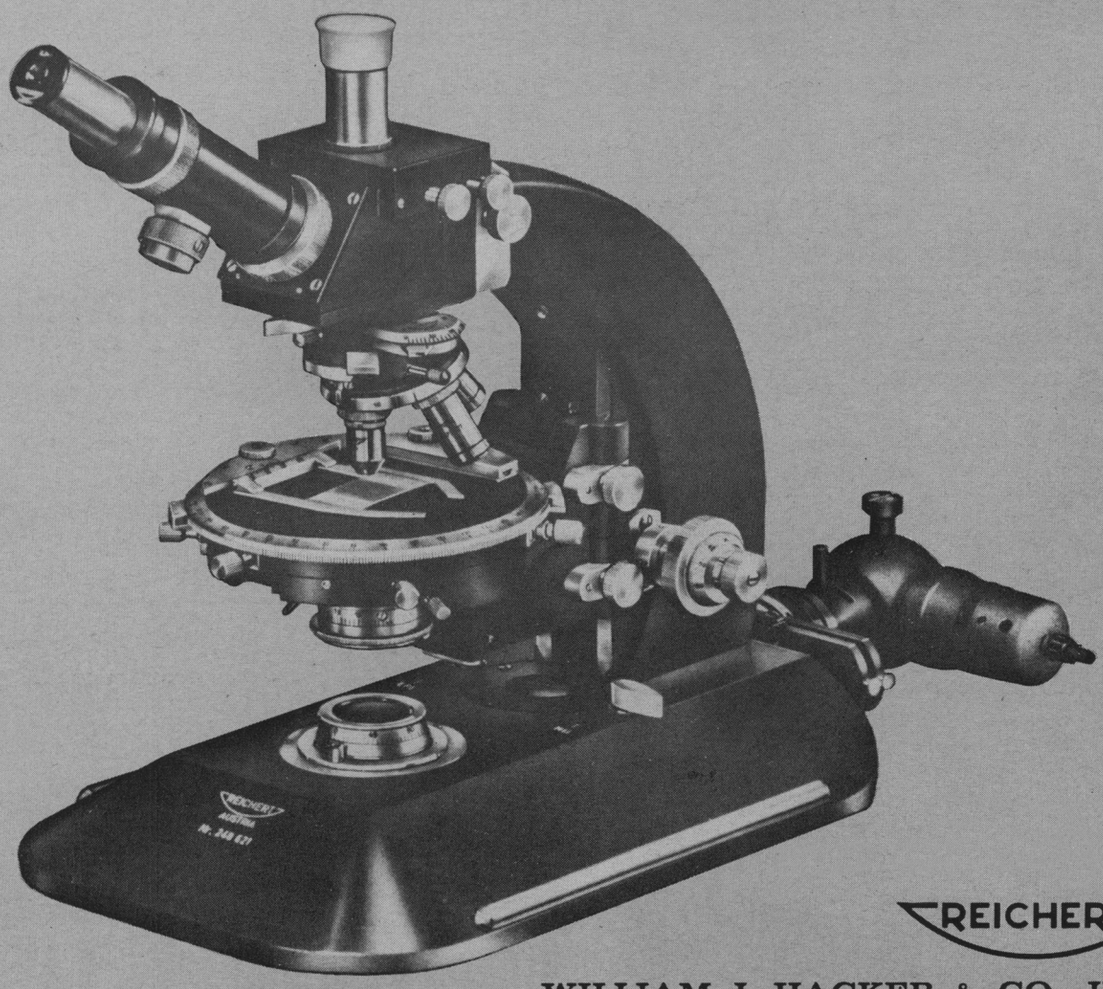

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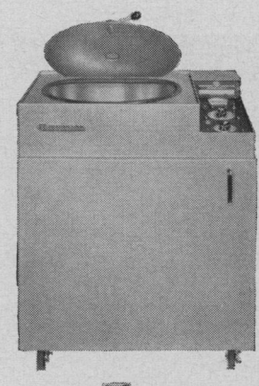

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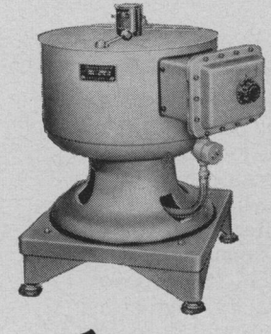
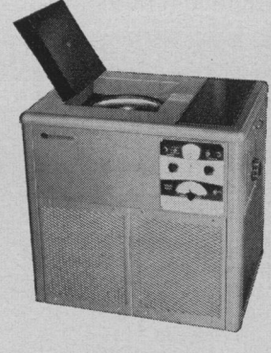
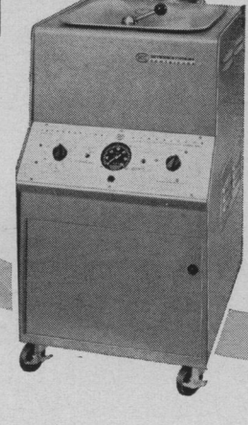
International's Model MB is first choice for implementing the micro-capillary method of blood cell volume testing. With this "Quiet Test" centrifuge, samples spun at 11,500 RPM, are ready for accurate reading in 3 or 4 minutes.





BENCH SIZE LEADER

International's Clinical Model has long been recognized as the most versatile centrifuge in the bench-size class. It swings more than 25 accessory combinations at speeds up to 6700 RPM.



TWO IN ONE!

International's Model CS Combines CM economy and SB speed and versatility in one all-new cabinetized centrifuge. Delivers up to 4,730 × G for routine work: up to 37,950 × G with multi-speed attachment. Swings horizontal, angle and basket heads.

of your next laboratory centrifuge

YOU'RE SURE OF SATISFACTION WHEN YOU CHOOSE INTERNATIONAL!

MOST VERSATILE

International's Model UV is the one model that meets all general-purpose laboratory demands. No other centrifuge in the world today offers a comparable combination of modern design, rugged dependability, wide-range versatility, most-wanted features . . . at such a moderate price.

EXPLOSION-PROOF

International's Model EXD, for use in Class I, Group D hazardous locations, is the only explosion-proof centrifuge listed by Underwriters' Laboratories and the Canadian Standards Association. It combines large capacity, high-speed and exceptional durability.

HIGH SPEED REFRIGERATED

International's Model HR-1 is the centrifuge of choice for high-speed angle separation at forces up to 40,000 × G and controlled temperatures between −20°C. and +10°C. The new Heliflow continuous flow unit, in addition to the four high-speed angle heads, gives the HR-1 unmatched versatility.

WIDE-RANGE REFRIGERATED

International's Model PR-2 gives positive temperature control within 1°C to blood fractionations and similar separations between −20°C and +10°C. Twenty-eight interchangeable heads for capacities between 7ml and 4 liters provide versatility unmatched in the refrigerated centrifuge class.

All eight laboratory centrifuges displayed here bear the IEC trademark... the International symbol of optimum value. No other single manufacturer offers all eight. Yet, these trusted friends of thousands of laboratory directors and technicians are only the highlights of the world's most diversified family of fine centrifuges.

During 59 years of concentrated and progressive research on laboratory centrifuges, International has developed more models, more accessories, more special tooling than all other sources combined.

This specialized pool of knowledge and resources is available to you through an International-trained representative of your authorized International dealer. Whether your centrifuging problems are many or few, his unbiased advice can help you select the versatile or special-purpose International Centrifuge that fits your needs at lowest practical cost.

Before you choose your next laboratory centrifuge, get all the facts from your nearby International dealer or write:

INTERNATIONAL IEC EQUIPMENT CO.

1219 SOLDIERS FIELD ROAD, BOSTON 35, MASS.

ways PICKER NUCLEAR can help you...

in the study of blood clearance of test medication and similar investigations

radioactivity assay of liquid samples automatically *by the hundred*

Picker Nuclear can help you count *automatically* the gamma radiation from large numbers of liquid samples to free your technicians for more important work. Typical samples might be those from blood analyses conducted on a mice colony, from large scale physiology studies, from routine T-3 uptake measurements, etc.

The automatic system can handle 100 samples at a time. Test tube diameter may be 24 mm (for 15 cc samples) or 14 mm (for 5 cc samples). The results of the measurement may be recorded in *counts* and *time* (permitting preset limits on both) or in *elapsed time* (for preset counts). A pulse height analyzer can be included in the system to permit selective measurements of a single radioisotope in a mixture of isotopes.

This Automatic Well Counter is one of the comprehensive Picker line of nuclear instrumentation and supplies: all marketed and serviced through a national network of company offices staffed by trained Picker people. (Picker alone in the nuclear field offers this caliber of local service).

For details call your district office (see 'phone book) or write Picker X-Ray Corporation, 25 South Broadway, White Plains, New York.

Picker nuclear

The automatic counting set-up: the Picker Automatic Well Counter at left, next the Magnascaler and Count and Time Printer.

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AMAZING SCIENCE BUYS for FUN, STUDY or PROFIT

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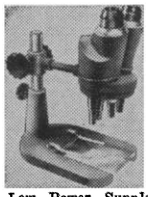

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Years in development. Precision American made. Used for checking, inspecting, small assembly work. Up to 3" working distance. Clear, sharp, erect image. Wide, 3 dimensional field. 2 sets of objectives on rotating turret. 23X and 40X. 10 Days Free Trial.

Stock No. 85,056-W ---------$99.50 f.o.b. Barrington, N.J.

Low Power Supplementary Lens Attachment for above Stereo—provides 15X down to 6X with clear, extra large 1½" field at 6X.

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Photographers! This is an actual photograph of the moon taken through our Astronomical Telescope by a 17-year student.

See the Stars, Moon, Planets Close Up!

3" ASTRONOMICAL REFLECTING TELESCOPE

60 to 180 Power. An unusual Buy! Famous Mt. Palomar Type

You'll see the Rings of Saturn, the fascinating planet Mars, huge craters on the Moon, Star Clusters, Moons of Jupiter in detail. Galaxies! Equatorial mount with lock on both axes. Aluminized and over-coated 3" diameter high-speed f/10 mirror. Telescope comes equipped with a 60X eyepiece and a mounted Barlow Lens, giving you 60 to 180 power. An Optical Finder Telescope, always so essential, is also included. Sturdy, hardwood, portable tripod—**FREE with Scope:—Valuable STAR CHART plus 272 page "HANDBOOK OF HEAVENS" plus "HOW TO USE YOUR TELESCOPE" BOOK.**

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4¼" Reflecting Telescope—up to 255 Power, all-metal pedestal mount.
Stock No. 85,105-W ----------$79.50 F.O.B. Barrington, N.J.

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Real time and labor saver for math teachers. If your tests require graph backgrounds—no need to attach separate sheets of graph paper and worry about keeping them straight. Simply stamp a graph pattern, 3" square as needed on each paper. Grading graph problems then become 100% easier. Stamps are 3" square overall—2 different patterns.

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Stock No. 50,351-W (16 blocks) ----------$3.00 Postpaid
Polar Coordinate Graph Stamp—3" Diam.
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Terrific Buy! American Made!

OPAQUE PROJECTOR

Projects illustrations up to 3" x 3½" and enlarges them to 35" x 30" if screen is 6½ ft. from projector; larger pictures if screen is further away. No film or negatives needed. Projects charts, diagrams, pictures, photos, lettering in full color or black-and-white. Operates on 115 volt, A.C. current. 6-ft. extension cord and plug included. Operates on 60 watt bulb, not included. Size 12" x 8" x 4½" wide. Weight 1 lb., 2 oz. Plastic case with built-in handle.

Stock No. 70,199-W ----------$7.95 Postpaid

CRYSTAL GROWING KIT

Do a crystalography project illustrated with large beautiful crystals you grow yourself. Kit includes the book "Crystals and Crystal Growing" and a generous supply of the chemical you need to grow large display crystals of potassium aluminum sulfate (clear), potassium chromium sulfate (purple), potassium sodium tartrate (clear), nickel sulfate hexahydrate (blue green) or heptahydrate (green), potassium ferricyanide (red), and copper acetate (blue green).

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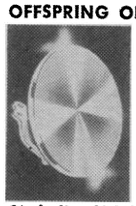

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1" diameter

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ANALOG COMPUTER KIT

Ideal introduction to the increasingly important electronic computer field. For bright students, or anyone interested in this new science. Demonstrates basic analog computing principles—can be used for multiplication, division, powers, roots, log operations, trig problems, physic formulae, electricity and magnetism problems. Easily assembled with screwdriver and pliers. Operates on 2 flashlight batteries. Electric meter and 3 potentiometers are mounted on die-cut box. Answer is indicated on dial. Computer is 20" long, 9" wide, 2" deep.

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'FISH' WITH A MAGNET

Go Treasure Hunting On The Bottom

Great idea! Fascinating fun and sometimes tremendously profitable! Tie a line to our 5-lb. Magnet—drop it overboard in bay, river, lake or ocean. Troll it along the bottom—your "treasure" haul can be outboard motors, anchors, fishing tackle, all kinds of metal valuables. 5-lb. Magnet is war surplus—Alnico V Type—a terrific bargain. Lifts over 125 lbs. on land—much greater weights under water. Order now and try this new sport.

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15½" VISIBLE WOMAN KIT

Astonishing counterpart of the famous VISIBLE MAN. Authentic, exact-scale, laboratory model of female figure lets you look right through transparent "skin"—examine vital organs, glands, nerves, veins, arteries. All parts removable, replaceable. Articulated skeleton permits inspection of 206 bones. Separate group of internal parts shows 7-months pregnancy. Ideal for students, doctors, nurses, hobbyists. Compares favorably with expensive medical school models. Complete with base. Authoritative 12-page Book, INTRODUCTION TO ANATOMY, included.

Stock No. 70,283-W ----------$4.98 Postpaid
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FLASHLIGHT POINTER FOR MOVIE SCREENS

Ideal for pointing out interesting features on movie, slide projection screens. Excellent lecture tool. For teacher use on maps, etc. Flashlight focuses an arrow where you point it.

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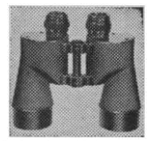
War Surplus American-Made 7x50 Binoculars



Big savings! Brand new! Crystal clear viewing—7 power. Every optical element is coated. An excellent night glass—the size recommended for satellite viewing. Individual eye focus. Exit pupil 7mm. Approx. field at 1,000 yds. is 376 ft. Carrying case included. American 7 x 50's normally cost $195. Our war surplus price saves you real money.

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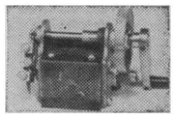

Brand new Signal Corps Electric Generator for scientific experiments, electrical uses, demonstrations. Generates up to 90 volts by turning crank. Use in high impedance relays. Charge ground and bring up night crawlers for bait or study. 2 Alnico Magnets alone worth original price. Wt. 2 lbs. Cost to Govt. $15.

Stock No. 50,225-W ----------$4.95 Postpaid

Same type generator, mounted, with light, as electricity demonstrator.

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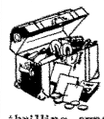

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Science Treasure Chest — Extra-powerful magnets, polarizing filters, compass, one-way-mirror film, prism, diffraction grating, and lots of other items for hundreds of thrilling experiments, plus a Ten-Lens Kit for making telescopes, microscopes, etc. Full instructions included.

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SOIL TESTING KITS

DE LUXE KIT for determining acidity and alkalinity, exisiting levels of nitrogen, phosphorus and potassium in the soil. Includes "Know Your Soil" dictionary, reagents and solutions to make 25 complete soil analyses (100 individual tests) in styrene 5" x 7" x 2" case.

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JUNIOR KIT—Basis for many fascinating experiments regarding growth of plants, etc.

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Take Telephoto Shots Thru 7 x 50 MONOCULAR

This is fine quality, American made instrument—war surplus! Actually ½ of U.S. Govt. 7 x 50 Binocular. Used for general observation both day and night and to take fascinating telephoto shots with your camera. Brand new, $95 value. Due to Japanese competition we close these out at a bargain price. Directions and mounting hints included.

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Invaluable educational aid! Fascinating conversation piece! Skull is anatomically correct—made of natural bone color, bone hard, lifetime plastic. Parts snap together—take apart. Spring-action lower jaw. Removable skull cap for examination of skull cavity, nasal passages, etc. Ideal for scientists, doctors, dentists, teachers, students, artists. Stand and Instruction and Anatomy Chart included.

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Huge selection of lenses, prisms, war surplus optical instruments, parts and accessories. Telescopes, microscopes, binoculars, infrared sniperscope, etc. Low-cost Science and Math Learning and Teaching aids.

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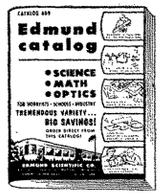

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EDMUND SCIENTIFIC CO. BARRINGTON, NEW JERSEY

The Vanguard Model 1000 VOLUMATIC Fraction Collector

The Vanguard VOLUMATIC is a completely transistorized self-contained unit employing an advanced technique for volumetric collection of chromatographic separations. Hold-up and mixing in volumetrically controlled separations are virtually eliminated when fractionation is performed with the Vanguard VOLUMATIC. Using a unique principle of repetitive cuts for a single separation, in conjunction with a photoelectric sensing device, the VOLUMATIC will collect from one to ten times the siphon volume in each test tube. The operator merely dials the number of times he wishes the siphon to fill and discharge before advancing to the next test tube. Employing this technique for collection of 5 X siphon volume for example, only the hold-up present from the last one-fifth of the first fraction is mixed with the first one-fifth of the second fraction, *an 80% reduction in mixing.*

Transistorization of all components assures absolute reliability of operation and allows continuous cold-room operation without modification.

The cast aluminum instrument cabinet affords the strength and rigidity needed for large columns and ancillary equipment, yet the entire unit weighs less than 50 lbs. Positive indexing of the stainless steel dispensing head to succeeding inner rows is achieved through mechanical gating which assures continued reliability. Compact size (25 in. wide x 30 in. long x 6 in. high) promotes maximum utilization of valuable laboratory and cold-room space. Heavy gauge, large capacity aluminum turntable (245 samples in 13mm. or 15mm. size) is supplied with handle and base-mounted rubber feet for easy removal and use as test tube tray.

Interchangeable turntables for 13mm., 15mm. and 18mm. test tubes are offered as standard accessories. To meet varying requirements a complete selection of siphons is also available. To increase the versatility of the Vanguard VOLUMATIC, transistorized time and drop counting plug-in units are also available.

Complete unit including siphon and turntable of choice with 4 ft. column support rod priced at $695.00, F.O.B. LaGrange, Illinois.

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La Grange, Illinois
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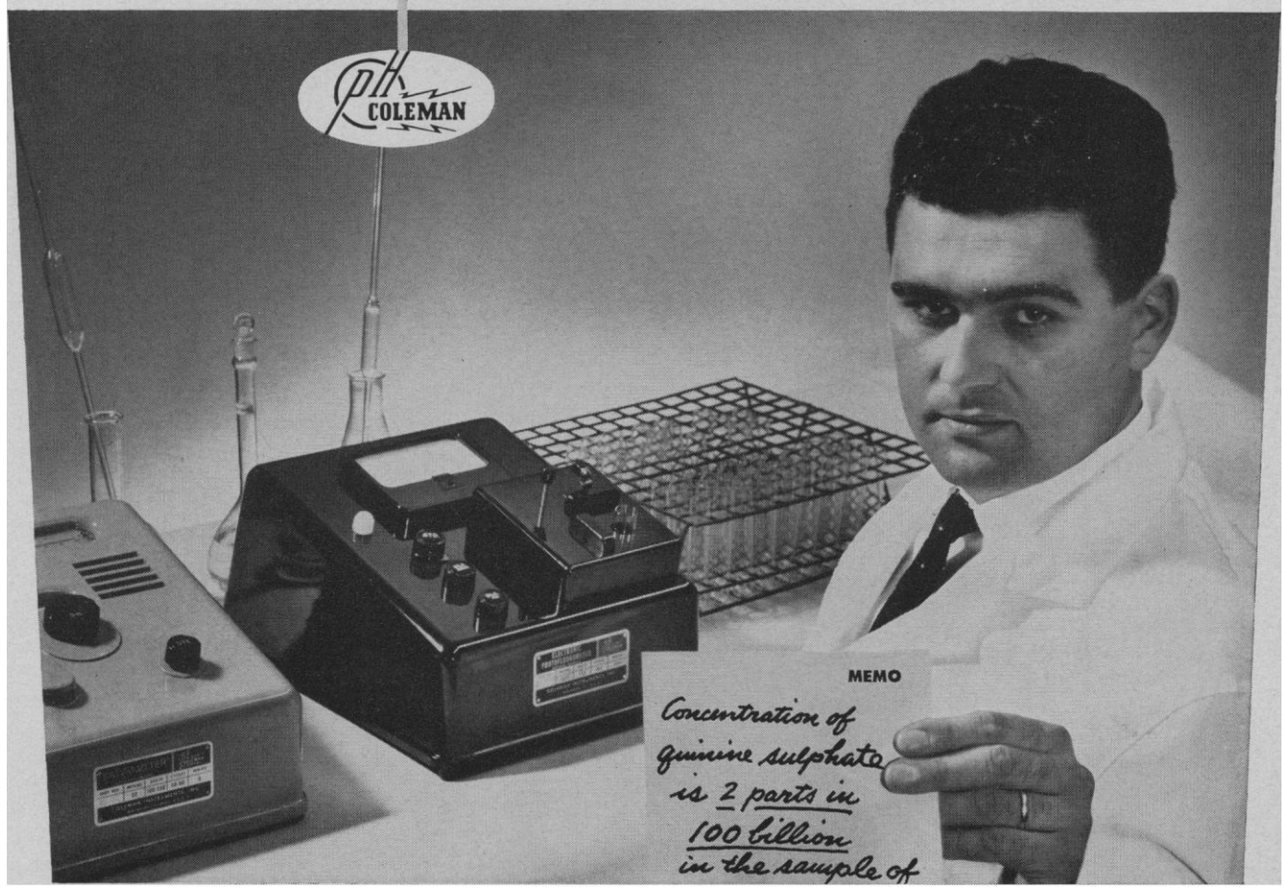
VANGUARD INSTRUMENT COMPANY

COLEMAN



The Coleman Photofluorometer® offers this surprising . . . and usable . . . sensitivity

It's true! The Coleman Photofluorometer® is sensitive enough to determine quinine concentration easily down to 3 parts in 10 billion—and when coupled with a Coleman Galv-O-Meter or Spectrophotometer, its sensitivity can be multiplied *15 times!* Determinations such as that shown above are *well* within its range.

This is one of the great advantages in using a Coleman Photofluorometer®—a tremendous sensitivity range without the use of photomultipliers.

In addition to high sensitivity, the Photofluorometer® remains unsurpassed for stability and ease of operation, with features like these:

Sample is placed directly in the optical system. No doors or carriers to handle.

Beam focuses within sample to eliminate interference by alien light or scratches and stains on the cuvette.

High intensity UV light plus continuous aperture control offer optimum excitation at all times.

Matched cuvettes save time and trouble.

Direct readings from large meter dial.

Eliminates need for expensive and troublesome photo-multiplier.

Decay of sensitive samples prevented by protecting sample from irradiation except during reading.

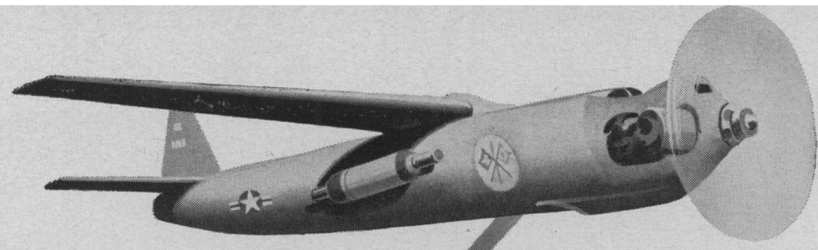
Filters are encased . . . protected from damage and many are pre-grouped for specific determinations.

Drift due to temperature change is prevented by the sample's position in the fan-cooled optical system.

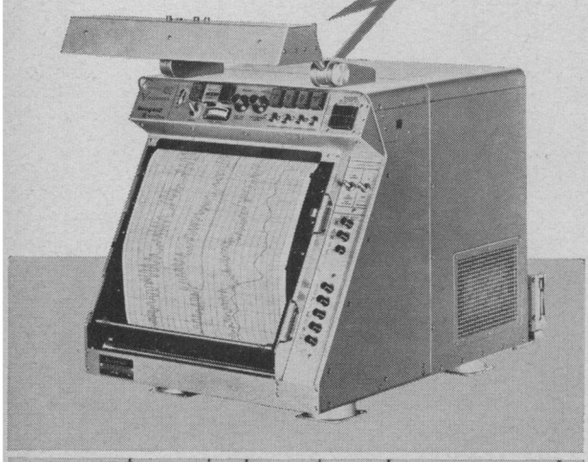
For complete data, ask for Bulletin SB-245A

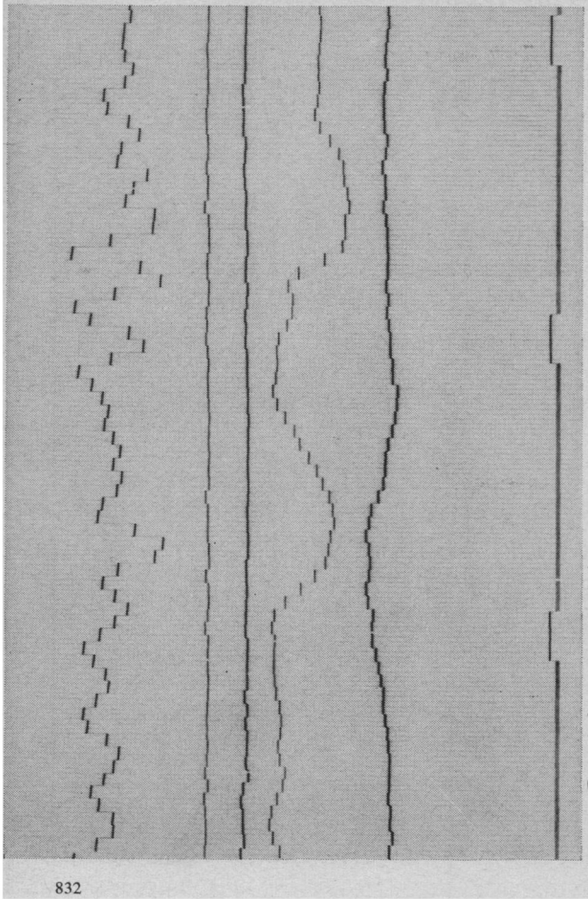
Order and simplification are the first steps toward mastery of any science

COLEMAN INSTRUMENTS, INC., MAYWOOD, ILLINOIS



Tracking a Surveillance Drone with the Visicorder





Record shown ¼ actual size.

Drone surveillance and reconnaissance gives U.S. Army combat units a high-altitude vantage point with much broader horizons from which to view battlefield action and terrain.

If effective use of the data gathered by the drone—the "eye in the sky"—is to be made, accurate instruments have to be on hand to monitor the drone's position and movement, its operational behavior and its response to flight commands. Telemetry supplies the radio link which transmits all this behavior information to a thoroughly-instrumented mobile tactical command post developed by Tele-Dynamics Division of American Bosch Arma Corp.

The Honeywell Model 1012 Visicorder has been selected as the direct readout unit in the Tele-Dynamics Drone Surveillance Telemetry system. In use with its companion instrumentation, the 36-channel Visicorder simultaneously displays the 22 channels of information required to track a drone, plus the timing traces.

In the Tele-Dynamics van, which serves as a tactical command post, the Visicorder provides both an instant "quick look" and a permanent record of the drone's operational parameters.

Signals are transmitted over a single channel by time-multiplexing. Signal and battery strength, engine speed and temperature, pitch and roll commands, altitude, airspeed, attitude (pitch and roll), yaw, acceleration (horizontal and vertical), and angle of attack are recorded by the Visicorder, along with three separate records of vibration.

Like the other units of the Tele-Dynamics system, these Honeywell Visicorders are built for rugged service . . . to deliver the data . . . when the drone is up and the chips are down.

Call your nearest Minneapolis-Honeywell Industrial Sales Office for a demonstration of how a Visicorder Oscillograph will save you time and money in data acquisition. OEM inquiries invited.

Reference Data: write for bulletins 906, 1012, 1108 and 1406.

Minneapolis-Honeywell Regulator Co.
Industrial Products Group, Heiland Division
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HONEYWELL INTERNATIONAL *Sales and Service offices in all principal cities of the world.*

NEW TORSION DIAL BALANCES with WEIGHT LOADERS

Torsion's new line of weight-loading dial balances retains all the proved advantages of the unique Torsion principle which eliminates knife edges and guarantees long-lasting accuracy.

Since Torsion introduced the "fine weighing" dial over a year ago, users have reported substantial savings in weighing time. Now Torsion has added a "weight-loading" dial which enables the user to "dial in" additional weights as described in the specifications for each new balance.

Both dials can be used without arresting the balance.

By using two dials, one for weight loading and one for fine weighing, Torsion has cut weighing time even more.

With Torsion's new two-dial feature, the time-consuming handling of small, loose weights has been eliminated. In addition to faster weighing, Torsion's new dial balances with weight loaders minimize the possibility of weights becoming inaccurate from rough handling.

Ask your laboratory supply salesman for a demonstration or write for complete specifications.

The Torsion Balance Company

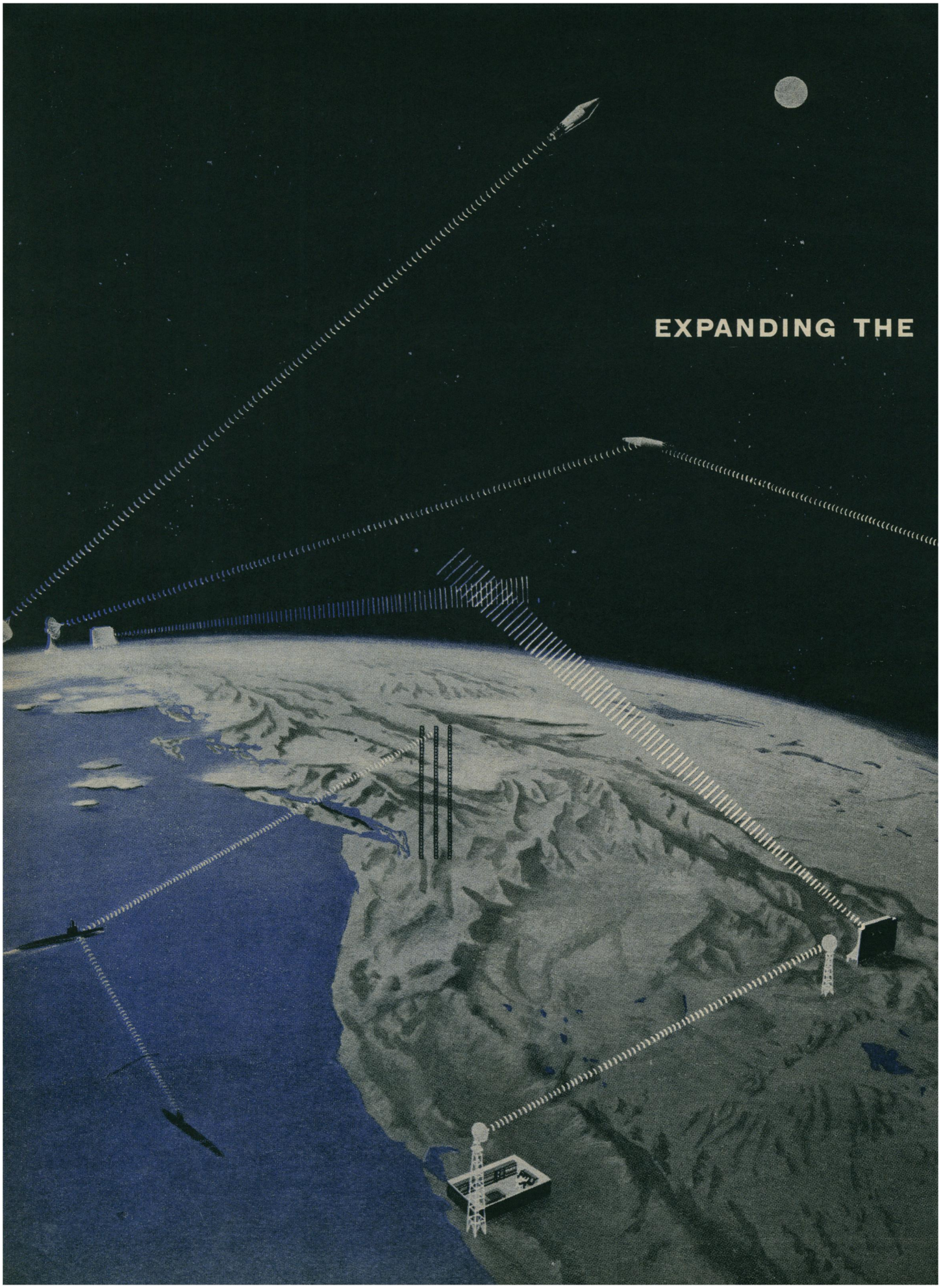
Main Office and Factory: Clifton, New Jersey
Sales Offices: Chi., Ill., San Mateo, Cal.

A TORSION MODEL DWL-3
Capacity: 200 grams
Weight-loading Dial: up to 9 grams by 1 gram increments
Fine Weighing Dial: 1 gram by .02 gram graduations
(Readability: .005 g)

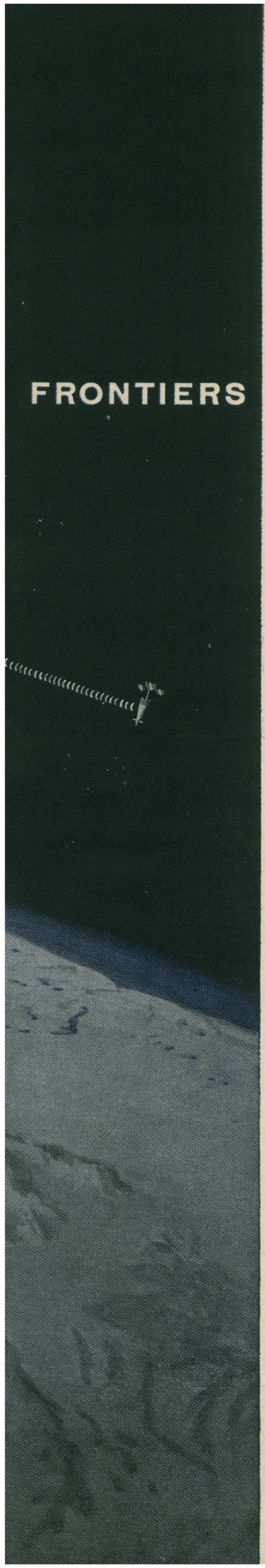
B TORSION DWL-5
Capacity: 500 grams
Weight-loading Dial: up to 90 grams by 10 gram increments
Fine Weighing Dial: 10 grams by .1 gram graduations
(Readability: .02 g)

C TORSION DWL-2
Capacity: 120 grams
Weight-loading Dial: up to 9 grams by 1 gram increments
Fine Weighing Dial: 1 gram by .01 gram graduations
(Readability: .002 g)

TORSION DWL2-1
Specifications are same as the DWL-2 except that this model has scoop for seeds or other bulky material.


EXPANDING THE

Herodotus, the historian, records (490 B.C.) the use of burnished shields for military signaling. This was the forerunner of the heliograph, invented by Sir Henry C. Mance, which came into wide use centuries later.



FRONTIERS OF SPACE TECHNOLOGY IN

COMMUNICATIONS

Lockheed's interest in developing the science of communications extends from the depths of the oceans to deep space. Its Missiles and Space Division research programs deal with the development and application of statistical communication and decision theory in such areas as countermeasures; telemetry multiplexing and modulation; scatter communications; multiple vehicle tracking; millimeter wave generation and utilization; sonic signal detection and processing; avoidance of multipath degradation; and interference avoidance.

Associated research and development efforts are directed toward propagation studies and advanced antenna design; low noise amplifiers; vehicle borne signal transmission and reception, data storage and processing; solid state materials and devices.

The scope of such activities extends from advanced studies of naval communication problems on and under the oceans; the many applications to satellite vehicles; on to the specialized communication problems of deep space explorations. Latter needs are exemplified by high frequencies, low weight and power, high stability, low effective bandwidth, extreme reliability and basic simplicity requirements.

Engineers and Scientists: Investigating the entire spectrum of communications is typical of Lockheed Missiles and Space Division's broad diversification. The Division possesses complete capability in more than 40 areas of science and technology – from concept to operation. Its programs provide a fascinating challenge to creative engineers and scientists. They include: celestial mechanics; communications; computer research and development; electromagnetic wave propagation and radiation; electronics; the flight sciences; human engineering; magnetohydrodynamics; man in space; materials and processes; applied mathematics; oceanography; operations research and analysis; ionic, nuclear and plasma propulsion and exotic fuels; sonics; space medicine; space navigation; and space physics.

If you are experienced in work related to any of the above areas, you are invited to inquire into the interesting programs being conducted and planned at Lockheed. Write: Research and Development Staff, Dept. M-18A, 962 W. El Camino Real, Sunnyvale, California. U.S. citizenship or existing Department of Defense industrial security clearance required.

Lockheed / **MISSILES AND SPACE DIVISION**

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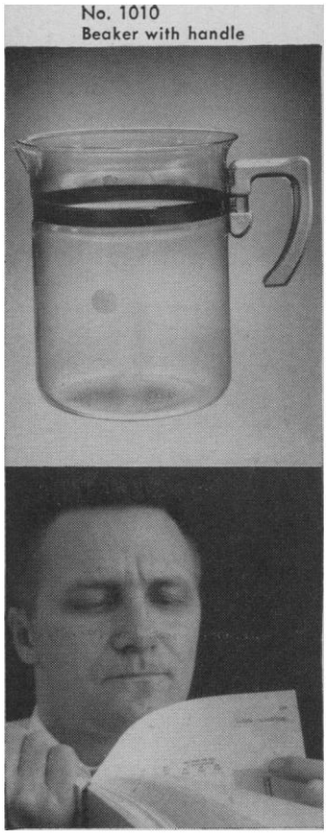

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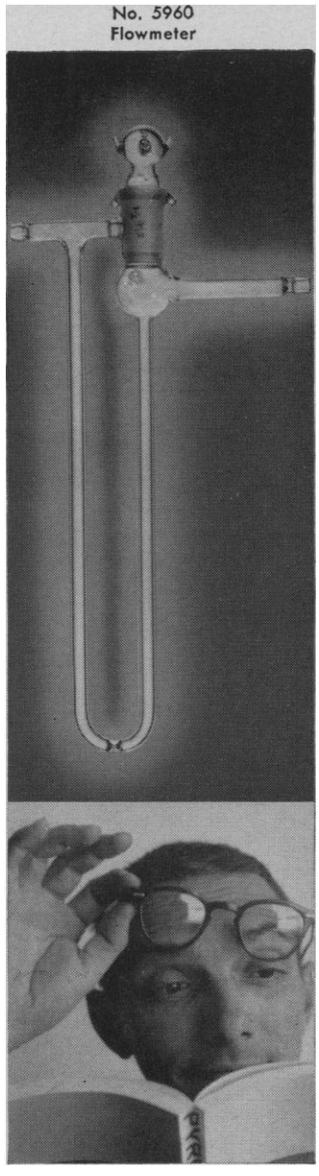

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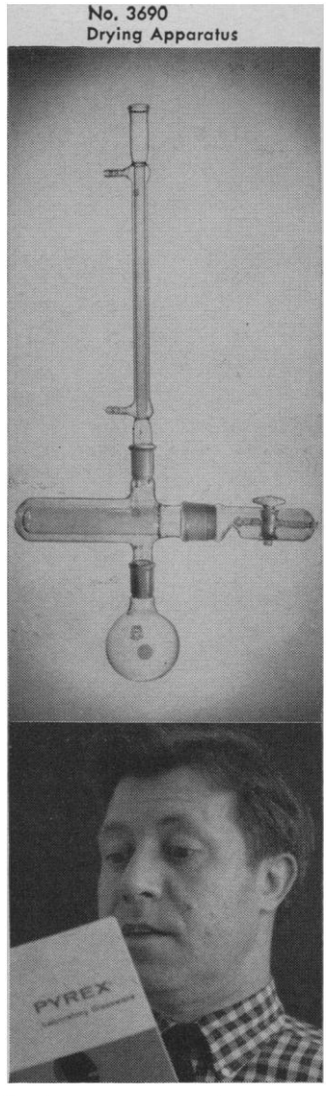

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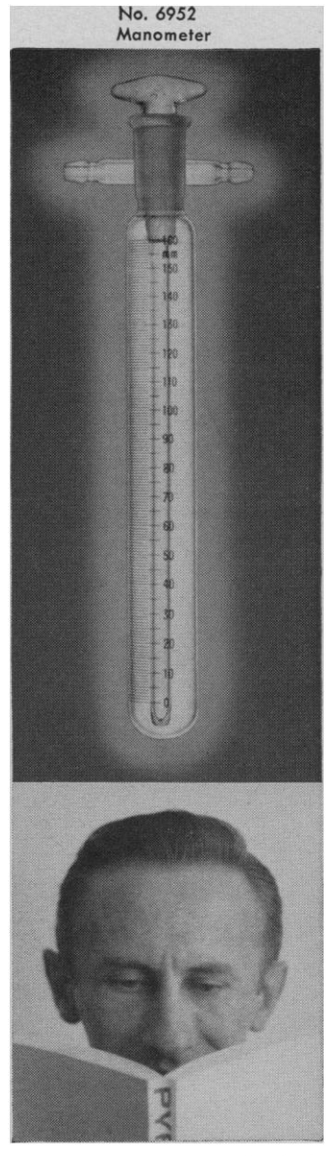

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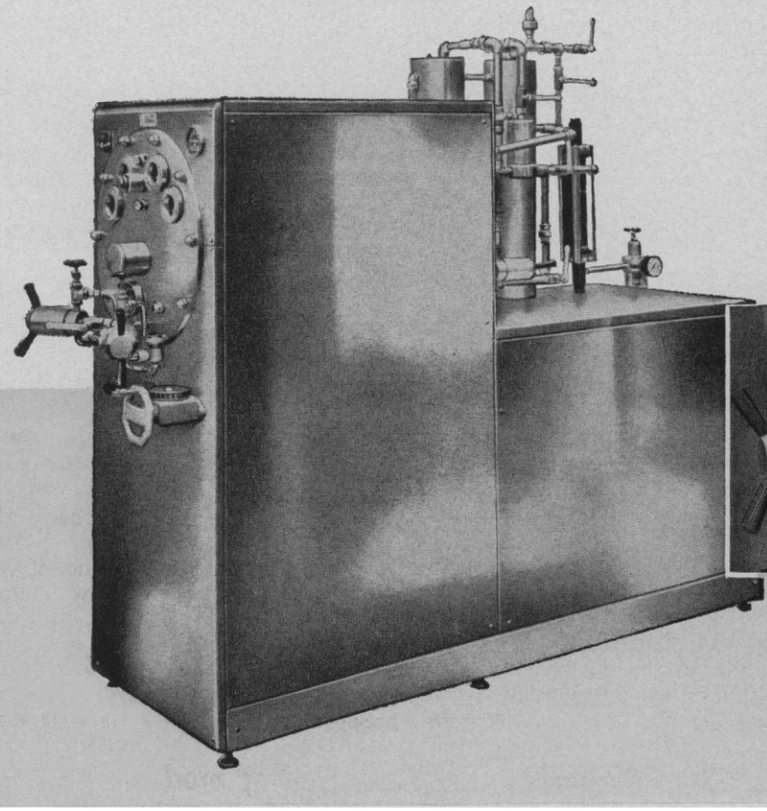

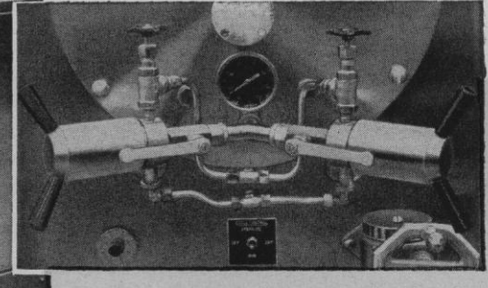

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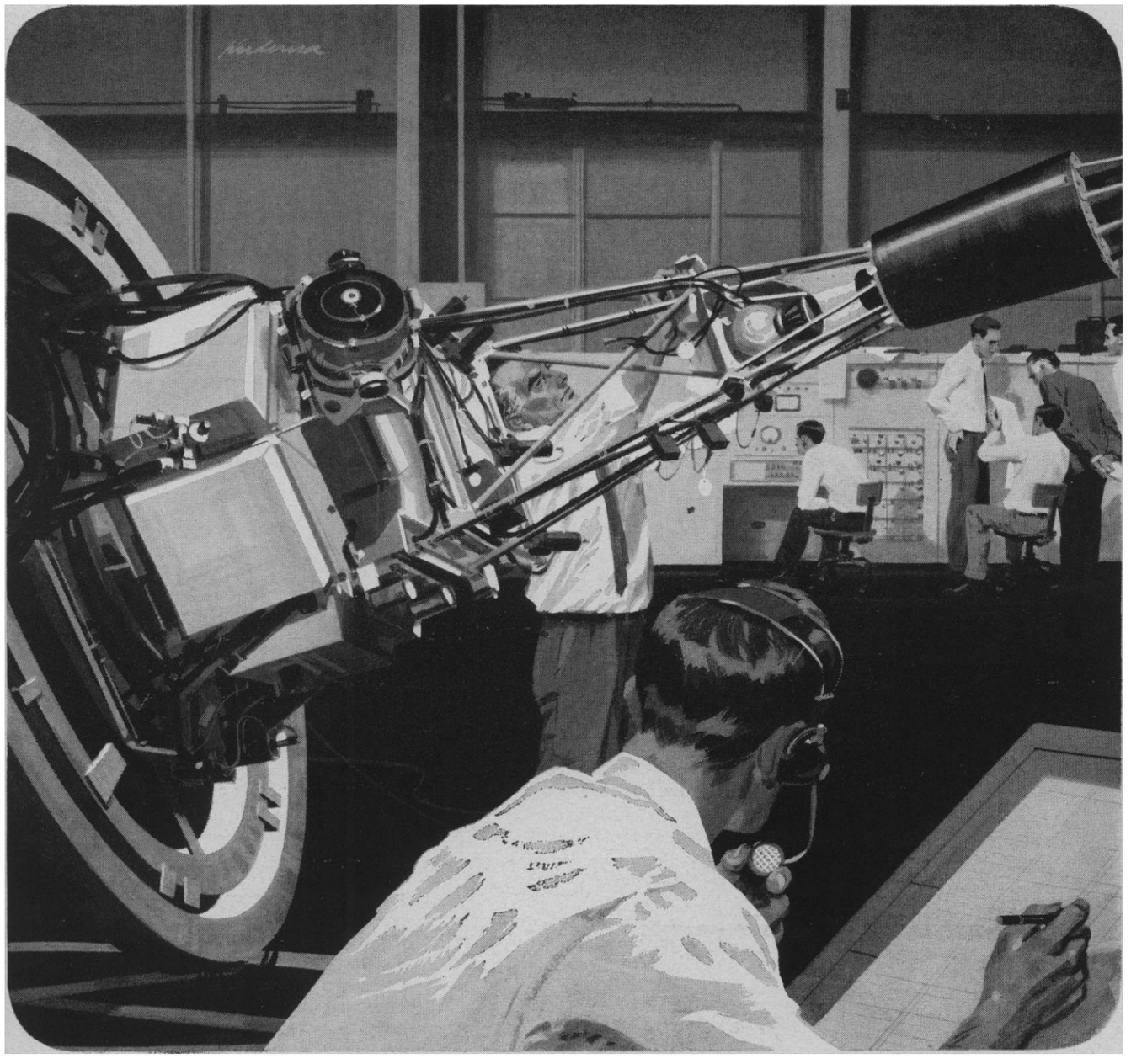

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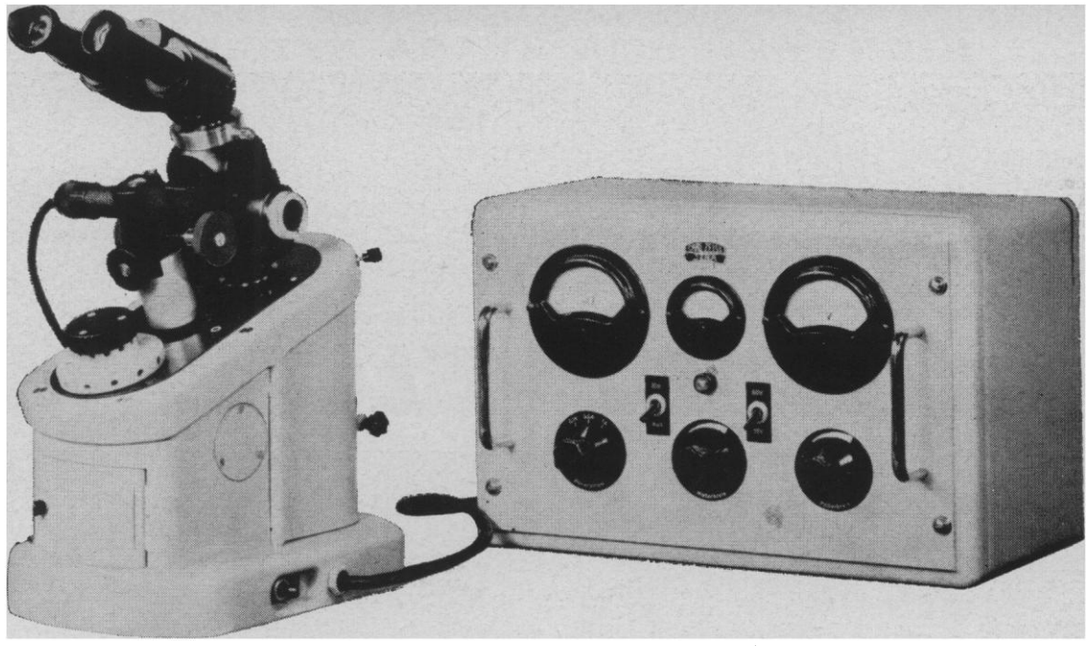

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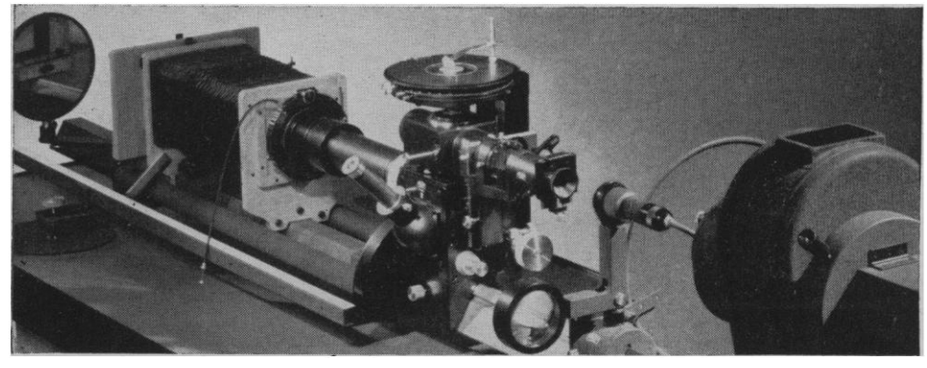

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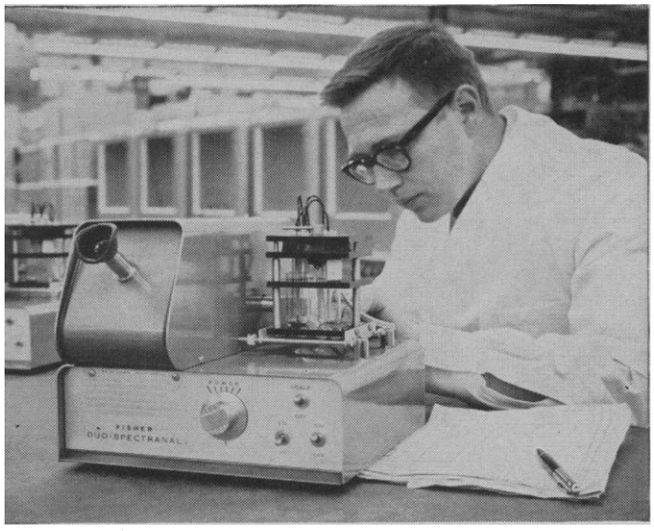

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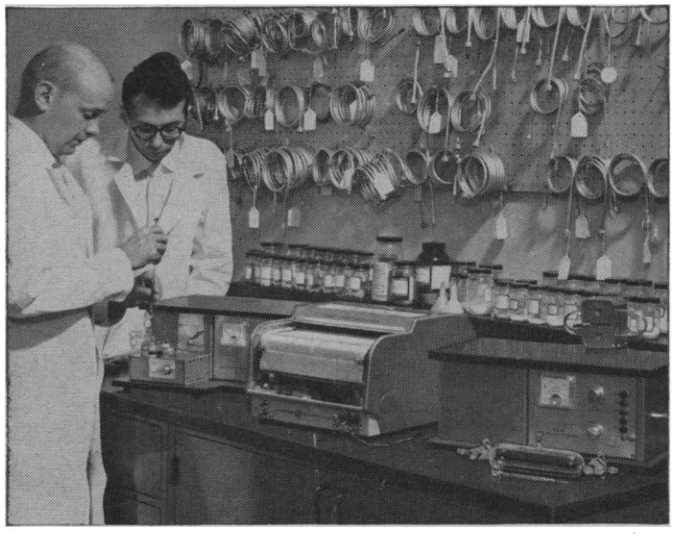

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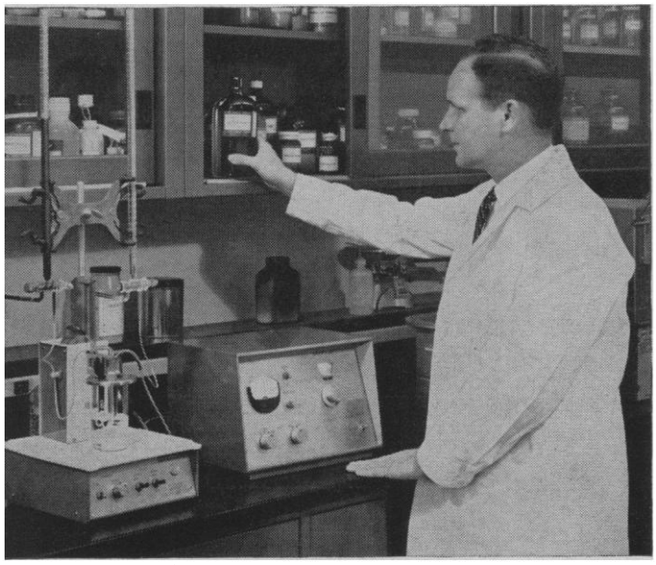

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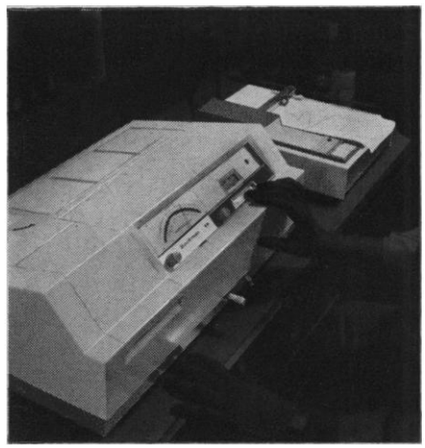

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Letters

Conservation

Thank you for your cover picture [*Science* **132** (9 Dec. 1960)] calling attention to the book, *This Is the American Earth*, one of the Sierra Club's most important achievements in 68 years of conservation. We are happy that your reviewer, ecologist Edward S. Deevey, author of one of the liveliest essays I have ever read ("The hare and the haruspex," in the *Yale Review*), liked the pictures [**132**, 1759 (1960)]. Other ecologists we have heard from liked the rest of the book as well.

Nancy Newhall chose a method of presentation that she hoped would jar readers out of the Conventional Response. The result may not be comfortable, but then it wasn't intended to be. As one who has watched the response to various kinds of conservation writing for more than 20 years, I think her choice, in its organic beauty, warrants the high praise that it has received elsewhere. Not Deevey's ridicule. He sticks labels on the conservation effort that will be much harder to scrape off than they were to put on.

Before undertaking a review that could severely impair the reception accorded a book by so important an audience as yours, I think a reviewer should read the book carefully, not just skim it. There is abundant evidence of skimming in the review, patent in the fact that a man as brilliant as Deevey missed the point of the book as no other reviewer has. In implying that Nancy Newhall's text is concerned only with scenic resources and not with broad conservation, he misses her thesis by a mile. The text is about the survival of man. It concerns the relation of man's spirit—the crucial resource—to his environment, on which he must live much more lightly than he has been doing. An ecologist, of all people, must know how totally inadequate the shallow definition of conservation ("wise use") is in the mid-20th century—even when qualified to read "ecologically sound use."

This Is the American Earth is not a book to be read in smug assurance that science has solved everything, or soon will have. It tells no one to relax and enjoy man's present course; it tries to change that course to one with good promise for our children. "Conservation is humanity fighting for the future," Nancy Newhall writes. It is not a methodological gathering of data indicating a need for further study about the rate of expenditure of resources.

Nor is conservation served by desiccating emotion out of its literature. One of the needs in conservation is the ability to express deep-felt opinion, to compress considerable scientific fact into poetic form, and especially to stir people into caring enough to act, and to act in time.

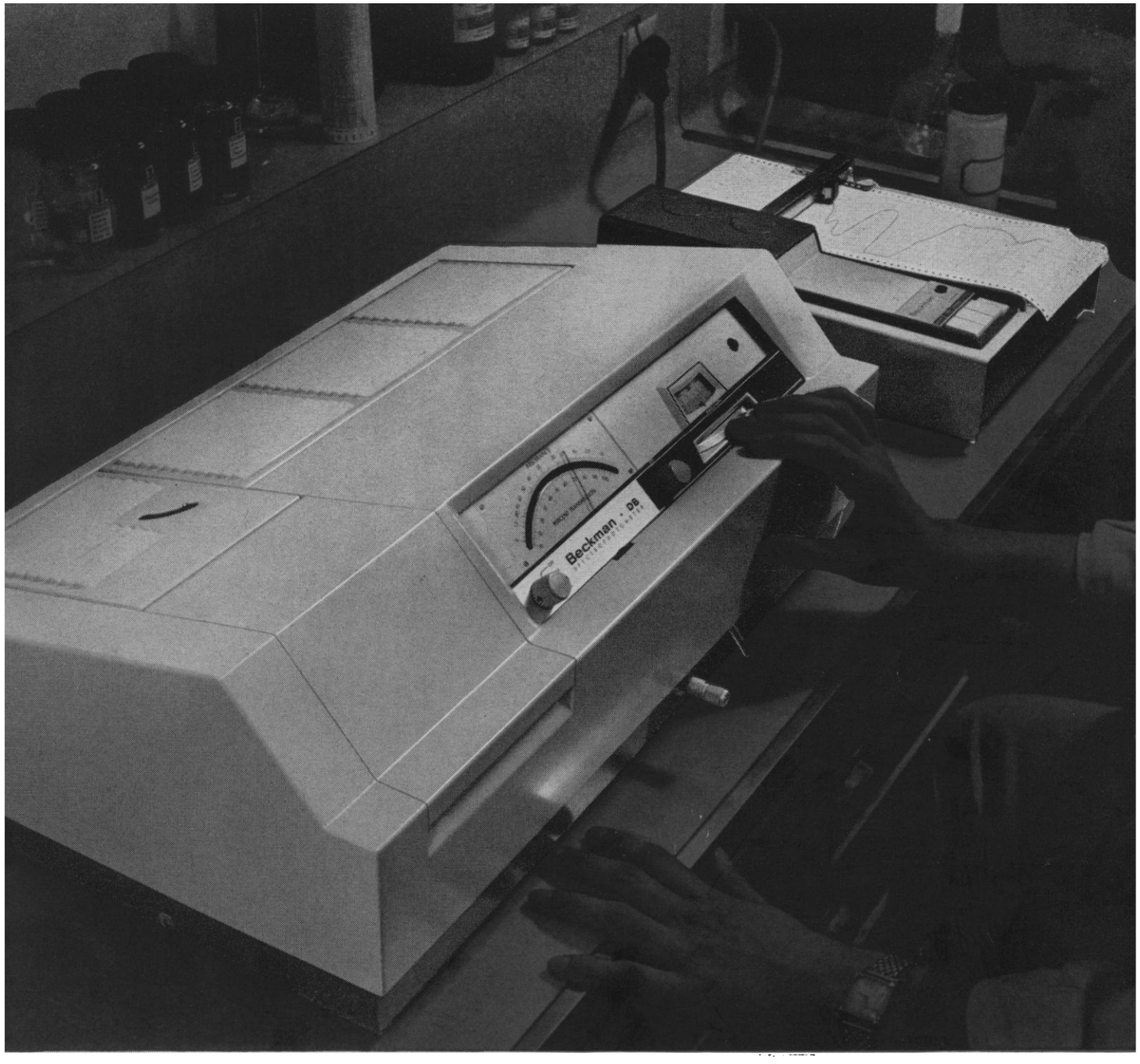
We wish your readers would check for themselves what Nancy Newhall, as an artist and writer lauded by artists and writers (if not by one ecologist) and as a conservationist and sciolist if not as a scientist, has been able to do. We think they will conclude it was worth doing. (I can't resist adding, about the text that Deevey didn't like, that Alfred Knopf, no mean connoisseur of the written word, has proposed publishing the text *without* the photographs!)

DAVID BROWER
Sierra Club, San Francisco, California

I am glad, with David Brower, that many reviewers liked Nancy Newhall's text, but I would be more penitent about being caught in a minority position if I were sure that the reviewers are not confusing their own commitment to the cause of conservation with literary discernment. In my review, which certainly was ungracious, I tried to nail an attitude that is wholly natural to movements of social reform, but which seems to me to pose a grave internal danger: the tendency of partisans to talk only to each other, in a private language that is bound to be misconstrued when overheard. I think it is poor tactics to broadcast this language. I quite agree that conservation is "humanity fighting for the future," but I suspect that field commanders like Brower, to whom we must all be grateful as they conduct our battles for us, cannot see all sectors of the front at once; and I think the movement is strong enough to stand some frank discussion, intramurally and behind the firing lines, of its methodology and goals.

To deal first with tactics, let me make plain that I ridiculed neither the book nor the movement, nor do I oppose emotion or art in the service of policy. What I said was that effusive overstatement is a dangerous political weapon, since its users can be made to *look* ridiculous. We agree, *entre nous*, that the book is propaganda—*Paradise Lost* was propaganda too. Now emotion is the very stuff of art, but when art is used as propaganda one wants to be quite sure that the emotion not only is honest but *sounds* honest—otherwise the irreverent opposition labels it sentimentality, meretriciousness, or cant. And it takes very great literary skill to express honest emotion nobly enough to spike such charges; sincerity is necessary but insufficient. I do not question Nancy Newhall's sincerity. I do find

(Continued on page 922)



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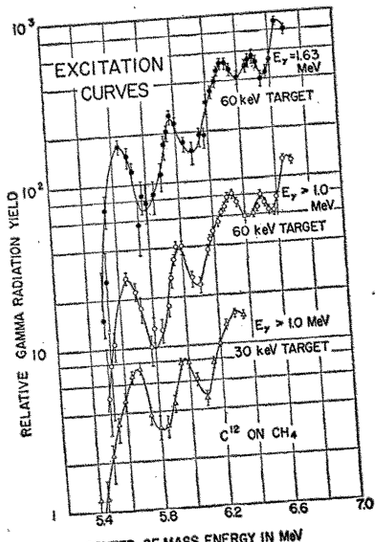

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[1]*H. E. Gove, Proceedings of the Second Accelerator Conference, Amsterdam, Oct., 1960 (North Holland Publishing Company, 1961) p. 63.*

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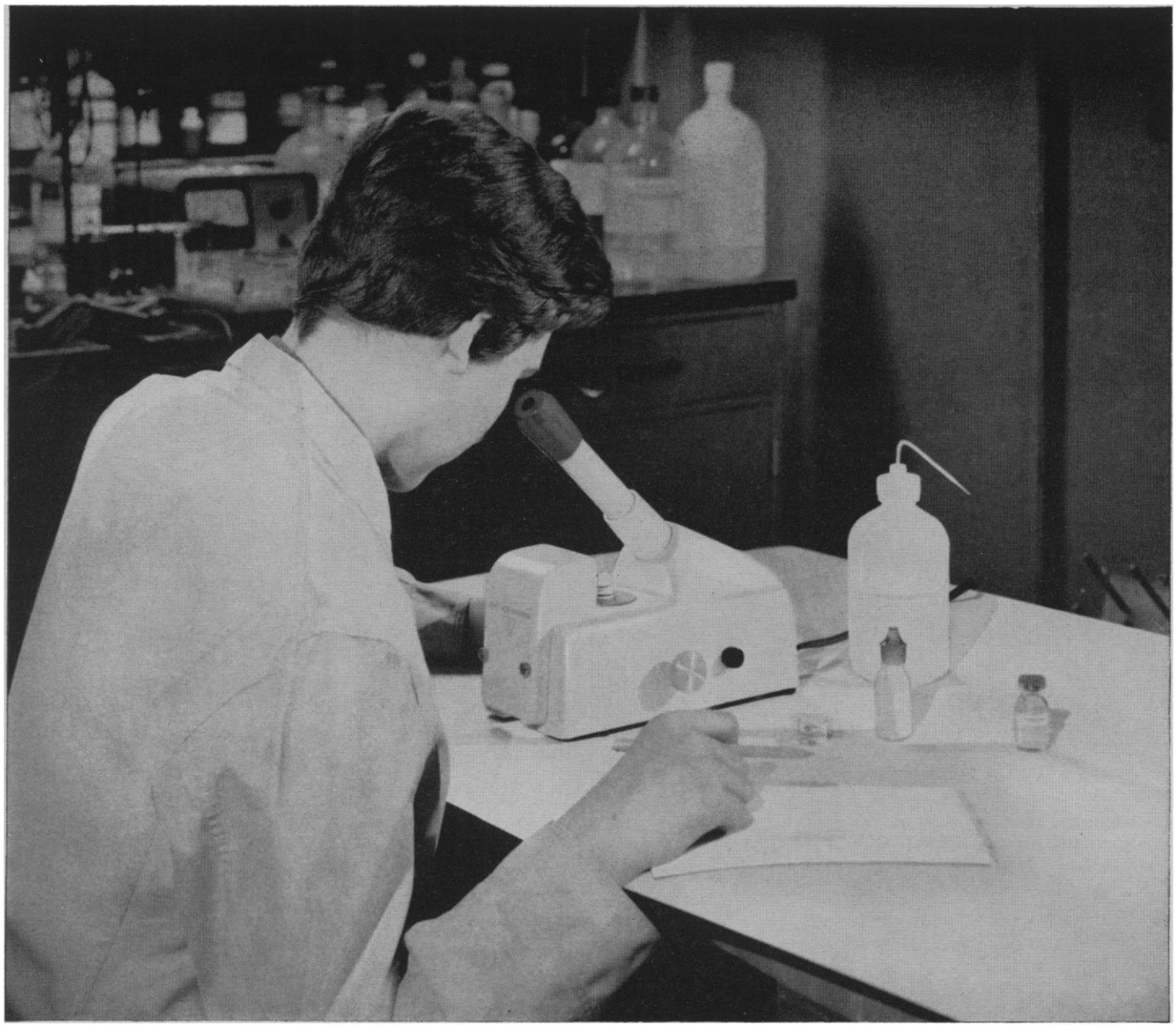
The Forum series is of special interest in that it makes no attempt to reach a mass audience: it is directed to intellectuals abroad. In the sciences, two series of Forum lectures were given in 1959: a 16-program series in medicine and a 20-program series in the behavioral sciences. In 1960 an 11-program series in chemistry was produced, and the first of 20 lectures in the biological sciences was broadcast last week. A few titles from the latest series and the names of the lecturers give some idea of the scope and quality of the programs. In chemistry, the following are representative: Chemical Research in Solar Energy, Farrington Daniels; Radioactive Isotopes in Chemical Research, Paul C. Aebersold; Proteins, John T. Edsall; and Origin of the Solar System, Harrison Brown. In biology some sample lectures are: Characteristics of Animal Populations, Edward S. Deevey; Plant Photoperiods, H. A. Borthwick; Biochemistry of Human Heredity, H. Bentley Glass; Chromosomes and Tissue Culture, Theodore T. Puck; Enzyme Feedback Controls of Living Processes, DeWitt Stetten.

The lectures in this series are available abroad not only on tape but also in the form of pamphlets which are obtainable for the asking at any of the 200 foreign posts of the United States Information Service or by direct request to the Voice of America in Washington.

Some Americans who are aware that these Forum lectures may be freely published or broadcast overseas have been under the impression that they cannot be used in a similar way in the United States. This is in part true, in that it is easier for a foreigner to obtain and publish the material: all he needs is a copy or a tape. A domestic user must in the first place get wind of the lectures and then get permission of the lecturers to use the material. Subject to the lecturers' permission, commercial publishers are, however, planning to bring out at least three of the Forum series in paperback editions, and the National Association of Educational Broadcasters will make the tapes available for further distribution. Thus it will soon be possible for Americans to have access to much of the material that is now virtually limited to a foreign audience. They then can judge for themselves whether an English chemist was justified in writing recently that these lectures would assist him and his colleagues in keeping up with modern developments in science.—G.DuS.



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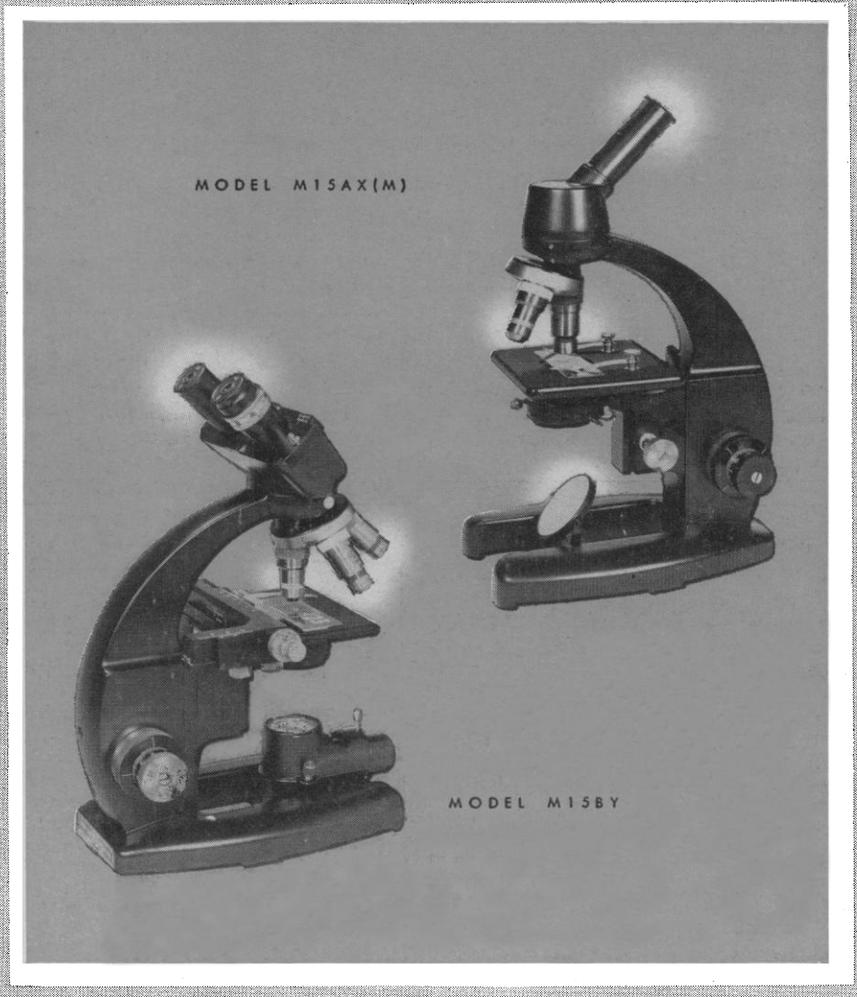

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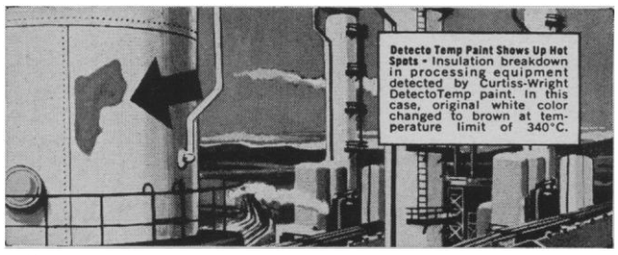

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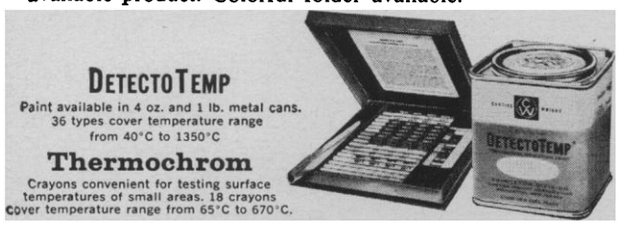

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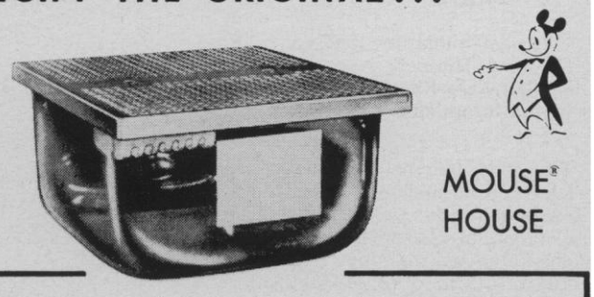

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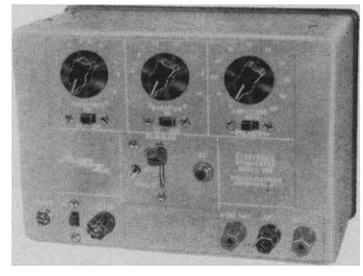

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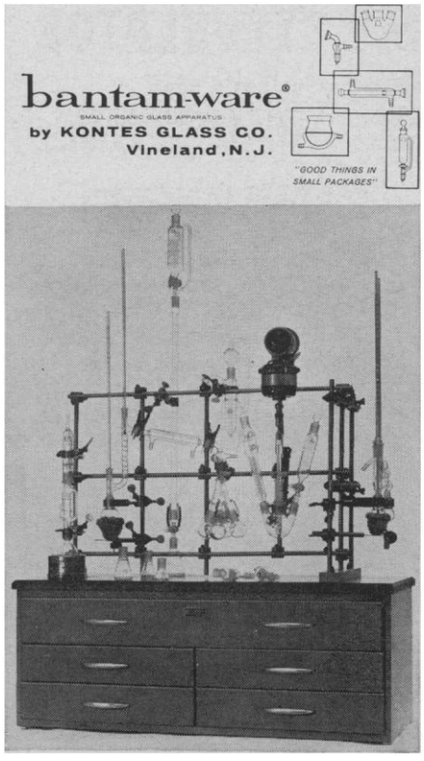

29 June. Special exchangers, column separations: R. Kunin, "Porous resins"; S. Lindenbaum, "Liquid ion exchangers"; W. J. Sloan, "Computers in column problems"; P. B. Hamilton, "Amino-acid separations."

30 June. Panel discussion: *Important unsolved problems in ion exchange* (G. E. Boyd and D. Reichenberg, *chairmen*).

Chemistry of Carbohydrates

3–7 July. (Program to be announced.)

Chemistry and Metallurgy of Semiconductors

A Rosenberg, *chairman*
P. Egli, *vice chairman*

10 July, Epitaxial crystal growth: A. P. Hale, "Vacuum deposition of silicon layers"; T. Renner, "Preparation of III–V compounds by vapor deposition"; V. J. Lyons, "Vapor growth of gallium arsenide"; P. I. Pollak, "Vapor phase growth of silicon crystals."

11 July. Crystal growth at high pressures: W. V. Wright, "Moderately high pressures"; R. H. Wentorf, "Ultra high pressures." *Thermodynamics*: M. B. Bever, "Properties of compound semiconductors"; W. Tiller, "Obtaining phase diagrams through controlled solidification."

12 July. Chemical bonds and electron energy bands: C. H. L. Goodman, A. J. Cornish, "Empirical relations between electrical properties and composition"; J. C. Slater and P. O. Lowdin, "Theoretical relationships."

13 July. Organic conductors: D. Fox, "Mechanisms"; M. I. Pope, "Phenomena."

14 July. Electroluminescence: E. E. Loebner, "Mechanisms"; H. F. Ivey, "Phenomena."

Microbiological Deterioration

James W. Clapp, *chairman*
Arthur M. Kaplan, *vice chairman*

17 July. Newer developments in industrial preservations (Charles C. Yeager, *chairman*): Richard C. Ross, "The microbiology of oil paints"; Robert L. Johnson, "Synthetic latex preservation." *Microorganisms and hydrocarbons* (Ray C. Allred, *chairman*): J. B. Davis, "Oxidation of alkyl-substituted cyclic hydrocarbons"; John O. Harris, "Oxidation of alphaltic hydrocarbons"; Ray C. Allred, "Oxidation of sulfonated alkylbenzenes."

18 July. Mechanisms of action of biocides (Robert E. Deems and Philip N. Gordon, *cochairmen*): Adrien Albert, "The physics and chemistry of fungicidal and bactericidal action"; J. L. Strominger, "Antibiotics as inhibitors of bacterial cell wall synthesis"; M. W. Allen, "Mechanism of action of nematocides"; A. S. Crafts, "Mechanism of ac-

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tion of herbicides"; Saul Rich, "Future trends in the chemical control of microorganisms."

19 July. Spore germination and physiology (Z. John Ordal, *chairman*): Hillel S. Levinson, "Bacterial spores"; S. G. Knight, "Fungal spores." *Genetics and the control of microorganisms* (Harold H. Smith, *chairman*): L. S. Baron, "Genotypic, phenotypic, and environmental effects of genetic transfer mechanisms in microorganisms"; Ellis Engelsberg, "Mutation to sensitivity to carbohydrate inhibition."

20 July. Degradation of cellulose at the enzymic level (R. C. Quittenton, *chairman*): D. R. Whitaker, G. Halliwell (subjects to be announced). *Microbiology of stored grains* (Robert L. Johnson, *chairman*): Clyde M. Christiansen, "Industrious fungi."

21 July. Summary and open discussion of the week's papers and their implications (Adrien Albert, *chairman and discussion leader*).

Biochemistry and Agriculture

James L. Liverman, *cochairman*
Frank L. Stark, Jr., *cochairman*

24 July. The role of cobalt in growth processes (James L. Liverman, *chairman*): Harold Evans, "Cobalt in plant nutrition"; Frank Salisbury, "Cobalt in photomorphogenic responses"; Lars Loercher, "Metabolic aspects of cobalt action"; Horace A. Barker, "Role of vitamin B_{12} in biochemical processes"; Gunther Eichorn, "Chelation in biological phenomena."

25 July. Progress in phenoxy herbicides (John B. Hanson, *chairman*): Corwin Hansch, "Relation of structure to activity" (*A*); Donald G. Crosby, "Relation of structure to activity" (*B*); J. L. Key, "Penetration and translocation"; Michael K. Bach, "Metabolism of the herbicides in plants"; J. B. Hanson, "Effects on the cytoplasm of plants."

26 July. Biochemistry and structure of the plant cell wall (A. C. Neish, *chairman*): S. T. Bayley, "The structure of the plant cell wall and its development"; Peter Albersheim, "The metabolism of pectic substances"; J. R. Colvin, "The mechanism of formation of cellulose fibrils"; M. B. Perry, "Biogenesis of carbohydrates"; H. S. Stafford, "Problems in lignification."

27 July. New aspects of plant disease control (Donald G. Crosby, *chairman*): Arthur Kelman, "Pectinolytic and cellulolytic enzymes in plant disease"; H. H. Flor, "Host-parasite relationships in plant disease"; Ernest Jaworski, "Genetic and antigenic relationships in plant disease." *Role and mission of the President's Science Advisory Boards.*

28 July. New developments in insect control procedures (Frank L. Stark, Jr., *chairman*): Martin Jacobson, "Insect sex attractants in survey and control procedures"; L. D. Christenson, "Promising new approaches to insect control and eradication"; R. C. Von Borstel, "Genetic methods for insect control."

Electrodeposition

Martin S. Frant, *cochairman*
Walter R. Meyer, *cochairman*

31 July. The mechanism of electrodeposition (W. R. Meyer, *chairman*): N. B. Hackerman, "Nature and kinetics of discharge species at the electrode surface"; P. Delahay and D. Mohilner, "Metal deposition: double layer effects and investigation of fast discharge processes." *The mechanism of electrodeposition* (continued) (E. Saubestre, *chairman*): J. O'M. Bockris, "Rate-determining steps and paths in the electrodeposition and electrodissolution of thin metallic layers"; J. O'M. Bockris, D. Drazic, H. Kitz, "Transient phenomena at the iron-solution interface, and the mechanism of the deposition and dissolution of iron."

1 Aug. The mechanism of electrodeposition (continued) (N. Hackerman, *chairman*): R. G. Barradas and B. E. Conway, "Electrochemical adsorption heterocyclic bases and ionic derivatives." Late papers and general discussion on mechanisms. *Addition agents in electrodeposition* (O. Kardos, *chairman*): H. Brown, "The role and structure of addition agents in electroplating, with special reference to nickel"; D. Trivich, "Some aspects of brightener ac-

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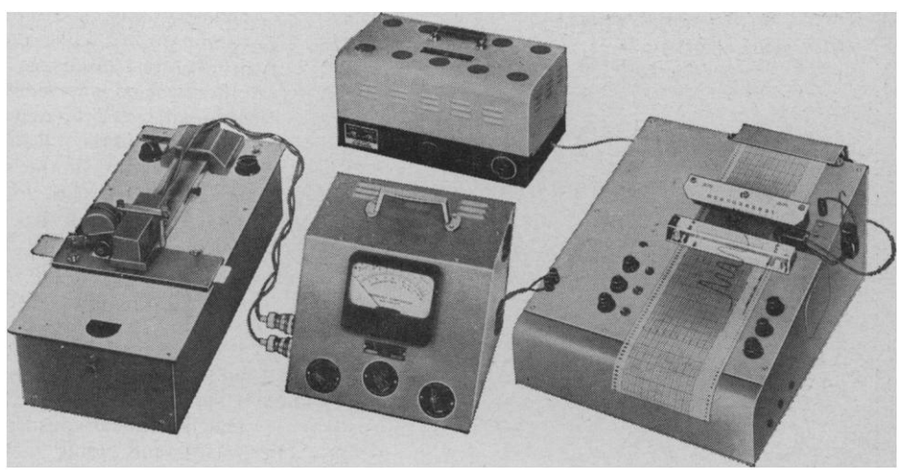

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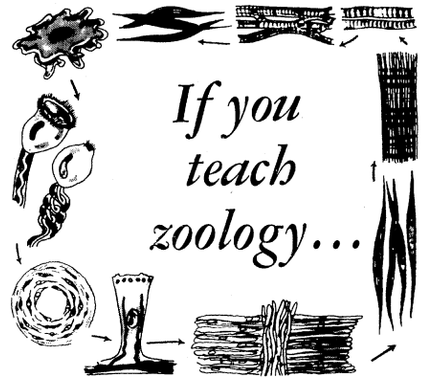

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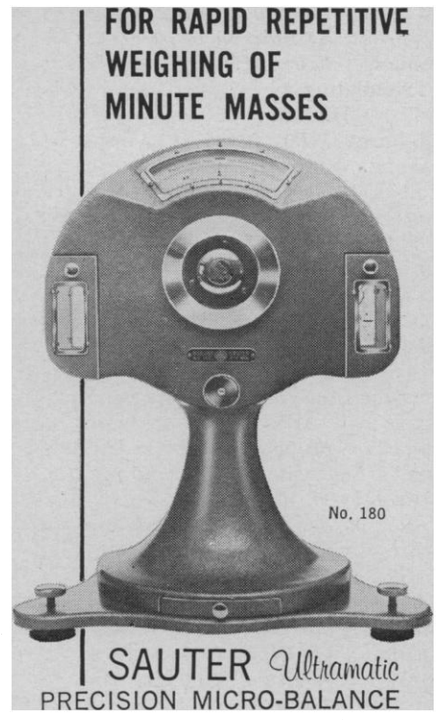

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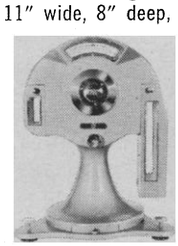

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2 Aug. The effect of structure on properties: A) Basis metal structure (A. Brenner, *chairman*): H. Leidheiser, "Nickelplating on copper single crystals"; M. H. Jones, "Effect of substrate metallurgy." *B) Coating metal structure* (A. Brenner, *chairman*): H. J. Read, "Plastic properties of electrodeposits"; A. M. Max, "Stress in electrodeposits."

3 Aug. Electrodeposition of alloys (F. A. Lowenheim, *chairman*): M. L. Holt, "Electrodeposition of alloys of some of the transition metals"; H. Koretzky, "Current research on electrodeposited thin magnetic films. A critical survey"; I. Wolf, "Factors affecting magnetic properties of iron-nickel films." *Electrodeposition on unusual substrates* (H. B. Linford, *chairman*): D. R. Turner, "Metal deposition on semiconductors"; E. Saubestre, "Plating on unusual metals."

4 Aug. Current problems in the utilization of plated coatings (N. Murphy, *chairman*): M. Frant, "Electrodeposited metals as electrical contacts"; F. A. Lowenheim, "Solderability of plated metals"; C. Levy, "Electro-deposited coatings for high temperature applications."

Electrical and Relaxation Processes in Glass

R. J. Charles, *chairman*
N. J. Kreidl, *vice chairman*

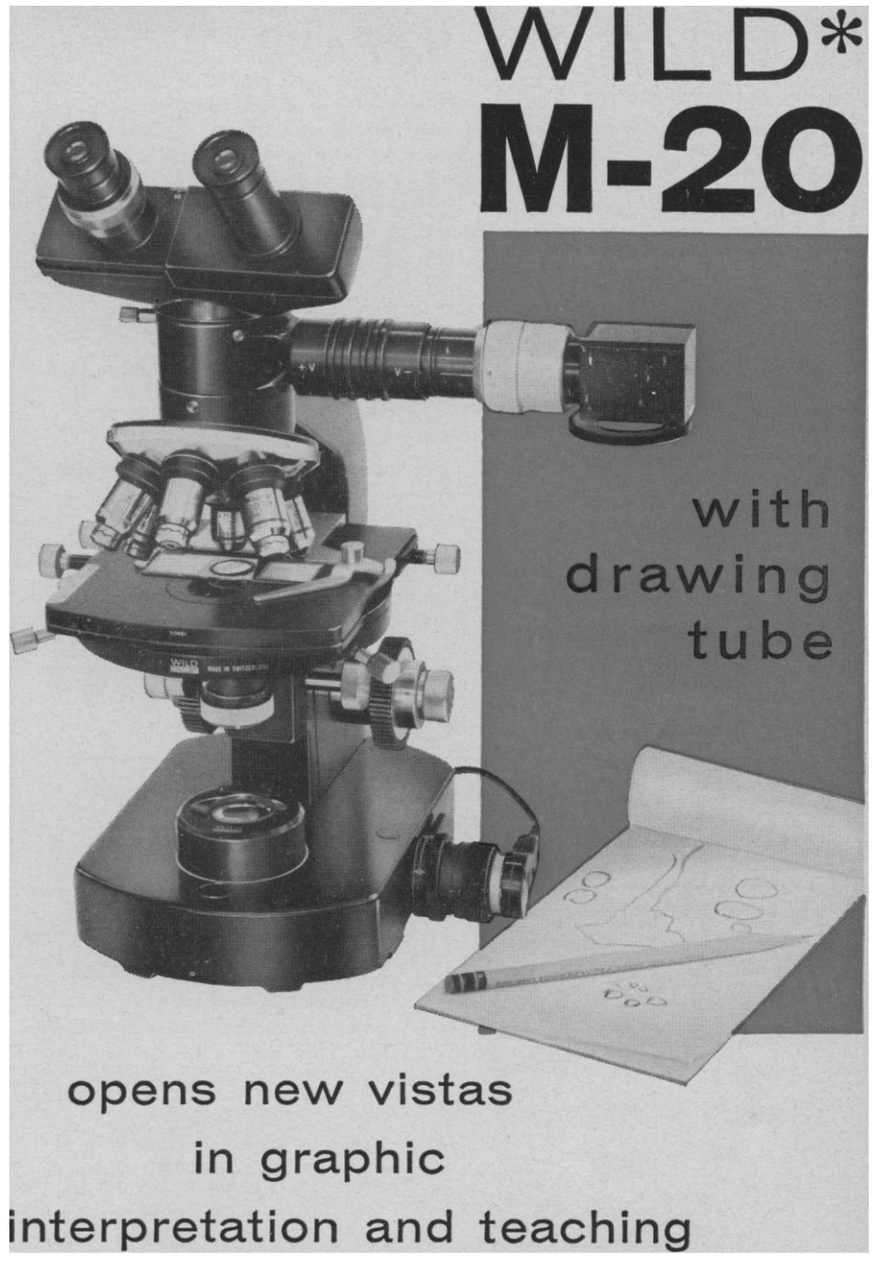
7 Aug. Electrical conduction and polarization (R. J. Charles, *chairman*): H. E. Taylor, "The dielectric relaxation of glass due to the movement of alkali ions"; J. I. Isard, "Electrical conduction in glasses." (N. J. Kreidl, *chairman*): A. E. Owen, "Comparison of conduction and relaxation processes in glass."

8 Aug. P. M. Sutton, "Space charge and electrode polarization"; R. J. Charles, "A defect model of diffusion in glass." *Alkali ion mobility* (F. M. Ernsberger, *chairman*): T. Abe, "On the free sodium ion in glass"; G. Eisenman, "The electrochemical properties of cation-sensitive glass electrodes and the atomic basis of their cation discrimination."

9 Aug. R. J. Ryder, "Internal friction of alkali silicate glasses"; I. I. Kitaigorodskii, (subject to be announced). *Glass structure and the liquid state* (A. B. Bestul, *chairman*): D. Turnbull, "The glass transition."

10 Aug. P. D. Bray, "Nuclear magnetic resonance studies of the structure of glass"; M. Aslanova, "Structure and properties of glass fibers"; T. A. Litovitz, "Structural relaxation in liquids (molten glasses)."

11 Aug. Discussions and business meeting.



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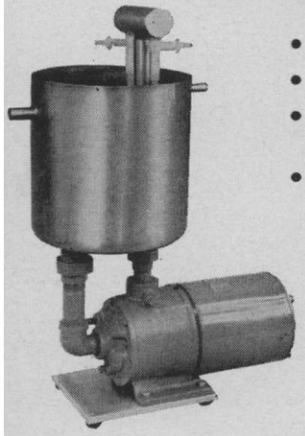

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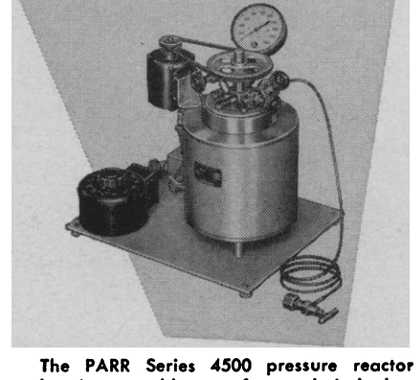

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Humid Tropics Vegetation

As a contribution toward the UNESCO Humid Tropics Programme, a group of scientists representing a number of fields came together from 14 to 20 September 1960, at Goroka, Territory of New Guinea, for a symposium on "The Impact of Man on the Vegetation of the Humid Tropics." Most of the participants were from southeast Asia and Australia and intermediate areas, but one, E. J. H. Corner, was from England and two, J. M. Blaut and F. R. Fosberg, were from the United States. The UNESCO Southeast Asia Science Cooperation Office and the Administration of the Territory of New Guinea cooperated in the enterprise, and the principal credit for superb local arrangements goes to John Womersley, of the Division of Botany, Lae, New Guinea.

The attention of the symposium was largely directed to the effects of the activities of man prior to the era of the chain saw and bulldozer. Goroka, in the middle of a vast area changed in prehistoric time from rain forest to grassland and cultivation by people who did not even have metal implements, was a truly appropriate place for discussion of such a subject. One had only to look out the window to see illustrations of what was being discussed. Present were literate representatives of peoples who, in 1930, had not yet seen a white man. Many of the participants and observers were members of the Administration of the Trust Territory of New Guinea, who deal daily with the matters under discussion. These factors gave a sense of reality that such conferences seldom have.

After a formal opening by J. T. Gunther, Assistant Administrator of the Territory, and discussion of the physical and human background, the subject was handled under seven broad headings, much more attention being given to the anthropological, social, and economic aspects than is indicated by the symposium title. (i) Consideration of the effect of selection and cultivation of food plants brought out that the exploitation of wild plants leads imperceptibly to cultivation, that wild species are changed by selection resulting in cultigens which may replace their wild ancestors, that the principal effect of food gathering on the forest is to increase the proportion of trees with edible fruits, and that gardening has far greater effect than this on the vegetation. (ii) The use of fire by early man, whether to get rid of debris, to destroy the forest because of its "nuisance value," to aid in hunting, or merely for fun, has been one of the strongest influences on vegetation and has played a large part in converting vast areas in the tropics from rain forest to grass and

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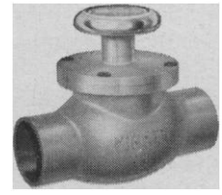

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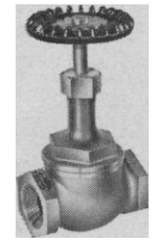

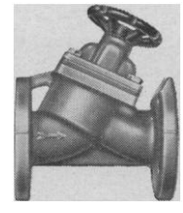

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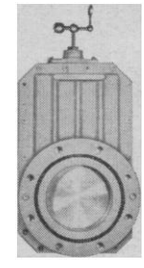

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agricultural land. (iii) Because grazing of domestic animals is not important in New Guinea, this topic received less attention than had been expected, but it was emphasized that in tropical grasslands intensive grazing is necessary, in the absence of fire, to prevent these man-made grass communities from reverting to woody vegetation. (iv) Shifting agriculture, admittedly of major importance in the alteration of tropical vegetation, came in for perhaps a disproportionate share of attention. It was asserted that an equilibrium between man and his environment is possible under this system of agriculture, but that population pressure leads to unbalance. One of the most interesting items brought out was the fact that in the highlands of New Guinea *Casuarina* has long been planted to increase soil fertility, even though its nitrogen-fixing properties were only recently determined scientifically. (v) It was pointed out that secondary plant communities resulting from man's activity are usually less mesophytic than the primary communities that they replace, that this often assists in the detection of such communities in an otherwise natural landscape, and that the reflection of the environmental pattern by vegetation becomes generally more obscured as a result of man's activities. (vi) A general review of the New Guinea grasslands showed that, of the two principal types, tall grass is a successional stage leading directly back to forest, while short grass results from much more profound alteration of the environment through man's activities. (vii) Public-health aspects of the alteration of the vegetation by man were noted, especially changed distribution patterns of disease vector populations.

As a result of the discussions a number of resolutions were formulated and sent to the UNESCO authorities. In essence, these called for the promotion, by UNESCO of (i) research into all aspects of secondary communities produced by human disturbance of the tropical forest; (ii) ethnobotanical research and cooperation in field work between anthropologists and botanists; (iii) investigations into traditional animal husbandry of preliterate societies, and into the psychosociological consequences of introduced pastoralism in primitive horticultural communities in the humid tropics, especially in Oceania; (iv) investigations into the susceptibility of humid tropical limestone soils and vegetation to damage by fire and other agents; (v) investigations into the ecological implications of root-physiology; and (vi) research into the socioeconomic adaptations to alien influence of recently contacted groups of people.

UNESCO was also asked to stimulate effective programs for conserving natural resources in the humid tropics, especially by creating nature reserves and national parks and by obtaining, through education and mass communications media, support of local populations for these measures. It was recommended that UNESCO convene, in two years' time, a symposium on the results of recent ecological research in the humid tropics.

F. R. FOSBERG
Geological Survey, U.S. Department of the Interior, Washington, D.C.

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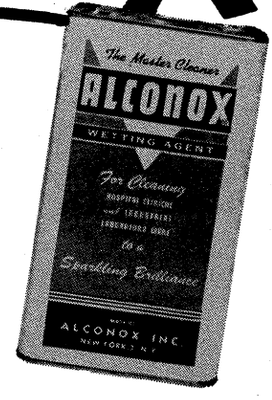

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17–18. Great Lakes Research, 4th conf., Ann Arbor, Mich. (C. F. Powers, Great Lakes Research Division, 1119 Natural Science Bldg., Ann Arbor)

17–19. Fluid Seal Meeting, intern., Ashford, Kent, England. (Information Officer, British Hydromechanics Research Assoc., South Road, Temple Fields, Harlow, Essex)

17–24. International Congress of Nurses, 12th quadrennial cong., Melbourne, Australia. (Miss D. C. Bridges, Secretary, 1 Dean Trench St., London, S.W.1, England)

18–20. Chemical Reactions in the Lower and Upper Atmosphere, intern. symp., San

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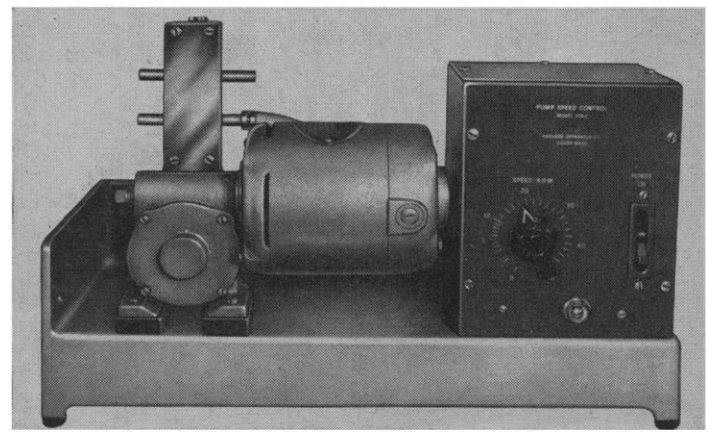

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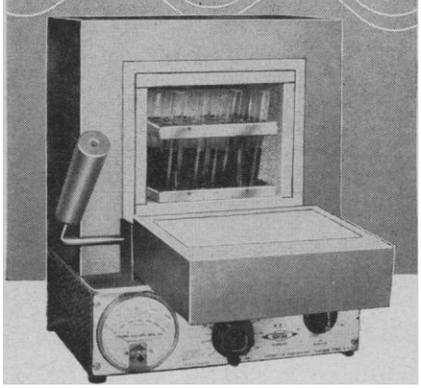

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18–21. American Geophysical Union and American Meteorological Soc., Washington, D.C. (American Geophysical Union, 1515 Massachusetts Ave., NW, Washington 5, D.C.)

19–21. Southwestern Inst. of Radio Engineers Conf. and Electronics Show, Dallas, Tex. (SWIRECO 61, P.O. Box 7443, Dallas 9)

20–21. Society of Chemical Industry, fungicide symp., London, England. (B. J. Heywood, 103 Harrow Drive, Hornchurch, Essex, England)

20–22. Association of Southeastern Biologists, Lexington, Ky. (H. J. Humm, Department of Botany, Duke Univ., Durham, N.C.)

20–24. Microbial Reactions in Marine Environments, intern. symp., Chicago, Ill. (C. H. Oppenheimer, Inst. of Marine Science, Univ. of Texas, Port Arkansas)

21–22. American Assoc. of Univ. Professors, Boston, Mass. (W. P. Fidler, AAUP, 1785 Massachusetts Ave., NW, Washington 6, D.C.)

21–23. American Soc. for the Study of Sterility, annual, Miami Beach, Fla. (H. H. Thomas, 920 S. 19 St., Birmingham 5, Ala.)

23. American Pharmaceutical Assoc., Chicago, Ill. (W. S. Apple, 2215 Constitution Ave., NW, Washington, D.C.)

23–26. American Assoc. of Colleges of Pharmacy, Chicago, Ill. (C. W. Bliven, George Washington Univ., Washington 6, D.C.)

23–27. American Ceramic Soc., 63rd annual, Toronto, Canada. (C. S. Pearce, 4055 N. High St., Columbus 14, Ohio)

23–27. Society of American Bacteriologists, Chicago, Ill. (E. M. Foster, 311 Bacteriology, Univ. of Wisconsin, Madison)

23–28. American Soc. of Hospital Pharmacists, Chicago, Ill. (J. A. Oddis, 2215 Constitution Ave., NW, Washington 7, D.C.)

24–26. Aerospace Medical Assoc., 32nd annual, Chicago, Ill. (W. J. Kennard, Secretary-Treasurer, c/o Washington National Airport, Washington, D.C.)

24–26. American Psychoanalytic Assoc., annual, Philadelphia, Pa. (J. N. McVeigh, 36 W. 44 St., New York 36)

24–27. American Assoc. of Petroleum Geologists, Denver, Colo. (G. V. Cohee, U.S. Geological Survey, Washington 25, D.C.)

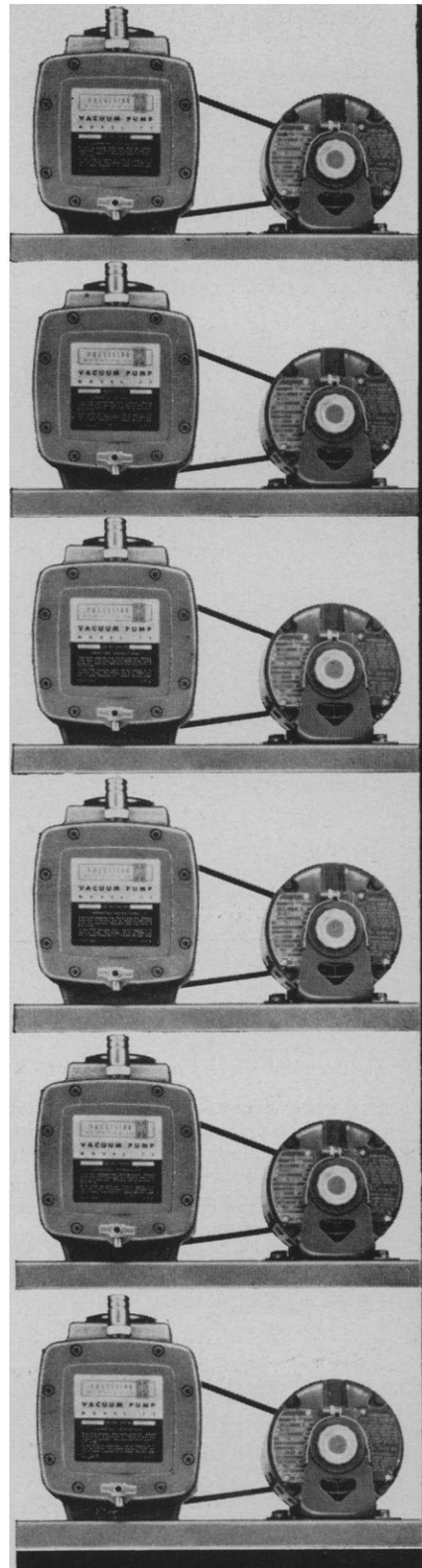
24–27. American Physical Soc., Washington, D.C. (K. K. Darrow, 538 W. 120 St., New York 27)

25–28. Society of Economic Paleontologists and Mineralogists, Denver, Colo. (J. Imbrie, Dept. of Geology, Columbia Univ., New York, N.Y.)

27–28. Diseases in Nature Transmissible to Man, 11th annual southwestern conf., College Station, Tex. (F. P. Jaggi, Jr., Agricultural and Mechanical College of Texas, College Station)

27–28. Health Education Conf., New York Acad. of Medicine, New York, N.Y. (I. Goldston, 2 E. 103 St., New York 29)

27–29. American Acad. of Neurology (members and guests), Detroit, Mich. (Mrs. J. C. McKinley, 4307 E. 50 St., Minneapolis 17, Minn.)



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27–29. Wildflower Pilgrimage, 11th annual, Great Smoky Mountains Natl. Park, Tenn. (A. J. Sharp, Dept. of Botany, Univ. of Tennessee, Knoxville)

27–5. American Psychiatric Assoc., annual, Philadelphia, Pa. (D. Blain, 1700 18 St., NW, Washington 6)

28–30. American Psychosomatic Soc., 18th annual, Atlantic City, N.J. (M. F. Reiser, 265 Nassau Road, Roosevelt, N.Y.)

30–4. Aero/Space Instrumentation Symp., 7th annual, Dallas, Tex. (W. J. Gabriel, Route 3, Box 36, Fort Worth, Tex.)

30–4. Electrochemical Soc., Indianapolis, Ind. (R. K. Shannon, 1860 Broadway, New York 23)

30–6. Conference on Internal Medicine, Nassau, Bahamas. (Bahamas Conferences, P.O. Box 1454, Nassau)

May

1–3. American Oil Chemists' Soc., St. Louis, Mo. (K. F. Mattil, Swift and Co., U.S. Yards, Chicago 9, Ill.)

2–3. American Pediatric Soc., Atlantic City, N.J. (C. M. Riley, Denver General Hospital, Denver 4, Colo.)

2–3. Association of American Physicians, Atlantic City, N.J. (P. B. Beeson, Yale Univ. School of Medicine, New Haven 11, Conn.)

2–5. Criticality Control in Chemical and Metallurgical Plant, intern. symp., OEEC, Karlsruhe, Germany. (European Nuclear Energy Agency, 38, Boulevard Suchet, Paris 16, France)

2–6. American Assoc. on Mental Deficiency, Cincinnati, Ohio. (N. A. Dayton, Mansfield Training School, Mansfield Depot, Conn.)

3–5. Nuclear Applications in Space Conf., Gatlinburg, Tenn. (J. J. Harford, American Rocket Soc., 500 Fifth Ave., New York, N.Y.)

3–6. American Goiter Assoc., Philadelphia, Pa. (J. C. McClintock, 702 Madison Ave., Albany 8, N.Y.)

3–6. Midwestern Psychological Assoc., Chicago, Ill. (I. E. Farber, Dept. of Psychology, State Univ. of Iowa, Iowa City, Iowa)

3–7. Student American Medical Assoc., Chicago, Ill. (R. F. Staudacher, 430 N. Michigan Ave., Chicago 11)

4–5. Human Factors in Electronics, 2nd natl. symp., Arlington, Va. (H. P. Birmingham, Human Engineering Development Section, U.S. Naval Research Laboratory, Washington 25)

4–5. Society for Pediatric Research, Atlantic City, N.J. (C. D. West, Children's Hospital, Cincinnati 29, Ohio)

4–6. American Ethnological Soc., Columbus, Ohio. (Miss N. F. S. Woodbury, Arizona State Museum, Univ. of Arizona, Tucson)

4–6. American Philosophical Assoc., western division, St. Louis, Mo. (L. E. Hahn, Washington Univ., St. Louis 30, Mo.)

4–6. American Soc. of Human Genetics, Atlantic City, N.J. (W. J. Schull, 1133 E. Catherine St., Ann Arbor, Mich.)

4–6. Pediatric Surgery, symp., New York, N.Y. (Office of the Associate Dean, New York Univ. Post-Graduate Medical School, 550 First Ave., New York 16)

4–6. Society for American Archaeology, Columbus, Ohio. (J. B. Wheat, Univ. of Colorado Museum, Boulder)

5–6. Population Assoc. of America, New York, N.Y. (K. B. Mayer, Dept. of Sociology and Anthropology, Brown Univ., Providence 12, R.I.)

5–7. American Soc. of Internal Medicine, Miami Beach, Fla. (G. T. Bates, 350 Post St., San Francisco 8, Calif.)

5–8. American Psychoanalytic Assoc., Chicago, Ill. (Mrs. H. Fischer, 1 E. 57 St., New York 22)

6–7. Academy of Psychoanalysis, annual, Chicago, Ill. (J. H. Merin, 49 E. 78 St., New York 21)

6–9. Circuit Theory, 5th midwestern symp., Urbana, Ill. (M. E. Van Valkenburg, Dept. of Electrical Engineering, Univ. of Illinois, Urbana)

7–10. American Inst. of Chemical Engineers, Cleveland, Ohio. (J. F. Van Antwerpen, ALChE, 25 W. 45 St., New York 36)

7–11. Institute of Food Technologists, New York, N.Y. (C. S. Lawrence, 176 W. Adams St., Chicago 3, Ill.)

7–12. Medical Library Assoc., Inc., Seattle, Wash. (Miss R. J. Mann, Mayo Clinic Library, Rochester, Minn.)

7–12. Society of American Bacteriologists, 62nd annual, Kansas City, Mo. (E. M. Foster, 311 Bacteriology, Univ. of Wisconsin, Madison 6)

7–12. Society of Motion Picture and Television Engineers, Toronto, Canada. (SMPTE, 55 W. 42 St., New York 36)

8–9. Titrimetric Methods of Analysis, symp., Cornwall, Ontario, Canada. [J. R. McCallum, Courtaulds (Canada) Ltd., Cornwall]

8–10. Aerospace Electronics Conf., 13th annual natl., Dayton, Ohio. (R. G. Stimmel, Institute of Radio Engineers, 1 E. 79 St., New York 21)

8–10. Instrument Soc. of America, Power Instrumentation Symp., 4th natl., Chicago, Ill. (H. A. Van Wassen, Duquesne Light Co., Pittsburgh 19, Pa.)

8–12. American College of Physicians, 42nd annual, Miami Beach, Fla. (ACP, 4200 Pine St., Philadelphia 4, Pa.)

8–12. American Psychiatric Assoc., 117th annual, Chicago, Ill. (C. H. H. Branch, 156 Westminster Ave., Salt Lake City, Utah)

9–11. Western Joint Computer Conf., Los Angeles, Calif. (W. F. Bauer, 8433 Fallbrook Ave., Canoga Park, Calif.)

10–12. Production Engineering Conf., Toronto, Canada. (R. B. Larson, 5701 Carnegie Ave., Cleveland 3, Ohio)

10–13. National Science Fair—International, 12th, Kansas City, Mo. (Science Service, 1719 N Street, NW, Washington 6, D.C.)

11–13. Acoustical Soc. of America, Philadelphia, Pa. (W. Waterfall, 335 E. 45 St., New York 17)

11–13. American Inst. of Industrial Engineers, annual, Detroit, Mich. (W. J. Jaffe, Newark College of Engineering, 367 High St., Newark 2, N.J.)

11–13. American Radium Soc., Colorado Springs, Colo. (C. G. Stetson, 350 Engle St., Englewood, N.J.)

15–16. Co-ordination Compounds, symp., Hamilton, Ontario, Canada. (R. J. Gillespie, McMaster Univ., Hamilton)

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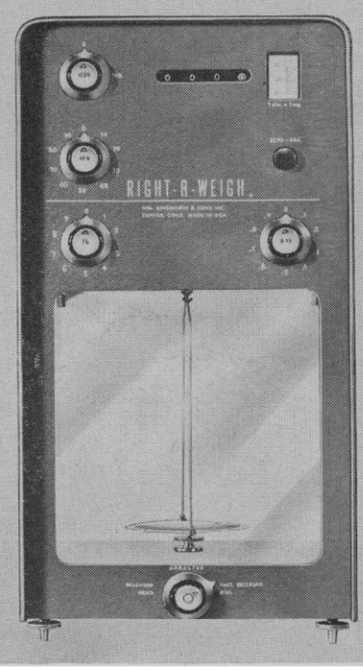

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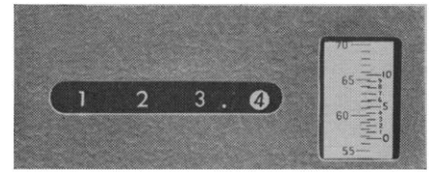

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15–17. Institute of Radio Engineers, natl. symp., Washington, D.C. (G. Shapiro, National Bureau of Standards, Washington 25)

15–17. Radiation Research Soc., annual, Washington, D.C. (E. L. Powers, Div. of Biological and Medical Research, Argonne National Laboratory, Argonne, Ill.)

15–18. Society of Aeronautical Weight Engineers, Akron, Ohio. (D. B. Block, 4004 Oxford Ave., NW, Masillon, Ohio)

15–18. Spectroscopy, 12th annual symp., Chicago, Ill. (W. Ashby, Continental Can Co., Inc., 7622 S. Racine Ave., Chicago 20, Ill.)

15–20. Conference on Nuclear Electronics, Belgrade, Yugoslavia. (J. Burt, International Atomic Energy Agency, United Nations, New York, N.Y.)

16–18. Western Conf. on Anesthesiology, biennial, Portland, Ore. (J. O. Branford, 2307 NW Overton St., Portland 9, Ore.)

16–20. American College of Cardiology, New York, N.Y. (P. Reichert, 350 Fifth Ave., Empire State Bldg., New York 1)

18–20. Host Tumor Interactions, intern. symp., Detroit, Mich. (M. J. Brennan, Oncology Div., Henry Ford Hospital, Detroit 2)

22–24. American Thoracic Soc., Cincinnati, Ohio. (F. W. Webster, 1790 Broadway, New York 19)

22–24. Global Communications, 5th natl. symp., Chicago, Ill. (R. D. Slayton, 5555 Touhy Ave., Skokie, Ill.)

22–24. Telemetering Conf., natl., Chicago, Ill. (J. Becker, AC Spark Plug Division, General Motors Corp., Milwaukee 1, Wis.)

22–25. American Urological Assoc., Los Angeles, Calif. (W. P. Didusch, 1120 N. Charles St., Baltimore 1, Md.)

22–25. Design Engineering Conf. and Show, Detroit, Mich. (ASME Meetings Dept., 29 W. 39 St., New York 18)

22–25. National Tuberculosis Assoc., Cincinnati, Ohio. (J. G. Stone, 1790 Broadway, New York 19)

22–26. Engineering Conf. and Exhibit, 29th annual, New York, N.Y. (G. E. Seeley, ASTME Headquarters, 10700 Puritan Ave., Detroit 38, Mich.)

22–26. Society of Photographic Scientists and Engineers, annual, Binghamton, N.Y. (M. G. Anderson, Ansco, Vestal Parkway East, Binghamton, N.Y.)

22–27. International Acad. of Legal Medicine and of Social Medicine, 5th cong., Vienna, Austria. (M. Helpern, Chief Medical Examiner, City of New York, 55 East End Ave., New York 28)

23–25. Large Capacity Memory Techniques for Computing Systems, symp., Washington, D.C. (Miss J. Leno, Code 430A, Office of Naval Research, Washington 25)

25. Gastroenterology Research Group, Chicago, Ill. (N. C. Hightower, Scott and White Clinic, Temple, Tex.)

25–26. Medical Technology, symp., Cleveland, Ohio. (J. W. King, Cleveland Clinic, 2020 E. 93 St., Cleveland 6)

25–26. Nitro Aliphatic Chemistry, symp., Lafayette, Ind. (Purdue Memorial Union, Purdue Univ., Lafayette)

25–26. Operations Research Soc. of America, 9th annual, Chicago, Ill. (D. Schiller, Gaywood-Schiller Associates, 203 N. Wabash Ave., Chicago 1)

26–27. American Otological Soc., Lake Placid Club, Essex County, N.Y. (J. A. Moore, 525 E. 68 St., New York 21)

26–3. American Acad. of Dental Medicine, cruise to Bermuda and Nassau. (H. Ward, 15 Bond St., Great Neck, N.Y.)

28–1. Special Libraries Assoc., 52nd annual, San Francisco, Calif. (B. M. Woods, SLA, 31 E. 10 St., New York 3)

29–31. American Gynecological Soc., Colorado Springs, Colo. (A. H. Aldridge, 899 Park Ave., New York 21)

29–31. Cancer Symp., 6th annual, Regina, Saskatchewan, Canada. (A. J. S. Bryant, Allan Blair Memorial Clinic, Regina)

29–31. Chemical Inst. of Canada, 44th annual, Ottawa. (Chemical Inst. of Canada, 48 Rideau St., Ottawa 2)

29–3. Molecular Spectroscopy, 5th European cong., Amsterdam, Netherlands. (D. H. Zijp, Secy., Nieuwe Achtergracht 123, Amsterdam-C.)

30. Nutrition Soc. of Canada, 4th annual, Guelph, Ontario. (E. V. Evans, Dept. of Nutrition, Ontario Agricultural College, Guelph)

31–2. Canadian Federation of Biological Societies, Guelph, Ontario, Canada. (E. H. Bensley, Montreal General Hospital, 1650 Cedar Ave., Montreal 25, P.Q.)

31–2. Radar symp., 7th annual, Ann Arbor, Mich. (Coordinator, 7th Annual Radar Symposium, Institute of Science and Technology, Box 618, Ann Arbor)

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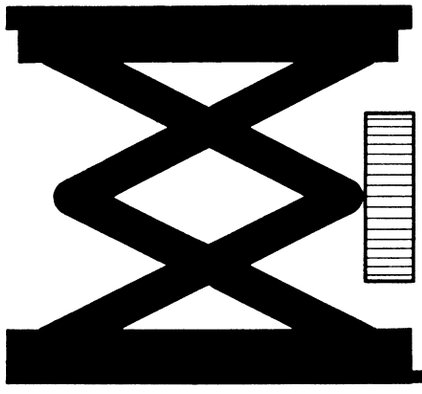

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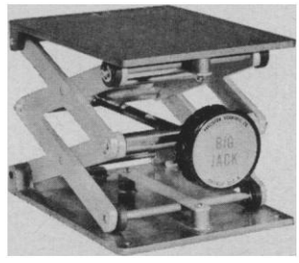

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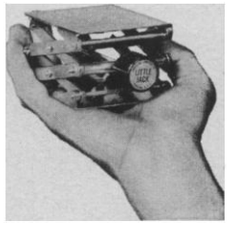

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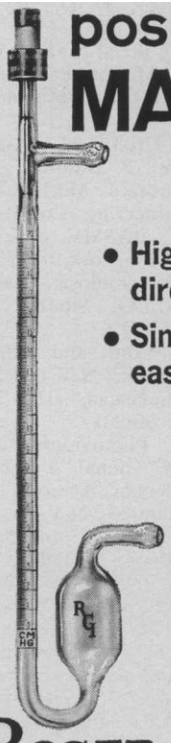

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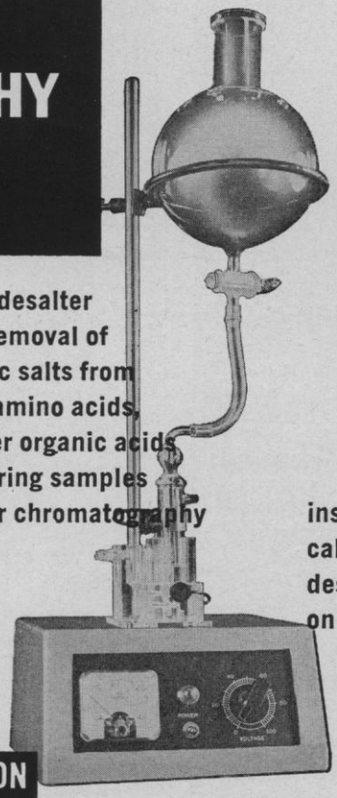

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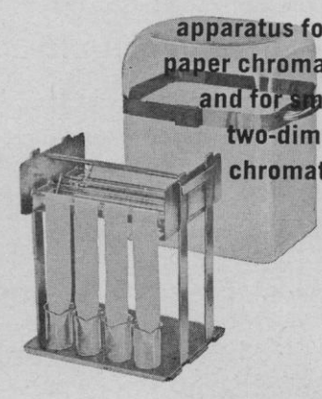

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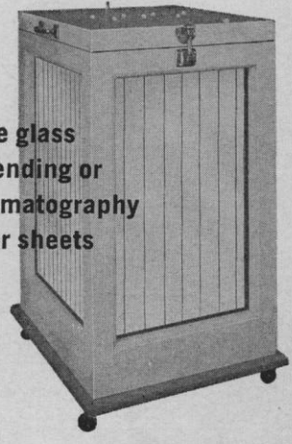

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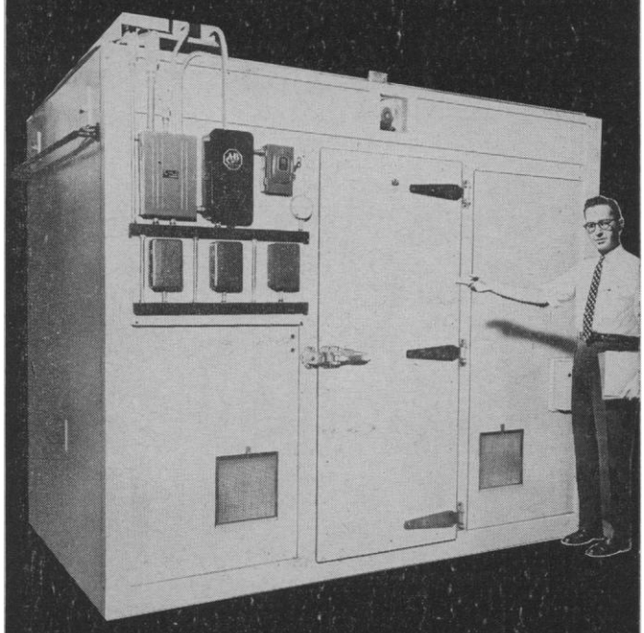

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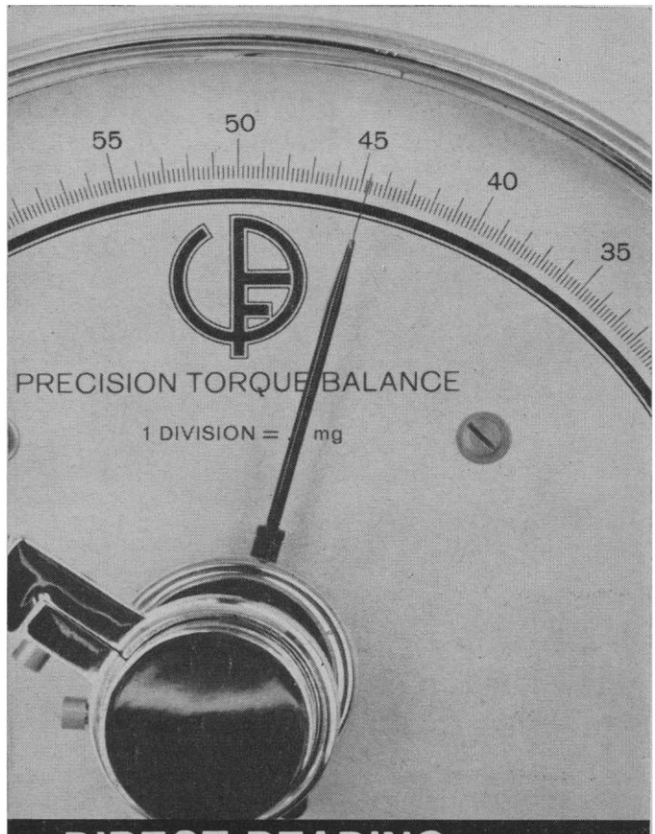

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■ MICROFILM FACSIMILE SYSTEM is capable of transmitting information at rates equivalent to a single-space typing rate of 43,200 words per minute in one model and 20,400 per minute in a second model. Input accepted by the transmitter is a positive or negative microfilm image. The information to be transmitted is scanned and dissected by a mechano-optical device which uses a fixed slot and a multiple-radial-slit rotating disc. The output signal is baseband video plus blacker-than-black synchronizing pulse. Bandwidth is approximately 400 kcy/sec in the faster model. Signal level is 1.0-volt peak; output impedance is 50 ohms. The receiving recorder is a multiple-stylus and printer type with 1800 styluses spaced 0.01 in. apart in the faster model. The record is produced on a special facsimile paper. In the faster model chart length is 1000 ft, chart width is 18.5 in., and chart feed rate is 4 in./sec. Signal input requirements are matched to the transmitter output. (Hogan Faximile Corporation, Dept. Sci123, 635 Greenwich St., New York, N.Y.)

■ ANGLE INDICATOR provides numerical indication of the angular position of mechanical devices to which remote two-speed (25:1) dual transmitters can be coupled. Accuracy of the two-speed unit is said to be ±15 sec of arc, and repeatability is said to be ±12 sec. Slewing speed is 180 deg in 9 sec; operating power is 115 v 400 cy/sec. (General Precision Inc., Dept. Sci115, 1150 McBride Ave., Little Falls, N.J.)

■ ANGLE ENCODING TRANSDUCER AND ROTATIONAL ANALYZER are combined into a system for study of rotation-associated phenomena in machinery (Fig. 1). The two units adapt an oscilloscope to provide horizontal trace deflection proportional to angular displacement of a rotating shaft. The system generates a horizontal sweep representing shaft angle at speeds from essentially 0 to 20,000 rev/min. Transduced data, such as velocity, pressure, acceleration, or vibration, appear on the cathode-ray-tube screen correctly referenced to instantaneous angular position. (Tektronix, Inc., Dept. Sci106, Post Office Box 500, Beaverton, Ore.)

■ LABORATORY PROCESS MONITOR is designed to program, monitor, and control reactions and distillations automatically. Variations in circuitry can be provided to fit individual program needs. Above ambient temperature reactions may be controlled with heaters; below ambient temperature they may be controlled with coolant solenoids activated as required. (Scientific Glass Apparatus Co., Dept. Sci124, Bloomfield, N.J.)

■ TRANSMITTING FLOWMETER combines the functions of an indicating rotameter and a pneumatic transmitter. The instrument measures liquid or gas flow with accuracy said to be within ±1 percent over its 10 to 1 range. The float in the metering tube is magnetically coupled to a rotating follower in the pneumatic transmitter. The transmitter can be used with any metering tube ½ to 2 in. in diameter; it covers flows equivalent to 1 to 100 gal of water per minute and 4 to 400 standard ft^3 of air per minute. Steam-jacketed and high-pressure models are available. (Fischer & Porter Co., Dept. Sci119, 676 Jacksonville Rd., Warminster, Pa.)

■ DISSOLVED OXYGEN ANALYZER measures the concentration of dissolved oxygen in a variety of fluid systems—including sewage; biological, natural, and fresh sea water; and some organic media. Operation is based on the measurement of current produced by the reduction of oxygen at a platinum electrode. The instrument also provides temperature measurements and temperature compensation. The instrument's transistorized amplifier is battery operated and is said to have no zero drift. (Jarrell-Ash Co., Dept. Sci103, 26 Farwell St., Newtonville 60, Mass.)

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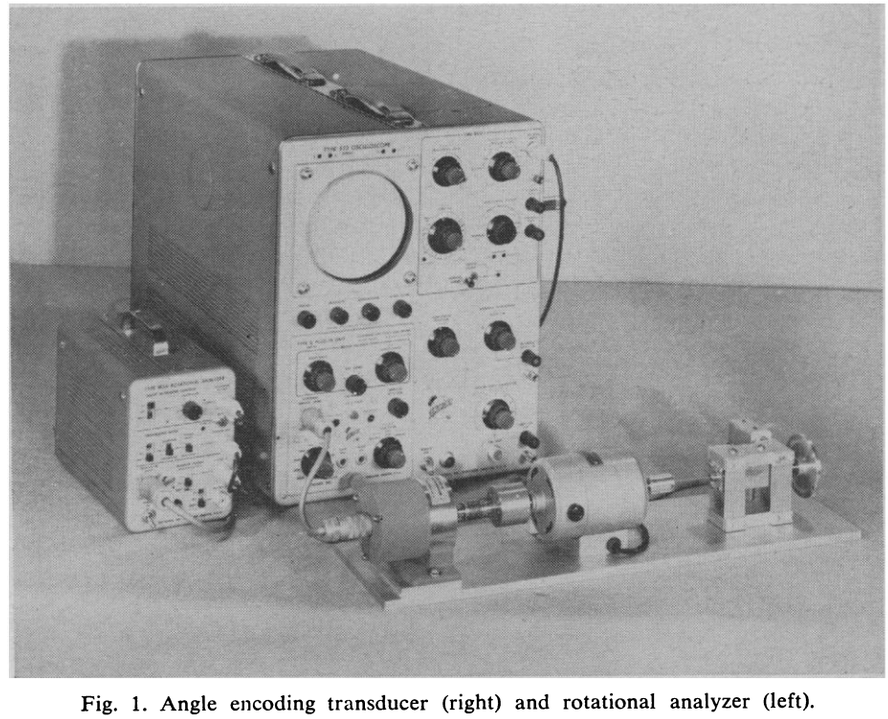

Fig. 1. Angle encoding transducer (right) and rotational analyzer (left).

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Neither Science *nor the writer assumes responsibility for the accuracy of the information. A Readers' Service card for use in mailing inquiries concerning the items listed is included on pages 815 and 929. Circle the department number of the items in which you are interested on this card.*

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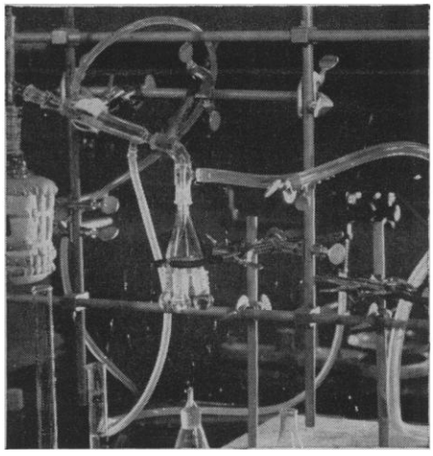

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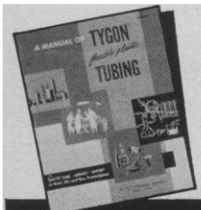

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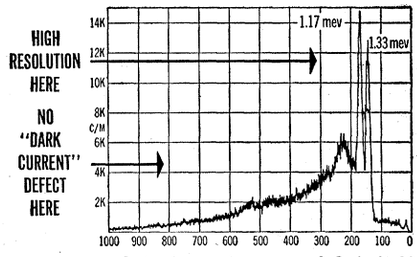
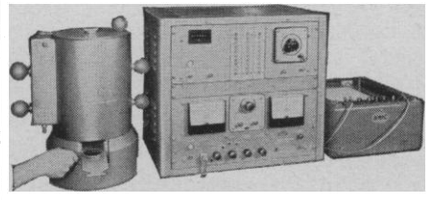


Actual spectrogram of Cobalt-60 made with a GSS-1. Window No. 3. Time constant: 2 sec.



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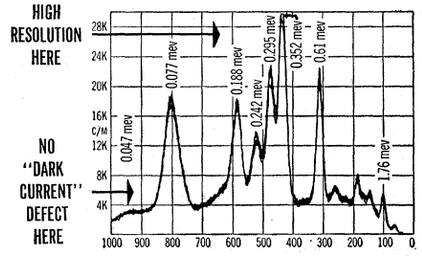

Actual spectrogram of Radium made with a GSS-1. Four runs over a period of 20 hours with no adjustment of instrument between runs, demonstrating utmost stability. Range: 30,000 C/M. Time constant: 3 sec.

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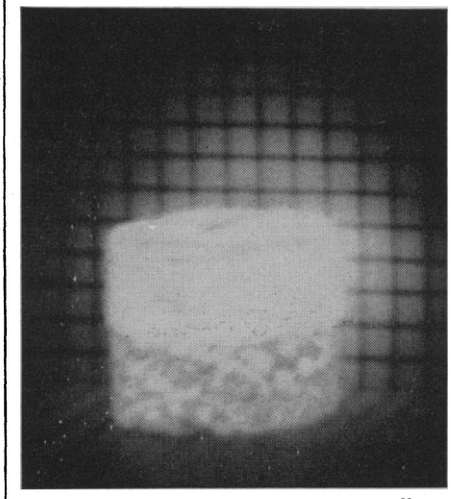

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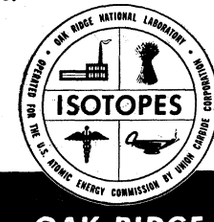

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■ VIBRATION CALIBRATOR consists of a transistorized electromechanical oscillator and a battery-operated cylindrical shaker. The instrument provides a standard acceleration for 1 grav r.m.s. at 100 cy/sec. Acceleration output appears at two pillbox-shaped, 50-g disks mounted on an internal cylinder extending through the instrument and projecting at the sides. Acceleration accuracy is said to be ±10 percent, frequency accuracy ±1 percent. Battery life is 100 hr. (General Radio Co., Dept. Sci120, West Concord, Mass.)

■ SWEEP DRIVE automatically sweeps oscillators and other tunable devices through their frequency ranges. The device has two sweep speeds and a neutral setting; in neutral position, tuning can be performed manually. Sweeps can be reversed or stopped automatically when limits are reached. Stops can be set for any sweep range from 5 deg rotation to 50 revolutions of the output shaft. One model provides an output voltage proportional to shaft position for driving the x axis of a recorder or oscilloscope. (Hewlett Packard Co., Dept. Sci125, 1501 Page Mill Rd., Palo Alto, Calif.)

■ HIGH VOLTAGE IMPULSE GENERATORS are available for 700, 1400, 2100, or 3500 kv for use in testing cables, insulating materials, electrical equipment, and lightning protection systems. The equipment utilizes a number of capacitors that are charged up to a maximum of 175 kv when connected in parallel. By means of spark gaps the capacitors are suddenly connected in series, and the test object receives a high voltage impulse which is approximately equal to the product of the number of capacitors and the charging voltage. Spark-gap distance can be adjusted for any

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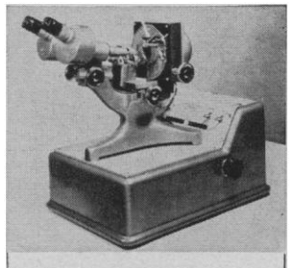

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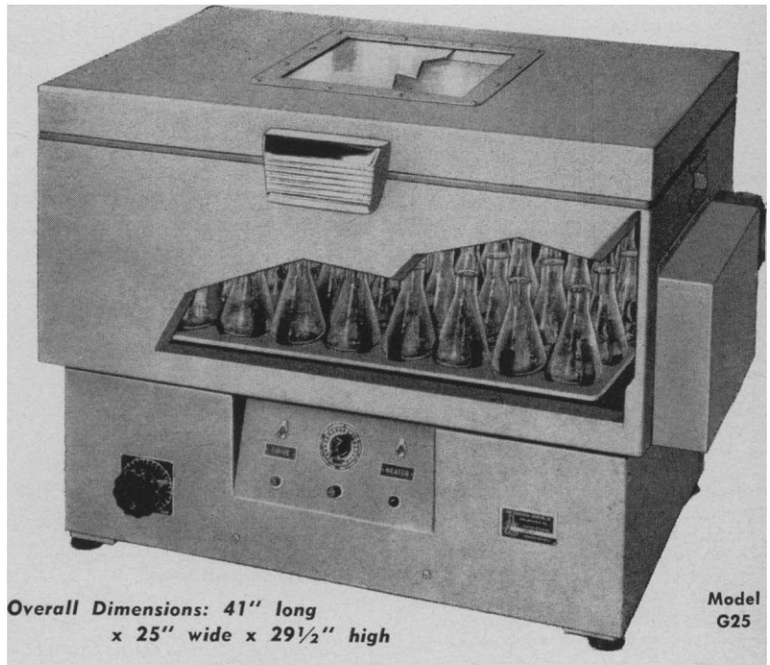

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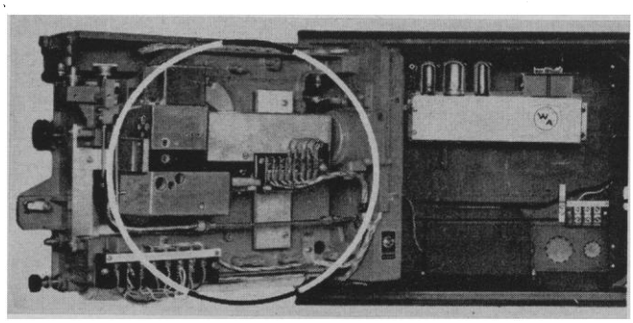

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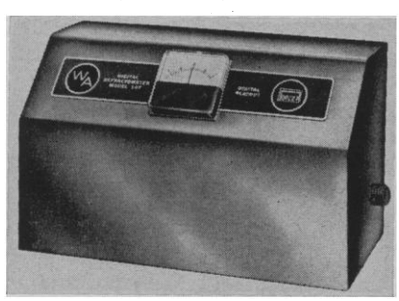

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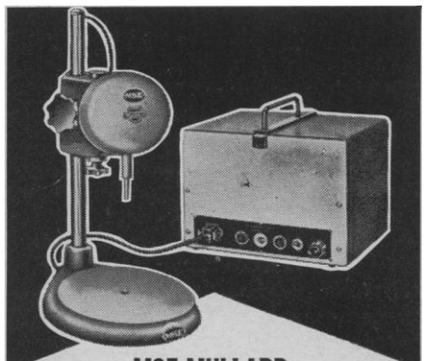

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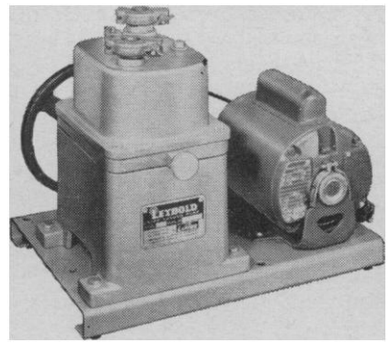

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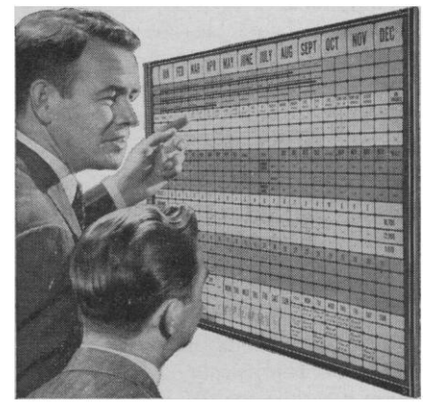

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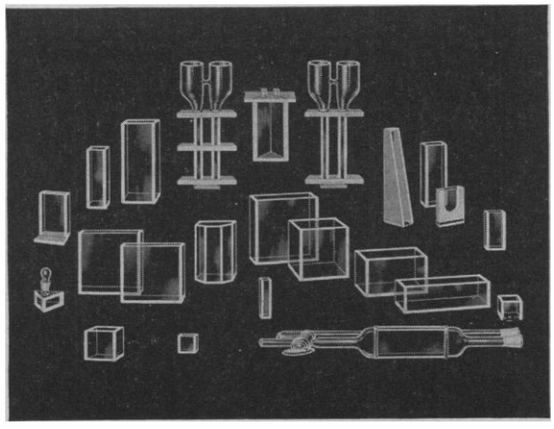

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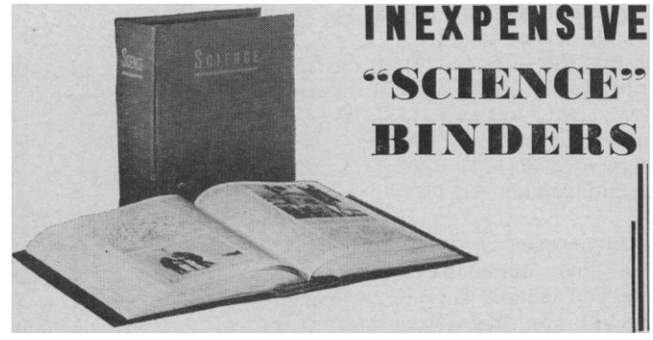

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Letters

(*Continued from page 844*)

that she is no Milton, which might seem irrelevant, except that I believe none but a great poet is qualified to deal, poetically, with the lofty themes she set herself. I will not try to discuss her imagery or style, but I must mention a thoroughly unpoetic trick, of throwing away her climaxes on resounding truisms like "The fecundity of life is to assure survival," or "To birth, death is a force reciprocal." One expects more of poetry than that, and when I say it lends itself to mockery I am not trying to be clever, I am thinking of its effect on the opposition.

As tactics, then, the poem can be described as a distinguished failure, but larger considerations are raised by asking why this failure was necessary—why did the work attempt so much? Its proximate occasion, of course, is the urgent need for preservation of wilderness. That is what the photographs plead for so eloquently, and when I wrote that the mood of the text is relentlessly epic rather than lyric I meant that it protests far too much to serve its obvious purpose. The preservation of wilderness *is* urgent, and it is a moral as well as a political issue, but who are its political opponents? Granted that some of them are short-sighted or mercenary exploiters who might be stirred into "caring enough to act, and to act in time," as Brower puts it, what is a reasonably foresighted developer to make of the call "to learn again to walk with Eden's angels"? More to the point, perhaps, I doubt that a psychiatrist or city planner will be impressed by the lines "How simple our basic needs—/a little food, sun, air, water, shelter, warmth, and sleep!" One can agree that "of all resources, the most crucial is Man's spirit," without necessarily supposing that passage of the Wilderness Bill will alleviate the problems of mental health and juvenile delinquency; in the arena where conflicting political demands can claim equal moral justification the apocalyptic tone of Nancy Newhall's text seems to me poor strategy. It preaches to the converted, and irritates potential allies.

I am sorry that so fine a book, and so nobly meant an effort, should have called forth these curmudgeonly reflections, but their intention is entirely friendly to the cause of conservation, if not to all its methods.

Perhaps I should add that any dispute Brower and I have about "ecologically sound use" of resources must be purely semantic, hinging on the word *ecology*. Otherwise I cannot guess why my definition is "totally inadequate." I include man's spiritual aspirations and his psychic need for solitude and beauty

(as well as for social companionship) within my definition, and I think I also know well the scientific or archival function that undisturbed areas must serve for future ecologists. Presumably Brower reads "multiple use" of wilderness preserves, a slogan of some of his opponents, and he knows, as I do, that by that self-contradictory philosophy Mount McKinley National Park will suffer the fate of Walden Pond. But as a historian of environments, I also know that *absolute* freedom from human disturbance has been unattainable since the Neolithic age began. I am just optimistic enough about human character to suspect that future generations will find their Waldens in places as tame as Thoreau's Walden must have seemed to John Muir. The melancholy fact is that most of them will have to.

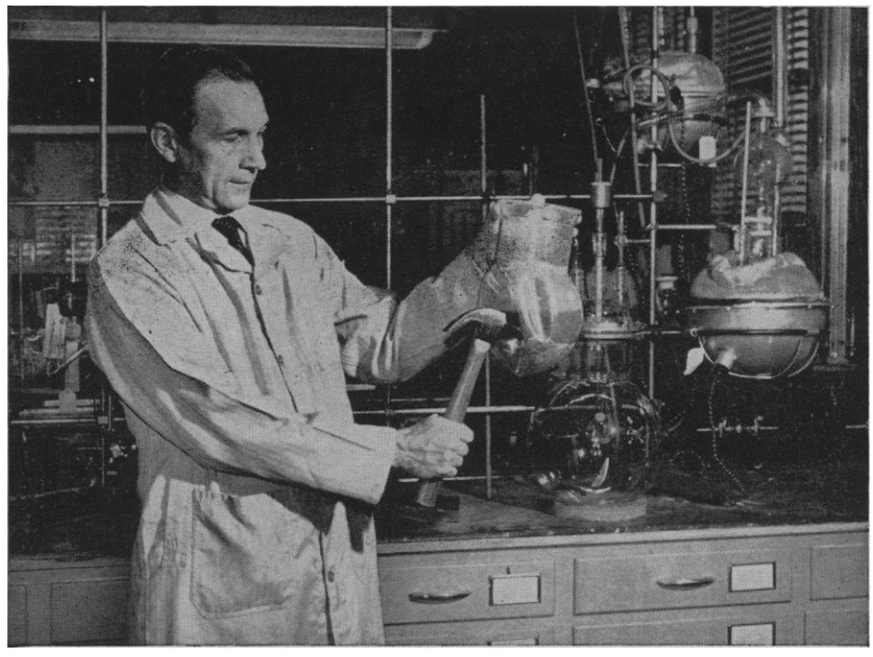
EDWARD S. DEEVEY
Yale University,
New Haven, Connecticut

Imprinting

It is regrettable that the term *imprinting* appears headed for the same semantic limbo that *instinct, innate,* and similar, once useful, terms attained some years earlier. This trend toward confusion has certainly not been retarded by the two most recent publications dealing with the subject of imprinting (*1, 2*).

Imprinting has generally been regarded as a somewhat distinctive form of learning (*3*). Its primary characteristics appear to be a restriction of its occurrence to a fixed and relatively brief period in the life of an organism, the absence of overt reenforcement apart from that provided by the subject's response, and a relative stability of the preference that develops for the imprinted surrogate. Hess (*4*) has adumbrated some additional characteristics, though the significance of some of these (for example, differential effects of drugs) is questionable.

Now it should be made clear that all but possibly one of these characters is common to forms of learning that have not, in the past, been considered instances of imprinting. Latent learning characteristically may occur in the absence of overt reenforcement; single-trial conditioning is also not unknown (*5*), nor is the stability of the imprinted response as irreversible as was originally supposed by Lorenz (*6*). The only factor in regard to which one can still assert the uniqueness of imprinting is its temporal fixity: if exposure to a surrogate does not occur within a limited period during the development of the organism—the critical period—the preference for that surrogate does not



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develop. If one argues that the concept of imprinting does not involve the notion of a critical period, limited in time, one can no longer defend the view that there is anything unique about imprinting as a learning process.

I have argued elsewhere (*7*) for the view that intermediate processes link imprinting to conventional types of learning. However, since we do know that some kinds of responses can be established only by exposure to the relevant stimuli during a specific and brief period in the organism's life, and that the response is linked to that stimulus in the absence of overt reenforcement, it does make sense to regard this type of learning as moderately distinct and to call it imprinting.

The papers originally cited, therefore, are deemed misleading on the following grounds.

1) Gray's (*1*) periods of exposure to the model extended for intervals of 24 hours and to ages of up to 5 days after hatching. How he can still assert that he has disproved James's contentions (*8*) when James adhered to our more precise definition of imprinting is difficult to understand. Under normal conditions, one might expect the result obtained by Gray to be attainable at *any* period in the life of the chick. In contrast, James's results can be expected only from chicks of a specific age, the critical period.

2) Moltz's (*2*) efforts to redefine "imprinting" operationally are manifestly pointless. When he ignores his own dicta and continues, in his discussion, to use *imprinting* in a manner differing from his own definition, he compounds confusion.

Finally, I wish to assert that much of the dissatisfaction with the critical-period criterion for the occurrence of imprinting has been assuaged. The difficulty has generally lain in the fact that no two workers could agree on the temporal definition of the critical period. It has recently been suggested (*9*) and demonstrated (*10*) that this has been due to age determinations having been based on the event of hatching (in birds, at least), an event notoriously susceptible to environmental influences. When age determinations are based upon developmental age—that is, time elapsed since the onset of blastulation—no such major discrepancies appear. Thus, it appears entirely reasonable and empirically valid to define imprinting as a rapid form of learning limited in its occurrence to specific developmental stages. That, after all, was what Lorenz (*11*) originally stated.

PETER H. KLOPFER
Zoology Department, Duke University, Durham, North Carolina

References and Notes

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11. K. Lorenz, *J. Ornithol.* **80**, 50 (1932).

I should like to observe that, besides being a capable investigator, Klopfer has an ability to identify the important points at issue in the theoretical treatment of imprinting. But I cannot agree with some of his criticisms.

First, he seems to object to the fact that I did not refer to a critical period in my most recent article on the subject of imprinting (*1*). The puzzling feature of the objection is that Klopfer did not mention any of the three earlier articles on imprinting of which I was author or co-author, wherein criticality *was* discussed (*2,3*). His apparent inclusion of me among those who do not give proper attention to criticality might therefore be construed as something less than correct. The reason I neglected criticality in the article in question was the absence of appositeness.

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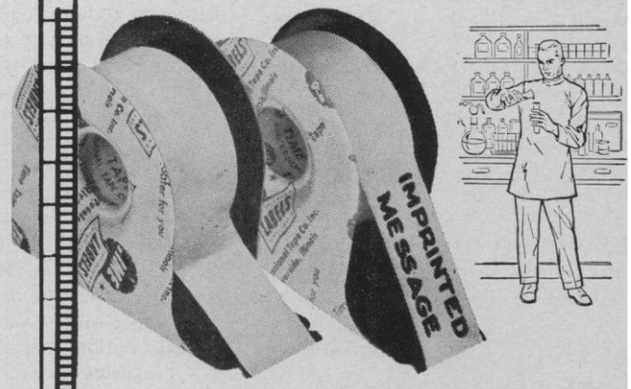

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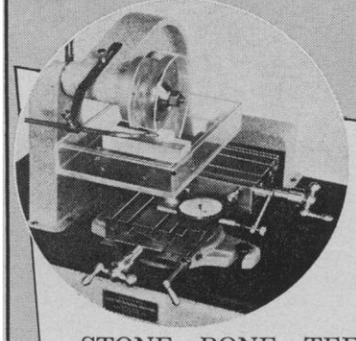

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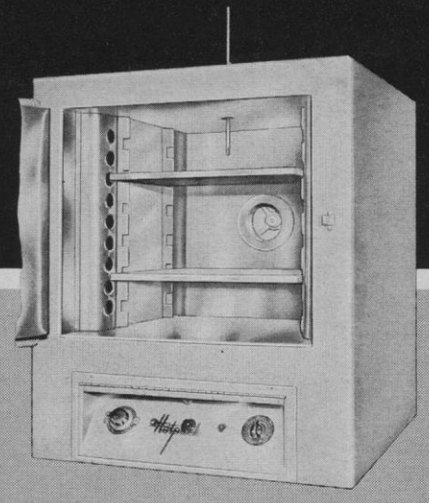

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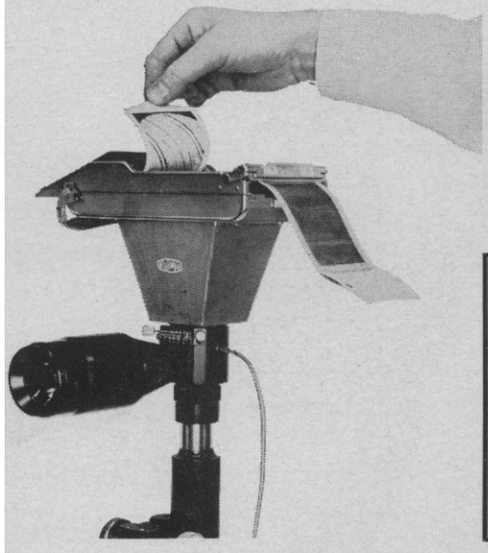

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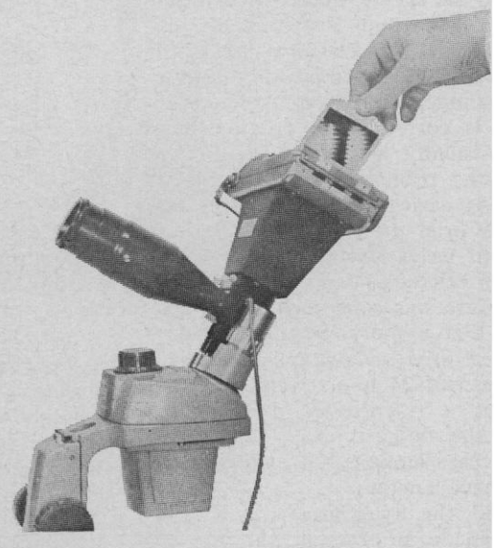

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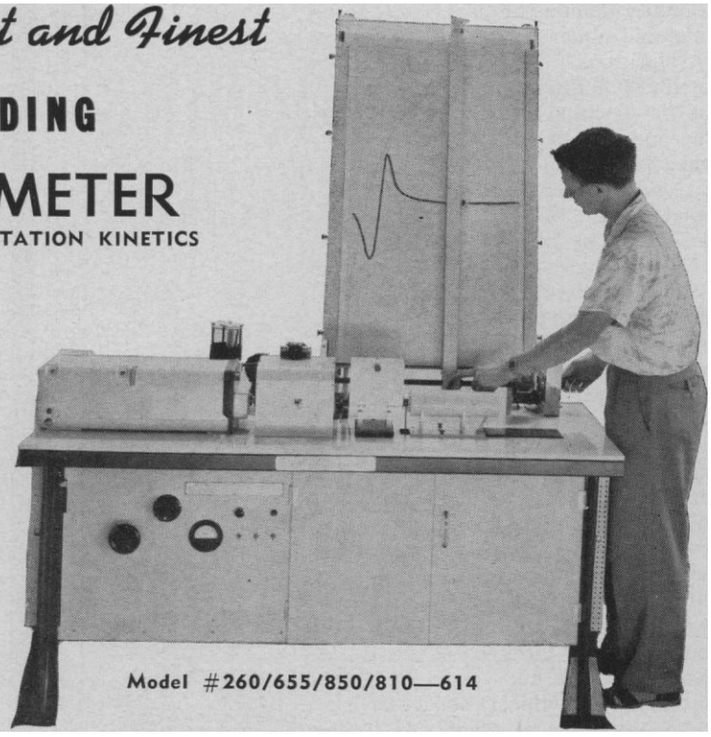

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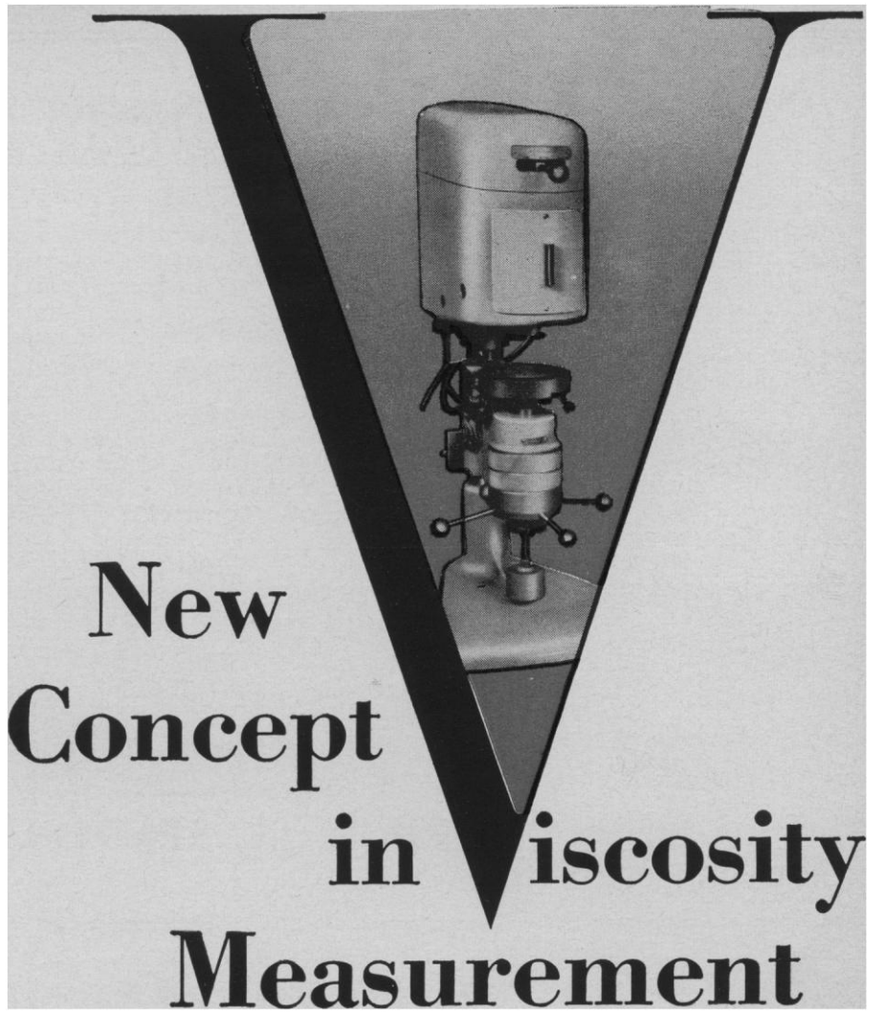
less objects was aimed at James's (*4*) discussion of retinal flicker in imprinting, a discussion which emanated from what I felt to be a misinterpretation of otherwise perfectly valid data. I have tried, and failed, to understand why Klopfer does not believe my experiment to be a test of James's interpretation. Since my first independent group was started at the age at which James began his dependent groups (experimental and control), Klopfer seems to be saying that I cannot adduce imprinting because I did use independent groups. If my inference is correct, then Klopfer does not consistently hold to his listing of one of the primary characteristics of imprinting as learning in a "fixed and relatively brief period." My period appeared to be as fixed as James's and was even briefer so far as range of age on exposure was concerned; if Klopfer implies that my experiment is invalid because I used a total exposure time per subject of 24 hours whereas James used but a fraction of this, then I haven't the faintest idea what to say except that Klopfer has one opinion and I have another.

Second, the hypothesis which Klopfer advances to account for the differences in critical-period topography from experimenter to experimenter is ingenious and, to a zoologist, undoubtedly plausible. I myself doubt that variation in the onset of blastulation would explain anything more than the subject variability demonstrated when a group is exposed under a certain condition to a certain model, and even here I doubt that the hypothesis is sufficient. This is not the place to present experimental data, but perhaps I may say that I have evidence indicating it is differences in the *models* which produce some, and perhaps most, of the discrepancies in the topography of the critical period.

This finding indicates to me at least, a genetic coding in the animal for reactivity to characteristics of the biologically natural social companion, which we may have been approximating in various degrees with this and that different kind of model. I have evidence that releasers can play a role in the responsiveness of chicks to novel objects, a more significant role than contemporary researchers may be prepared to accept (*5*). I wonder if Klopfer's hypothesis, whatever its validity, does not encourage disdain of the fact that developmental level of behavior can be identified only through behavioral research. We are not yet in that ideal (and perhaps mythical) stage of science where physiological events can predict behavioral events previously unknown. Until that time, and provided Klopfer's hypothesis is not absolutely valid, I suspect that dating the age of a subject from birth (or hatching) will work as handily as dating from blastulation.



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Third, as a zoologist Klopfer ought to realize that much of the reason for dispute about imprinting is that theorists refuse to consider a functional theory of imprinting. It is not that such a theory is nonexistent, because several years ago I published my opinion that the function of the process of imprinting is the establishment of a social bond between the young and its parents, whether in animals or man (*3*). The study of imprinting is the analytical investigation of this process, with emphasis on the behavior of the young. By seizing upon the more romantic elements of imprinting in birds, such as the rapid learning evidenced in some cases and the ability of any researcher to become a Pied Piper of sorts, investigators and theorists alike have ignored the most elementary of all questions in the delineation of a behavioral process: what the process does for the species to help it survive and procreate. Behaviorists may yet regret the day they forgot their Darwin.

Fourth, while Klopfer's allusion to the history of the study of imprinting is by way of being an expository device, I should like to submit a few words about this history, if the rather inadequate knowledge now commonly met with can be called history (*6*). Imprinting was discovered by Spalding, who was also, as nearly as I can determine, the man who first isolated critical periods (*7*). William James gave us our first systematic definition of criticality in behavior and first stated the opinion that the process we now call imprinting is ended by the onset of the fear period (*8*). That aspect of imprinting theory accredited to Lorenz (*9*)—that imprinting involves a rapid learning of the first moving object that the hatchling sees—was previously stated in its essentials by Heinroth (*10*), who apparently mixed long-known research facts with the quite peculiar learning theory of the German philosopher Hermann Samuel Reimarus (*11*). It was Reimarus the Cartesian who originated the conception that lower animals learn what they need to learn in a rapid manner to complement their instincts (compare Lorenz's similar conception in regard to imprinting); from Reimarus's point of view this rapid learning was possible because animals cannot learn very much.

While Lorenz should be given all possible credit for emphasizing the importance of imprinting, he cannot be given credit for a theory the basic outlines of which are not his. Nor should he be given credit for inventing the term *imprinting*, which is a translation of the German term *einzuprägen* used by Heinroth (*10*), which in turn bears strong resemblance to the term *stamping-in* frequently employed by Douglas Spalding (*7*).

PHILIP HOWARD GRAY
Department of Psychology,
Montana State College, Bozeman

References and Notes

1. P. H. Gray, *Science* **132**, 1834 (1960).
2. ——— and K. I. Howard, *Perceptual Motor Skills* **7**, 301 (1957); D. M. Baer and P. H. Gray, *ibid.* **10**, 171 (1960).
3. P. H. Gray, *J. Psychol.* **46**, 155 (1958).
4. H. James, *Can. J. Psychol.* **13**, 59 (1959).
5. P. H. Gray, *J. Comp. Physiol. Psychol.*, in press.
6. For example, H. Moltz [*Psychol. Bull.* **57**, 291 (1960)] has reviewed the history of imprinting with practically no consideration of the long list of researchers, from Spalding onward, who investigated the phenomenon of imprinting without express use of that rubric. The names of some of the men who so contributed to our knowledge of this behavioral process would, I feel sure, be not a little surprising to the nonhistorian.
7. D. A. Spalding, *Macmillan's Mag.* **27**, 282 (1873).
8. W. James, *Principles of Psychology* (Holt, New York, 1890).
9. K. Z. Lorenz, *Auk* **54**, 245 (1937).
10. O. Heinroth, *Verhandl. intern. ornithol. Kong. 5. Kong. 1910* (1911), vol. 5, p. 589.
11. The writings of H. S. Reimarus are not generally available, but a critical discussion of his theory may be found in the English translation by his contemporary, C. G. Leroy [*The Intelligence and Perfectibility of Animals from a Philosophic Point of View with a Few Letters on Man* (Chapman and Hall, London, 1870)]. There is also the more sympathetic discussion in G. S. Brett, *A History of Psychology: Medieval and Early Modern Period* (Allen and Unwin, London, 1921).

Klopfer states that my "efforts to redefine 'imprinting' operationally are manifestly pointless." Considering his emphasis on the critical period, I suspect that his dissatisfaction stemmed

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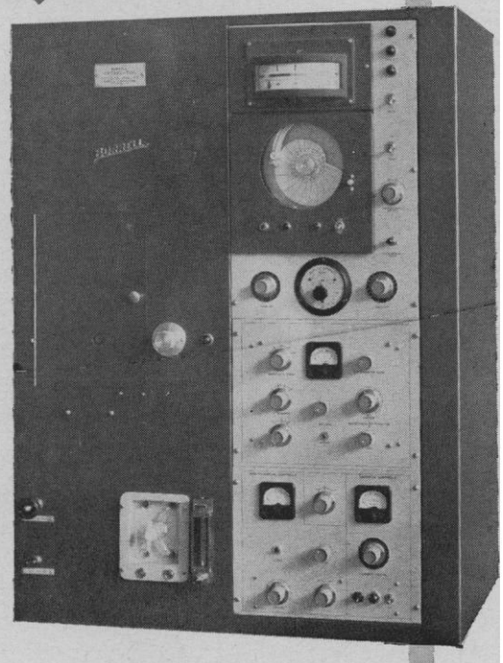

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from the belief that I ignored the "temporal fixity" of imprinting, a characteristic which he regards (and rightly so) as unique. The following definition, contained in the article to which Klopfer refers, makes it evident that I did no such thing: "Thus, imprinting will be defined as the procedure of visually presenting to an animal a large moving object *during the first several hours of its life* under conditions that insure that the object is not associated with such conventional reinforcing agents as food and water" (italics added).

Klopfer also states that I ignored my "own dicta" and that I thereby compounded confusion. I must admit that I am unceratin as to what he intended to convey. To which dicta (or even dictum) is Klopfer referring? What is the nature of the confusion? To what extent have I compounded it?

In conclusion, may I say that it does not appear unreasonable to expect a scientist to be explicit when criticizing the work of another and to offer at least some evidence in substantiation of a sweeping dismissal.

HOWARD MOLTZ
Department of Psychology, Brooklyn College, Brooklyn, New York

Handling Scientific Information

In a recent issue of *Science* [**132**, 1922 (1960)], Helen Brownson, in the article "Research on handling scientific information," makes the following statement: ". . . the essential problem of applying machines to the handling of scientific information on a large scale has yet to be solved. This unsolved problem has to do with means of analyzing the subject content, meaning, and relevance of documents for mechanized handling. Research directed toward this end is making progress but is still in its infancy."

What Helen Brownson calls *the* unsolved problem is really a pseudo-problem which cannot delineate or define a fruitful field for research. In *The Mathematical Theory of Communication*, by Shannon and Weaver, the following two statements appear: (i) "The semantic aspects of communication are irrelevant to the engineering aspects." (ii) "This does not mean that the engineering aspects are necessarily irrelevant to the semantic aspects."

If one properly understands these two statements, one can also understand why mechanized systems and coding can contribute to the semantic aspects of information storage and retrieval systems and why semantic considerations cannot contribute to the solution of problems of mechanization (engineering aspects). Suppose one wished to develop a high-fidelity system

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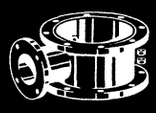

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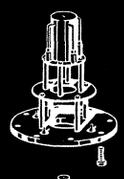

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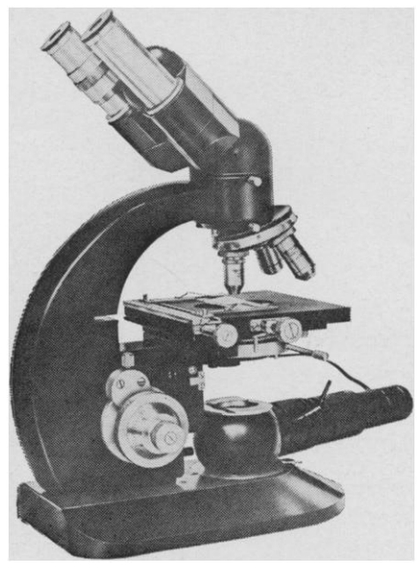

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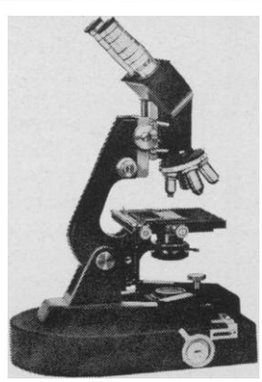

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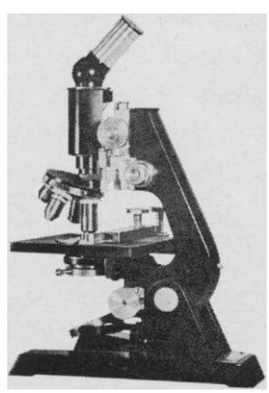

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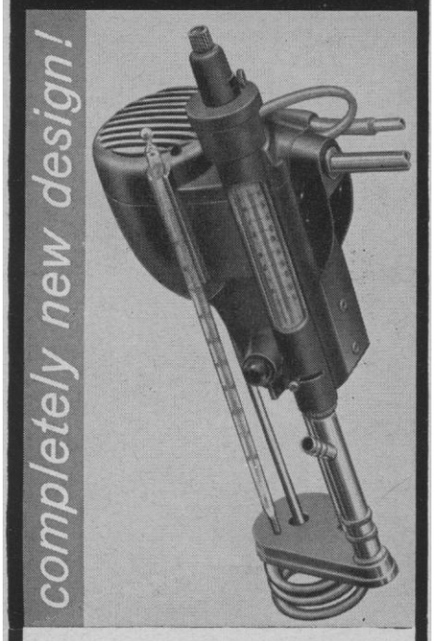

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In brief, the type of research described by Helen Brownson can only be justified by denying Shannon's statement that the semantic aspects of communication are irrelevant to the engineering aspects. This is not usually understood, because this statement is confused with the converse statement—that engineering aspects are relevant to semantic aspects.

MORTIMER TAUBE
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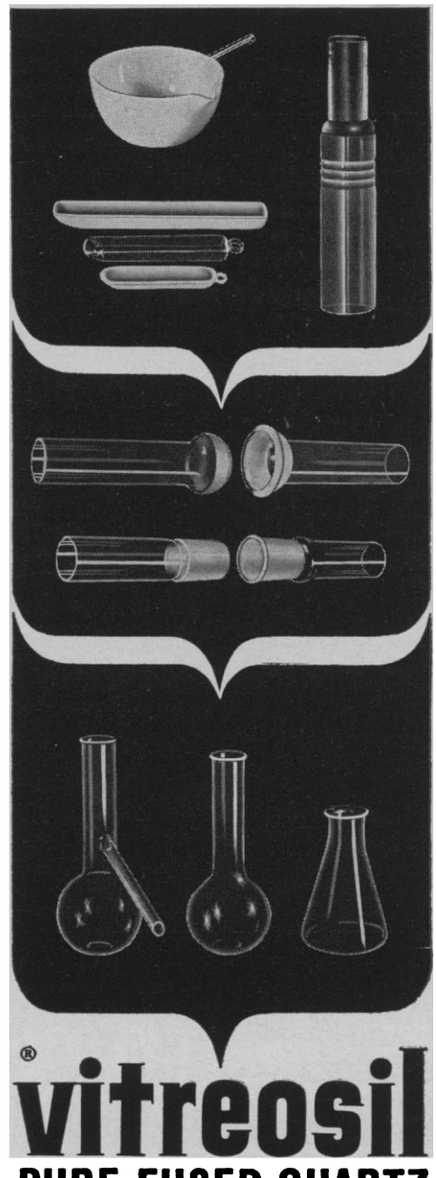
I do not believe the quotations Taube gives from Shannon and Weaver are relevant to the broad problem discussed in my article, which is much more than an engineering problem. In his statement about "the semantic aspects of communication," Shannon was using *communication* in a very special sense—namely, the transmission of messages from one point to another. The paragraph containing that statement begins: "The fundamental problem of communication is that of reproducing at one point either exactly or approximately a message selected at another point" (p. 3). Weaver provides further clarification: "The mathematical theory of the engineering aspects of communication . . . admittedly applies in the first instance only to . . . the technical problem of accuracy of transference of various types of signals from sender to receiver" (p. 97). He emphasizes that the word *information*, in this theory, is used in a special sense that must not be confused with its ordinary usage; in particular, it must not be confused with meaning. At this point, Weaver states, "It is this, undoubtedly, that Shannon means when he says that 'the semantic aspects of communication are irrelevant to the engineering aspects' " (p. 99).

In discussing the interrelationship of the technical, semantic, and effectiveness problems of communication, Weaver points out that the mathematical theory "contributes to the problem of translation from one language to another, al-



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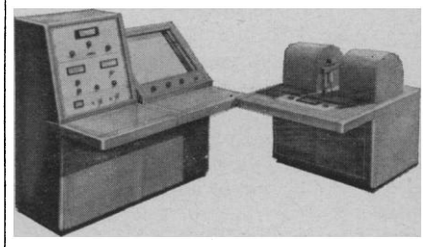

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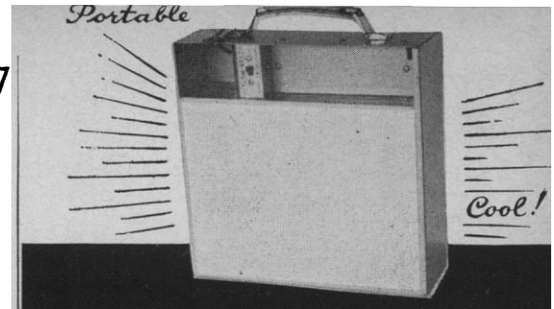

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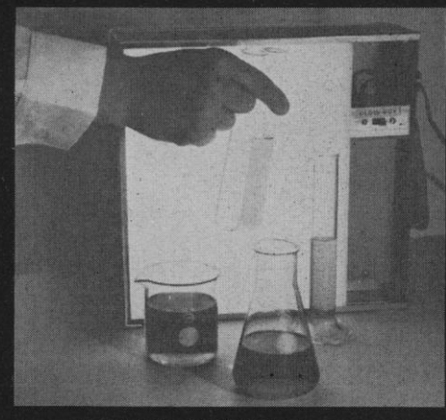

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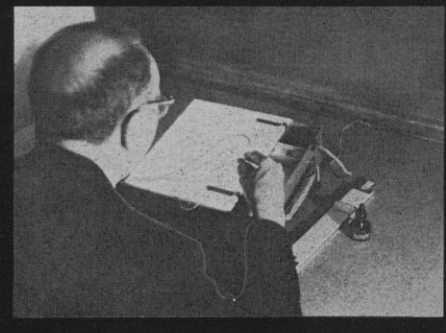

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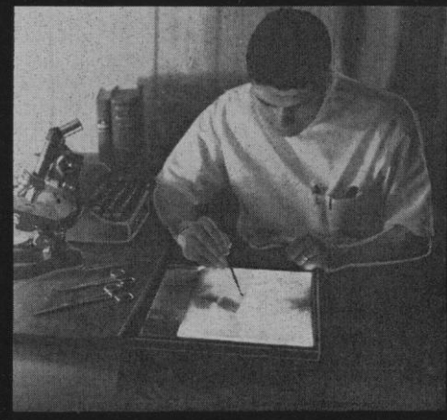

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though the complete story here clearly requires consideration of meaning, as well as of information" (p. 115). Translation is only a part of the broader problem of dealing effectively with the content of scientific literature.

Arguing by analogy is confusing if the analogy is not a close one. With respect to mechanization of the handling of scientific information, the problem is not that of designing something equivalent to a high-fidelity system for the reproduction or transmission of music. We are not merely trying to develop means for undistorted reproduction or transmission of scientific writings. A closer analogy might be the mechanization of some or all procedures involved in handling written music so as to facilitate searches for, say, compositions of a particular period in a particular style, rhythm, and tempo, in which a certain combination of notes is used. The essential problem then would be how best to obtain and to store coded representations of the compositions' characteristics and contents, in machine-searchable form, so that compositions with the desired characteristics could be readily identified—after, of course, first determining what musicologists are likely to want to search for.

I don't know for whom Taube speaks when he says, "We are only interested in storage and retrieval systems because individuals can index material. . . ." It is clear that many persons doing research or administering funds for research in this field believe it worth while to explore the possibility of mechanizing the indexing process or its equivalent. It is important, of course, to work on the engineering problem of efficient manipulation of index data. Such work, however, will contribute little toward the broad problem of mechanizing the retrieval of scientific information if the indexing, whether human or mechanized, is poor. Mechanized information-handling systems will serve us well only if human analysis and indexing of the "input," or whatever mechanized procedures may substitute for them, are sufficiently reliable for scientists to have confidence in the systems.

HELEN L. BROWNSON
National Science Foundation, Washington, D.C.

Doomsday

The article "Doomsday" by von Foerster, Mora, and Amiot [*Science* **132**, 1291 (1960)], although perhaps written and published with an obvious tongue-in-cheek attitude, has received some publicity in the newspapers, and there is danger that it may be taken too

seriously. At least one well-established biological fact has been omitted from the calculations: in human beings it still takes about 270 days from conception to delivery. This fact sets an ultimate limit upon the productivity factor a. If we consider only the reproductive female population (assuming the presence of enough males to maintain the necessary conception rate), it is apparent that the doubling time cannot ever be much less than ¾ year. If von Foerster's equation is valid until this doubling time is reached, the curve at this point has to depart from the power function and revert to an exponential,

$$N = e^{-\alpha t},$$

where α cannot exceed

$$0.69315/0.75 = 0.925 \text{ yr.}$$

From von Foerster's Eq. 12,

$$\alpha = 0.99/\tau,$$

so the power function fails at

$$\tau = 0.99/0.925,$$

or 1.07 years before "dooms-time," when the world population would only be, from von Foerster's Eq. 11,

$$N = 1.79 \times 10^{11}/\tau^{0.99}$$
$$= 1.67 \times 10^{11},$$

a value which corresponds to a population density less than 5 times that of Japan at present. Of course, males and children add something to the problem, but 1.7×10^{11} is far short of infinity, so there is still a ray of hope.

J. S. ROBERTSON
V. P. BOND
E. P. CRONKITE
Medical Research Center,
Brookhaven National Laboratory,
Upton, New York

The essay in doomsmanship of von Foerster, Mora, and Amiot is to be commended. With the exception of their remarks about Malthus, their conclusions are essentially correct. I say this because essentially these same conclusions can be arrived at from Malthusian principles.

People who are without food and water for any extended time die first of dehydration and then of starvation. Hence one can predict with confidence that food and water supplies, F, will limit human populations (deserts, highway U.S. 66, 110°W, 1960). This idea can be expressed symbolically as follows:

$$dN/dt = k_1F \qquad (1)$$

where N is the population size, t is the time, and k_1 is the rate constant for the conversion of food into people.

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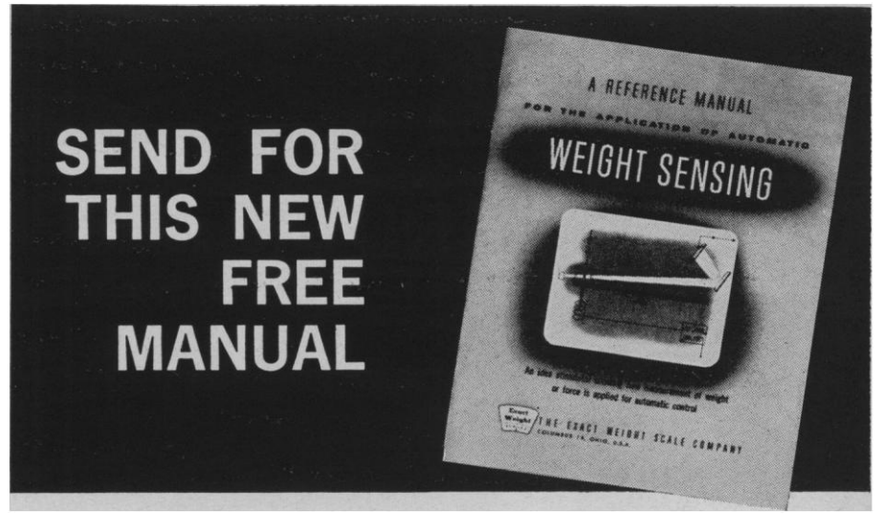

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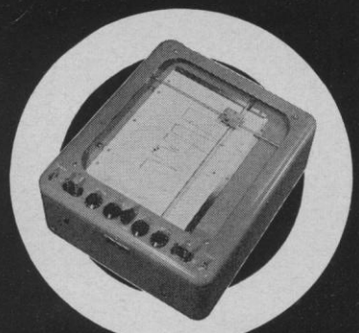

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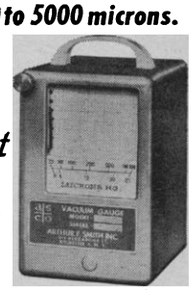

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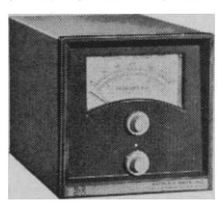

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energy, we can also predict, also confidently, that the limiting rate of food supply is constant. The rate of food accumulation is zero, however, because the limiting population is hungry. It is also certain that in order to reach a limiting population, all surplus food supplies would have to be consumed, or

$$N = N_{max},\ F = 0 \qquad (2)$$

Substituting Eq. 2 in Eq. 1 and integrating, we find, sure enough, that the human population becomes maximal; thank goodness it wasn't infinite after all.

It should be carefully noted that the limiting population may turn out to be larger than that estimated above because people may become smaller.

In conclusion, we should not sell Malthus short. His work in theoretical demography is so nearly contemporary as to make one wonder. There is a solution that has not yet been suggested (except by Swift, for one special case): cannibalism.

WILLIAM E. HUTTON
5133 Waterman Boulevard,
St. Louis, Missouri

In the article "Doomsday," the assumption is made that the fractional rate of growth of population will increase with the population; consequently, as the population becomes larger, the fractional rate of growth becomes larger and before long exceeds the maximum possible rate of increase permitted by the biology of the human species. It seems obvious that such a theory has no relation to reality and is of no value whatever in predicting future populations.

It is possible, however, to use the methods of this article, starting from more plausible assumptions, and to arrive at population growth curves which not only are in agreement with the facts of the past but which do yield helpful suggestions as to how the population will grow in the future. Such a formulation has been made, in accordance with the ideas of Raymond Pearl reflected in Eq. 2 of von Foerster *et al.*

The basic assumption is that the population increases at a rate which is proportional to the product of the population and another term which is equal to the supportable population of the region minus the population itself at that time, all divided by the supportable population at the same time. This is the same as Pearl's basic differential equation except that he calls the so-called supportable population the ultimate population and treats it as a constant. In the new formulation the supportable population is considered to be a function of time—namely, a constant plus another constant times time. The resulting differential equation is easily solved in general form, and curves have been con-

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structed in terms of general parameters which make it possible and convenient to extend the historical data of population of a given city or region into the future. The assumption that the supportable population increases with time is in agreement with the assumption of von Foerster *et al.*—namely, that science and technology do increase the ability of a region to support its population.

Using these theories and the set of curves that have been constructed, we find that the population of the United States agrees remarkably well with the appropriate curve from the family of curves referred to, starting with census data for 1790 and ending with data for 1960. The simpler logistic curve of Pearl fails to give agreement after 1940. One's prediction of future population of the United States depends of course upon the choice of constants, and this in turn depends upon one's estimate of the rate of increase of the ability of our territory to support the future population. Whether this ability increases linearly with time or at a faster rate seems to me to be a matter of conjecture at this time. In any case, such a formulation does offer promise of assistance to those who wish to predict future populations, and the absurd results reported in the article "Doomsday" should not discourage us from making attempts of this sort.

W. E. HOWLAND
Purdue University, Lafayette, Indiana

The article by von Foerster, Mora, and Amiot would be too ridiculous to comment on if it were not such an outstanding example of the inadmissible use of mathematics to prop up a manifestly absurd conclusion. I suppose that the authors are aware of that absurdity, although the tone of the article gives little ground for the supposition, but I wonder why they are not also aware that such articles run the very real danger of increasing the mistrust that many have always shown even of the legitimate uses of mathematics.

The article is so easy to criticize on the basis of the too free use of unsupported hypotheses (particularly Eq. 3) that I shall not do so. Instead, I shall show that *even if the stated hypotheses are accepted the conclusion does not follow.*

It is assumed in the article that the "productivity" α of a population with N members is given by

$$\alpha = \alpha_0 N^{1/k}$$

(Eq. 3), where α_0 and k are constants. The authors then use the "fact" that the rate of change of population is given by

$$\begin{aligned} dN/dt &= \alpha N \\ &= \alpha_0 N^{1 + 1/k} \end{aligned} \qquad (1)$$

to conclude that N goes to infinity at some finite value (A.D. 2027) of the time.

I wish only to point out that this nonsense does not arise if one only recalls that the size of a population is always an integer. As a result, the expression dN/dt has no real meaning except as an approximation, a fact the authors do not bother to point out. Eliminating this approximation, we see that Eq. 1 should read

$$N(n) - N(n-1) = \alpha_0 [N(n-1)]^{1 + 1/k}, \quad n = 1,2,...,$$

where n refers to the generation under consideration and the unit of time has been taken as a generation. Recalling that $N \geq 1$ for all n since N is an integer, we see that

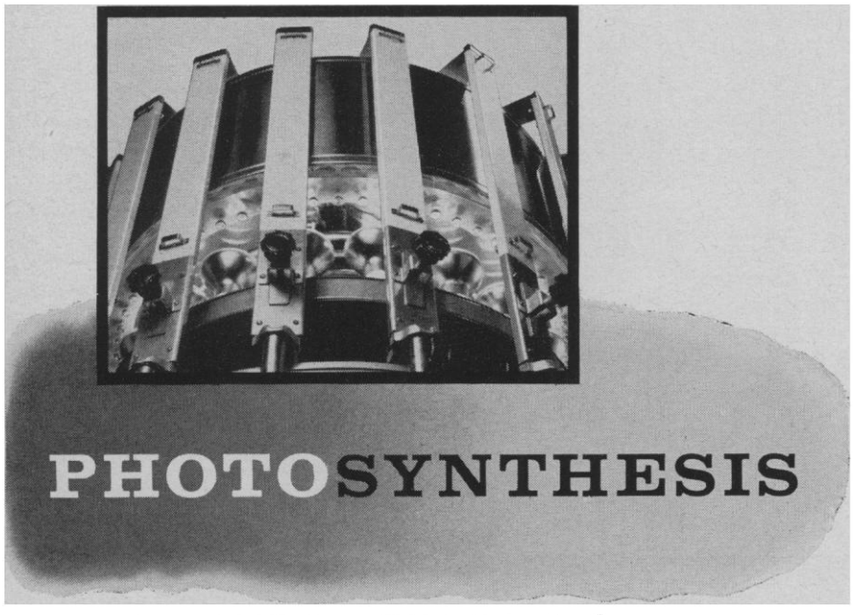
$$\begin{aligned} N(n) &\leq N(n-1) + |\alpha_0| \, [N(n-1)]^{1 + 1/k} \\ &\leq (1 + |\alpha_0|) \, [N(n-1)]^{1 + 1/k} \end{aligned}$$

Thus,

$$N(n) \leq (1 + |\alpha_0|)^{k[(1 + 1/k)^n - 1]} [N(0)]^{k^n}$$

which is clearly finite for all n.

The argument here should not be misconstrued. The point is not that the world's population growth is not a serious problem but only that progress toward resolution of the problem is in no



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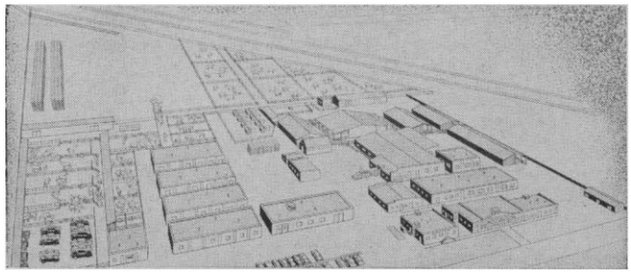

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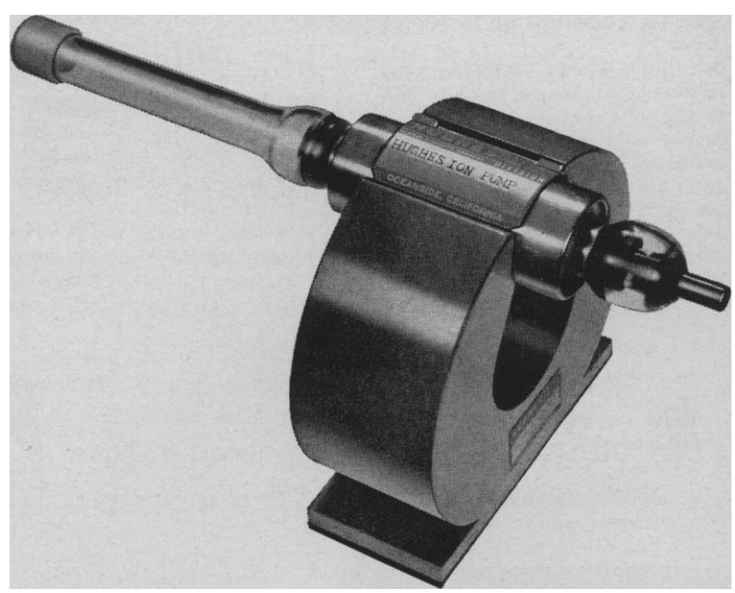

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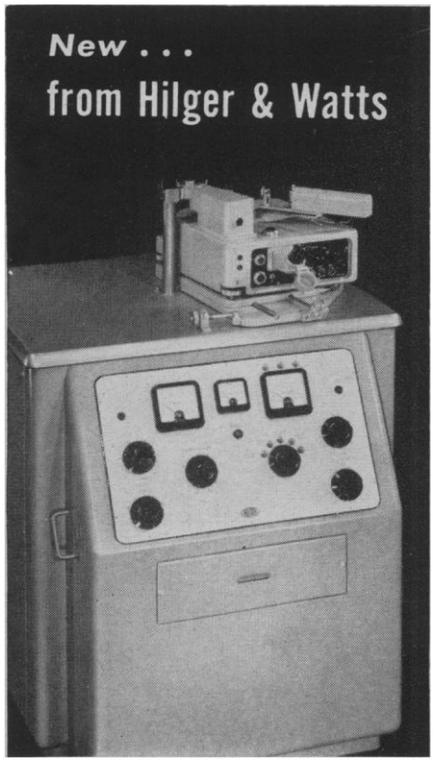

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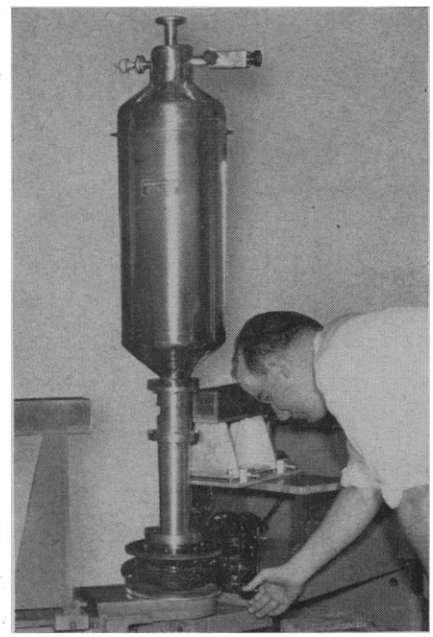

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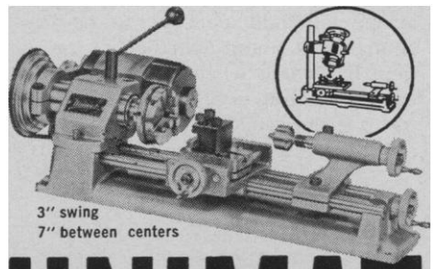

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way served by publication of arguments which, on their face, must be false and which may do some incidental harm. The authors express the hope that their article will "add some fuel to the heated controversy about whether or not the time has come when something has to be done about population growth control." If the article has this effect, it can only be on a controversy among fools.

MARVIN SHINBROT

Stanford University, Stanford, California, and *Lockheed Aircraft Corporation, San Jose, California*

We appreciate the opportunity to comment on the remarks which have been made with respect to our article "Doomsday." There are two points which seem to need further clarification. Since we erroneously believed that these points are part of the household furniture of the scientific community, we apologize for having neglected to restate them explicitly. The first refers to the relation between theory and reality, and to the supportability of a hypothesis. We believe that support of a hypothesis is gained through compatibility with experimental observation (*1*) rather than by arguments about what should be the case or what should not be the case. This compatibility establishes the relation between theory and reality and serves as a touchstone for accepting or rejecting a hypothesis. If some of our readers express doubt whether or not our simple hypothesis (Eq. 3) has any connection with reality, we obviously failed to keep them interested in this subject long enough to turn to our Fig. 1, which offers a comparison between theory and observation. Although we know that such a comparison, however favorable, will never prove the "truth" of a hypothesis, we pointed out that it seems that our Eq. 11 may, at least, "serve as an adequate empirical formula for presenting most of our recorded data on human population growth" (*2*).

The second point refers to the interpretation of singularities of the form

$$\lim_{x \to x_0} y = \infty$$

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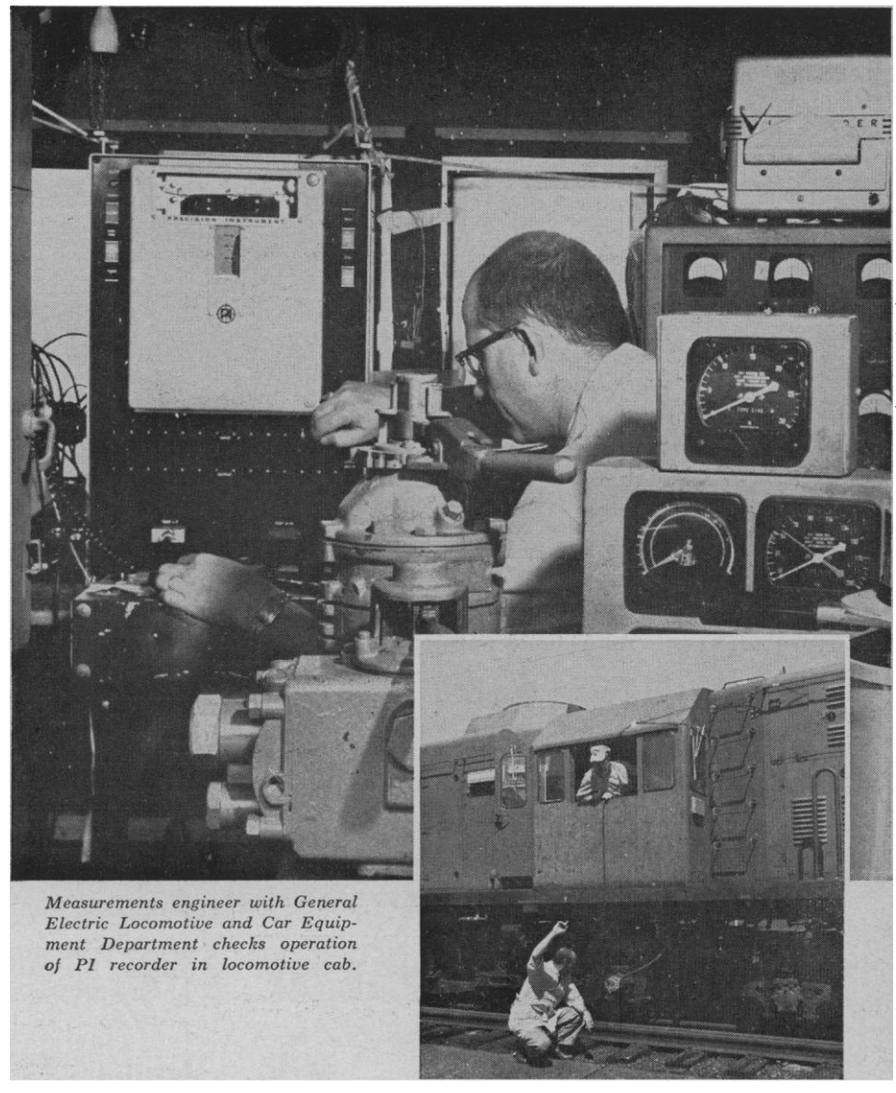

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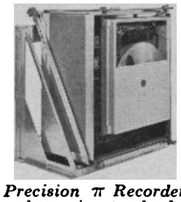

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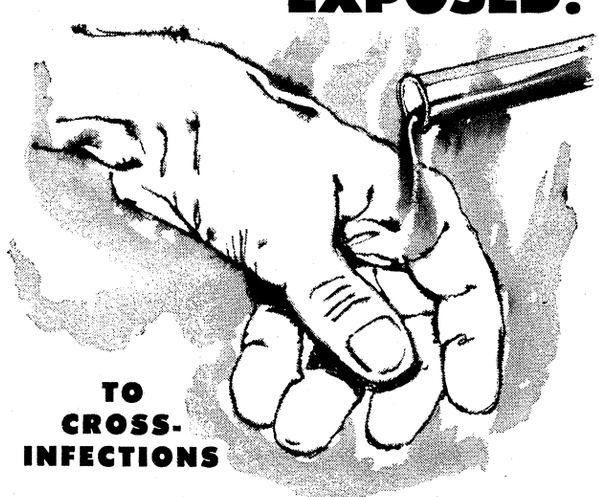

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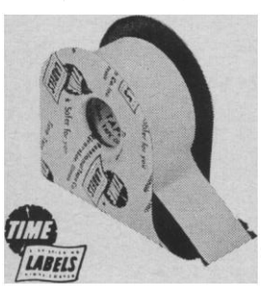

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BIOLOGICAL AND CHEMICAL CONTROL OF PLANT AND ANIMAL PEST

Editor L. P. Reitz **April 1960**

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that such theories have predictive value because of these singularities. On the contrary, since the generally accepted interpretation of expressions such as these, in which a parameter increases rapidly beyond all bounds, is that the system as a whole becomes highly instable in the vicinity of the critical value x_0 of the corresponding parameter, these singularities serve as welcome warning signals that some breakdown of the system's structure is to be expected.

With respect to the first letter, by Robertson, Bond, and Cronkite, we are very happy to note that this medical research team went along so well with our proposed thesis of "adequate technology," because they obviously must have in mind some tricks for reducing the age of puberty in the human female—the greatest bottleneck in speeding up the rate of reproduction. But who are we to argue with doctors about such points of physiology? However, we may argue their mathematics, because (i) they used a wrong equation for calculating dooms-time for a particular doubling time, and (ii) they failed to follow up their own argument by omitting to calculate the population at doomsday according to the proposed exponential. With our expression for doubling time Δt_2 (that is, Eq. 13, and *not* Eq. 12), one finds the corresponding dooms-time to be $\tau_1 = 2.25\ \Delta t_2$, and invoking Eq. 11, one obtains N_1, the population on that date. With the aid of the suggested exponential we have N_D, the "finite" population at doomsday:

$$N_D = N_1 2^{\tau_1/\Delta t_2} = 3.70 \cdot 10^{11}/(\Delta t_2)^{0.99}$$

With the suggested value of $\Delta t_2 = 0.75$ one obtains $N_D = 5 \cdot 10^{11}$. This corresponds to a population density 15 times that of Japan and about 10 percent that of New York City today. We predicted that this population density would occur on 1 January, A.D. 2024, plus or minus 5.5 years. But according to the arguments advanced by Robertson, *et al.*, we will have this squeeze just 1000 days later. If this is considered to be a ray of hope, the ray is very dim indeed.

We share Hutton's admiration for T. R. Malthus, whose omnipresence in the minds of pessimists as well as optimists we believed we had pointed out.

Howland's suggestion for an approach to population problems is formulated in the differential equation

$$\frac{dN}{dt} = \varepsilon_0 N\ (1 - N/N_0)$$

where ε_0 is a constant and N_0 is the "supportable population." Although this hypothesis may be plausible, it has unfortunately no relation to reality when confronted with estimates of the human global population, unless, as Howland points out, *ad hoc* adjustments for N_0 are made as time goes on. Thus, this theory requires development of a theory for N_0 as a function of t or N. No such function, to Howland's and our knowledge, has as yet been suggested which would fit past data over a period longer than, say, ten generations. In this dilemma we would like to propose, in all modesty, to try tentatively the following, perhaps not too implausible, hypothesis —namely, that $N_0\ (N)$, the supportable population, is almost always somewhat larger than the instaneous population N. We suggest:

$$N_0 = N/(1 - \tfrac{\alpha_0}{\varepsilon_0} N^{1k})$$

with the constants α_0/ε_0 and k to be determined by observation. We hope that this suggestion meets with Howland's approval, because it catches three flies with one stroke. First, it expresses, in some sense, our principle of "adequate technology," to which Howland has no objections; second, it will enable Howland's proposed differential equation, when properly integrated, to represent human population growth over more than a hundred generations with a mean deviation of less than 7 percent; and, third, it eliminates guesswork about a quantity which is, in principle, inaccessible to experimental observation—namely, N_0, the size of the supportable

Fig. 1

population. This is easily seen by inserting our suggested function into Howland's proposed differential equation, which leads, after integration and adjustment of the constants by the method of least squares, to our Eqs. 11, 12, and 13, which are free of unobservable parameters. We hope that with this little excursion we have supplied Howland with precisely that formulation

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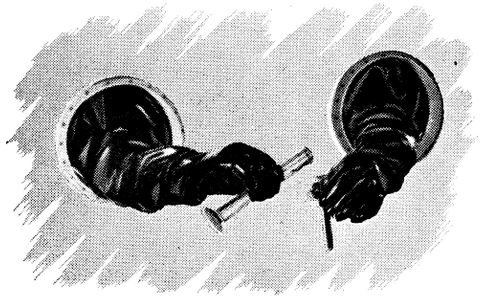

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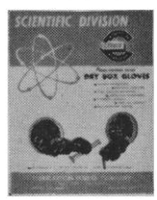

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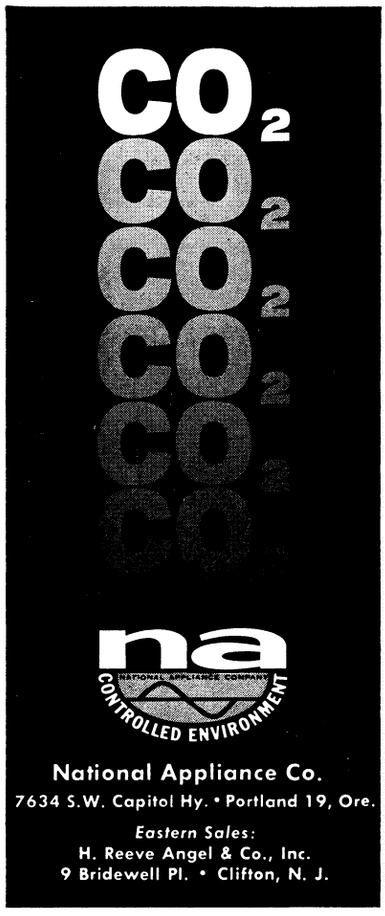

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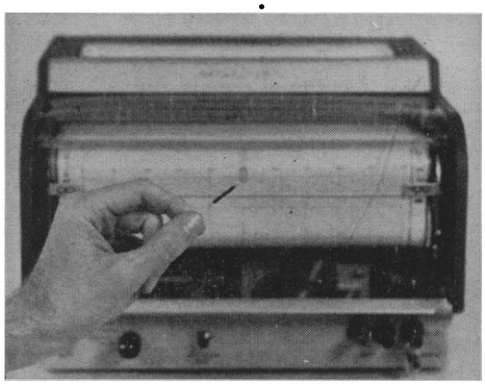

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which, according to him, "does offer promise of assistance to those who wish to predict future populations."

Unfortunately, politeness forbids us to respond to Shinbrot's remarks because this would involve him in a controversy which—in his own words—can only be on "a controversy among fools." Otherwise we would have pointed out our agreement with his feeling that it is unkind to perform a Dedekind cut on a man. On the other hand we could *not* write our differential equation in the form suggested by Shinbrot because we do not know of any integer-triple $N(n),N(n-1)$, and α_0, which would fit for $k \neq 1/i$ $(i=1,2,3....)$, his sugested difference equation. Obviously he must know such triples, and thus his suggested relationship will remain forever "Shinbrot's last theorem."

In the meantime, while we were displaying our wits and know-how in more or less learned discussions about the perennial question of how many angels can dance on a pin point, over ten million real people of flesh and bone, with hopes and desires, with sorrows and pain, have been added to our family of man. Our responsibility demands that we be ready with an answer when these millions ask for their right to live the span of their human condition in dignity.

Let us join forces so that we will not be caught in a dispute seen prophetically by Francesco de Goya y Lucientes: "Of what will they die?"

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LAWRENCE W. AMIOT
Department of Electrical Engineering, University of Illinois, Urbana

References and Notes

1. B. Russell, *Human Knowledge* (Simon and Schuster, New York, 1948), p. 481.
2. For comparison of our Eq. 11 with estimates of the prehistoric human population, we are grateful to F. Meyer for having drawn our attention to his article "L'Accélération de l'evolution," in *L'Encyclopédie Française* (Larousse, Paris, 1959), vol. 20, p. 24.
3. E. V. Condon and H. Odishaw *Handbook of Physics* (McGraw-Hill, New York, 1958).

History of the Microscope

In Paul Klopsteg's article, "The indispensable tools of science" [*Science* **132**, 1913 (1960)], there are several statements on the historical aspects of microscopes and microscopical discoveries which are inaccurate. In the interest of keeping the historical record correct, I submit the following.

It is considered [see, for example, A. J. Kluyver's notes on Leeuwenhoek's letter to the Royal Society dated 9 October 1676, in *Collected Letters of Antoni van Leeuwenhoek*, Swets and Zeitlinger, Eds. (1939, 1941), vols.

1, 2] that bacteria were undoubtedly observed and described by Leeuwenhoek as early as 24 April 1676, and *not* 1681, as stated. Further, De Waard [see A. Schierbeek, *Measuring the Invisible World* (Abelard-Schuman, 1959)] has discovered that Zacharias Janssen was born in 1588, and his son Hans, in 1611, so that neither could have invented the compound microscope in 1590.

RAYMOND N. DOETSCH
Department of Microbiology, University of Maryland, College Park

Food Additives

The 27 May 1960 issue of *Science* [**131**, 1581 (1960)] gave editorial approval to the report of the Panel on Food Additives of the President's Science Advisory Committee. The principal recommendation of the panel was to set up an advisory board "to weigh evidence and make recommendations to the Secretary of the Department of Health, Education, and Welfare on the basis of available scientific data on applications for the approval of food additives." In evaluating this recommendation two facts should be considered. First, the panel probably would be under heavy pressure from corporations who would want exemption *now* for additives for which there is *some* evidence of carcinogenic effect in animals. Second, on the basis of present data and techniques, there is no way to make a reliable prediction of the "safe" level of a carcinogenic compound, and—to quote the report—"definitive answers useful in extrapolation to man may not be expected for many years to come."

While the report discusses a number of the major difficulties in the path of scientific decision-making in this area, there is one particular difficulty (which gets bare mention in the report) that we would like to stress here because it is often overlooked. This difficulty arises because (i) the population at risk is of the order of 10^8 persons; (ii) our primary emphasis is on controlling the *number* (rather than the proportion) of cancer cases; and (iii) direct estimates of the risk probabilities would be based on relatively small experiments (10 to 10^3 animals). Since we would be concerned if an agent produced, say, 100 cancer cases, a "safe" level would require risk probabilities of the order of 10^{-6}. Statistical theory indicates that to obtain adequate direct estimates of such small risk probabilities would require a sample of 10^6.

From this standpoint, consider the decision rule: If no cancers develop in 1000 test animals, classify the corresponding level of the agent as "safe."

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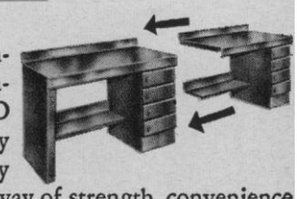

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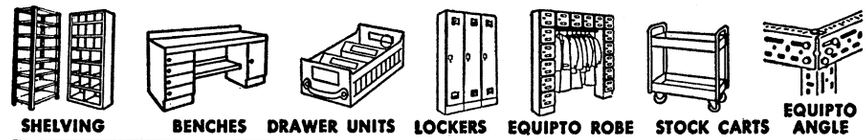

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The probability of obtaining zero cases in a trial sample of size n, when the true incidence is p, is given by the last term of the binomial expansion $[p + (1-p)]^n$, $(1-p)^n$. Thus, if an agent were capable of producing 100,000 cases of cancer in the United States population at risk ($p = .001$), there would be about one chance in three $[(1 - .001)^{1000}]$ that the agent would be classified as "safe." Even if we make the common assumption (which is not always legitimate) that dividing the dose level by 100 would be equivalent to obtaining no cancers in 100,000 test animals, in such a test of an agent which could produce 1000 cases in a population of 100 million ($p = .00001$), there is a one-in-three chance $[(1-.00001)^{100,000}]$ that no experimental tumors would occur.

The present alternative to direct estimation of risk probabilities is extrapolation from dose-response curves. The report states that "dose-response curves for certain potent carcinogens in animals have been worked out from which can be reliably predicted the probability of an individual, in a given size population, developing a tumor from a given dose of carcinogens." This statement requires qualification. While a given technique (such as probit analysis) will often be adequate for ordinary applications (which involve interpolation or very limited extrapolation), the extrapolation required here makes the estimate heavily dependent on the assumption about the underlying distribution (such as the normal distribution). This point is evident when several alternative linearizing transformations (probits, logits, angits, and so on) are used on the same data. While all may provide a fair fit to the observed points and very similar estimates for the LD_{50} (50-percent probability), the *extrapolated* estimates for very small probabilities will not even be of the same order or magnitude. Such predictions are clearly not reliable enough to be used in a decision where human lives are involved.

Until reliable decision-making procedures for the food additive situation are developed—and to develop them is certainly not an easy task—we would question the advisability of vesting an advisory board with power to exempt chemicals that have some experimental carcinogenic effect from the present Food Additive Amendment of the Food, Drug, and Cosmetic Act. An advisory board to review procedure to be considered adequate for testing chemicals for carcinogenic effect in man would, of course, be useful. The creation of such a board probably does not require any amendment to existing legislation.

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Goals of Secondary School Teachers

As a secondary school teacher (in biology), I feel I must reply to Merritt A. Williamson's letter in *Science* [**132**, 1732 (1960)].

In his statement, "college teaching, as contrasted with secondary school teaching, is concerned with the development within the student of the power to think, reason, appreciate, and discriminate . . . ," he implies that these are not the objectives of the secondary school teacher. He is very wrong. These are the objectives I had when I taught sixth-grade and eighth-grade biology and which I now have in teaching tenth-grade biology. That I am not alone is evidenced in the fact that, through the American Institute of Biological Science's Biological Sciences Curriculum Study program, hundreds of secondary school teachers (among others) contributed to the development of three different approaches to the teaching of biology, all of which embodied these same objectives.

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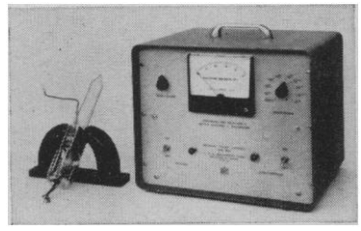

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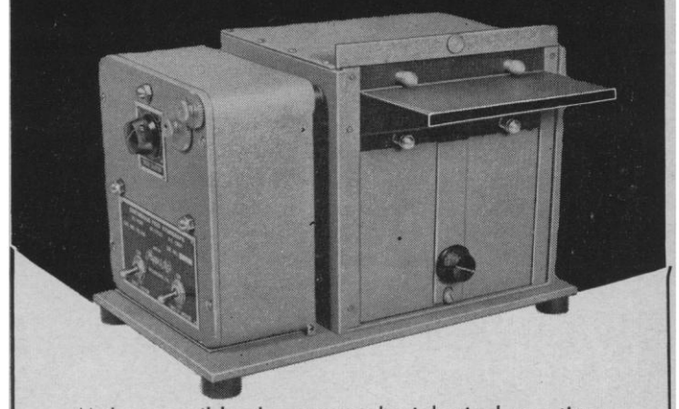

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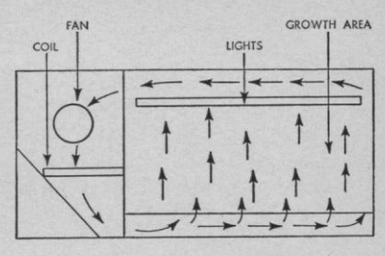

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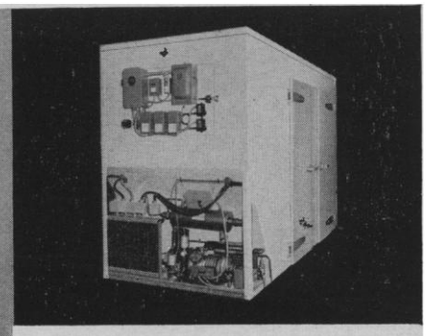

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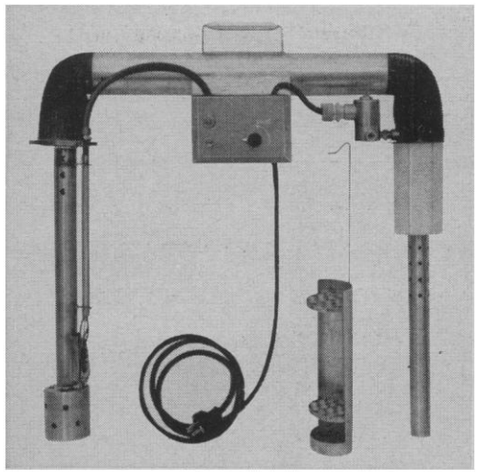

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these three BSCS approaches this year, and I know secondary school teachers in other areas as well as in science who work toward these same goals in their regular teaching programs.

That we do not achieve nearly so much as we would like can be explained by the fact that time is necessary for continuous planning, evaluation, and reorganization of any teaching program as it relates to the individual student and his progress. At the elementary and secondary levels this time is available each day only after a continuous sequence of periods of meeting students in either academic or extracurricular pursuits (periods that often include the noon hour), broken only by the 3-minute interval for changing classes.

Even so, secondary (and elementary) school teachers are concerned and do work toward helping the student develop his ability to think, reason, appreciate, and discriminate. We need, somehow, to provide time for regular professional interchange of ideas in the school day, both within a school system and between school systems, so that all teachers will be stimulated to work more directly to accomplish these aims in spite of many seemingly insurmountable difficulties.

MARON E. STEWART
Ionia High School, Ionia, Michigan

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W. H. Oldendorf [*Science* **133**, 198 (1961)] should be advised that one very good reason for not contaminating books with advertising as he suggests is the very costly increase in postage that results.

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Radiation Exposure

The article by Newell and Naugle on radiation in space [*Science* **132**, 1465 (1960)] is an interesting and timely treatment of the subject. However, it contains several references to ionizing radiation exposure standards for human beings which I feel may be misleading.

A figure of 0.3 r per quarter is referred to as an exposure standard for radiation workers. To my knowledge, this has not been proposed by any group. It probably represents a simple decimal-point slip from the 3.0 rem (close enough to the roentgen for this discussion) per quarter recommended by the National Committee on Radiation Protection and Measurements (NCRP), the International Commission on Radiological Protection (ICRP), and

the Federal Radiation Council (FRC). This error then resulted in the value 1.2 r per year given in Table 4. Neither 0.3 r per quarter nor 1.2 r per year is consistent with the value 5.0 r per year given in the same table.

The long-term occupational dose in the numerical recommendations of the three groups mentioned above is $5(N-18)$ rem, where the individual's age is N and greater than 18. Thus, a person over the age of 18 would be permitted 12 rem every year (3 rem times 4 quarters) until he reached the dose derived by the formula.

The reference to a 25-roentgen "maximum permissible emergency dose" leaves the impression that some serious biological effect will ensue from a higher dose. The article seems to have taken a portion of the NCRP's *Handbook 59* (as revised) out of context. The complete thought is, "An accidental or emergency dose of 25 rems to the whole body or a major portion thereof, occurring only once in the lifetime of the person, need not be included in the determination of the radiation exposure status of that person. . . ."

The NCRP and ICRP are unofficial groups. More recent in origin, and more directly related to NASA, is the FRC, whose recommendations have been approved by the President for the guidance of federal agencies. One recommendation of the FRC would permit a dose exceeding that set forth in the radiation exposure guides after careful consideration of the reason for the larger dose. Surely, a man in space would qualify for consideration.

THOMAS S. ELY
Office of Health and Safety,
U.S. Atomic Energy Commission,
Washington, D.C.

As noted by Ely, our article contains an error in Table 4. The maximum permissible dosage for radiation workers should be 3 r per quarter and 12 r per year, provided the individual's total long-term occupational dose does not exceed $5(N-18)$ r, where N is his age in years. The statement, "In 10 hours a man would receive his allowable yearly dose even with this amount of shielding," should then read, "In 6 hours a person would receive his allowable quarterly dose even with this amount of shielding."

The statement, "After taking such a dose [25 r] the man would not be permitted to take any more radiation in his lifetime," should, as noted by Ely, be deleted.

It was our intent in the article to give the relative orders of magnitude of the radiation levels in space and permissible dosages to indicate the magnitude of the problem presented by this radiation environment. It was not our intent to give the impression that we

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were specifying the permissible dosages for manned space flight.

There are three errors in the second section of Table 2. Item *b* should read: "Electrons, $E>200$ kev: omnidirectional intensity: $\leqq 1 \times 10^{8}$ cm^{-2} sec^{-1}." Item *c* should read: "Protons, $E>60$ Mev: omnidirectional intensity: $\leqq 10^{2}$ cm^{-2} sec^{-1}" (*1*).

HOMER E. NEWELL
JOHN E. NAUGLE
National Aeronautics and Space Administration, Washington, D.C.

Note

1. The symbol $\leqq$ here means "less than or approximately."

Sustained Swimming in Dolphins

Johannessen and Harder, authors of the report "Sustained swimming speeds of dolphins" [*Science* **132**, 550 (1960)], imply that the "length of time at observed speed" (in their Table 1) necessarily represents in each case a time during which the animals swam continuously and unaided at the indicated speeds. It is this implication on which I wish to comment.

Establishing the sustained work capacity of dolphins by the observational methods used by these authors requires identification of the individual animals during the indicated timing periods. The valdity of using groups of dolphins for this purpose is questionable. How can the authors be sure that a group, seen from a quarter of a mile to several miles away, is necessarily made up of the same individuals, or is even the same group, as one seen a few minutes earlier or later?

Part of the problem of proving the marine animal's capacity for sustained swimming at high speed seems to be that of showing that a portion or all of the required energy is not derived from waves. Observations have shown that in some cases no apparent swimming effort is required for dolphins in a bow wave to move through the water at 10 knots (*1*). They have also been seen riding natural waves near shore (*2*).

The numerous observations of "wave-riding" dolphins have been variously explained as resulting from gravity (*3*), buoyancy (*4*), and pressure (*5*)—forces associated with the waves. The question of the origin of the force or forces actually producing the "wave riding" seems at present unresolved.

The work referred to above suggests strongly that observational programs designed to demonstrate the work capacity of marine animals swimming near the surface should give particular attention to waves. The sizes and directions of motion of local wind waves and of swell may be important, especially as they are related to the directions and speeds of motion of the observing ship and of the animals observed.

If dolphins and other marine animals can indeed utilize the energy of waves on the open sea, as well as bow and coastal waves, then the virtual absence of wave data in the observations reported by Johannessen and Harder makes it seem doubtful that these observations can be regarded as clear evidence of the sustained-work capacity of the animals concerned.

A. H. WOODCOCK
Woods Hole Oceanographic Institution, Woods Hole, Massachusetts

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2. D. K. Caldwell and H. M. Fields, *J. Mammal.* **40**, 454 (1959).
3. A. H. Woodcock and A. F. McBride, *J. Exptl. Biol.* **28**, 215 (1951).
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5. P. F. Scholander, *Science* **129**, 1085 (1959); A. A. Fejer and R. H. Backus, *Nature* **188** (1960).

In answer to Woodcock's comments we suggest that the questions raised are not applicable to our report to the extent that Woodcock infers. He wonders at our using groups of dolphins instead of individuals. Anyone experienced in shepherding even a well-disciplined group of children will testify that group velocity is equal to and usually less than the velocity of the individual. This

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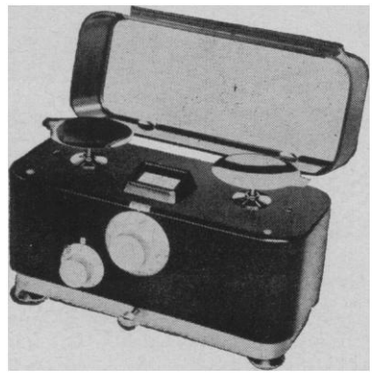

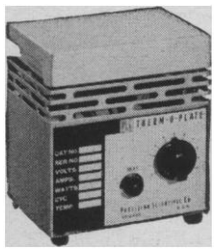

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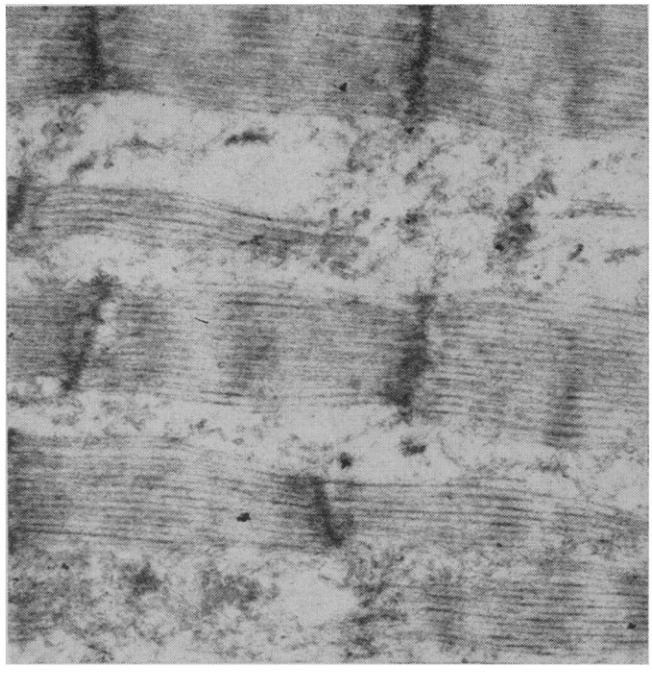

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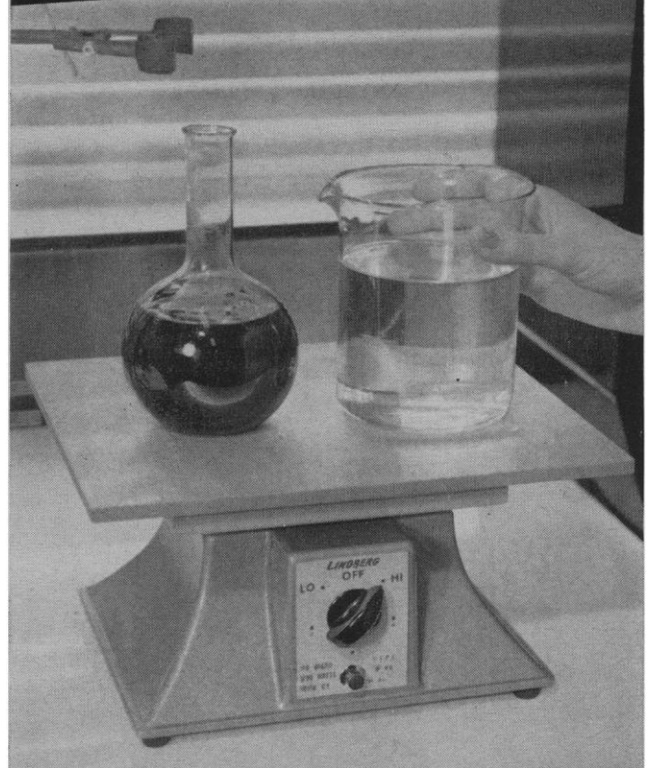

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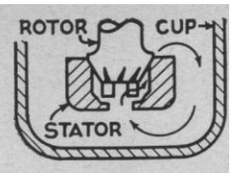

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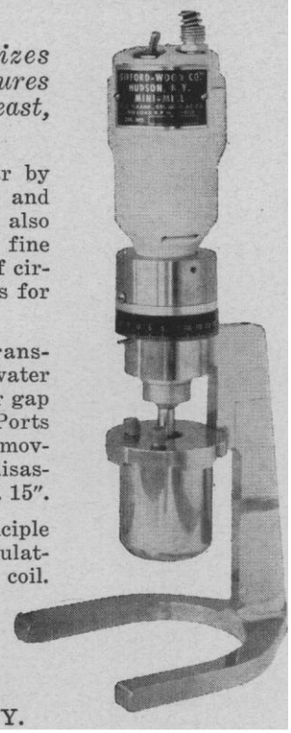

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indicates that the group velocity is a conservative indicator of the maximum capability of sustained swimming speed in dolphins. The infrequency with which large groups were observed is a reasonable guarantee that the observer, Andrews, did not see two disparate groups, at the beginning and end of each observation. A group of 200 individuals swimming at 14 to 18 knots is well delineated by a zone of splashing water.

Although it is well known that dolphins can and do ride bow waves of ships, there is not even unofficial report, to our knowledge, that they can ride the random waves of the open sea. Of course one should not confuse the "lift" a pelagic mammal might obtain from an ocean current with the riding of random waves. The sightings reported by us were made during times of general calm, and during the sighting of the fourth group (200 to 300 individuals) the observer reported an exceptionally smooth, or "glassy," sea.

Finally, it should be pointed out that we are not reporting on the "sustained-work capacity" of dolphins but on their sustained swimming speeds.

CARL L. JOHANNESSEN
Department of Geography, University of Oregon, Eugene

JAMES A. HARDER
Department of Civil Engineering, University of California, Berkeley

Woodcock gives a number of references in support of his conclusion that the origin of forces producing the "wave riding" is unresolved. This conclusion requires examination. He and McBride (*1*) asserted that only the underwater weight of the dolphin could be effective in providing a wave-induced propulsive force, and they reported on their killing a *Tursiops* to find this weight. Subsequently W. D. Hayes (*2*) showed that, from hydrodynamic theory, the forces on the dolphin would be equal to the component of its *total* weight acting parallel to the water surface. More exactly, the component acting is parallel to the surface of constant pressure passing through the animal (the free surface is one surface of obvious constant pressure), for there is no corresponding pressure gradient in this direction.

Woodcock is not alone in believing the hydraulic explanation of questionable validity. Scholander (*3*) proposed yet another mechanism for wave riding and reported on an experiment designed to test the "Hayes effect." He concluded that there was none. When challenged by Hayes to produce data (*4*), he published a figure showing his measurements of the drag on a small fishlike object towed in various parts of a ship's bow wave (*5*). The expected value of the "Hayes effect" force, based on a

reasonable 10- to 15-degree inclination of the equal-pressure surface, was from 110 to 170 grams in this case, in which the object weighed only 650 grams. Although his results were admittedly crude (the scatter in many of the experimental runs exceeded the magnitude of the expected "Hayes effect" itself) and were certainly not conclusive, Scholander in effect invited Hayes to abandon hydrodynamics and to compete with him in the experimental verification of the balance-of-force principle of mechanics. Hayes's explanation is based on such fundamental principles that to deny it is to deny Newtonian mechanics; thus, I cannot agree that the question is "unresolved."

It is curious that whereas Woodcock's original analysis of the forces required for wave riding was in error, his conclusion, that dolphins experience less friction than an equivalent solid body, seems to be true. Quite apart from the evidence of low friction inferred from the unusually high swimming speed of dolphins, mechanical models of dolphin skin made of rubber have been shown to exhibit only about 40 percent of the surface drag coefficient of otherwise equivalent rigid skins (*6*). The present question seems to be, not whether dolphins have an anomalously low friction drag, but rather how low this drag is. Our report of the sustained swimming speeds of dolphins was intended to provide some of the data needed to answer that question.

J. A. HARDER
University of California, Berkeley

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2. W. D. Hayes, *Nature* **172**, 1060 (1953).
3. P. F. Scholander, *Science* **129**, 1085 (1959).
4. W. D. Hayes, *ibid.* **130**, 1657 (1959).
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6. M. O. Kramer, *J. Am. Soc. Naval Engrs.* **72**, 25 (1960).

Comets

In Thornton Page's report on the Fourth Berkeley Symposium on Mathematical Statistics and Probability [*Science* **132**, 1870 (1960)] there are several points on which I would like to comment.

First, in the legend of Fig. 4, Page states that the semimajor axis of an orbit is one-half the maximum distance from the sun. Instead, the semimajor axis is one-half the *sum* of the maximum and the minimum distances from the sun. Only in the case of the "sun-grazing" comets can the closest approach distance be ignored, because it is small in comparison with the maximum distance. In fact, in most parabolic or near-parabolic orbits it is quite impossible to give a good value for the semimajor axis.

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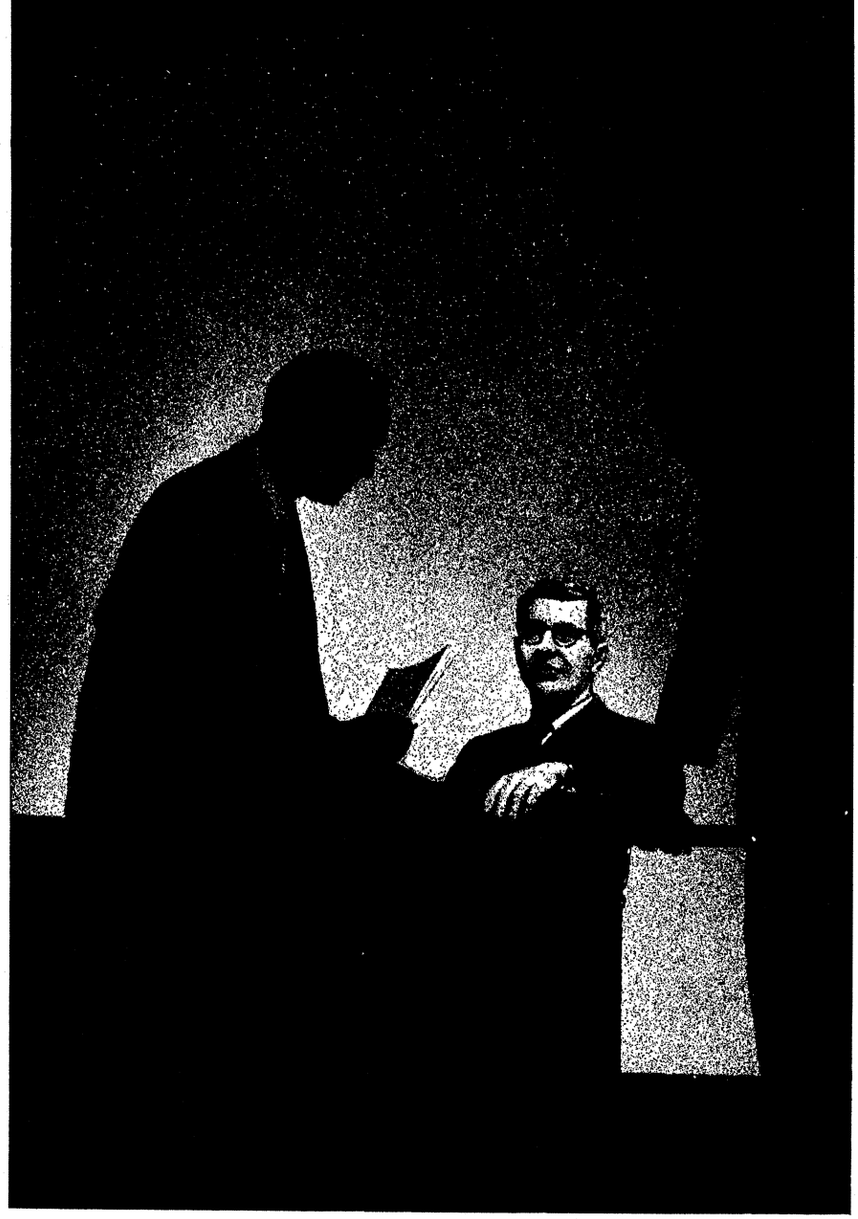

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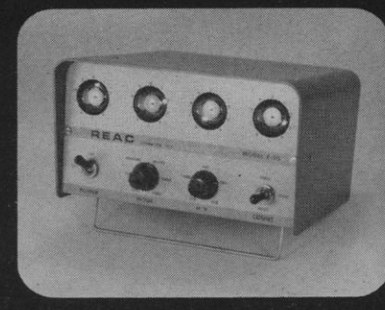

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Secondly, Page says (p. 1875), "Because the space about the sun is so nearly empty, comets move with frictionless ease. . . ." If this were true, one would have to neglect the effect of solar radiation pressure on the comet—an effect which is probably responsible for the anomalous acceleration of some periodic comets after all classical perturbations have been taken into account. This effect seems to be difficult to evaluate for the large mass of data required for a statistical study such as Page described in the report.

Although I have not yet had an opportunity to examine the papers discussed in the article, I feel that the conclusions drawn concerning Lyttleton's theory are unwarranted at this time for the following reason. Most of the comets that can be observed at present or that have been observed in the past have been those that come in relatively close to the sun and to the major planets, and they are probably not a fair sample of the comet population. The question cannot be decided with certainty until observations are made of comets that remain invisible from the earth. Those that do not become visible are objects that miss the sun by a great distance. If one finds that these are much more numerous than the ones that come close to the sun, one might have to adopt the Lyttleton theory. If they are less numerous, the Oort theory would fit the data better. Of course, it is possible that *both* views are correct in a restricted sense. Further research is required on the orbital mechanics and physical nature of comets before any definite conclusions can be reached.

DAVID D. MEISEL
Association of Lunar and Planetary Observers, Fairmont, West Virginia

Meisel is of course correct in his comments concerning the definition of semimajor axis (a) and the effect of radiation pressure on the orbits of comets, but I believe that both are of little consequence. Values of a are so large, and the eccentricity is so near unity for "new" comets, that the difference between $2a$ and aphelion distance is less than one part in several thousand. Moreover, in these orbits of high eccentricity, "new" comets spend most of their time so far from the sun that the effect of solar radiation pressure is limited to two small (and nearly opposite) impulses near each perihelion passage. These amount to a very small bias in the random planetary perturbations considered by Kendall and Hammersley.

In connection with these comments it should be emphasized that Kendall based his study on a carefully selected set of 23 "*new*" comets, excluding of course the periodic comets which, on Oort's theory, have suffered large or many perturbations from their original orbits. Kendall's analysis takes account of the residual observational selection (due to fewer approaches of comets of longer period) but does not concern itself with comets of large perihelion distance simply because neither Oort nor Lyttleton predict large angular momentum of newly formed cometary material about the sun.

The statistical studies I reported cannot be said to disprove Lyttleton's theory or to prove Oort's, as yet, but the limited observational data certainly indicate that "new" comets fall toward the sun from considerably greater distances than Lyttleton's theory would predict. Moreover, Lyttleton's own analysis of the directions of major axes of comet orbits failed to show the expected preference for directions associated with the solar motion relative to nearby stars and interstellar clouds.

The greatest weakness of these statistical studies, as Meisel possibly implies, is the selection of "new" comets for comparison with either theory. Such selection is essential, since random

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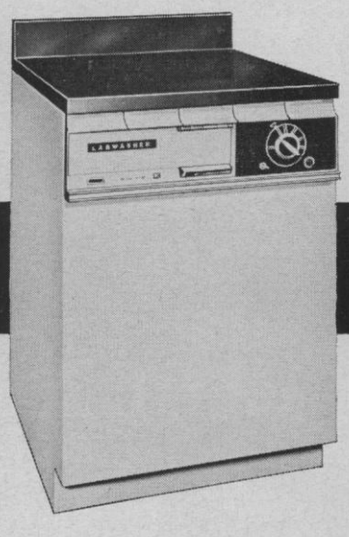

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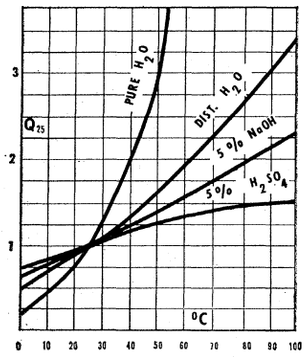

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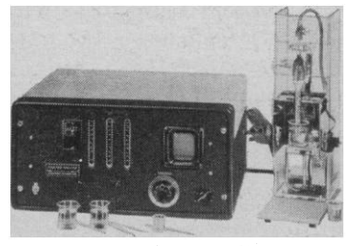

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planetary perturbations soon smear out any record of the original direction or distance of fall toward the sun. Even if we could observe all comets within 10 or 20 astronomical units of the sun, the key to their origin would lie in recognizing the new ones that preserve some record of the initial conditions.

THORNTON PAGE
Department of Astronomy, Wesleyan University, Middletown, Connecticut

Exposures in Lunar Photography

If the errors in *Outer Space Photography for the Amateur*, reviewed by Charles H. Smiley in a recent issue of *Science* [**133**, 271 (1961)], are typified by the example given in the review, they must be "few and . . . unimportant" indeed.

It is well known among astronomers that the full moon is about nine times as bright as the first and last quarters. But since the quarter moon is only half illuminated, the surface brightness of the full moon is only about four and a half times that of the quarters. Thus, the book's suggestion that the exposure for the quarter moon be four times that for the full moon is substantially correct, and the reviewer's "correction," giving the factor of nine, is wrong.

However, to paraphrase the reviewer, if a professional overexposes his first moon photograph, he can make corrections on his second try.

ANDREW T. YOUNG
Harvard College Observatory, Cambridge, Massachusetts

I shall leave my statement as it is, with the factor nine. Young's arithmetic is satisfactory, as far as it goes, but some judgment is needed in addition. The full moon, flat-lighted, is low in contrast; most astronomers expose and develop to increase the contrast. If one is to develop to a high gamma and yet have a reasonable maximum density, one will choose an exposure on the low side, down one or two stops from that indicated by Young's arithmetical solution.

For the moon at either quarter, the situation is different. Then the interesting lunar area is that near the terminator, where the natural contrast is high. One may reasonably choose to expose for the partly illuminated areas and develop for less than full contrast. One might also take into account the fact that the surface brightness of the moon at first quarter is about 20 percent greater than at third quarter.

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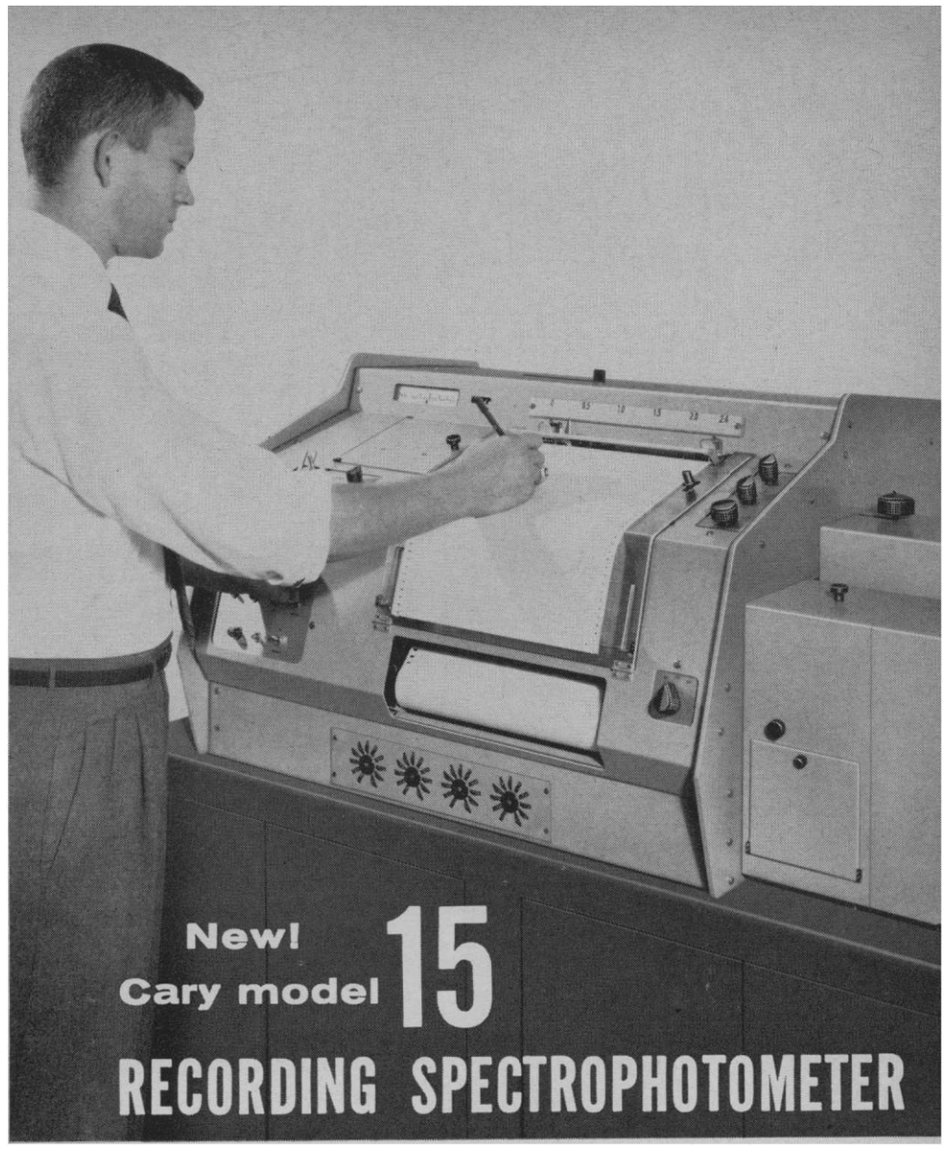

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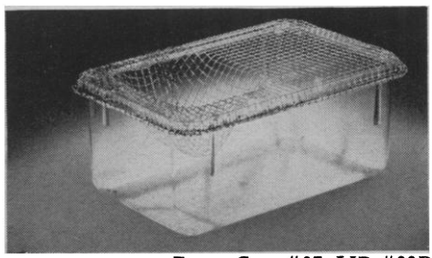

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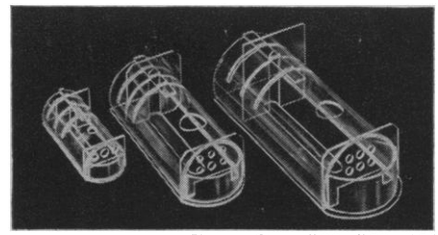

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CHARLES H. SMILEY
Ladd Observatory, Brown University, Providence, Rhode Island

Inquiry into Racial Differences

I agree with the ideas expressed in the letter by Leon S. Mickler on "Racial differences" [*Science* **133**, 202 (1961)].

The proposition that all races are genetically equal in mental abilities has become a part of conventional wisdom, but, in my opinion, none of the supporting evidence meets requirements for proof. It is also unproven that racial differences in mental abilities and achievement have a genetic basis, but it seems to me that the weight of evidence is strongly in favor of this conclusion. The lack of culture-free tests of abilities, problems of sampling and control, and the fact that racial groups are not pure are all barriers to proof. There are methods of studying the problem that have not been tested, and the question could be answered with reasonable certainty, although the procedures would be tedious and costly. We should support inquiry and debate of this question for two reasons. First, science should continue as the free pursuit of knowledge; we should make no rules which stop people from thinking. Second, additional information on racial differences may be required in order for society to work intelligently toward removing the causes of racial problems.

I agree with Mickler that new information on the genetic basis of mental abilities should not threaten the legal or moral rights of any race. It is possible, however, to hold to the principle that each individual be appraised on his aptitudes and behavioral standards without regard to race and, at the same time, to face the possibility that the random mixing of races in schools and housing as a means of achieving desegregation is neither scientifically sound nor morally right. It may well be, if civilization survives and racial bias disappears and each individual is free to move ahead according to his aptitudes and drives, that, although individuals of every race will achieve excellence in every field, there will continue to be important racial differences in interests, aptitudes, and kind of achievement.

DWIGHT J. INGLE
Department of Physiology, University of Chicago, Chicago, Illinois