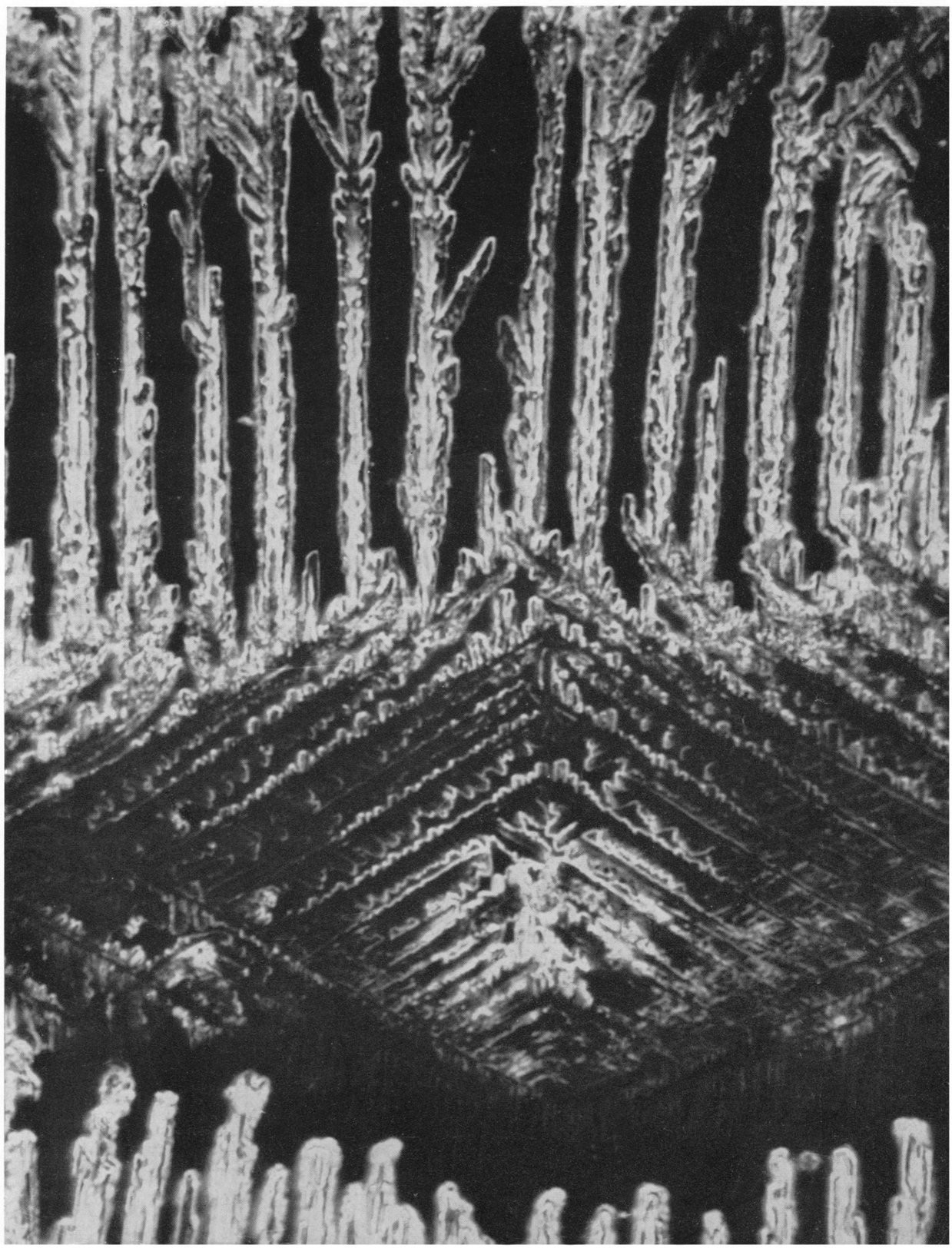
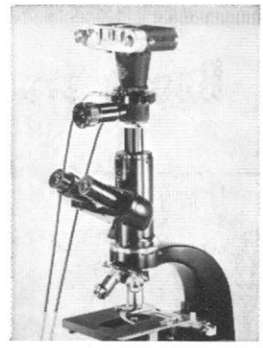
SCIENCE

19 May 1961

Vol. 133, No. 3464

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE





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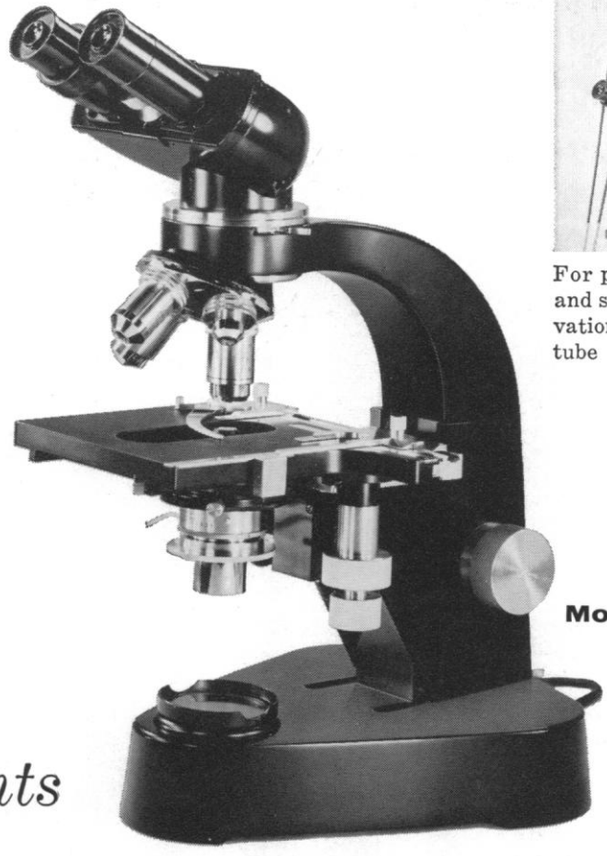
the laboratory microscope which fulfills research requirements



Model S 48/76

the new LEITZ LABOLUX IIIa

The Leitz Labolux IIIa is a new laboratory microscope with built-in illumination and the famous Labolux ball-bearing focusing control, which combines both coarse and fine focusing in a single knob. Available with a wide variety of accessories, the Labolux IIIa is recommended for all routine laboratory work and, in addition, can be equipped to fulfill the most exacting research.

A variety of interchangeable tubes is available: monocular, binocular or trinocular (binocular viewing plus photo tube for photomicrography). Tubes can be rotated 360° so that the Labolux IIIa may be faced away from the observer, for increased accessibility to all controls and to the object stage, and to make "conference-viewing" by two consultants more convenient.

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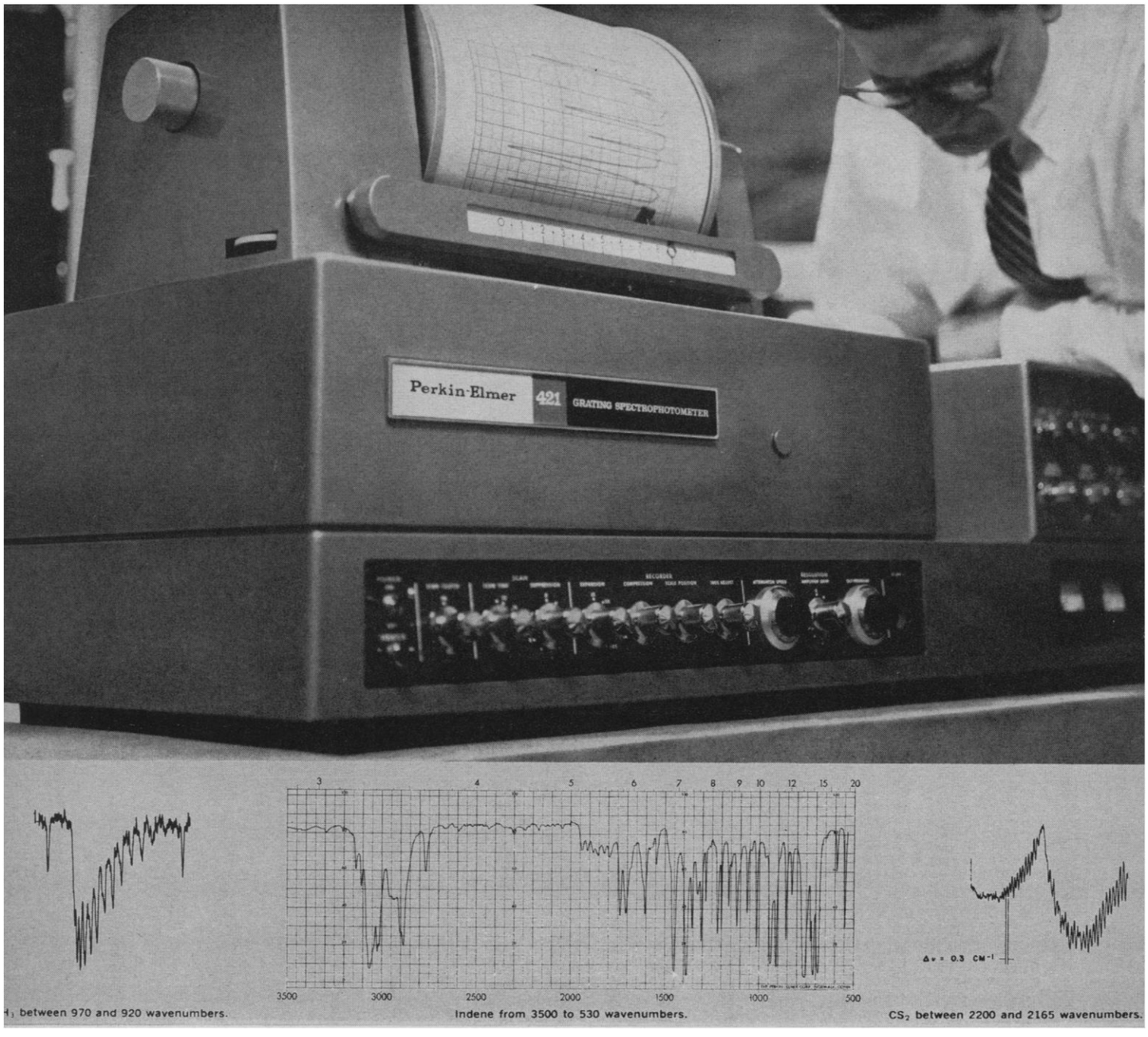

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NEW MODEL 421 HIGH-RESOLUTION SPECTROPHOTOMETER OFFERS UNINTERRUPTED SCAN FROM 2.5 TO 18 MICRONS

Highest grating resolution and greatest flexibility over an extended wavelength range—with no gaps or overlaps—the new Perkin-Elmer Model 421's performance means these advantages:

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19 May 1961, Volume 133, Number 3464

SCIENCE

3 of the 33 NEW ITEMS

featured in our new issue of

WHAT'S NEW FOR THE LABORATORY

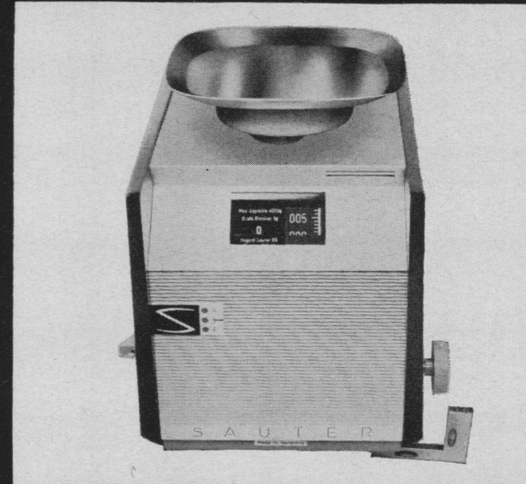
Ask for No. 43!

It's easy to separate, detect and identify the components of a syringe-injected sample with this new "Precision" instrument—designed for ambient temperature work. The *Chronofrac* can be set up ready for use in ten minutes. Practically any gases and volatile liquids with boiling points to 105°C can be analyzed. Either air or helium may be used as the carrier. *Details are given in bulletin 619, sent on request.*

C-4280X "Precision" Chronofrac, 11"x13"x5" high $250.00

"Precision" CHRONOFRAC

This *Toppan* scale has a unique full-floating indicating mechanism for fast, correct weighing—regardless of off-level conditions. Simply place it on a table, put the sample on the pan, and read the weight. For weighing beyond the optical range, just remove a substitution weight. The added capacity is indicated in the aperture adjacent to the projection scale. Features include front and rear direct-projection reading . . . magnetic damping for quick oscillation and stability of projection scale . . . extra-large interchangeable pans. There are six models (three with tare, three without) in three capacities—1000, 2000 and 4000g. Prices range from $580.00 to $730.00. *Write for descriptive brochure.*



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Even the minutest change in moisture is detected by this new portable unit which monitors water vapor continuously in ppm ranges of 0-10, 0-30, 0-100, 0-300 and 0-1000—selected by a 6-position switch. Accuracy is ±5% of the full scale in any of the ranges. Response is rapid. Reading is direct. The sample flow rate is 100cc per minute, adjustable for any gas stream. Sample pressures are 10 to 100 psig. The unit plugs into any 100-125 volt AC outlet—or it may be operated from the self-contained 67½-volt battery. Comes complete with electrolytic cell. *For the entire story, ask for bulletin 773.*

H-9560X Beckman Hygromite, 11" wide x 6.7" high x 10.7" deep $495.00

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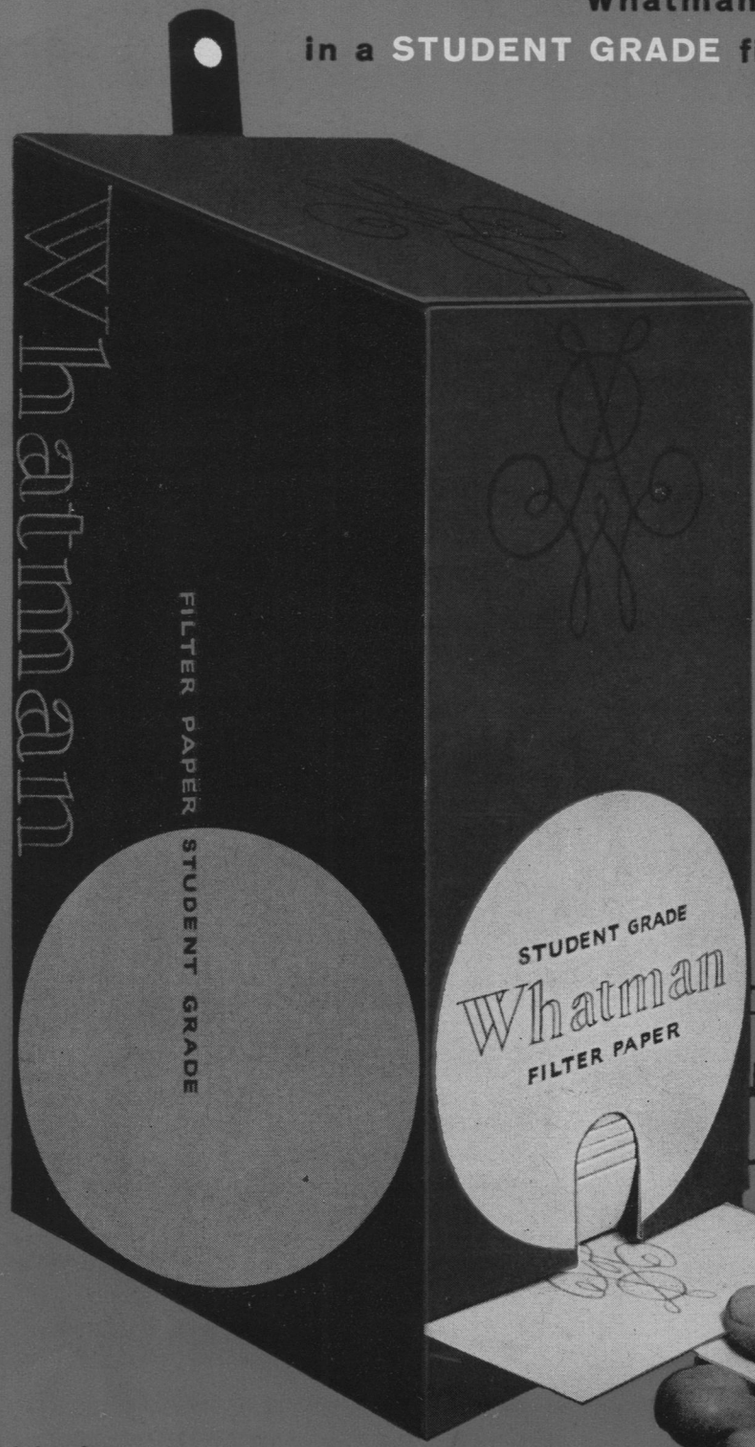

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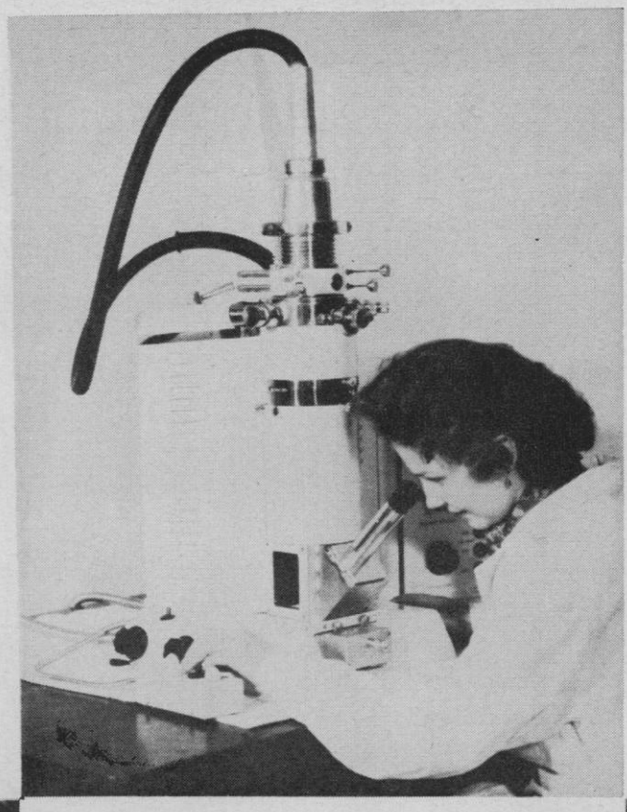

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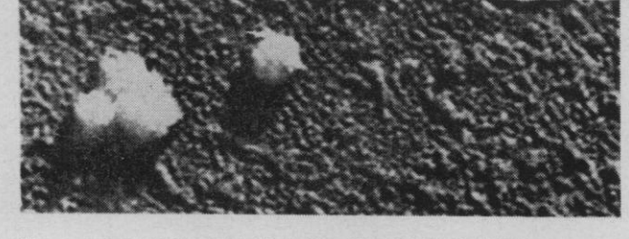

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NEW TORSION DIAL BALANCES with WEIGHT LOADERS

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*

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Capacity: 200 grams
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(Readability: .005 g)

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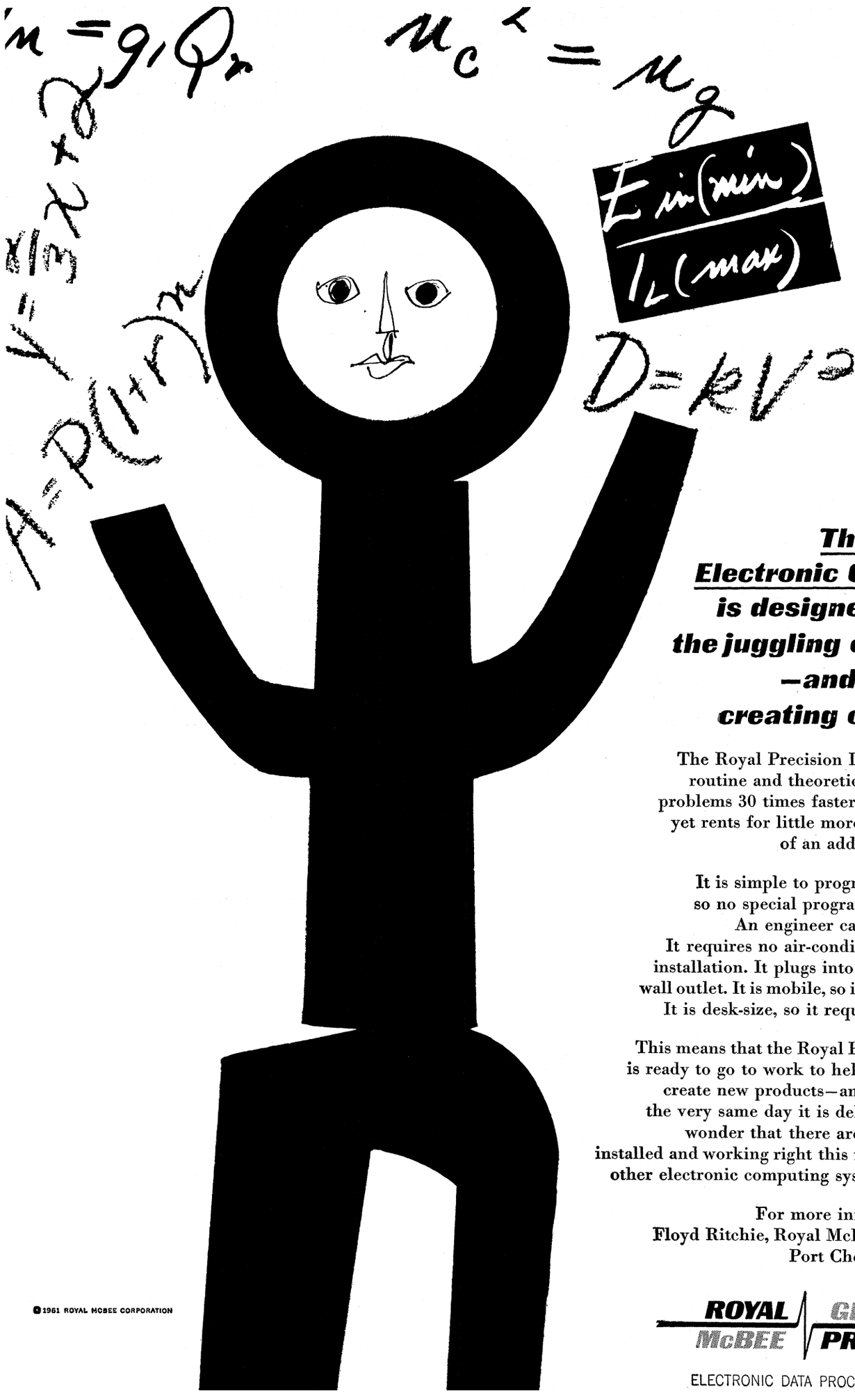
Fine Weighing Dial: 10 grams by .1 gram graduations
(Readability: .02 g)

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.



The LGP-30 Electronic Computer is designed to stop the juggling of figures —and start the creating of profits.

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It is simple to program and operate, so no special programmer is needed. An engineer can use it himself. It requires no air-conditioning or costly installation. It plugs into any 110-volt AC wall outlet. It is mobile, so it rolls anywhere. It is desk-size, so it requires little room.

This means that the Royal Precision LGP-30 is ready to go to work to help your company create new products—and fresh profits—the very same day it is delivered. Can you wonder that there are more LGP-30's installed and working right this minute than any other electronic computing system in its class?

For more information: write Floyd Ritchie, Royal McBee Corporation, Port Chester, New York.

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** The Picker Nuclear Division is prepared to help interested institutions in drawing up training programs in the use of nuclear techniques in biology, chemistry, medicine, agriculture, physics, and other fields.*

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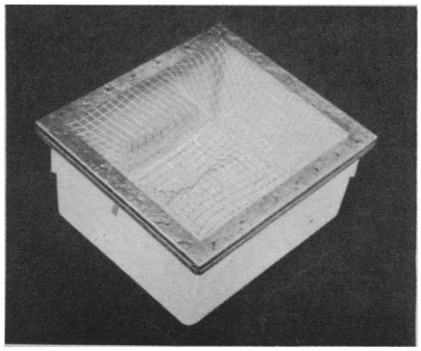
A Mousellany of Animal Care Developments

New Standard Of Living For 750,000 Hamsters

Last year more than 750,000 hamsters were used in research. Following about 20 million mice, 10 million rats, and 800,000 guinea pigs used annually, hamsters are now the fourth most widely used experimental animal. Hamsters are increasingly important in infectious and parasitic disease investigations, vascular studies, nutrition and dental research, the study of estrogenically active compounds, and in cancer research.

The Institute of Laboratory Animal Resources, an agency of the NAS, as part of its research animal standards program, has just published "Standards for the Breeding, Care and Management of Syrian Hamsters." These Standards are designed to minimize biological variability by suggesting methods of standardizing and controlling genetic and environmental factors regarding hamsters.

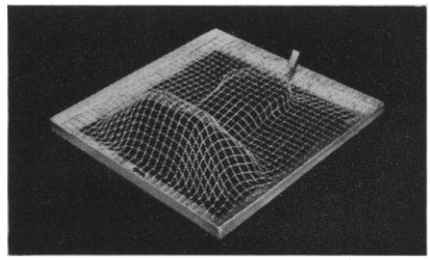
The Standards, prepared by a committee of animal care experts, make recommendations regarding construction and air conditioning of facilities, and regarding such equipment as cages, racks, food hoppers, water bottles, and mechanical cage washers. The Standards also cover food and bedding, cleaning techniques, refuse removal, disease and parasite control, and record keeping. *Copies of the Hamster Standards are available on request from the Executive Secretary, Institute of Laboratory Animal Resources, National Academy of Sciences, Washington 25, D.C.*



Econo-Cage #55, LID #52ES

Hamster Housing That Meets The New Standards

Now, the new "50 Series" of cages and lids designed especially for hamsters is available from the Econo-Cage Division of Maryland Plastics, Inc. This series complies in all respects with the new ILAR Hamster Standards, just as the "20" and "30 Series" more than meet ILAR mouse standards and the "40 Series," ILAR Rat Standards.



Econo-Cage Lid #52ES

The hamster standards recommend a minimum cage area of 150 square inches per nursing female including litter, or a minimum of 15 square inches for each animal over 3 months old, and a depth of not less than six inches. The "50 Series" Econo-Cages *exceed* these specifications in all dimensions. They are 12″ by 14″ by 6½″ deep (i.d.) and 12⅞″ by 14⅞″ by 6⅝″ deep (o.d.), a floor area of 168 square inches... room for 11 adult hamsters per cage.

In further accordance with the standards, the cages are molded with a smooth seamless finish and sides, bottoms and corners are slightly rounded so that there are no dirt-catching crevices. Because they are impervious to liquids and moisture, "50 Series" Econo-Cages are far more corrosion resistant than galvanized pans (and they are far less expensive than stainless).

Polypropylene Econo-Cage #55 can be exposed to repeated cage washings—even to repeated autoclaving—without distortion. Its impact resistance is excellent. The clear hamster unit, Econo-Cage #53 made of acrylonitrile-styrene copolymer, affords constant and immediate visual access at reasonable cost. This unit should not be exposed to temperatures above 200°F, but can take repeated washings at that temperature.

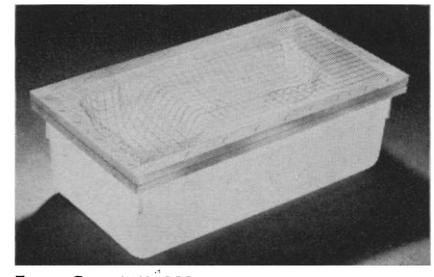
Clamping The Lid On Animal Housing

Newly designed lids, with built-in food hoppers and water bottle pockets, *have special spring steel hold-down clamps which securely fasten the lid to the cage.* The cages are escape proof as the hamsters cannot move the lids from the inside. The lid can be easily snapped into place or removed by the operator.

Lids for the "50 Series" of Econo-Cages are made entirely of metal. The new cover, 52ES, a single piece of #2 heavy gauge galvanized mesh, is deep drawn to form a feeder and water bottle pocket, and riveted to a heavy galvanized frame.

The deep feeder is designed so that animals can obtain food from the entire surface. The food hopper complies with the standards which recommend at least 1″ of clearance between food hopper and bedding. The water bottle pocket, a new built-in Econo-Cage feature, holds most bottle styles and sizes, saves space too.

Lids are also available with a flat mesh top for use with internal feeding systems. Either deep drawn feeder or flat style lids, can be made with or without hold down clamps, deep drawn feeder with spring clips (52ES), deep drawn feeder without spring clips (52E), Flat Lid with spring clips (52FS), flat lid without spring clips (52F). All four lid styles are interchangeable with "50 Series" Cages.

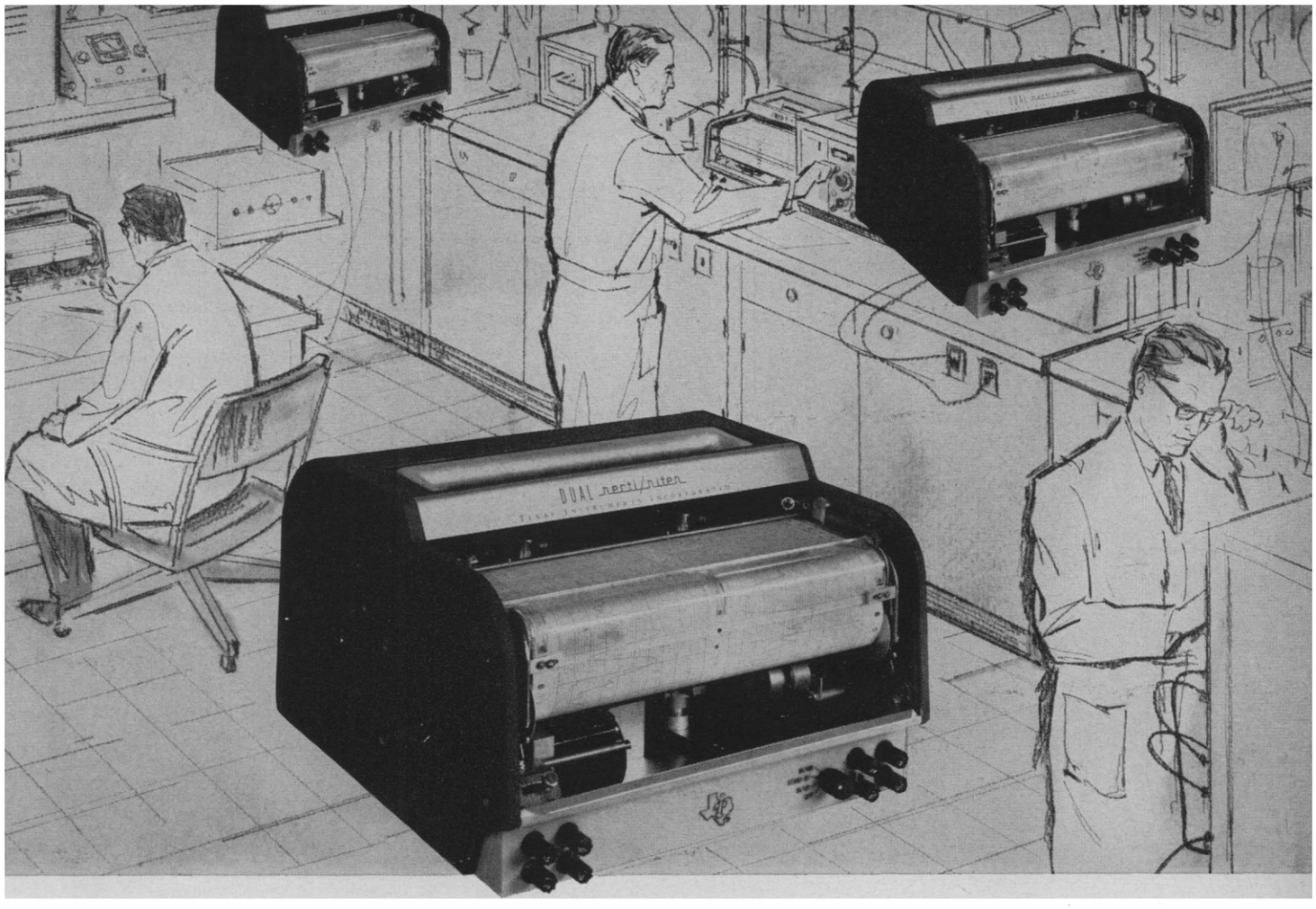


Econo-Cage #45, LID #42E

A Note About Versatility

New "50 Series" cages and lids not only meet ILAR Hamster Standards, but comply in all respects as well with standards established for the breeding and housing of laboratory rats. Conversely, "40 Series" Econo-Cages (10½″ by 19″ by 6⅛″ deep), originally designed for rats, meet the hamster standards. "40" or "50 Series" Econo-Cages now can be used interchangeably for hamsters and/or rats.

For more details on Econo-Cage line write for complete information, including "30 Series" breeding and holding cages for mice (10½″ by 19″ by 5⅛″) available in fibreglass as well as in the same materials as the "40" and "50 Series."



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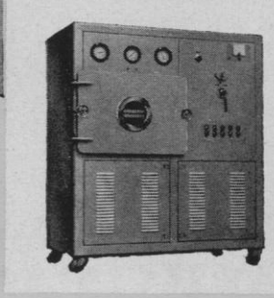

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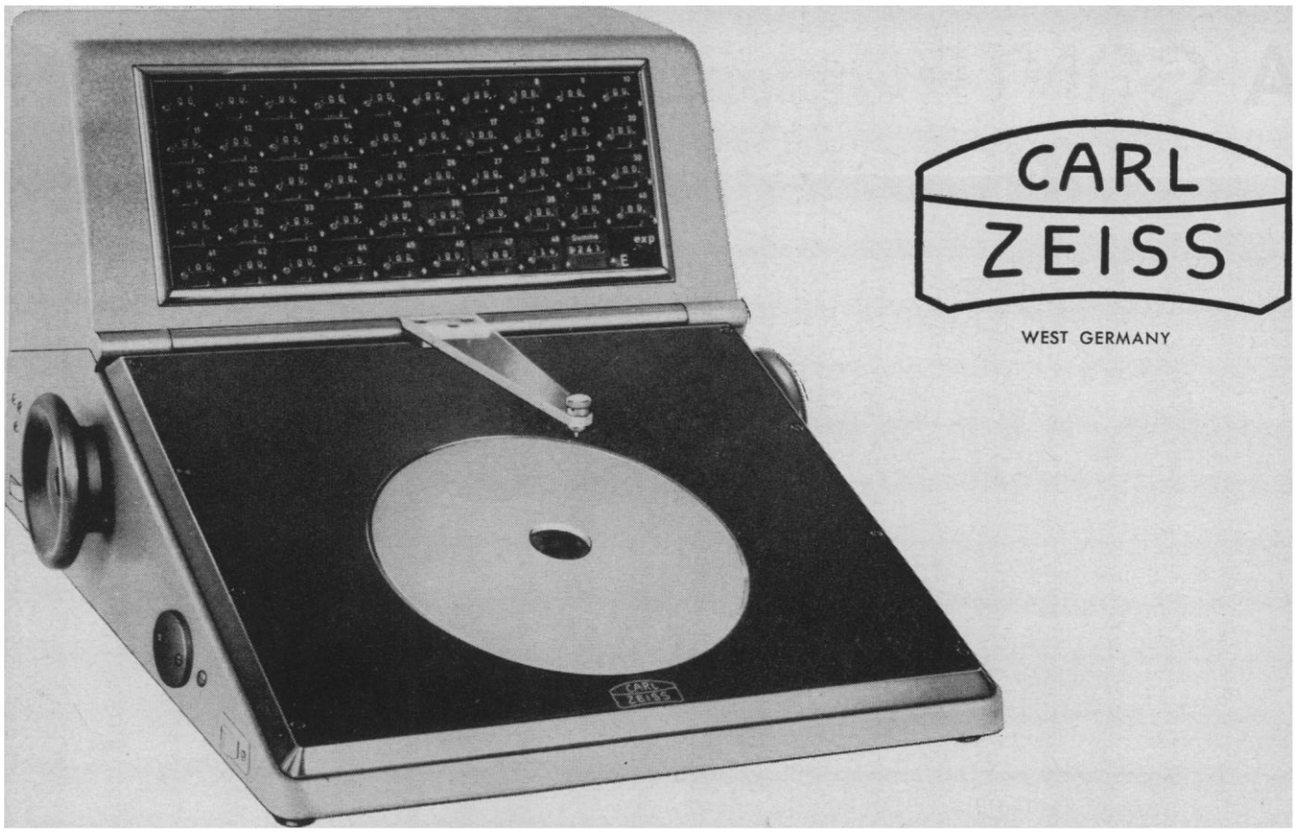

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There are many interchangeable shaker platforms. They have large capacities for flasks, tubes, and other culture and reaction vessels.

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The degree of agitation can be selected and the temperature controlled for the growth of aerobic and anaerobic organisms. Models are available with either Gyrotory® or reciprocal agitation, and illumination for photosynthesis studies. The PSYCROTHERM can also be used as a BOD incubator.

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CHAMBER DIMENSIONS: Width 32½", Depth 21", Height 26"

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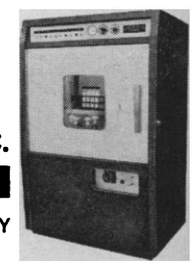


NO. 23 EPR AT WORK SERIES

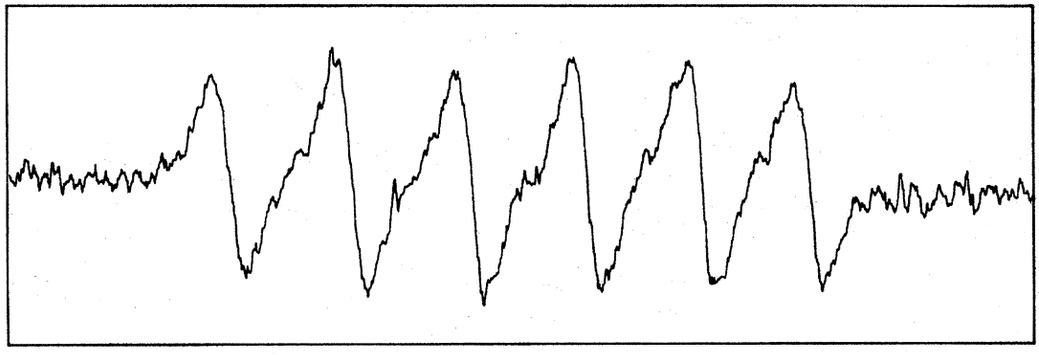
EPR HIGH SENSITIVITY IN AQUEOUS SOLUTIONS

(ELECTRON PARAMAGNETIC RESONANCE)

The V-4501 100 kc EPR Spectrometer, when used with the V-4548 Aqueous Solution Sample Cell, offers to the scientist excellent sensitivity for investigating paramagnetic species in solvents of high dielectric loss. Manganese in aqueous solutions at 10^{-6} molarity can be observed with approximately 10:1 signal-to-noise ratio.

10^{-6} Molar Mn^{++}

Modulation . . . . . 20 gauss peak-to-peak
Response . . . . . 1 second
Power at Cavity . . . 180 mW
Sample at Room Temperature



Electron paramagnetic resonance in aqueous solutions is complicated by the fact that water has a high dielectric loss at typical microwave frequencies. The V 4501 Spectrometer employs a rectangular TE_{102} resonant sample cavity. The difficulty of dielectric loss in this sample cavity can be overcome by using a flat sample cell which constrains the sample in the nodal plane of minimum r-f electric field (and maximum r-f magnetic field).

With the increasing use of EPR in various fields of biology, this development is of considerable significance. For example, there is a rising interest in the role of metals in biological systems. Many of these metals happen to be paramagnetic with concentrations which vary from 10^{-4} to 10^{-7} molar. Use of high sensitivity EPR equipment often permits positive identification of the metal, a determination of its valence state, and a quantitative measurement of concentration.

A particularly important metal is manganese, which, as Mn^{++}, has been detected in enzymes and living cells. A dilute aqueous solution of this ion is therefore a suitable material to investigate the sensitivity of EPR equipment for biological applications. The spectrum above is a trace of 10^{-6} molar Mn^{++} obtained with the Varian EPR Spectrometer system. The well known six line hyperfine pattern arising from the 5/2 nuclear spin of Mn^{55} is evident. To check the reproducibility, quantitative 10^{-6} M solutions of $MnCl_2$, $MnSO_4$, and $MnNO_3$ were prepared, and the observed signal heights of these three samples were found to be the same within $\pm 5\%$.

For literature which fully explains the 100 kc EPR Spectrometer and its application to basic and applied research in physics, chemistry, biology and medicine, write the Instrument Division.

VARIAN associates
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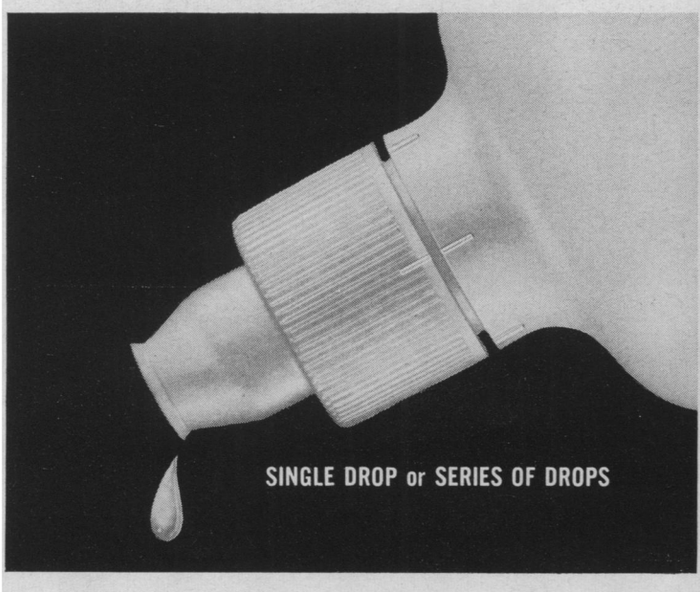
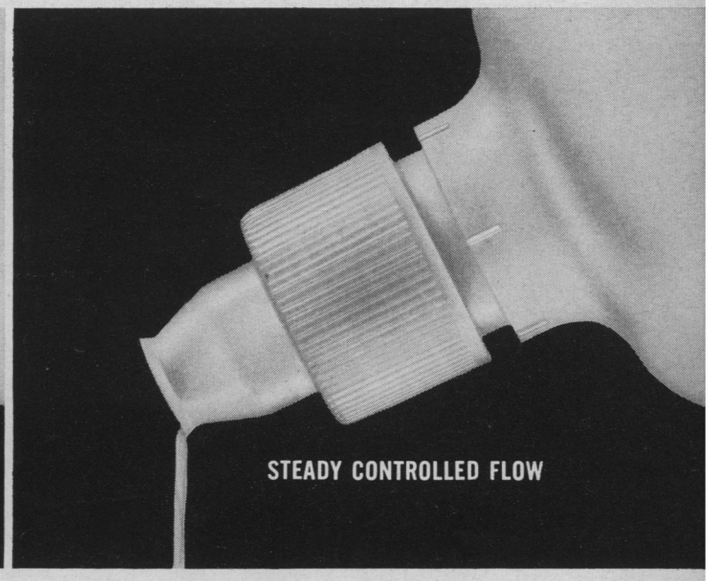
New ADJUSTA-FLOW HF bottle* lets you "dial" precise rate of flow





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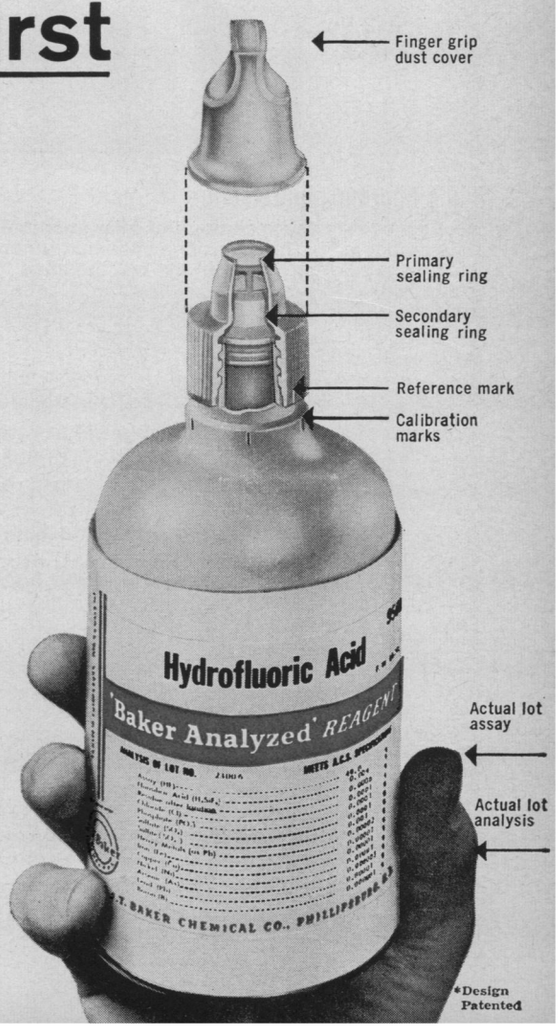

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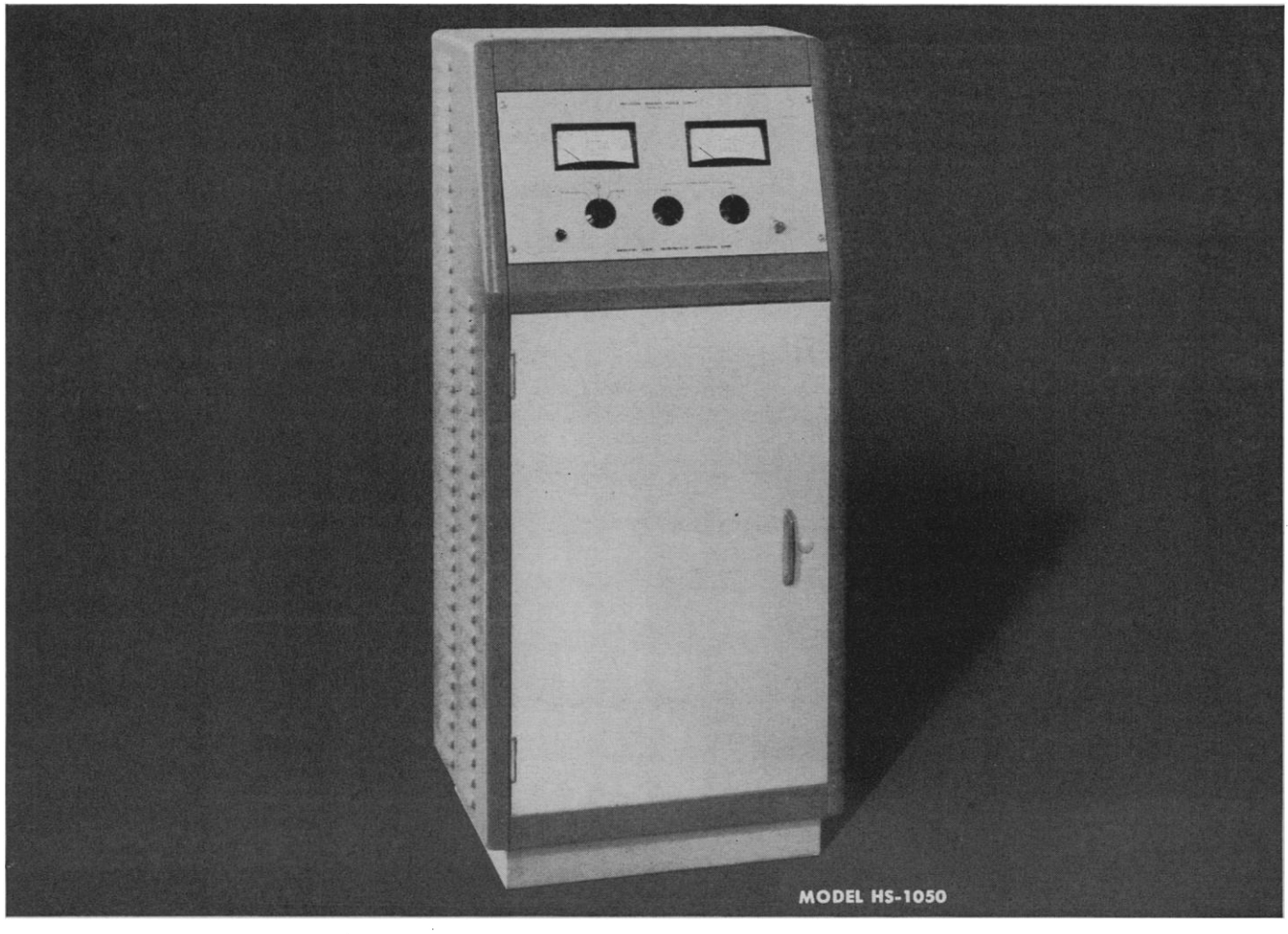

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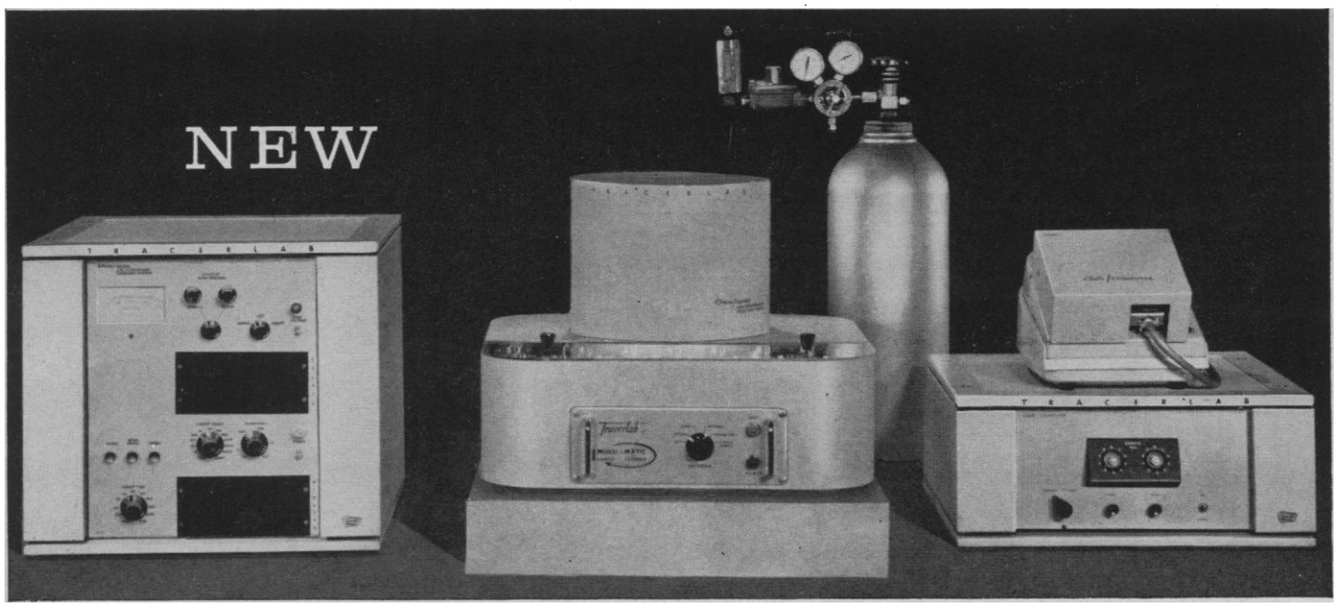

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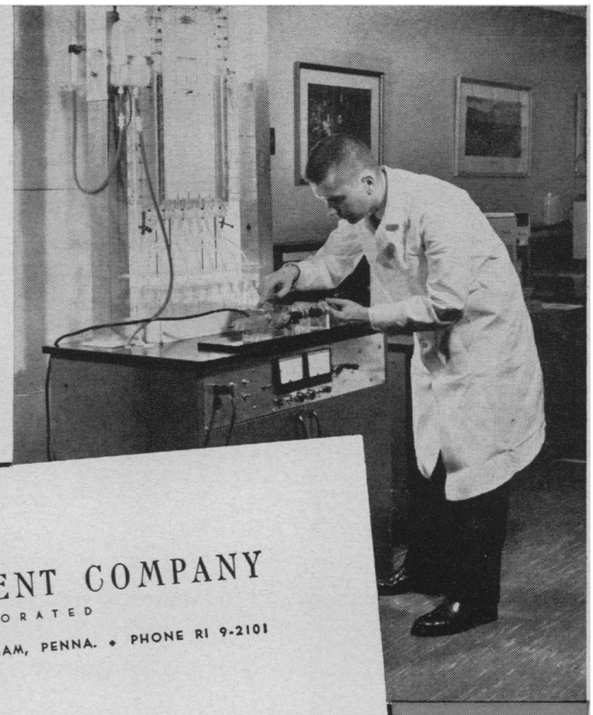

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Basic Research at Honeywell
Dr. Finn Larsen
Vice President for Research

Studies in the Magnetic Properties of Thin Metallic Films

Temporary or transient memories of electronic computers consist of small doughnut-shaped ferrite cores hand-assembled into many complex matrices. Bulk, speed of response and costly manufacture create inherent limitations. It now appears possible to overcome these by replacing ferrite cores with tiny spots of magnetic film vapor deposited on a smooth flat surface.

Today's electronic computer has a memory which is part of the brain of the machine. Larger machines commonly have two memories: one for permanent storage of information, the other for temporary storage of more transient information. The temporary memory consists of a collection of ferrite cores, each core shaped like a tiny doughnut and having a number of wires laced between it and other cores forming a matrix or grid. The wires carry either the pulse of electricity which magnetizes the core, or a similar pulse which is the core's response to interrogation.

A series of these pulses, handled in a binary number system, have become the language of the computer. To function binumerically, circuits represent "0" by not conducting current, and represent "1" by conducting. Each memory core can be magnetized in one direction for "0", the opposite direction for "1." To avoid ambiguities, cores are made so they are not readily magnetized in any direction other than these two.

Each small ferrite core can be magnetized or interrogated in about a microsecond (one-millionth second). Unfortunately, the assembly of ferrite cores discourages automation processes, making manufacture slow and costly. In addition, the tremendous bulk of many millions of cores properly assembled prohibits machines requiring considerably larger transient memories.

Current basic research indicates that one of the most promising successors to the ferrite core is a tiny spot of magnetic film about 1,000 Angstroms (four millionths of an inch) thick, deposited on a smooth flat surface. These films have been prepared in Honeywell's Research Laboratories from an alloy of nickel and iron by heating the alloy until it vaporizes in a vacuum. Each freed vapor particle travels until it strikes a cooler surface. There it condenses and stays, if the surface is suitable and immaculately clean.

It might be assumed that the task would be simple. However, as the vapor condenses and becomes solid, it seems to become peculiarly sensitive to the nature of the surface on which it is being deposited. Unless oriented by a magnetic field (created by large coils that encircle the vacuum chamber), the films could be magnetized in a number of directions instead of along the desired single line. When we obtain uniformly bi-stable spots, we are in effect duplicating the action of ferrite cores. We also may use the same cycle by which bits of information are stored and extracted by reversing direction of the magnetic field.

The coercive force necessary to reverse (or "flip") the direction of magnetization within a thin film is very low. Another important advantage stems from the fact that reversal may be accomplished either by employing a rotational mechanism (simultaneous rotation of all atomic magnetic moments) or a wall-motion mechanism (sequential rotation of the atomic magnetic moments in the form of a moving wall). Both may be induced through application of a coercive force as small as one Oersted. Of the two mechanisms, rotational is much the faster; it makes possible the reading and writing of 100,000,000 bits of information per second on a single spot, as compared to about 100,000 for ferrite cores.

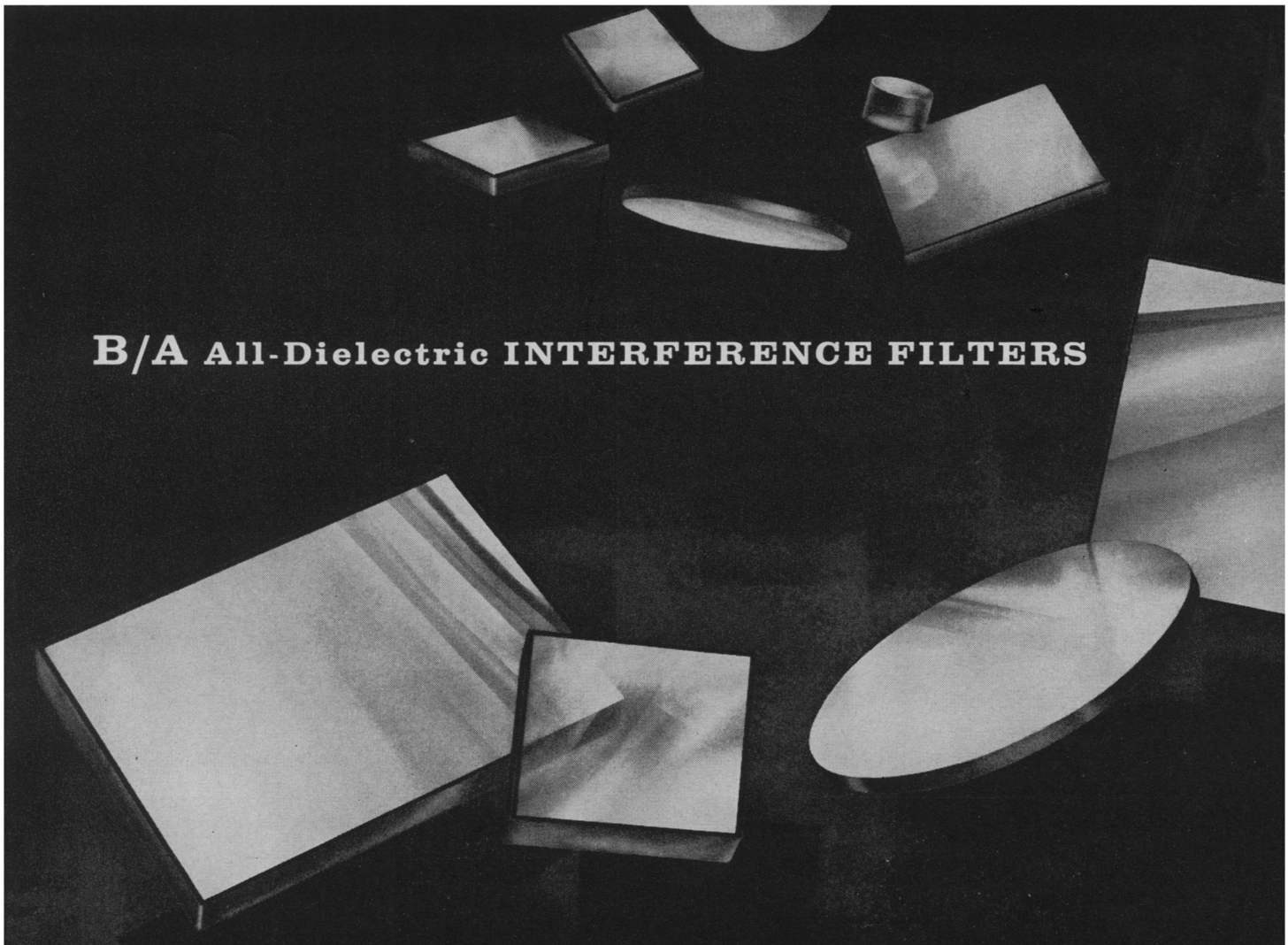
Honeywell scientists have consistently produced 256 bit (16x16) matrices uniform to plus or minus 5% of energy. Only this uniformity makes it possible to use the films in circuits, since a given small electrical pulse applied to any film must flip that film.

Uniformity has been achieved in part through study of deposition techniques and experiments both with various types of substrata and with various methods of cleaning them before deposition. It has resulted also, through broader understanding of the mechanisms involved, in causing reversal of the magnetic field. Even more important, however, have been detailed investigations into the factors that lead to non-uniformity, and subsequent development of techniques that tend to eliminate them.

The most difficult task remaining seems to be linking the film spots with printed circuits which will probably replace the wires used with the ferrite cores.

Our research on thin films is both basic and applied. Applied, since our scientists are trying to create better, faster, smaller memory systems for the commercial and military computers our engineers design; and basic, since they are trying to understand and explain all the phenomena described, as well as others that are completely baffling.

If you are engaged in magnetics research and would like to know more about Honeywell's work on thin magnetic films, you're invited to correspond with Dr. Richard Prosen, Honeywell Research Center, Hopkins, Minnesota. Or, if you would like a simplified explanation of the binary number system and how to perform standard mathematical manipulations using this system, write to Honeywell Research, Minneapolis 8, Minnesota.



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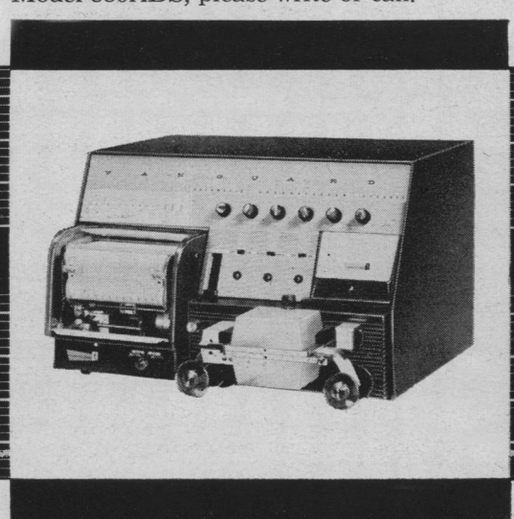
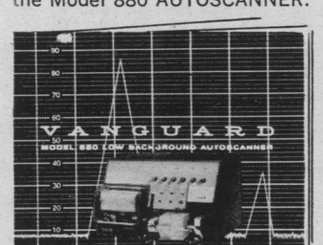
NEW STANDARD ACCESSORY, TOO! Also available is the new, exclusive Vanguard Model 880ADS, a completely automatic system for quantitative integration and digital presentation of radioactive zones. For complete details concerning either the Model 880 or Model 880ADS, please write or call.

VANGUARD 880



REQUEST THIS BROCHURE— See how you can achieve the highest detection efficiency available for chromatogram scanning. Booklet outlines distinctive features and lists all operational characteristics of the Model 880 AUTOSCANNER.



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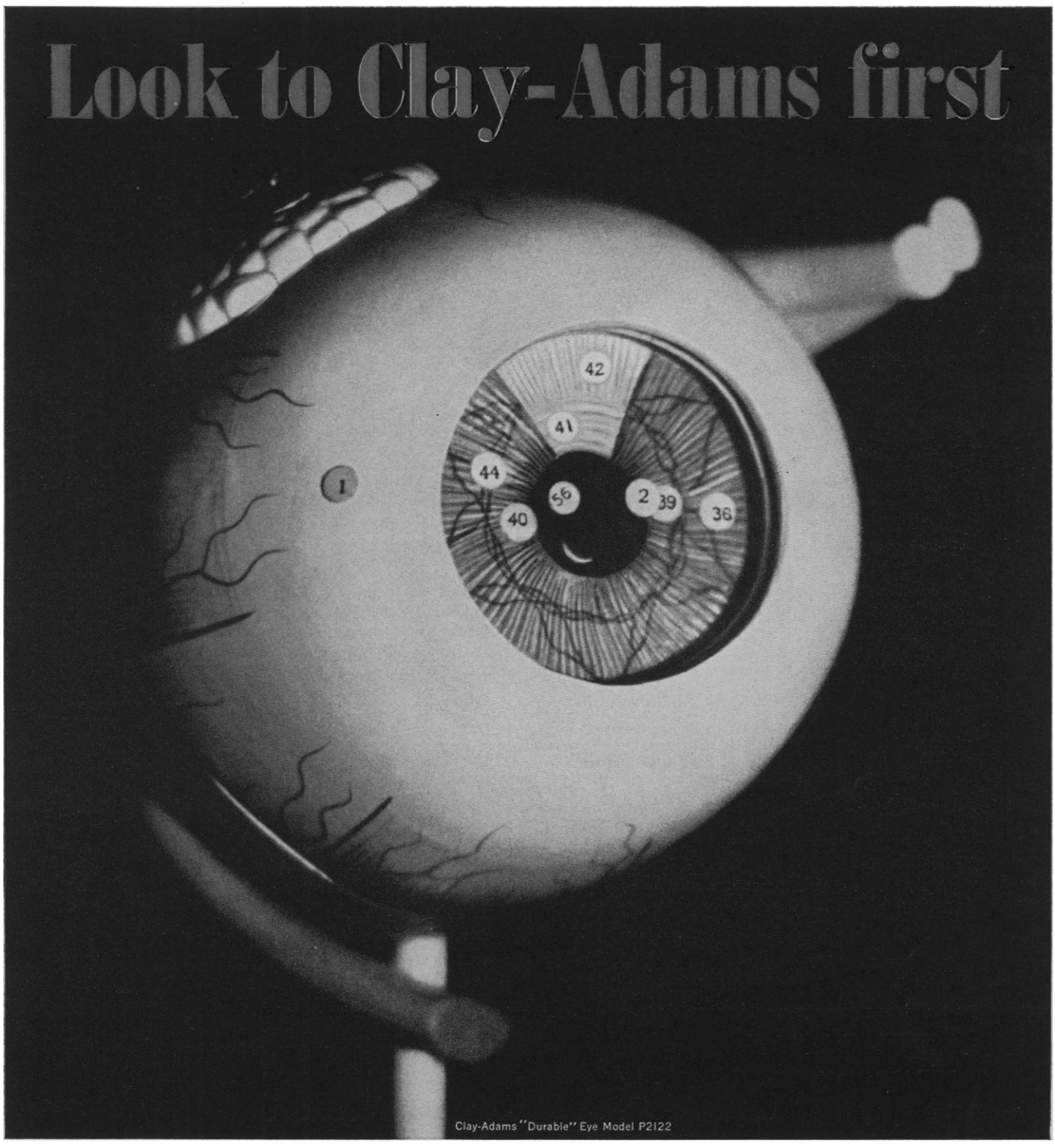

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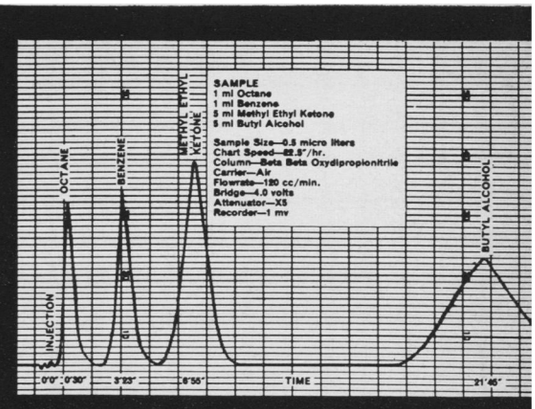

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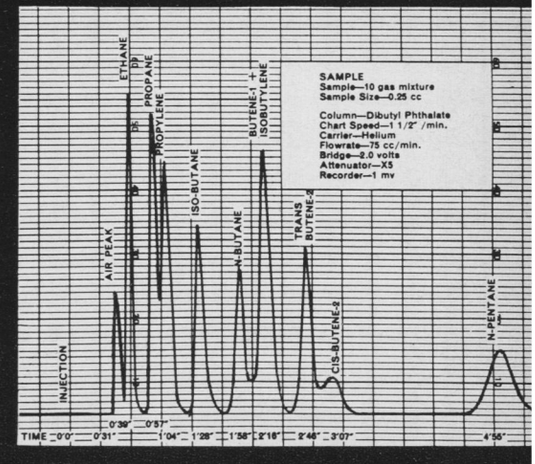

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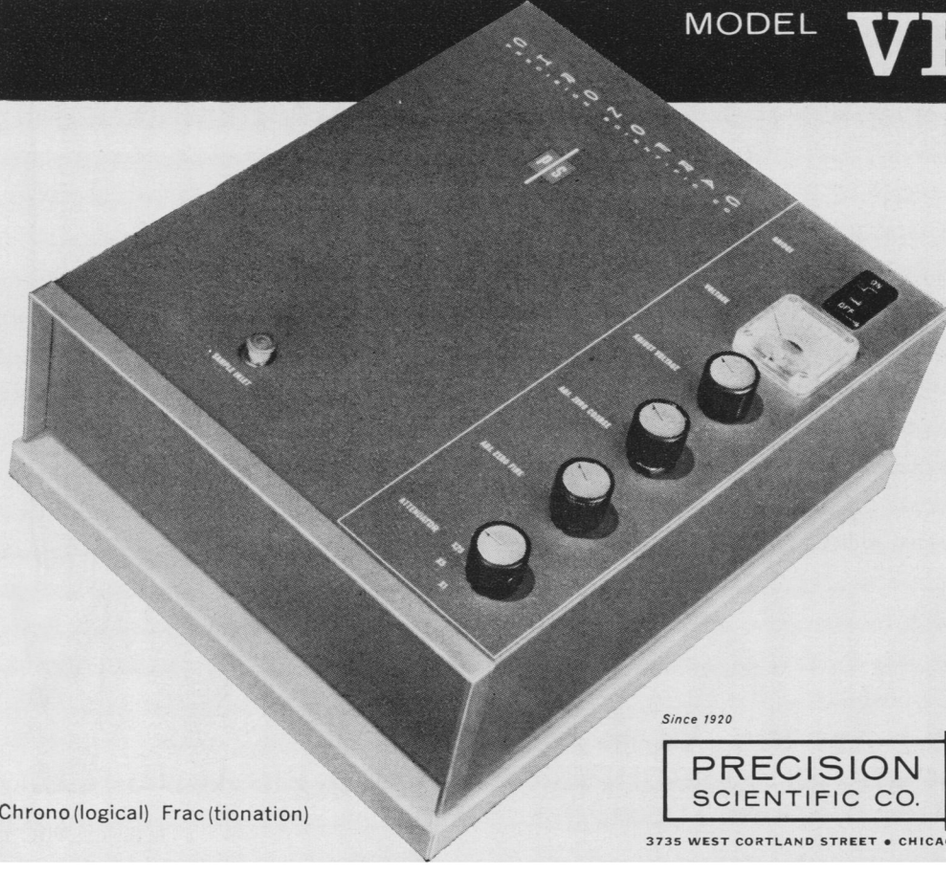
MODEL VP-I



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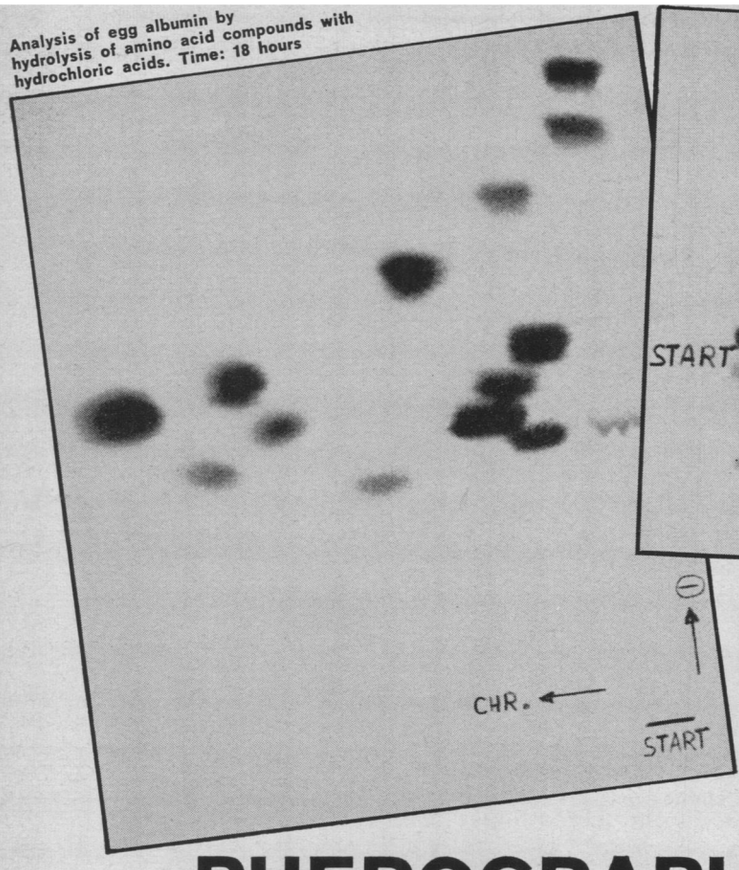
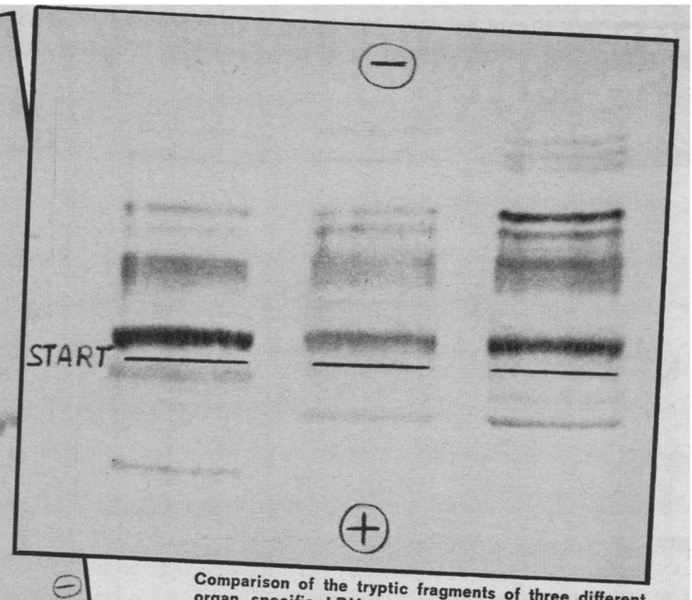


Analysis of egg albumin by hydrolysis of amino acid compounds with hydrochloric acids. Time: 18 hours



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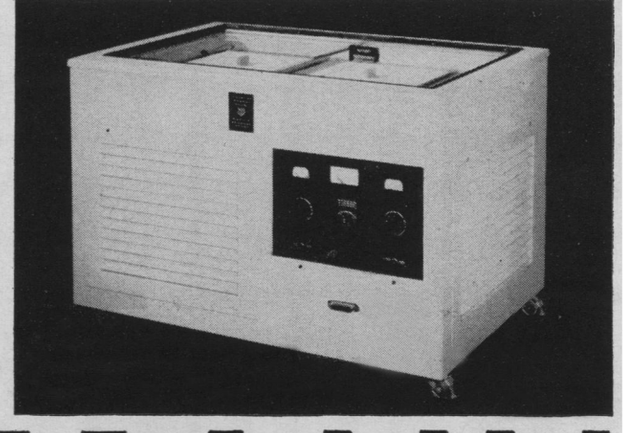

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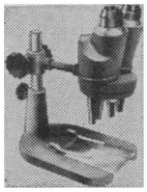

Years in development. Precision American made. Used for checking, inspecting, small assembly work. Up to 3" working distance. Clear, sharp, erect image. Wide, 3 dimensional field. 2 sets of objectives on rotating turret. 23X and 40X. 10 Days Free Trial.

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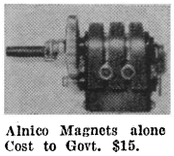

Brand new Signal Corps Electric Generator for scientific experiments, electrical uses, demonstrations. Generates up to 90 volts by turning crank. Use in high impedance relays. Charge ground and bring up night crawlers for bait or study. 2 Alnico Magnets alone worth original price. Wt. 2 lbs. Cost to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

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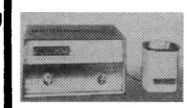

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All colors withstand the chemical durability tests outlined in Federal Specification DD-V-581-a.

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Your dealer has these color coded pipets in stock. For a complete listing of these pipets, send for Supplement No. 2 to Laboratory Glassware Catalog LG-2.

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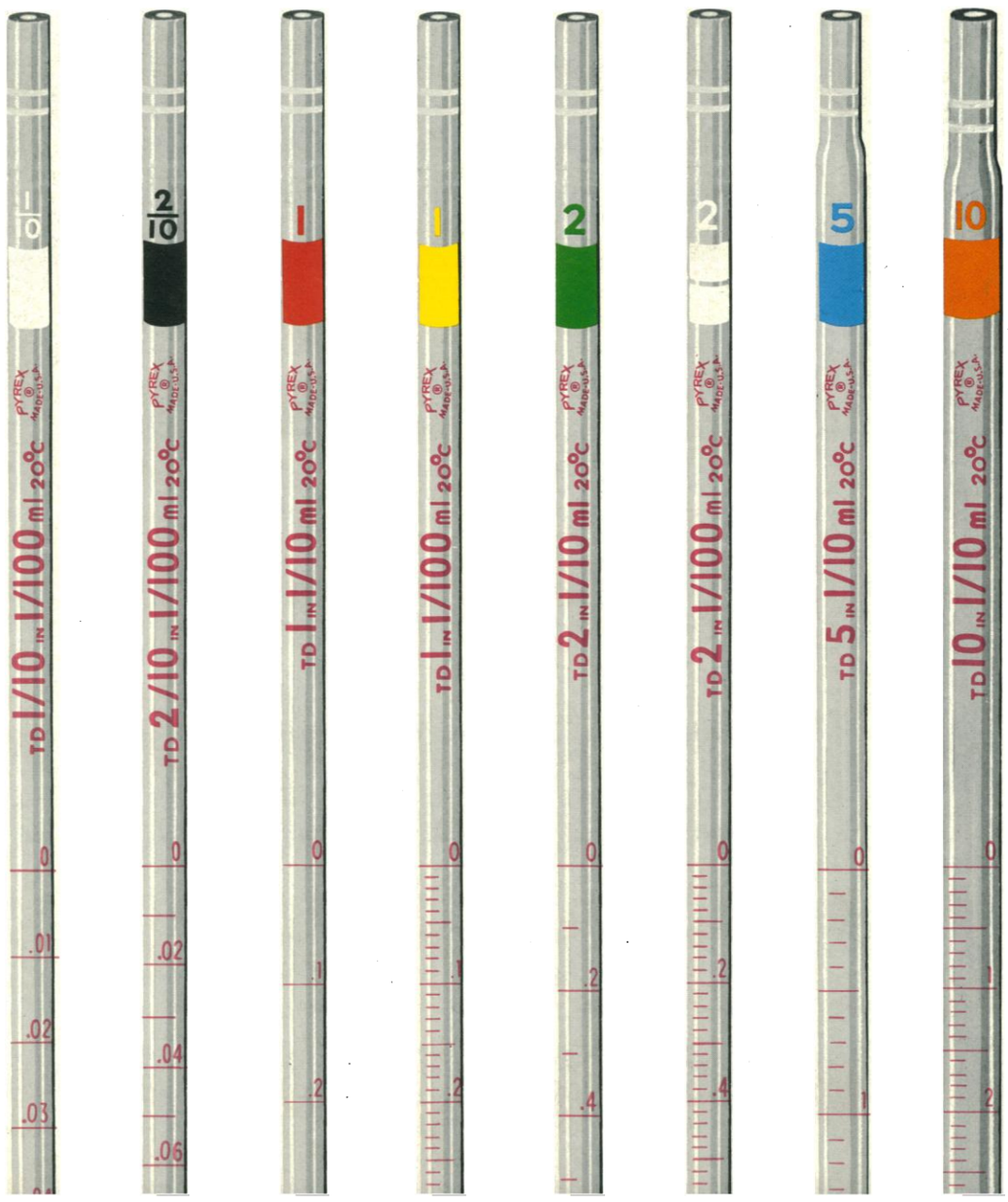
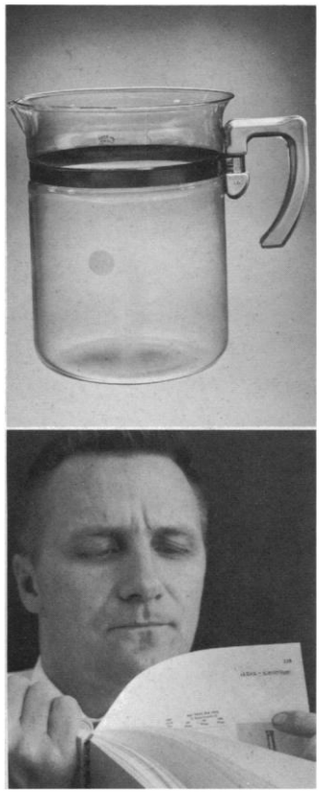
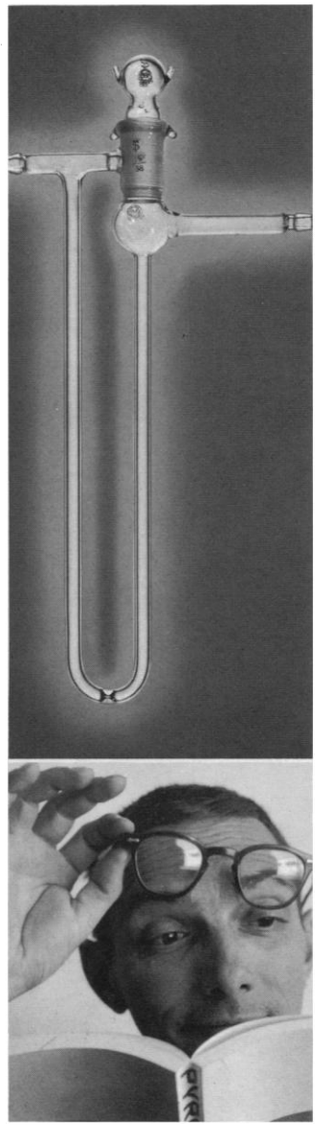


No. 1010
Beaker with handle



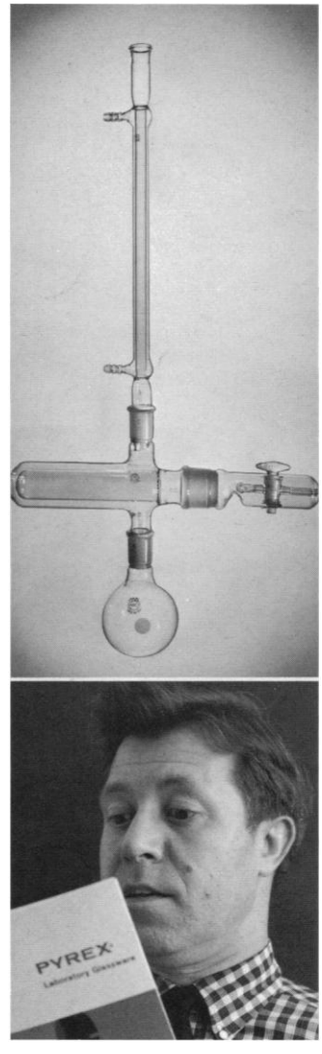
Hmm, a beaker with a handle. Makes sense. Should lift and pour as easy as a coffeepot – even when it's boiling hot. Won't need those clumsy tongs now.*

No. 5960
Flowmeter



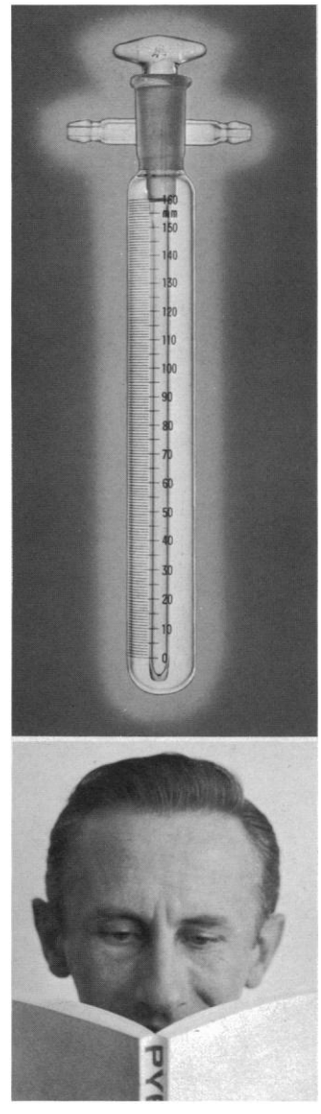
Four orifices on this flowmeter. From ¼ to 2 mm. Says you just turn the stopper to select the one you want. That's pretty simple.*

No. 3690
Drying Apparatus



Drying apparatus for small quantities. Works at constant temperature, under reduced pressure. I won't have to buy separate parts and make my own after all.*

No. 6952
Manometer



A "t" manometer. Scaled from 0 to 160 mm. The scale is red for easy reading. Available in single outlet also. Real easy to fill.*

*MORAL?

Whatever you need, it's probably in LG-2, the new PYREX Labware Catalog. It's the widest line anywhere. More than 9000 items.

Take beakers, for instance. There are 10 types of beakers in 55 sizes. Beakers with or without spouts. Tall or low form beakers. Conical or graduated ones. Micro beakers.

Need a beaker for everyday use? Try a PYREX brand standard model. One for work with light-sensitive substances? PYREX Low Actinic Ware is designed for it. For gas absorption or washing? Filtration? There's a PYREX brand fritted beaker. Using alkalies? Corning brand Alkali Resistant Ware is your best bet.

For high-temperature work up to 1500 degrees C., there's a VYCOR brand glass beaker of 96% silica.

Whatever you need, check the quantity discount allowances. They run as much as 23.5%. And, if you don't have a copy of our new catalog, LG-2, write for yours.

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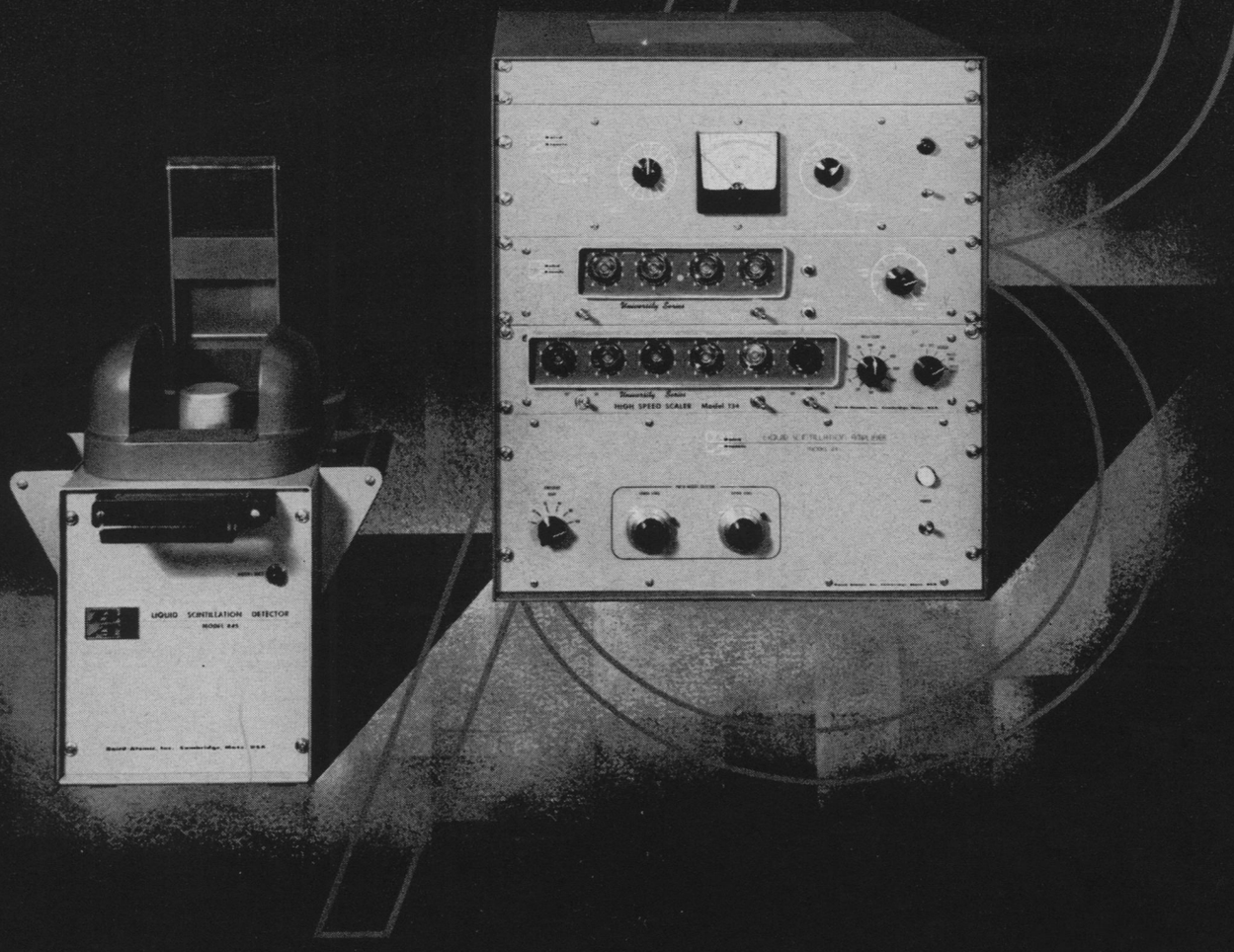

The development, field test, and customer acceptance of a versatile radioisotope manual liquid scintillation counting assembly has been completed. Here is a system for Carbon 14 and Tritium that incorporates simplicity and flexibility – that is in the price range of all laboratories.

Among its field proven features: simplified circuitry which assures reliable operation, negligible field maintenance; the high voltage supply, scaler and timer are all standard B/A instruments – they can be used with other systems and tailored to your special needs; specially designed, dark preadaption sample holder accepts a variety of sample configurations and sizes; the system is compact and versatile.

These – and other features of the Model 745 Liquid Scintillation unit – are available at the lowest cost you would expect to pay for reliable performance with liquid scintillation. You owe it to your budget to get more information – write or call your nearest B/A representative today.

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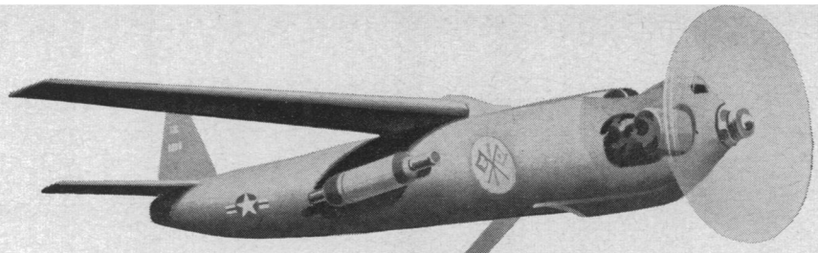
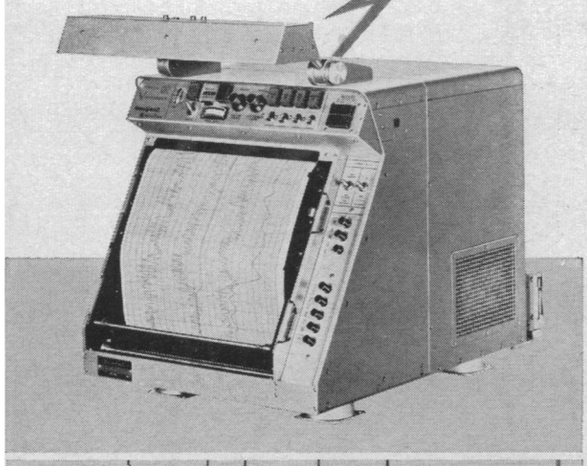
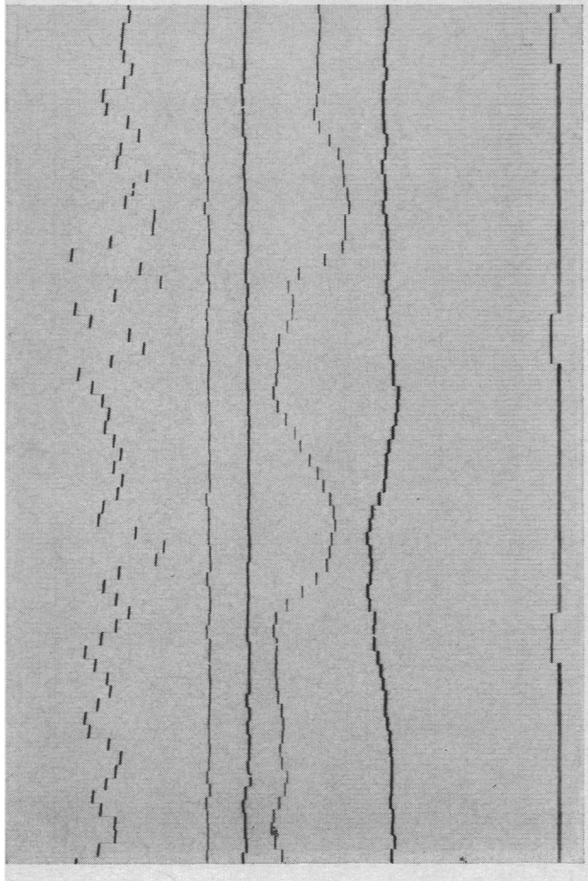


Tracking a Surveillance Drone with the Visicorder





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.

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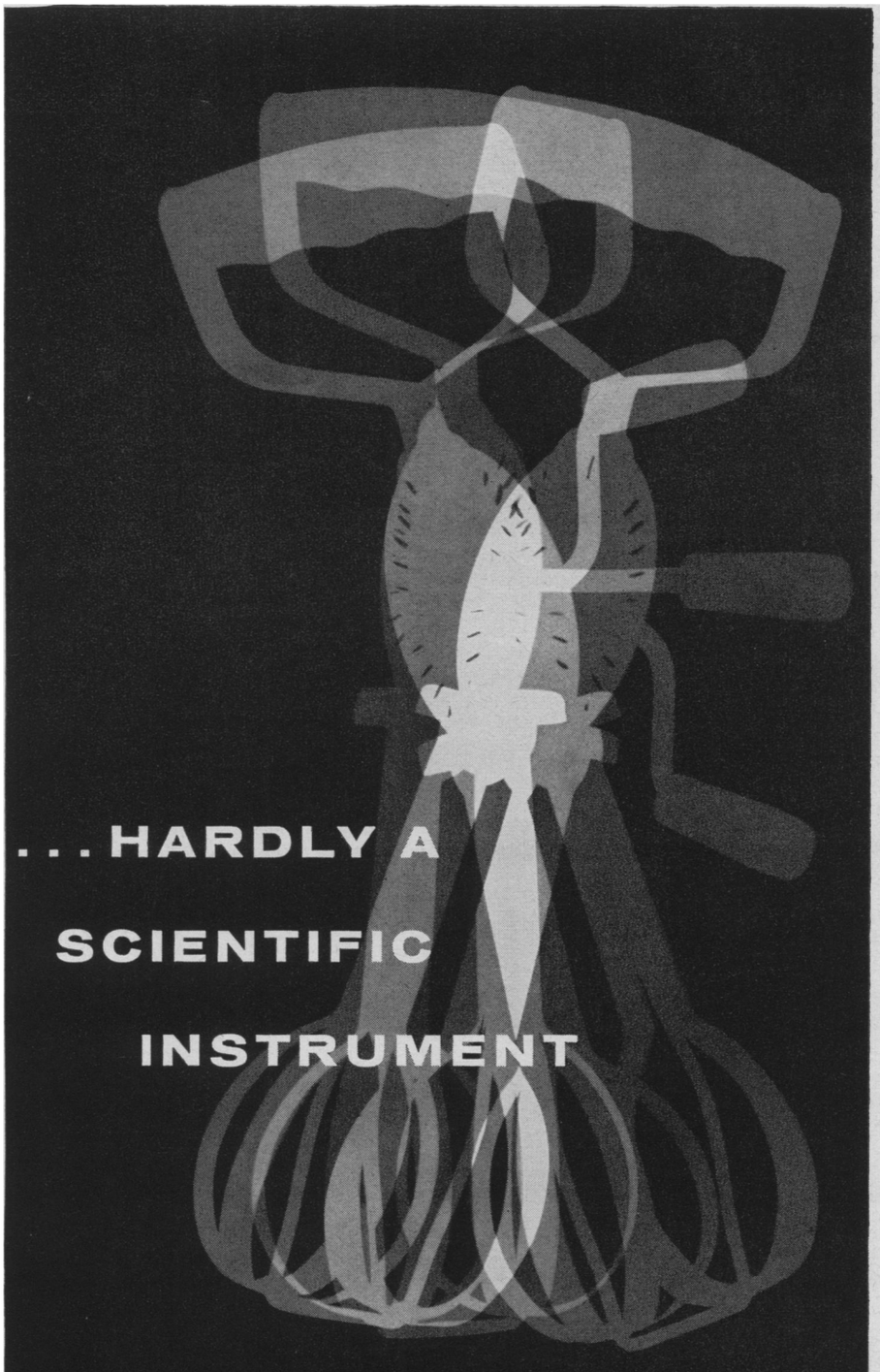

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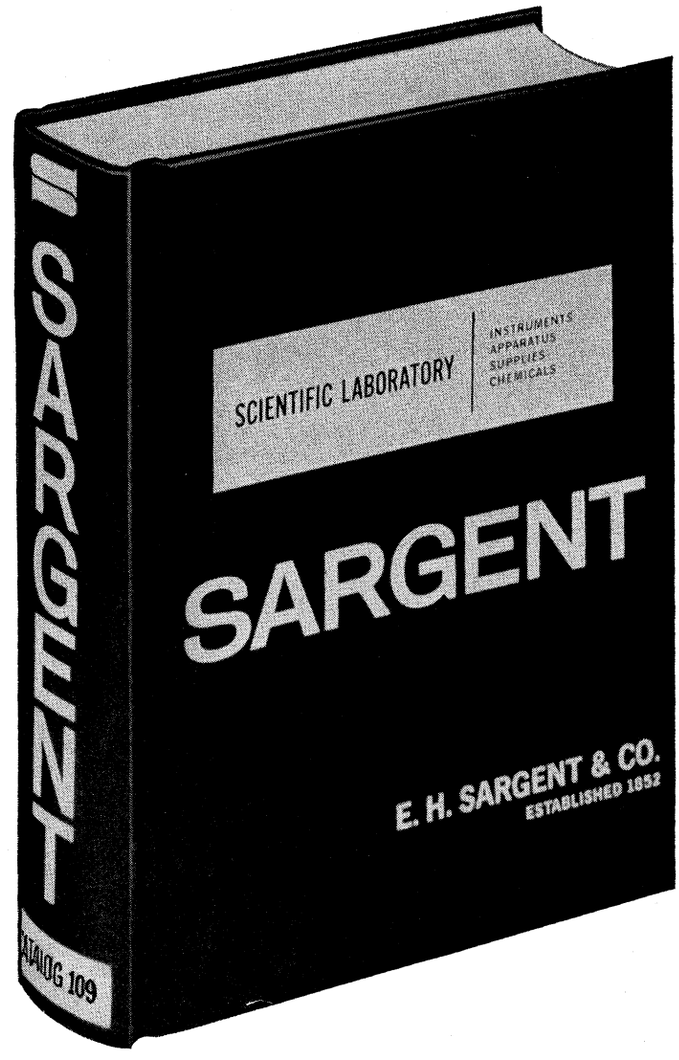

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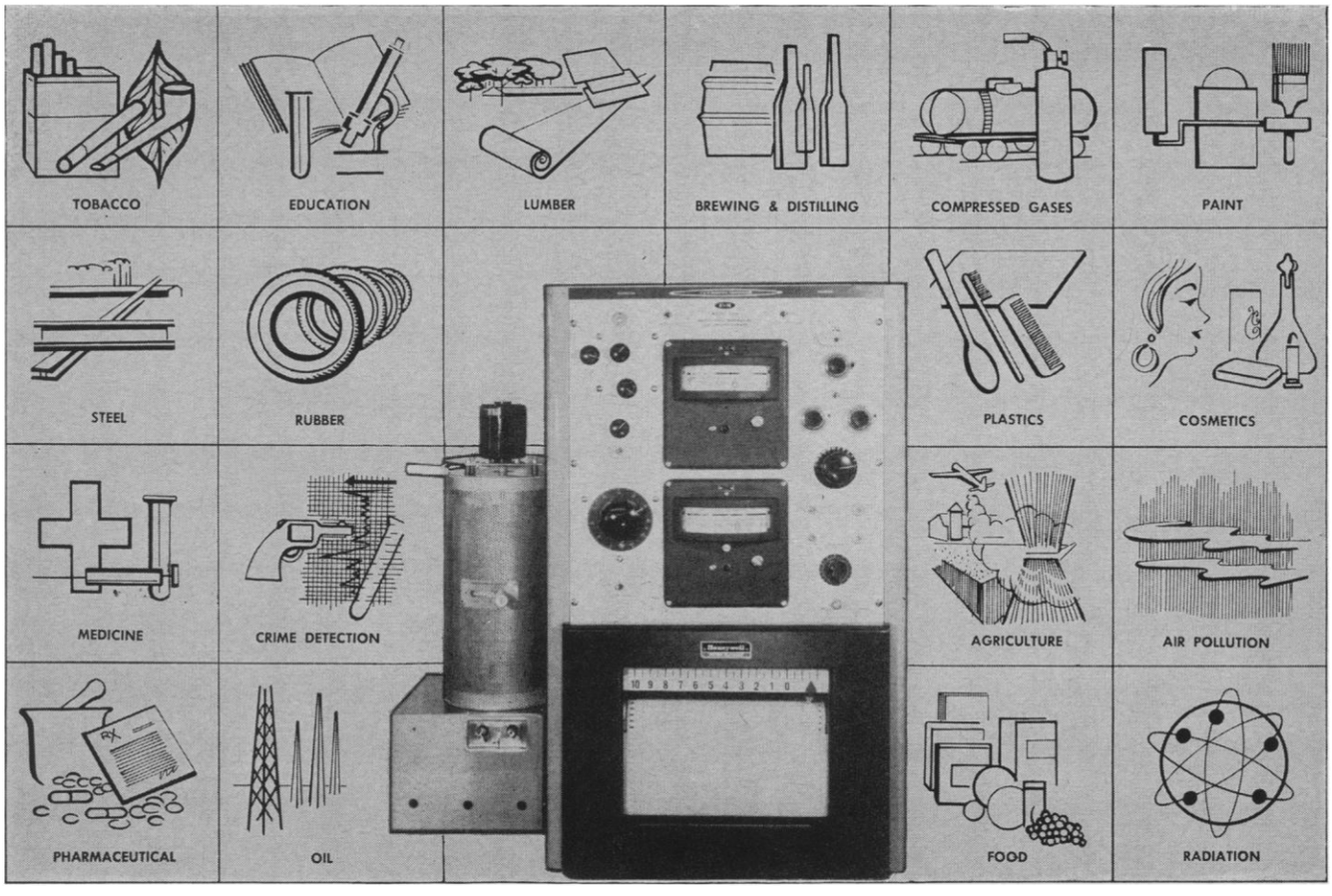

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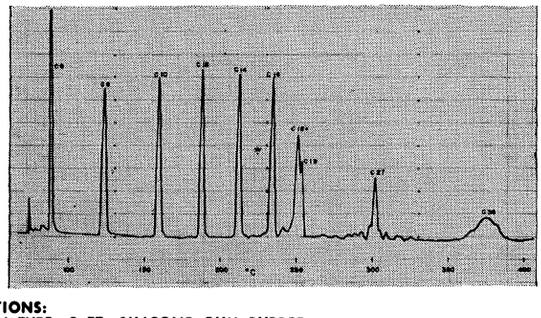
MODEL 500 LINEAR PROGRAMMED HIGH TEMPERATURE GAS CHROMATOGRAPH

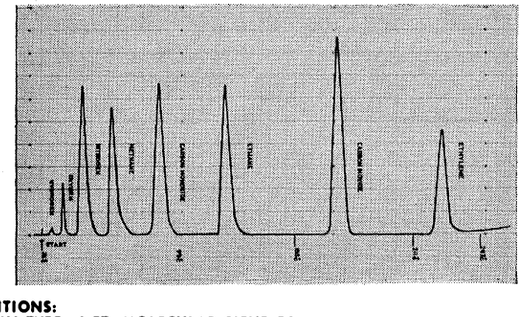
The technique of linear programmed temperature gas chromatography combined with high temperature operation makes possible these previously difficult analyses: (1) wide boiling mixtures; (2) gases (H_2, O_2, N_2, CO, CO_2 and light hydrocarbons); (3) trace quantities of high boiling residues in low boiling solvents; (4) correlation of retention times with analytical distillation data; and (5) temperature sensitive materials.

Two such analyses are shown below. Note that with programming, both separations required only one column. Without programming, the methyl ester separation would have required a fixed high column temperature with a loss in resolution of the lower boiling components. In the separation of gases at a constant column temperature, ethane, carbon dioxide and ethylene would not have eluted. To chromatograph this mixture at constant temperature, two columns and two samples are necessary.

With a Model 500, column scouting is easily performed because columns can be changed and baselines re-established in ten minutes. Heating from room temperature to 300°C takes about 10-15 minutes (27 minutes to 500°C) and cooling back to room temperature takes about the same time. Model 500 has separate and independent temperature controls, each to 500°C, for column, detector, and injection port. In addition, eighteen linear heating rates from 0.29 to 42°C/min. provide a wide selection for optimum column conditions.



CONDITIONS:
COLUMN TYPE: 2 FT. SILICONE GUM RUBBER
COLUMN TEMP. PROGRAMMED AT 5.6°C/MIN. FROM 75°C to 400°C.
This is a typical programmed temperature chromatogram demonstrating a separation of methyl esters with widely differing boiling points. Observe the sharp peaks, shaped nearly alike.



CONDITIONS:
COLUMN TYPE: 4 FT. MOLECULAR SIEVE 5A
COLUMN TEMP. PROGRAMMED AT 4.0°C/MIN. FROM 35°C TO 247°C.
By programming column temperature, H_2, N_2, O_2, CO, CO_2, ethane, and ethylene all separate on a single column in one run. Using the same column, the programmed temperature technique works equally well for the separation of nitrous and nitric oxide in the presence of these gases.

***Similar analytical advantages with Model 500** apply to the other industries illustrated above. Write for our bulletins on Model 500. These describe in detail all the specialized analyses.*

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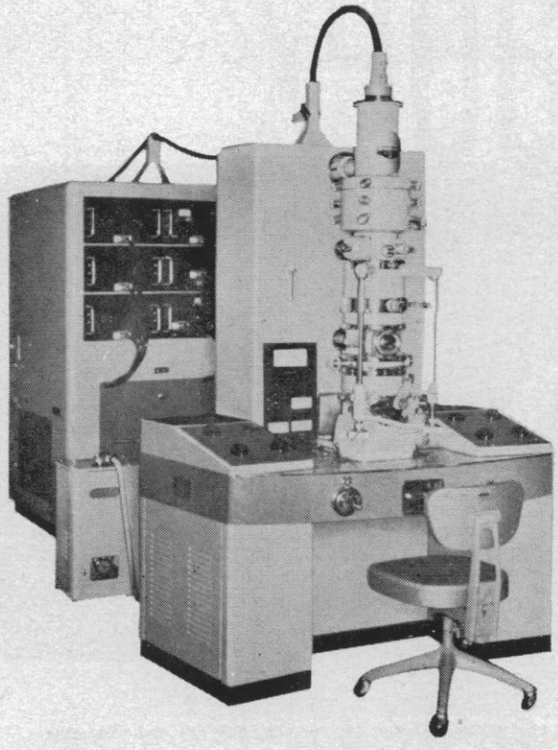

featuring the new HU-11

Hitachi, Ltd. of Japan, in advance of world-wide competition, announces the successful production of the HU-11, the latest in electron microscopes.

The new HU-11 is an enlarged, high efficiency electron microscope, guaranteed 8-10 Angstrom Unit resolution, capable of probing the very basic structure of matter. This is possible in the HU-11 because it is equipped with a chromatic aberration compensating lens system, a development uniquely Hitachi.

Other improvements include an enlarged specimen chamber facilitating handling of accessories, an exhaust system trap reducing contamination to a minimum (therefore eliminating the need to disassemble the column), increased exposure area of photography permitting recording of high resolution diffraction rays.

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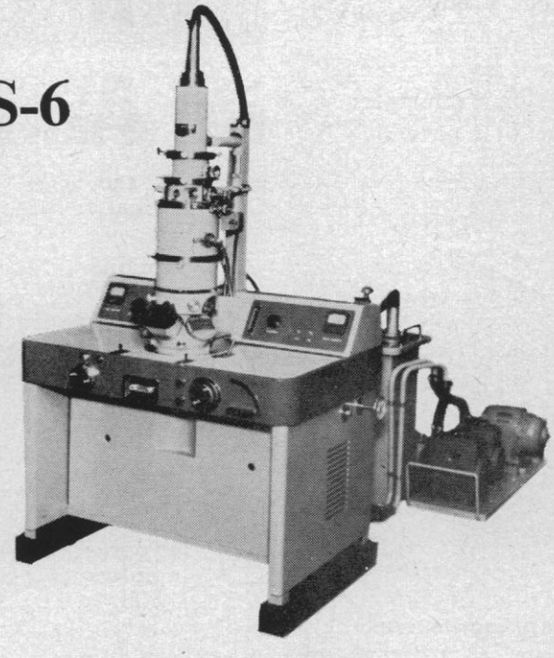

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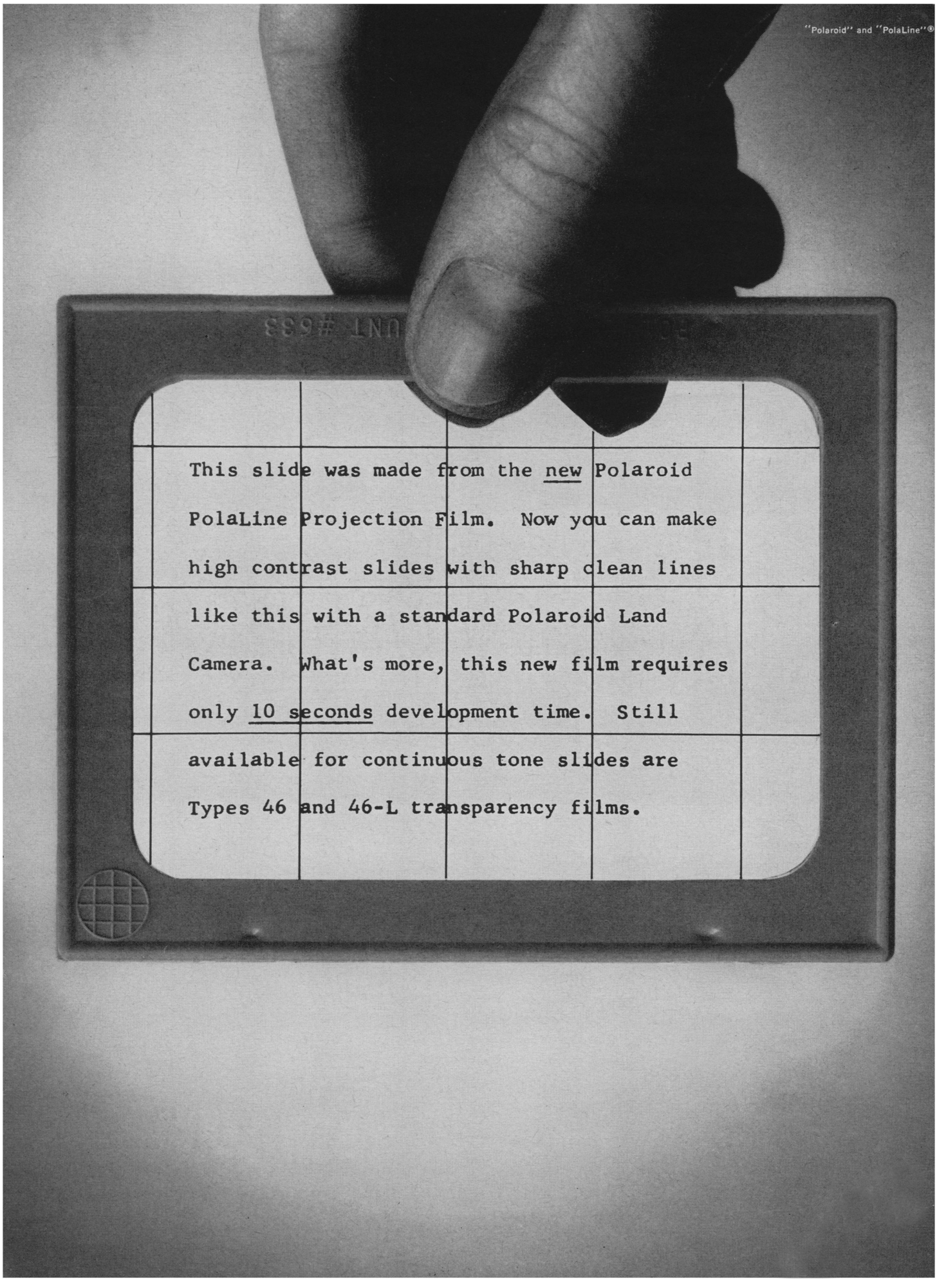
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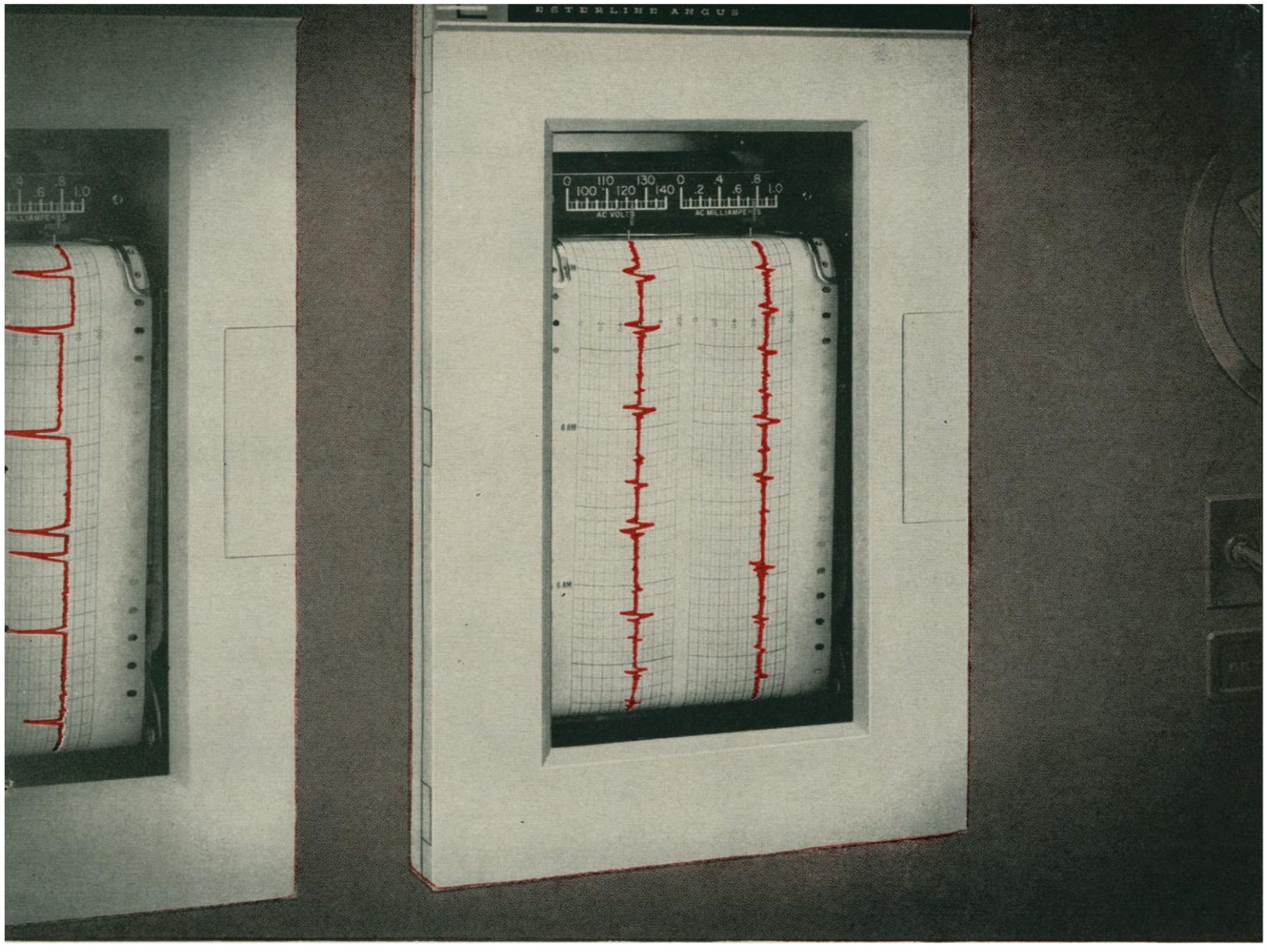

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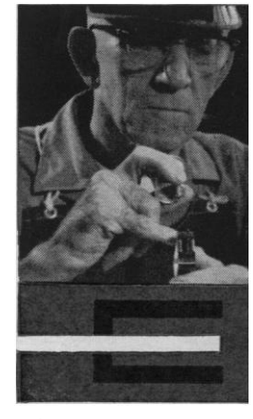

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19 May 1961, Volume 133, Number 3464

SCIENCE

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Diploma Diplomacy

In his recent *The Voice of the Dolphins*, a slim volume of commentary on contemporary affairs in the form of science fiction, Leo Szilard, of the Enrico Fermi Institute of Nuclear Studies, at one point predicts the outcome of one part of our program of educational assistance for less-developed countries. The prophecy is that our fellowship program for bringing African students to this country, educating them in American colleges, and then sending them home, a program that began last year with action by the Kennedy Foundation and the State Department, will grow through the years to produce an Africa that is developed but that is unyielding in the ill will it bears us. The basis for this prediction is the expectation that the visiting students, who will be the leaders of the new Africa, will be treated by white Americans with the same courtesy they give Negro Americans.

As a piece of political satire, this example of an unexpected and undesired consequence of educational assistance may not be entirely successful. The fact is that if we look to the present, instead of the future, the difficulty that is already upon us is rather the reverse. In the matter at least of advanced scientific training of many of our foreign students, the trouble is not what attitudes our visitors display toward us on their return but their reluctance, in view of the educational and research facilities they enjoy here, to return at all. But if Szilard's example is not altogether convincing, it makes a valid point. In educational assistance and in other parts of public affairs, we frequently get into trouble because we are not prepared, as we are in scientific investigation, to explore the consequences of our ideas.

A proposal has been offered, however, that meets both the contingency that the scholars we have helped will not like us and the contingency that they will like us only too well. Arthur F. Burns, professor of economics at Columbia University, has suggested that instead of bringing students to this country we send the universities to them, instead of importing students we export universities. The suggestion was offered in a brief speech given last year at the University of Chicago and now published under the title "Why not diploma diplomacy?" in the first number of that university's new magazine, *Context*. Burns suggests that we build universities, for those countries that ask for them, in which such professions will be taught as engineering, agriculture, medicine, and public administration. He also proposes that we provide as much assistance in the way of staffing these institutions as is wanted.

This suggestion was offered in the context of the more general problem of financial grants and easy loans to other countries, and Burns finds that it compares favorably with other forms of economic aid. The cost of the universities could be met by using some of our present funds for economic aid. From the viewpoint of foreign policy, the building of universities on foreign soil, Burns says, would be free of the distrust of our motives that characterizes some of our other efforts at aid, as when, for example, because of the deficit in our international accounts, we seek arrangements requiring that the credits we offer be spent on American goods. From the viewpoint of economics, and this from a professional economist, the benefits of building universities could repay the cost a thousand fold.—J.T.

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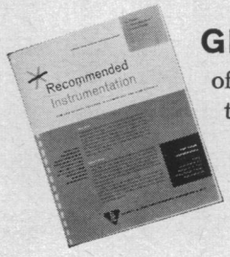

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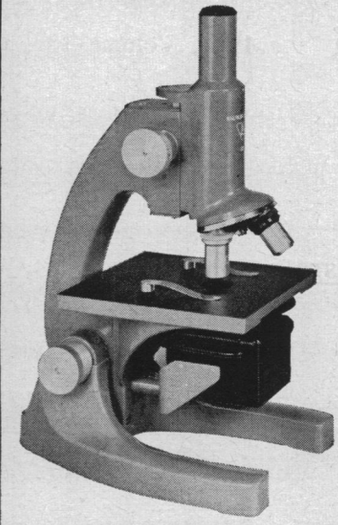

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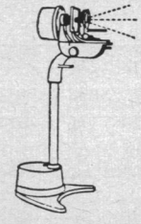

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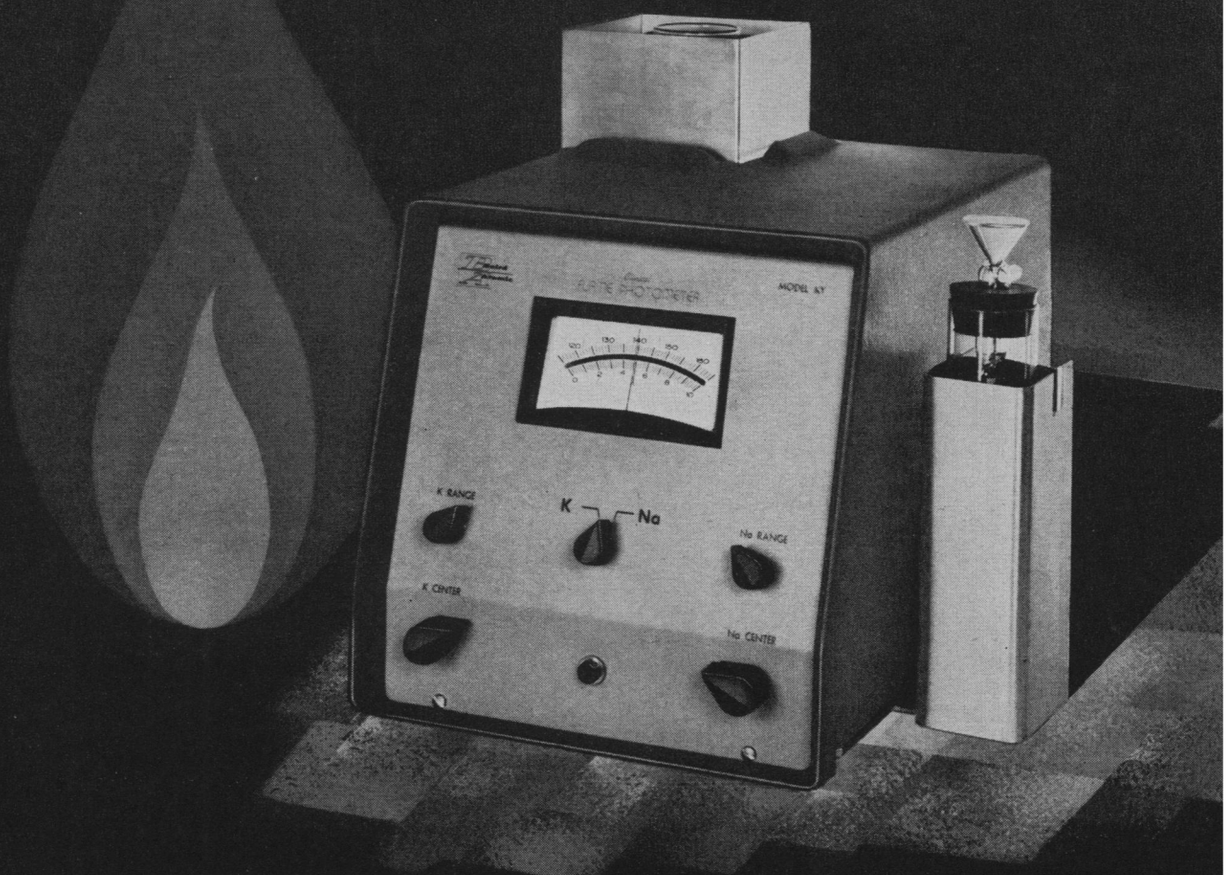

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ful results can actually be obtained. We should be sure that the significant physical processes are not likely to be masked by backgrounds due to uninteresting particles also produced in the interactions.

Summary

Summarizing, one can say that the construction of ultrahigh-energy accelerators will be justifiable even at presently predictable costs. There is little doubt that eventually physicists will push into the domain of superintensity as well as that of superenergy. If they do this, it will be because the information that will then become available will be needed in order to formulate a more complete picture of nature. For the time being, we should examine the results of the 30-Gev synchrotrons in the light of their bearing on these large constructions of the future. But if these projects are to be realized in a time comparable to our lifetime, then those study projects which have become a necessary prelude to actual construction should be started now (*3*).

Notes

1. During a discussion with G. I. Budker concerning plasma instabilities and their deleterious effect on plasma accelerators, I asked if this did not mean we should be very pessimistic about any success. "Yes," said Budker, "but don't forget that a plasma is like a woman, the outlook can change most rapidly!"
2. K. Symon estimated that the cost of producing colliding beams of protons attaining 30 Gev in the center of mass would be about $200 million. The collision yield would be about 10^5 $cm^{-3}sec^{-1}$, with 10 percent gas background at 10^{-8} mm-Hg. G. K. O'Neill says that the cost of a storage ring set might be similar to that of the alternating-gradient synchrotron used as its injector; for example, addition of storage rings to the Brookhaven National Laboratory alternating-gradient synchrotron would cost approximately $35 million, for 60 Gev in the center of mass ("equivalent energy," about 2150 Gev).
3. I gratefully acknowledge the assistance, in preparation of this report, of R. F. Mozley and W. D. Walker, who prepared conference notes, and of Robert Oppenheimer, Gerald Pickavance, and Keith Symon, who sent additional comments and summaries.

Forthcoming Events

June

5–16. Operations Research and Systems Engineering, Baltimore, Md. (Dean, School of Engineering, Johns Hopkins Univ., Baltimore 18)

6–8. Tissue Culture Assoc., 12th annual, Detroit, Mich. (F. E. Payne, Dept. of Epidemiology, Univ. of Michigan, Ann Arbor)

8–11. American Electroencephalographic Soc., Atlantic City, N.J. (G. A. Ulett, Malcolm Bliss Mental Health Center, 1420 Grattan, St. Louis 4, Mo.)

8–18. International Organization for Standardization, general assembly (members only), Finland. (American Standards Association, 70 E. 45 St., New York 17)

9–11. Society of Biological Psychiatry, Atlantic City, N.J. (G. N. Thompson, 2010 Wilshire Blvd., Los Angeles 57, Calif.)

9–17. European Convention of Chemical Engineering, Frankfurt, Germany. (DECHEMA, Postfach No. 7746, Frankfurt/Main 7)

11–15. American Soc. of Mechanical Engineers, summer annual, Los Angeles, Calif. (O. B. Schier II, 29 W. 39 St., New York 18)

12–13. Radio Frequency Interference, 3rd natl. symp., Washington, D.C. (E. F. Mischler, National Engineering Service, Washington, D.C.)

12–14. American Dairy Science Assoc., Madison, Wis. (H. F. Judkins, 32 Ridgeway Circle, White Plains, N.Y.)

12–14. American Neurological Assoc., Atlantic City, N.J. (M. D. Yahr, Neurological Inst., 710 W. 168 St., New York 32)

12–14. Society for the Study of Development and Growth, regeneration symp., Williamstown, Mass. (A. C. Braun, Rockefeller Inst., New York 21)

12–15. Nature of the Real, conf., Milwaukee, Wis. (E. D. Simmons, Dept. of Philosophy, Marquette Univ., Milwaukee 3)

12–15. Physics of Electronic and Atomic Collisions, intern. conf., Boulder, Colo. (B. Bederson, Physics Dept., New York Univ., New York 53)

12–16. Association of Official Seed Analysts, Richmond, Va. (D. D. Forsyth, Agronomy Building, Madison 6, Wis.)

12–16. Molecular Structure and Spectroscopy, symp., Columbus, Ohio. (R. A. Oetjen, Dept. of Physics and Astronomy, Ohio State Univ., Columbus 10)

12–18. European Assoc. for Animal Production, 8th intern. congr., Hamburg, Germany. (European Assoc. for Animal Production, Via Barnaba Oriana 28, Rome, Italy)

12–24. European Inst. of Scientific Studies for the Prevention and Treatment of Alcoholism, Amsterdam, Netherlands. (D. Ehlbeck, Intern. Bureau against Alcoholism, Case Gare 49, Lausanne, Switzerland)

12–29. Statistical Quality Control Intensive Courses for the Chemical and Processing Industries, 18th annual, Rochester, N.Y. (H. M. Kentner, Extended Services Div., Rochester Inst. of Technology, Rochester 8)

13–14. Product Engineering and Production, 5th natl. conf., Philadelphia, Pa. (P. J. Riley, R.C.A., Building 10-6, Camden 2, N.J.)

13–16. Gas Chromatography Symp., 3rd biennial, East Lansing, Mich. (J. E. Callen, Procter and Gamble Co., Miami Valley Laboratories, P.O. Box 175, Cincinnati 39, Ohio)

13–16. Institute of Aerospace Sciences and American Rocket Soc., Los Angeles, Calif. (Inst. of Aerospace Sciences, 2 E. 64 St., New York 21)

13–18. Nuclear Congress, 6th, Rome, Italy. (Ufficio Stampa e Relazioni Pubbliche-CNEN, Via Belisario 15, Rome)

14–16. Applied Mechanics Conf., Chicago, Ill. (American Soc. of Mechanical Engineers, Meetings Dept., 29 W. 39 St., New York 18)

14–16. Semiconducting Compounds, conf., Schenectady, N.Y. (W. W. Tyler, General Electric Research Laboratory, Schenectady)

14–16. Theory of Weak and Strong Interactions, conf., La Jolla, Calif. (T. A. Manar, Scripps Institution of Oceanography, La Jolla)

14–17. American Assoc. of Bioanalysts, Dallas, Tex. (L. D. Hertert, 490 Post St., Room 1049, San Francisco 2, Calif.)

16–17. Meteoritical Soc., Nantucket, Mass. (G. L. Rowland, Long Beach City College, Long Beach 8, Calif.)

17–21. American Nuclear Soc., Boston Mass. (O. J. Du Temple, ANS, 86 E. Randolph St., Chicago 1, Ill.)

18–21. American Astronomical Soc., Nantucket, Mass. (J. A. Hynek, Dearborn Observatory, Northwestern Univ., Evanston, Ill.)

18–23. American Meteorological Soc., 193rd natl., and Pacific Div., AAAS, 42nd annual, Davis, Calif. (AMS, 45 Beacon St., Boston 8, Mass.)

18–23. American Soc. of Medical Technologists, Seattle, Wash. (Miss R. Matthaei, Suite 25, Hermann Professional Bldg., Houston 25, Tex.)

19–21. American Soc. of Pharmacognosy, annual summer meeting, Houston, Tex. (R. S. Westby, Eli Lilly and Co., 740 S. Alabama St., Indianapolis 6, Ind.)

19–21. Space Flight and Re-entry Trajectories, symp. by Intern. Acad. of Astronautics, Paris, France. (Secretariat, IAA, 12 rue de Gramont, Paris 2)

19–23. Conference on Carbon, 5th bi-

ennial, University Park, Pa. (Fifth Carbon Conf., Pennsylvania State Univ., Conference Center, University Park)

19–23. Current Aspects of Internal Medicine, postgraduate course, American College of Physicians, Iowa City, Iowa. (E. C. Rosenow, Jr., Executive Director, ACP, 4200 Pine St., Philadelphia 4, Pa.)

19–24. Feed Microscopy, annual meeting and special short course, Denver, Colo. (C. Jones, Colorado Department of Agriculture, 3130 Zuni St., Denver 11)

19–30. Astrophysics Seminar, Cloudcroft, N.M. (J. R. Foote, P.O. Box 1053, Holloman Air Force Base, N.M.)

21–1. International Plastics Exhibition and Convention, London, England. (British Plastics, Dorset House, Stanford St., London, S.E.1)

22–23. American Rheumatism Assoc., New York, N.Y. (F. E. Demartini, 622 W. 168 St., New York 32)

22–23. Computers and Data Processing, 8th annual symp., Estes Park, Colo. (W. H. Eichelberger, Denver Research Inst., Univ. of Denver, Denver, Colo.)

22–24. Endocrine Soc., New York, N.Y. (H. H. Turner, 1200 N. Walker, Oklahoma City 3, Okla.)

22–26. American College of Chest Physicians, New York, N.Y. (M. Kornfeld, 112 E. Chestnut St., Chicago 11, Ill.)

23–25. American College of Angiology, 7th annual, New York, N.Y. (A. Halpern, Secretary, 11 Hampton Court, Great Neck, N.Y.)

25–28. American Soc. of Agricultural Engineers, annual, Ames, Iowa. (J. L. Butt, 420 Main St., St. Joseph, Mich.)

25–29. Morphological Precursors of Cancer, intern. symp. (by invitation only), Perugia, Italy. (L. Severi, Div. of Cancer Research, Univ. of Perugia, P.O. Box 167, Perugia)

25–30. American Medical Assoc., 110th annual, New York, N.Y. (AMA, 535 N. Dearborn St., Chicago 10, Ill.)

25–30. American Soc. for Testing Materials, Atlantic City, N.J. (R. J. Painter, 1916 Race St., Philadelphia, Pa.)

25–30. International Union of Leather Chemists Societies, 8th congr., Washington, D.C. (F. O'Flaherty, Dept. of Leather Research, Univ. of Cincinnati, Cincinnati 21, Ohio)

25–30. National Education Assoc. of the U.S., Atlantic City, N.J. (W. G. Carr, 1201 16 St., NW, Washington 6)

26–27. Conference on Vacuum Metallurgy, 5th annual conf., New York, N.Y. (R. F. Bunshah, Dept. of Metallurgical Engineering, New York Univ., New York 53)

26–28. American Soc. of Heating, Refrigerating and Air-Conditioning Engineers, 68th annual, Denver, Colo. (J. H. Cansdale, ASHRAE, 234 Fifth Ave., New York 1)

26–28. Control of Noise, symp., Teddington, England. (Director, Natl. Physical Laboratory, Teddington, Middlesex)

26–28. European Symp. on Space Technology, London, England. (Secretary, British Interplanetary Soc., 12 Bessborough Gardens, London, S.W.1)

26–28. Military Electronics, 5th natl. convention, Washington, D.C. (H. Davis, SAFRD, Pentagon, Washington 25)

26–30. American Soc. for Engineering Education, annual, Lexington, Ky. (M. Baker, Univ. of Kentucky, Lexington)

26–30. Concepts and Design in Aerospace Electricity, Philadelphia, Pa. (D. H. Scott, General Electric Co., No. 3, Penn Center Plaza, Philadelphia 2)

26–30. Reading Conf., 3rd annual, Syracuse, N.Y. (R. A. Kress, Syracuse Univ., Syracuse 10)

26–9. Large Dams, 7th intern. congr., Rome, Italy. (U.S. Committee on Large Dams, c/o Engineering Joint Council, 29 W. 39 St., New York 18)

27. Colloid Symp., by Faraday Soc., Glasgow, Scotland. (A. S. Hyde, Chemistry Dept., Royal College of Science and Technology, Glasgow, C.1)

27–29 Analytical Astrodynamics, intern. symp., Santa Barbara, Calif. (Capt. J. L. Gilbert, Air Force Office of Scientific Research, Washington 25)

27–29. Society for Investigative Dermatology, Inc., New York, N.Y. (H. Beerman, 255 S. 17 St., Philadelphia 3, Pa.)

27–30. American Home Economics Assoc., Cleveland, Ohio. (Miss M. Warren, School of Home Economics, Univ. of Oklahoma, Norman)

27–30. Hurricanes, 2nd technical conf., American Meteorological Soc., Miami Beach, Fla. (AMS, 45 Beacon St., Boston 8, Mass.)

28–30. International Gas Conf., 8th Stockholm, Sweden. (R. H. Touwaide, Union Internationale de l'Industrie du Gaz, 4, avenue Palmerston, Brussels 4, Belgium)

28–30. Joint Automatic Control Conf., Boulder, Colo. (R. Kramer, Massachusetts Inst. of Technology, Cambridge 39)

28–1. Institute of Navigation, annual, Williamsburg, Va. (C. T. French, General Precision, Inc., 777 14 St., NW, Suite 611, Washington, D.C.)

29–1. American Assoc. of Physics Teachers, Stanford, Calif. (R. P. Winch, Williams College, Williamstown, Mass.)

July

1–3. Astronomical League, Detroit, Mich. (W. A. Cherup, 4 Klopfer St., Millvale, Pittsburgh 9, Pa.)

2–7. American Physical Therapy Assoc., Chicago, Ill. (Miss L. Blair, Executive Director, APTA, 1790 Broadway, New York 19)

2–9. Rural Medicine, 1st intern. congr., Tours, France. (Prof. Vacher, Secretaire General, c/o Institut National de Medecine Agricole, Ecole de Medecine, Tours).

3–6. Clay Minerals, colloquium on genesis and synthesis of, intern., Paris, France. (Prof. Hocart, Faculté des Sciences, Université de Paris à la Sorbonne, 47 rue des Ecoles, Paris 5)

3–8. Treatment of High Level Radioactive Wastes, symp., Intern. Atomic Energy Agency, Vienna, Austria. (IAEA, 11 Kärtner Ring, Vienna 1)

3–16. Durability of Concrete, symp., Intern. Union of Testing and Research Laboratories for Materials and Structures, Prague, Czechoslovakia. (B. Hacar, Director, Inst. of Theoretical and Applied Mechanics, Czechoslovak Acad. of Sciences, Solínova 7, Prague 6-Dijvice)

4–8. Latin-American Assoc. of Physiological Sciences, 4th meeting, Ribeirão

Preto, Brazil. C. R. Diniz, Caixa Postal 301, Ribeirão Preto, Estado de São Paulo)

5–8. European Organization for Research on Fluorine and Dental Caries Prevention, 8th meeting, London, England. (J. R. Forrest, Senior Dental Officer, Ministry of Health, Savile Rd., London, W.1)

5–8. Optical Materials, colloquium, Intern. Commission for Optics, Paris, France. (Institut d'Optique, 3, Boulevard Pasteur, Paris 15)

5–9. International Convention on Radio Techniques and Space Research, Oxford, England. (British Institution of Radio Engineers, 9 Bedford Sq., London, WC.1.)

5–12. International Ophthalmic Optical Congr., London, England. (G. H. Giles, Intern. Optical League, 65 Brook St., London, W.1)

6–7. Free Radicals, intern. symp., 5th, Uppsala, Sweden. (Symposium Secretariat, c/o Inst. of Physical Chemistry, Uppsala)

6–12. Agricultural Medicine, 1st intern. congr., Tours, France. (J. Vacher, Institut National de Medecine Agricole, Ecole de Medecine, Tours)

6–12. Ribonucleic Acids and Polyphosphates: Structure, Synthesis and Function, intern. colloquium, Strasbourg, France. (Prof. Ebel, Faculté de Pharmacie, Université de Strasbourg, Strasbourg)

9–14. Bio-Medical Electronics, 4th intern. conf., New York, N.Y. (H. Schwan, Moore School of Electrical Engineering, Univ. of Pennsylvania, Philadelphia 4)

9–15. American Library Assoc., annual conf., Cleveland, Ohio. (D. H. Clift, 50 E. Huron St., Chicago, Ill.)

9–15. International Dental Federation, 49th annual session, Helsinki, Finland. (Office of Secretary General, IDF, 35 Devonshire Place, London, W.1, England)

10. Bibliographical Soc. of America, Cleveland, Ohio. (E. Wolf II, Library Co. of Philadelphia, Broad and Christian Sts., Philadelphia 47, Pa.)

10–14. Institute in Technical and Industrial Communications, 4th annual, Fort Collins, Colo. (Director, Inst. in Technical and Industrial Communications, Colorado State Univ., Fort Collins)

10–14. International Congr. of Dietetics, 3rd, London, England. (Miss D. F. Hollingsworth, British Dietetic Assoc. 251 Brampton Rd., London, S.W.3)

10–14. International Diabetes Federation, 4th congr., Geneva, Switzerland. (B. Rilliet, Secretary General, 4 Boulevard des Tranchees, Geneva)

10–14. Optical Instruments and Techniques, conf., London, England. (K. J. Habell, Natl. Physical Laboratory, Teddington, Middlesex, England)

10–20. Plant Exploration and Introduction, technical meeting on, Food and Agriculture Organization of the U.N., Rome, Italy. (Intern. Agency Liaison Branch, Office of the Director General, Viale della Terme di Caracalla, Rome)

10–24. Medical Electronics, 4th intern. conf., New York, N.Y. (L. E. Flory, David Sarnoff Research Center, Princeton, N.J.)

11–25. World Meteorological Organization, 3rd South American session, Rio de Janeiro, Brazil. (WMO, 1 Avenue de la Paix, Geneva, Switzerland)

12–18. Radioactivity in Food and Agriculture, Expert Committee on the Organi-

zation of Surveys for FAO, Rome, Italy. (Intern. Agency Liaison Branch, Office of the Director General, Viale della Terme di Caracalla, Rome)

13–14. Data Acquisition and Processing in Biology and Medicine, conf., Rochester, N.Y. (Office of Public Information, Univ. of Rochester, River Campus Station, Rochester 20)

15–18. Life Insurance Medicine, 7th intern. congr., Lisbon, Portugal. (L. de Carvalho Cancella, Secretary, Parede, Portugal)

16–18. British Congr. of Obstetrics and Gynaecology, 16th, Bristol, England. (Secretary, British Congr. of Obstetrics and Gynaecology, University Dept. of Obstetrics, Southmead Hospital, Bristol)

16–22. International Soc. for Clinical and Experimental Hypnosis, Rio de Janeiro, Brazil. (ISCEH, 33 E. 65 St., New York 21)

17–22. Soil Mechanics and Foundation Engineering, 5th intern. conf., Paris, France. (E. Caminade, Secretaire General, 23 rue de Cronstadt, Paris 15)

18–20. Pulmonary Structure and Function, Ciba Foundation Symp. (by invitation only), London, England. (Ciba Foundation, 41 Portland Pl., London, W.1)

18–21. Inorganic Polymers, intern. symp., Nottingham, England. (General Secretary, Chemical Soc., Burlington House, London, W.1, England)

21–22. World Power Conf. (members only), Moscow, U.S.S.R. (Central Office, 201-2 Grand Buildings, Trafalgar Sq., London, W.C.2, England)

23–28. Otolaryngology, 7th intern. congr., Paris, France. (H. Guillon, Secretary General, 6 Avenue Mac-Mahon, Paris 17)

24–28. Nematology Symp., 6th intern., Ghent, Belgium. (J. van den Brande, Soc. of European Nematologists, Rijksland-boushogeschool, Coupure links 235, Ghent)

24–29. Medical Electro-Radiological Societies, Latin Federation of, 5th congr., Paris, France. (C. Proux, Secretary, 9 rue Daru, Paris 8)

24–30. Urology, 12th intern. congr., Rio de Janeiro, Brazil. (J. Silva de Assis, Secretary, P.O. Box 1275, Belo-Horizonte, Brazil)

26. International Commission for the Prevention of Alcoholism, 7th annual meeting, Washington, D.C. (International Headquarters, 6840 Eastern Ave., NW, Washington 12)

26–28. Detection and Assay of Hormones by Immuno-Clinical Means, Ciba Foundation Colloquium (by invitation only), London, England. (Ciba Foundation, 41 Portland Pl., London, W.1)

27–1. Macromolecular Chemistry, intern. symp., Montreal, Canada, (Organizing Committee, P.O. Box 816, Sarnia, Ontario, Canada)

28–29. Linguistic Soc. of America, Austin, Tex. (A. A. Hill, Box 7790, University Station, Austin 12)

30–2. Soil Conservation Soc. of America, Lafayette, Ind. (H. W. Pritchard, 838 Fifth Ave., Des Moines 14, Iowa)

30–3. International Psycholanalytical Congr., 22nd, Edinburgh, Scotland. (Miss C. de Monehaux, 53 York Terrace, Regents Park, London, N.W.1, England)

31–4. Biophysics, 1st intern. congr., Stockholm, Sweden. (B. Lindström, Dept. of Medical Physics, Karolinska Institutet, Stockholm 60)

31–4. Differential Equations in Non-Linear Mechanics, Air Force Acad., Colorado Springs, Colo. (J. P. Lasalle, 7212 Bellona Ave., Baltimore 12, Md.)

31–11. Physics of the Solar System and Re-entry Dynamics, conf., Blacksburg, Va. (Bureau of Public Relations, Virginia Polytechnic Inst., Blacksburg)

31–12. Electric Power and Problems of Nuclear Power, seminar, U.N. Economic Commission for Latin America, Mexico, D.F. (A. Dorfman, Chief, Energy and Water Resource Program, Avenue Providencia 871, Santiago, Chile)

August

1–26. Functional Analysis, 8th American Mathematical Soc. summer institute, Stanford, Calif. (P. D. Lax, AMS, 190 Hope St., Providence 6, R.I.)

2–5. International Conf. of Pure and Applied Chemistry, 21st, Montreal, Canada. (R. Morf, Hoffmann-LaRoche, S.A., Grenzachterstrasse 124, Basel 2, Switzerland)

3–5. Canadian Chemical Conf. and Exhibition, 44th, Montreal. (Chemical Inst. of Canada, 48 Rideau St., Ottawa 2, Ont.)

5–9. International Rorschach Soc., 5th congr., Fribourg-en-Brisgau, Germany. (A. Friedemann, Chemin des Pêcheurs 6, Bienne, Switzerland)

6–10. Occupational Medicine and Toxicology, 3rd Inter-American conf., Miami, Fla. (W. B. Deichmann, School of Medicine, Univ. of Miami, Coral Gables, Fla.)

6–12. Atmospheric Ozone and General Circulation, symp., Arosa, Switzerland. (H. U. Duetsch, 20 Carl Spittelerstrasse, Zürich 53, Switzerland)

6–12. Chemical and Thermodynamic Properties at High Temperatures, symp., Montreal, Canada. (N. F. H. Bright, Natl. Research Council, Ottawa, Canada)

6–12. International Congr. of Pure and Applied Chemistry, 18th, Montreal, Canada. (L. Marion, Natl. Research Council, Ottawa 2, Canada)

7–9. Guidance and Navigation Conf., American Rocket Soc., Palo Alto, Calif. (J. J. Harford, ARS, 500 Fifth Ave., New York, N.Y.)

7–9. International Committee of Electro-Chemical Thermodynamics and Kinetics, 13th meeting, Montreal, Canada. (N. Ibl, Eidg. Technische Hochschule, Laboratorium für Physikalische und Elektrochemie, Universitätsstrasse 6, Zürich 6, Switzerland)

7–9. Space Age Astronomy, intern. symp., Pasadena, Calif. (D. W. Douglas, Jr., Douglas Aircraft Co., Inc., Santa Monica, Calif.)

7–10. National Medical Assoc., New York, N.Y. (J. T. Givens, 1108 Church St., Norfolk, Va.)

7–11. High Temperature Chemistry and Thermodynamics, symp., Montreal, Canada. (L. Brewer, Dept. of Chemistry, Univ. of California, Berkeley)

7–11. Seminar on Fast and Intermediate Reactors, International Atomic Energy Agency, Vienna, Austria. (IAEA, 11 Kärtner Ring, Vienna 1)

8–11. Poultry Science Assoc., State College, Pa. (C. B. Ryan, Texas A & M College, College Station)

8–16. Society of Protozoologists, Prague, Czechoslovakia. (N. D. Levine, College of Veterinary Medicine, Univ. of Illinois, Urbana)

10–16. International Congr. of Biochemistry, 5th, Moscow, U.S.S.R. (N. M. Sissakian, Leninsky prospekt, 33, Moscow, B-71)

10–16. International Union of Biochemistry, 4th general assembly, Moscow, U.S.S.R. (R. H. S. Thompson, IUB, Dept. of Chemical Pathology, Guy's Hospital Medical School, London, S.E.1, England)

12–19. Fast Reactions, summer school, Cambridge, England. (Secretary of the Summer School, Dept. of Physical Chemistry, Lensfield Road, Cambridge)

13–18. Microchemical Techniques, intern. symp., University Park, Pa. (H. J. Francis, Jr., Pennsalt Chemical Corp., P.O. Box 4388, Chestnut Hill Post Office, Philadelphia 18, Pa.)

13–18. Theoretical Aspects of Magnetohydrodynamics, seminar, University Park, Pa. (Conference Center, Pennsylvania State Univ. University Park)

13–19. International Assoc. of Applied Psychology, 14th congr., Copenhagen, Denmark. (Congress Secretariat, 19 Sankt Pederstraede, Copenhagen K.)

13–19. Training for Research in the Processes of Vision, 1st intern. conf., Rochester, N.Y. (Office of Public Information, River Campus Station, Rochester 20)

14–17. Calorimetry Conf., intern., Ottawa, Canada. (J. E. Kunzler, Bell Telephone Laboratories, Murray Hill, N.J.)

14–19. International Medical Conf. on Mental Retardation, 2nd, Vienna, Austria. (Miss E. Langer, Div. of Maternal and Child Health, State House, Augusta, Maine)

14–19. Symposium on Radiation, Vienna, Austria. (World Meteorological Organization, 1 Avenue de la Paix, Geneva, Switzerland)

14–25. Israel Medical Assoc., 5th world assembly, Jerusalem, Israel. (Beth-Harofeh, 1 Heffman St., Tel-Aviv, Israel)

14–26. Plant Pathology, conf., Lafayette, Ind. (J. F. Schafer, Dept. of Botany and Plant Pathology, Purdue Univ., Lafayette)

14–26. World Eucalyptus Conf., 2nd, São Paulo, Brazil. (Intern. Agency Liaison Branch, Office of the Director General, Food and Agriculture Organization, Viale delle Terme di Caracalla, Rome, Italy)

15–17. International Assoc. of Milk and Food Sanitarians, Jekyll Island, Ga. (H. L. Thomasson, P.O. Box 437, Shelbyville, Ind.)

15–18. Technical Assoc. of the Pulp and Paper Industry, 12th testing conf., Montreal, Canada. (TAPPI, 155 E. 44 St., New York 16)

15–24. International Astronomical Union, 11th general assembly, Berkeley, Calif. (D. H. Sadler, Royal Greenwich Observatory, Herstmonceux Castle, Hailsham, Sussex, England)

16–18. Hypersonics Conf., intern., Cambridge, Mass. (J. J. Harford, American Rocket Soc., 500 Fifth Ave., New York, N.Y.)

18–21. Association of American Geographers, East Lansing, Mich. (M. F. Burrill, 1785 Massachusetts Ave., NW, Washington 6)

19–30. Agricultural Economists, 11th intern. conf., Cuernavaca, Mexico. (J. Ackerman, Farm Foundation, 600 S. Michigan Ave., Chicago, Ill.)

20–23. International Ergonomics Assoc., 1st congr., Stockholm, Sweden. (T. Olson, Dept. of Industrial Physiology, G.C.I. Lidingövägen 1, Stockholm)

20–24. American Veterinary Medical Assoc., Detroit, Mich. (H. E. Kingman, AVMA, 600 S. Michigan Ave., Chicago 5, Ill.)

21–23. International Hypersonics Conf., Cambridge, Mass. (F. Ridell, Avco Research Laboratory, 301 Lowell St., Wilmington, Mass.)

21–24. Biological Photographic Assoc., Chicago, Ill. (Mrs. J. W. Crouch, Box 1668, Grand Central P.O., New York 17)

21–24. International Conf. on Photoconductivity, Ithaca, N.Y. (E. Burstein, Dept. of Physics, Univ. of Pennsylvania, Philadelphia)

21–26. International Congr. of Psychotherapy, 5th, Vienna, Austria. (W. Spiel, Lazarettg. 14, Vienna 9)

21–26. World Traffic Engineering Conf., Washington, D.C. (Intern. Road Federation, 1023 Washington Bldg., Washington 5)

21–27. International Assoc. of Dental Students, congr., London, England. (D. H. Clark, Royal Dental Hospital, Leicester Sq., London, W.C.2)

21–27. International Congr. of Zoology, 16th, Washington, D.C. (The 16th Congr., Natl. Acad. of Sciences, 2101 Constitution Ave., NW, Washington 25)

21–31. United Nations Conf. on New Sources of Energy, Rome, Italy. (United Nations, New York, N.Y.)

21–2. International Congr. of Practical Medicine, Merano, Italy. (Bundesärztztekammer, 1 Hädenkampfstrasse, Cologne, Germany)

21–6. Pacific Science Congr., 10th, Honolulu, Hawaii. (Secretary General, 10th Pacific Science Congr., Bishop Museum, Honolulu)

22–25. International Pharmacological Meeting, 1st, Stockholm, Sweden. (A. Wretlind, Karolinska Institutet, Stockholm 60)

22–30. International Conf. on Protozoology, Prague, Czechoslovakia. (N. D. Levine, College of Veterinary Medicine, Univ. of Illinois, Urbana)

23–25. Gas Dynamics, symp., biennial, Evanston, Ill. (J. J. Harford, American Rocket Soc., 500 Fifth Ave., New York, N.Y.)

23–26. Electron Microscope Soc. of America, Pittsburgh, Pa. (Miss M. L. Rollins, Agricultural Research Service, U.S. Department of Agriculture, P.O. Box 19,687, New Orleans 19, La.)

23–26. Institute of Management Sciences, 8th annual intern., Brussels, Belgium. (W. Smith, Inst. of Science and Technology, Univ. of Michigan, Ann Arbor)

23–1. Radioisotopes in the Biological Sciences, conf., Intern. Atomic Energy Agency, Vienna, Austria. (IAEA, 11 Kärtner Ring, Vienna 1)

24–26. Physiology of the Hippocampus, intern. colloquium, Montpellier, France. (Mme. Mineur, Centre National de la Recherche Scientifique, 13 Quai Anatole France, Paris 7)

26–1. Radiology, 10th intern. congr., Montreal, Canada. (C. B. Peirce, Suite 204, 1555 Summerhill, Montreal 25)

26–2. History of Science, 5th intern. congr., Ithaca, N.Y., and Philadelphia, Pa. (Secretary, 5th Intern. Congress of the History of Science, Cornell Univ., Ithaca)

27–29. International Congr. of Group Psychotherapy, 3rd, Paris, France. (W. Warner, P.O. Box 819, Grand Central Station, New York 17)

27–29. Psychosomatic Aspects of Neoplastic Disease, 2nd annual conv., Paris, France. (L. L. LeShan, Intern. Psychosomatic Cancer Study Group, 144 E. 90 St., New York 28)

27–31. American Soc. of Plant Psysiologists, Lafayette, Ind. (C. O. Miller, Indiana Univ., Bloomington)

27–1. American Congr. of Physical Medicine and Rehabilitation, Cleveland, Ohio. (D. C. Augustin, 30 N. Michigan Ave., Chicago 2, Ill.)

27–1. American Inst. of Biological Sciences, annual, Lafayette, Ind. (J. R. Olive, AIBS, 2000 P St., NW, Washington 6)

27–1. Coordination Chemistry, 6th intern. conf., Detroit, Mich. (S. Kirschner, Dept. of Chemistry, Wayne State Univ., Detroit 2)

28–30. Mathematical Assoc. of America, Stillwater, Okla. (H. L. Alder, MAA, Univ. of California, Davis)

28–30. Oak Ridge Inst. of Nuclear Studies, 8th annual summer symp., Gatlinburg, Tenn. (Symposium Office, University Relations Division, Oak Ridge Inst. of Nuclear Studies, P.O. Box 117, Oak Ridge, Tenn.)

28–30. Scandinavian Symp. on Fat Rancidity, 3rd, Sandefjord, Norway. (E. Törnudd, Gaustadallen 30, Blindern, Norway)

28–31, American Soc. for Pharmacology and Experimental Therapeutics, Rochester, N.Y. (K. H. Beyer, Merck, Sharp and Dohme Research Laboratories, West Point, Pa.)

28–31. Botanical Soc. of America, Lafayette, Ind. (B. L. Turner, Dept. of Botany, Univ. of Texas, Austin 12)

28–31. Chemical Physics of Nonmetallic Crystals, intern. conf., Evanston, Ill. (O. C. Simpson, Argonne National Laboratory, 9700 South Cass Ave., Argonne, Ill.)

28–1. Heat Transfer Conf., intern., Boulder, Colo. (S. P. Kezios, American Soc. of Mechanical Engineers, 29 W. 39 St., New York 18)

28–1. Ionization Phenomena in Gases, 5th intern. conf., Munich, Germany. (Secretariat, Oskar von Miller Ring 18, P.O. 463, Munich 1)

28–1. Radioactive Metrology, symp., Oxford, England. (B. W. Robinson, Applied Physics Division, National Physical Laboratory, Teddington, Middlesex, England)

28–1. Rockets and Astronautics, 3rd intern. symp., Tokyo, Japan. (Japanese Rocket Soc., 1-3, Ginza-Nishi, Chuo-Ku, Tokyo)

New Products

■ AUTOMATIC PARTICLE COUNTER AND SIZER (Fig. 1), manufactured by Casella Electronics Ltd., sizes and counts a wide range of materials either on a standard 3- by 1-in. slide or in a liquid counting chamber. An image of the particles is projected onto an adjustable slit by a conventional microscope system. The sample is mechanically scanned past the slit, and the signals produced by the image of the particle passing over the slit are recorded electronically. Particle size range is 1 to 200 μ. A particle crossing the slit produces a voltage pulse of amplitude proportional to the extent to which the slit is obscured. The reciprocating stage on which the specimen is mounted allows the scanning slit to be located on the optical axis of the microscope. The filament-lamp illumination gives high-intensity and uniform illumination by the Kohler system. Intensity is sufficient to provide a good signal-to-noise ratio for slit sizes less than 1 μ wide.

The number of particles per unit area is obtained by scanning the specimen twice with two slits of different widths but constant sensitivity; "sensitivity" means the length of slit that must be obscured for the particle to be counted. The size distribution of particles is obtained by differentiating the curve of the proportion of particles having sizes greater than specified sizes.

Compensation for variations in opacity of the materials being counted is provided by a gain control. Instead of using the particles themselves to adjust for this compensation, a drum carrying a series of neutral density filters is provided for use once the optical density of the specimen has been determined.

Scanning is automatically performed in two series of 100 passes that bring the specimen back to its starting point but on a slightly offset track. Each traverse is 10 mm long, and the distance between tracks is 50 μ. The total length swept out in this way is 200 cm.

Models are available with up to five amplitude discrimination channels. The use of the multichannel instrument permits a corresponding reduction in the time necessary for accumulation of data required to define a size distribution. (Cooke, Troughton and Simms, Inc., 91 Waite St., Malden 48, Mass.)

Circle 1 on Readers' Service card

■ FLAME ABSORPTION SPECTROMETER is a double-beam instrument with an automatic readout system for the range 2000 to 7700 A. The instrument includes a hollow-cathode lamp source, a source power supply, double-beam source optics, a monochromator equipped with grating, a detector unit with two multiplier-phototube detectors, and associated electronic equipment. Sampling is done by a standard burner with preset regulator, equipped with accessories for burning acetylene, propane, natural gas, or manufactured gas, and with a standard liquid sample nebulizer. To perform an analysis, a liquid sample is atomized in the flame burner. Light from the hollow-cathode source, which must emit the spectrum of the metal being analyzed, crosses the flame and is focused on the entrance slit of the monochromator previously set to read the intensity of the chosen spectral line. The degree of absorption of the source light by the flame is a function of the concentration of the metal in the sample. Typical sensitivities (in μg/ml) quoted by the manufacturer are: calcium, 0.1; magnesium, 0.03; copper, 0.02; zinc, 0.1; cadmium, 0.1; and iron, 0.1. (Perkin Elmer Corp., Norwalk, Conn.)

Circle 2 on Readers' Service card

■ DESALTER for removal of inorganic salts from laboratory solutions prior to chromatographic separation uses ion-exchange membranes. The device includes transformer, half-wave rectifier, potentiometer, and three compartments separated by ion-exchange membranes. The central compartment has a total capability of 6 ml; it is divided into cells of 1, 2, and 3 ml. Outer compartments contain platium-clad electrodes. (Torsion Balance Co., Clifton, N.J.)

Circle 3 on Readers' Service card

■ SEQUENCE REMOTE CONTROL SYSTEM controls the operating time of cameras placed along missile ranges to collect data on test drone flights. The fully

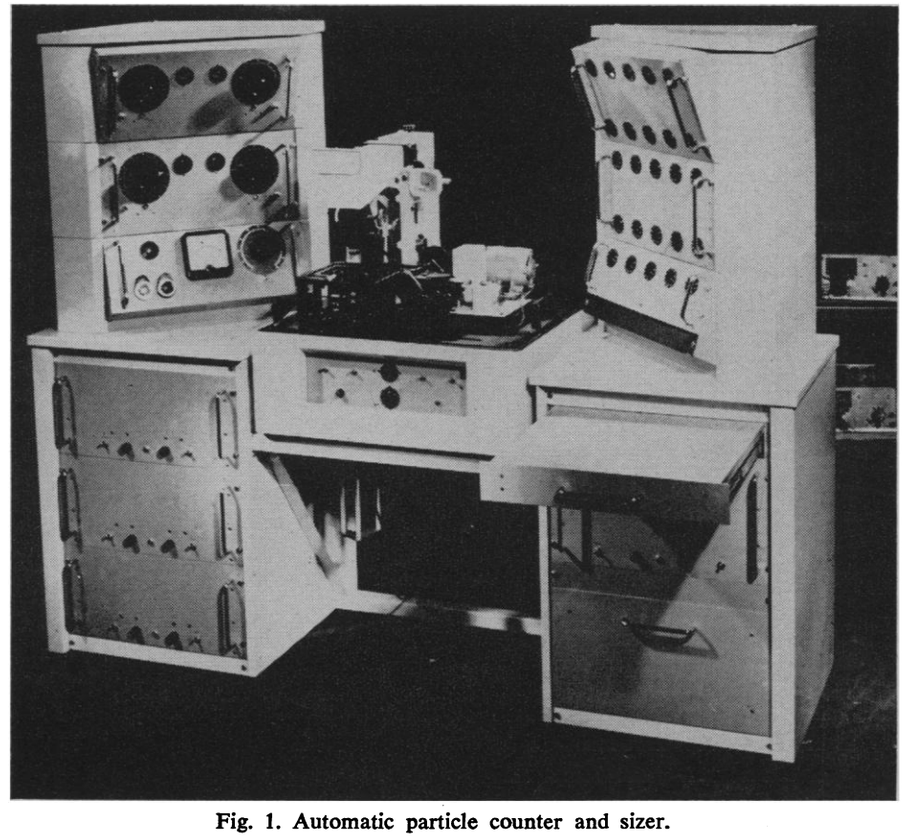

Fig. 1. Automatic particle counter and sizer.

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 1537 and 1627. Circle the number of the items in which you are interested on this card.*

solid-state system is comprised of a console that programs information to launch the drone and to operate any number of cameras in time sequence. This is achieved by presetting a decade counter to the countdown time while additional counters are set to the start and stop times for each camera. All timing is visually indicated, and provision is made for overriding the program (Telemetrics, Inc., 12927 S. Budlong Ave., Gardena, Calif.)

Circle 4 on Readers' Service card

■ DATA CONVERTER (Fig. 2) extracts data from punched tape and writes it on magnetic tape. Conversion of the data is accomplished so that input and output data are completely identical; the output record is a bit-for-bit image of the input tape. A universal code conversion feature is available at extra cost. The system will accept paper, foil, or plastic tapes in widths of five to eight channels. It can write on magnetic tape in formats compatible with the IBM 727 and 729 model 1 and Remington Rand computer inputs. The system includes a paper tape reader capable of reading 120 characters per second, a magnetic tape handler, and necessary electronic circuitry for control and tape format. (Tally Register Corporation, 1310 Mercer St., Seattle, Wash.)

Circle 5 on Readers' Service card

■ ELECTRONIC MULTIPLIER offers single-quadrant multiplication and squaring with accuracy said to be ± 0.01 percent and four-quadrant multiplication accuracy of ± 0.05 percent of full scale. Units are available with two, four, or six channels. Inputs are four independent voltages (x_1, y_1, x_2 and y_2) in the range of ± 100 volts. Outputs are two independent products ($-0.01\ x_1y_1$ and $-0.01\ x_2y_2$) in the range of ± 100 volts at 10 ma maximum load current. Specifications quoted by the manufacturer include: drift, less than 100 mv over an 8-hr period; noise, less than 100 mv, peak; phase shift, less than 1 deg at 100 cy/sec; zero error, with one variable at zero and the other ranging over ± 100 volts, maximum 40 mv. No external power supplies are required for operation with analog computing equipment. (Donner Scientific Co., 888 Galindo St., Concord, Calif.)

Circle 6 on Readers' Service card

■ OPTICAL LEVEL (Fig. 3) manufactured by Hilger and Watts maintains a level line of sight even when its telescope is tilted. The self-leveling device consists of a fixed prism rotating above two swinging prisms. When the telescope is tilted, the suspended prisms change their relation to the axis of the telescope to maintain a level line of

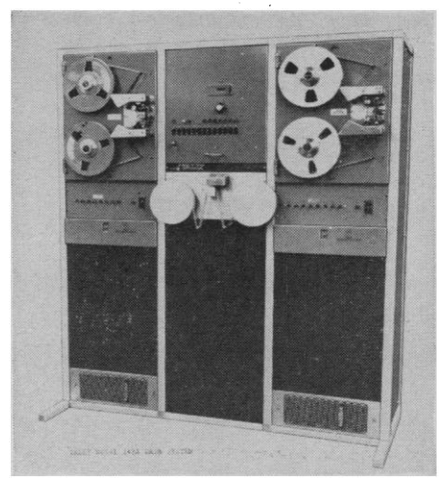

Fig. 2. Data converter.

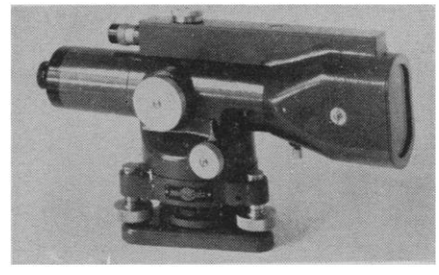

Fig. 3. Optical level.

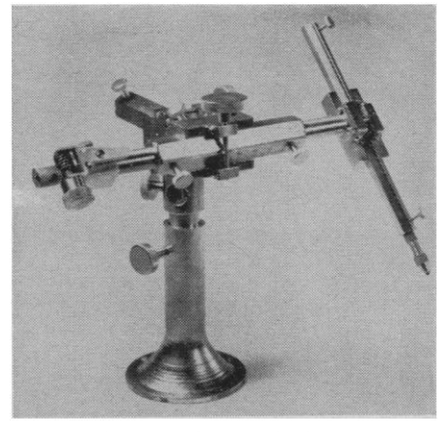

Fig. 4. Micromanipulator.

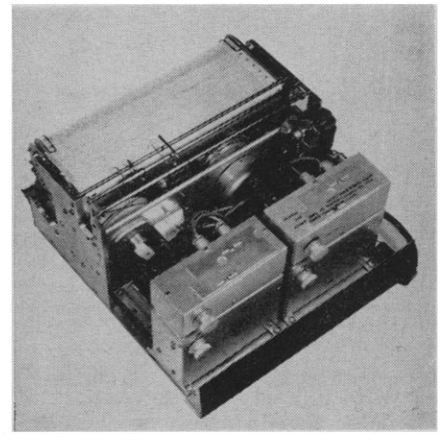

Fig. 5. Integrating strip-chart recorder.

sight. Repetitive setting action is said to be closer than 1 sec of arc. Oscillation of the prisms is prevented by an air-damped piston that also limits swing to ±20 min of arc. The instrument reads directly to 0.001 in. over ½ in. Focusing range is 6 ft to infinity. (Engis Equipment Co., 431 Dearborn St., Chicago 5, Ill.)

Circle 7 on Readers' Service card

■ MICROMANIPULATOR (Fig. 4) is made up of three independent traversing units that may be assembled in any desired combination. Advance of each unit is controlled by a knurled knob directly coupled to a spring-loaded friction wheel that tracks in a groove in the manipulator body. Fine and coarse positioning are independent for each traverse. Motion range is 0 to 125 mm, and sensitivity is said to be 10 μ. Materials of construction are stainless steel and chrome-plated bronze; the entire unit may be autoclaved. (Process & Instruments, 15 Stone Ave., Brooklyn 33, N.Y.)

Circle 8 on Readers' Service card

■ RECORDER-PEN PROGRAMMER, for recorders using solenoid operated pen-lift machanisms, operates the pen lift in five distinct code patterns for trace identification. The programmer is specifically designed for use with antenna pattern recorders and *x-y* recorders when multiple recordings on the same chart are desired. (Scientific-Atlanta, Inc., 2162 Piedmont Rd., NE, Atlanta 9, Ga.)

Circle 9 on Readers' Service card

■ INTEGRATING STRIP-CHART RECORDER (Fig. 5) provides five different maximum count rates, obtained by an adjustable gear system, up to a maximum of 40,000 area counts per minute. Counting rate linearity is said to be ± 0.5 percent of full scale. The integrator channel, installed as a second channel in a standard single-channel recorder, consists of a potentiometer-amplifier-servo arrangement and actuates an area trace simultaneously with the signal trace. Between each peak or at the start of each integration, the integrator pen may be reset to either margin. The integrating circuit may be set to any assumed zero point in the span of the recorder signal. (Texas Instruments, Inc., 3609 Buffalo Speedway, Houston 6, Tex.)

Circle 10 on Readers' Service card

■ MERCURY-VAPOR METERS are offered in three models. Model 21 is a single-range analyzer operating from 0.03 to 3 mg/m³. For operation, the instrument is adjusted to zero in an area known to be free of vapor. Model 23 is a dual-range instrument with ranges 0.005 to 0.1 mg/m³ and 0.03 to 3

mg/m³. The instrument uses two phototubes in a bridge circuit with internal standards to eliminate the need for external calibrating devices. Model 24, similar to model 23, features an audio alarm; alarm concentration is adjustable over the entire range of the instrument. All models operate on 115-volt a-c. (Beckman Instruments, Inc., 2200 Fullerton Rd., Fullerton, Calif.)

Circle 11 on Readers' Service card

■ SAMPLING VALVE (Fig. 6) for chromatographs or mass spectographs has 12 intake ports connected to a common vacuum chamber. Each valve is sealed by a spring-loaded ball except at the time the port is open for sampling. A cam controls opening and closing of each port by moving a rod that unseats the ball. A 3-sec electrical pulse actuates motion of the valve from one position to the next. The valve detents to a new position in 10 sec. (Gelman Instrument Co., 106 N. Main St., Chelsea, Mich.)

Circle 12 on Readers' Service card

■ POTENTIOMETER is a four-dial, six-figure, dual-range d-c instrument with a total range of 2.101010 volts. The manufacturer guarantees accuracy of ± 0.001 percent for a period of 5 years, initial adjustment within ± 0.0002 percent, stability within ± 0.00015 percent per year, thermal electromotive forces less than 0.1 μv, and resolution of 0.1 μv. The potentiometer also functions as a resistance comparator accurate to two parts per million and a saturated–standard-cell comparator that will detect differences of 1 μv. (Sensitive Research Instrument Corp., 310 Main St., New Rochelle, N.Y.)

Circle 13 on Readers' Service card

■ TRACKING FILTER is designed to improve the signal-to-noise ratio of Doppler signals by reducing circuit bandwidth. The device is in effect a bandpass filter whose center frequency automatically tracks the Doppler signal frequency. The tracking bandwidth is adjustable over a wide range by means of a front-panel control even while tracking is being performed. Input frequency range is 100 to 20,000 cy/sec; input signal level is approximately 3 volts (r.m.s.); tracking bandwidth is adjustable to 1.0, 2.5, 5.0, 10, 25, or 50 cy/sec. (Interstate Electronics Corp., 707 E. Vermont Ave., Anaheim, Calif.)

Circle 14 on Readers' Service card

■ X-RAY IMAGE INTENSIFIER produces an image said to be 3000 times brighter than that available on a conventional fluoroscopic screen. The system accommodates 16- and 35-mm motion-picture cameras or still cameras, either in combination with television or separately. These cameras can be used in conjunction with direct viewing. An automatic brightness control maintains consistent brightness levels when viewing or quickly scanning objects of varying density and thickness. All photographic-exposure factors are automatically compensated, so the operator has only to push a button to record photographically. The intensifier can be used with x-ray generators at voltages up to 250 kv. (Tracerlab Industrial Division, 1601 Trapello Rd., Waltham, Mass.)

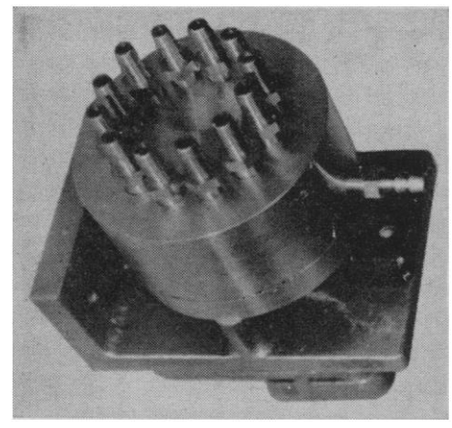

Fig. 6. Sampling valve.

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■ RESISTANCE THERMOMETER uses a hermetically sealed platinum resistance element to sense temperature. The sensitive element is 0.138 in. in diameter and 0.4 in. long. Precision of the sensor is said to be better than ± 0.05 ohm or ± 0.05°F; measurement range is -100° to ± 400°F; linearity deviation is said to be less than 1 percent of full range. Nominal resistance is 470 ohms at 32°F; time constant is 2 sec in moving liquid. (Minco Products, Inc., 740 Washington Ave. N., Minneapolis 1, Minn.)

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■ MAGNETIC MATERIAL SEPARATOR (Fig. 7) achieves efficient separation by preventing the trapping of nonmagnetic particles within agglomerates of magnetic particles. The mixture to be

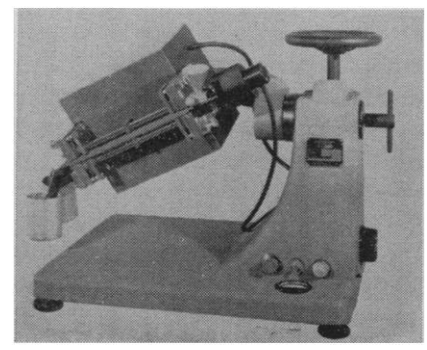


Fig. 7. Magnetic material separator.

separated is fed down a vibrating chute that is inclined sideways as well as forward. An alternating magnetic field is used. Several times in each second the magnetic field is switched off for a few cycles which allows time for the vertical agglomerates to collapse, releasing the nonmagnetic particles and allowing them to migrate toward the lower side of the chute. The magnetic particles are kept toward the higher side by the nonuniform magnetic field that is produced by a current of several hundred amperes flowing in accurately spaced rods that are parallel to the chute. A divider near the lower end conducts the fractions to separate receptacles. (S. G. Frantz Co., 121 Kline Ave., Trenton 6, N.J.)

Circle 17 on Readers' Service card

■ THICKNESS-MEASURING INSTRUMENT measures the thickness of organic and other nonmagnetic coatings on iron and steel. The instrument operates on the magnetic-amplifier principle. The measurement is performed by applying a two-pole probe to the surface and reading a meter deflection. Hardened beryllium copper foils are furnished for calibrating the instrument. Four probes are available with pole separations from 5/32 to 1 in. for thicknesses up to ¾ in. An attachment for applying the probe at constant pressure when measuring soft materials and wires of small diameter is available. (Twin City Testing Corp., Tonawanda, N.Y.)

Circle 18 on Readers' Service card

■ MICROTOME CRYOSTAT provides for mounting a rust-proof microtome in the evaporator. The drive wheel of the microtome is mounted on the outside of the cryostat so that the operator exposes his hands to the low temperature only when introducing or removing the tissue sample. Temperature is maintained between −10° and +20°C. The unit may be operated with the cover open. (International Equipment Co., 1284 Soldiers Field Rd., Boston 35, Mass.)

Circle 19 on Readers' Service card

■ PULSE GENERATOR features rise times of 0.3 nsec, minimum pulse width of 0.7 nsec, and built-in calibrated widths of 5, 10, and 20 nsec. Other pulse widths can be produced with lengths of coaxial cable used externally. Repetition rate is variable from 20 to approximately 300 per second. Pulse generator is a coaxially mounted mercury-wetted switch with an adjustable damping vane for adjustment of undershoot and overshoot. (Lumatron Electronics, Inc., New Hyde Park, N.Y.)

Circle 20 on Readers' Service card

JOSHUA STERN
National Bureau of Standards, Washington, D.C.

Ninety-six percent of our respondents reported no medical debts, and exactly the same percentage was found by Steiner and Dorfman for 1951.

The most recent study of medical expenses of the aging known to us is based on data collected through the National Opinion Research Center. Odin W. Anderson, Patricia Collette, and Jacob J. Feldman, in "Family Expenditures for Personal Health Services" (Health Information Foundation, 1961), present findings comparable to our own. The "Profile" study showed that 97 percent of respondents had expenditures for physicians below $50 for one month, and that 2 percent had expenses above $50 but below $100. Anderson *et al.* found that 86 percent of their aged respondents had expenditures for physicians below $100 for an entire year. The "Profile" study showed that 95 percent of the respondents had no hospital expenditures in one month and that 3 percent had hospital expenditures below $100. Anderson reports that 86 percent of his aged respondents had no hospital expenditures in a year, and that 5 percent had hospital expenditures below $100. According to the "Profile" study, 98 percent of the aged had expenditures for medicines of less than $50 in a month, while Anderson reported that 88 percent had spent less than $100 for (prescribed) medicines in a full year.

If a few of our regional associates in the study, in response to a request from a subcommittee of the United States Senate, have felt it their duty to support the subcommittee, we may expect the data to be biased in favor of universal misery. If, in spite of the data they delivered and certified to us, some associates wish to believe that the aging are in a grave plight, it is a tribute to their professional competence and scholarly objectivity that they furnished the data as obtained by the interviewers. It has often been said that a chief mark of the scientist is that he even reports findings he does not like.

JAMES W. WIGGINS
HELMUT SCHOECK
Emory University, Atlanta, Georgia

Our reporter did try to contact Wiggins and Schoeck before publishing the news article. He telephoned Atlanta, but was unable to reach them. His report was based not on the press releases of the Senate subcommittee but on an examination of the letters in the files of the subcommittee; interviews with American Medical Association officials in Washington; the report, under the by-line of Wiggins and Schoeck in the Wall Street Journal *summarizing the findings of their study; and the A.M.A. press release interpreting their work.*—ED.

Degrees and Titles

This letter is a commentary on your most interesting editorial in *Science* [**133**, 441 (17 Feb. 1961)] entitled "A question of degrees." In 1920 the Society for the Rationalization of the Title of Doctor was organized at the University of Virginia and immediately received a great deal of favorable publicity. I would like to call your attention to the stand the society took at the time, but I have to rely on my memory alone. I believe the following numbered statements give the society's position.

1) The title of Doctor was to be limited to doctors of medicine, dentists, druggists, ministers of the Gospel, and Ph.D.'s of less than 1 year's standing, although, on occasion, it could be applied to a Ph.D. in either affection or derision.

2) The title of Professor was to be limited to high school teachers (male), to aviators giving exhibitions (they did in those days), and to any professional wrestler who owned a gymnasium and taught wrestling.

3) Any student who called a member of the faculty "Professor" was to be corrected on the spot.

4) All members of the faculty were to be called "Mister" (there were no female members), unless they taught in the Medical School. In this latter case it was just too much trouble to tell the M.D.'s and the Ph.D.'s apart, so they were lumped together and called "Doctor."

5) The president of the university should never be addressed as "Your Excellency."

The above standards have the virtue of being precise and definite. I admit that sometimes I am in doubt about the proper way to address a colleague—whether to call him "Doctor" or "Professor." I fear I take the easy way out and compromise. When in doubt I address him as "Colonel."

CONWAY ZIRKLE
Botanical Laboratory, University of Pennsylvania, Philadelphia

Your editorial "A question of degrees" was a timely one. I was surprised, however, that you failed to take a stand on the issue. Members of the medical profession have been only too eager to appropriate the term *doctor* for themselves and fully exploit its value (even medical students are called "Doctor" in hospitals), and, due no doubt to various group pressures, the Washington *Post* has been following a highly discriminatory editorial policy under the very eyes of professional societies. Were it not for the lack of intelligent action by professional societies and for the timidity of some Ph.D.'s, the "question of degrees" would probably not have arisen in this country.

Incidentally, most surgeons and physicians in England are called "Mister" because, very simply, they do not have a doctorate. The minimum requirement for the practice of medicine in England is a Bachelor of Medicine degree similar to our Bachelor of Laws degree; the British M.D., which is higher than an American M.D., requires the completion of original research work and proof of many years of professional competence.

STEPHEN D. BRUCK
4401 East West Highway, Bethesda, Maryland

In your editorial "A question of degrees" you write, "The degree of doctor of philosophy was . . . modeled after the German Ph.D."

Some information, almost certainly well known to you, that you could have presented in the last half of your editorial is that common courtesy, almost anywhere in the world, indicates that an individual is most appropriately

addressed by his highest title. For example, the president of a university who once was a professor and who also had been granted a Ph.D. degree would be addressed in Germany as "Herr President," and if it was needful to repeat all of his titles, they would appear as "Herr President Professor Doctor." Discerning persons notice the impropriety of addressing a given individual as "Doctor" instead of "Professor" if he is entitled to both titles—provided, of course, there is intent to follow the German system.

The English title of Mister depends on matters nonacademic.

In summary, the problems that you pose might be answered in one way if the German system were followed and in another way if the British system were followed. It seems to me that there is no American system. If there is an American system, perhaps someone like you who has given thought to the matter ought to outline it. Those of us who are teachers might find it useful to have a recommended system in order to teach students in American universities how to avoid unintentional discourtesies.

E. RAYMOND HALL
Museum of Natural History,
University of Kansas, Lawrence

Loyalty Oath

Jack P. Hailman's letter regarding the loyalty oath [*Science* **133**, 251 (27 Jan. 1961)] reawakens a grave concern regarding the growing tendency of Americans (not only scientists) to shrink from an opportunity to reaffirm love for, belief in, and loyalty to their country. This tendency is approaching a stage of neurosis, or negative thinking, in which a loyalty oath is regarded as being as surely preliminary to adoption of the cloak of the Fifth Amendment as a Bach toccata is indicative of an impending fugue. Such concern was in no way allayed by the eloquent appeal of Bentley Glass on behalf of the resolution, adopted by the AAAS Council at the Chicago meeting in 1959, recommending elimination of this requirement for the grant of National Science Foundation fellowships.

I would suggest to Hailman that he might ponder whether a President-elect of the United States should feel, concerning the not dissimilar oath he is required to take at his inauguration, "How unnecessary!" It seems to me that, if one is loyal to his country, taking such an oath is the least undertaking he can make, and rather than regard it as an insult, he might better be willing to take the oath at every available opportunity.

On moral grounds it can be ques-

tioned whether falsely subscribing to an oath can be regarded as an "insignificant offense," even in a comparative sense. And it matters little whether the oath is made in reference to the Bible, the Koran, or the Talmud. If one is a believer in the sanctity of oaths, as one would expect a believer in American ideals to be, there can be no crossing of the fingers, no seeking of special immunity for the field of science. Not to acknowledge the sanctity of oaths is to flirt with the moral dangers of agnosticism and with social beliefs inimical to the Western world.

Let us remember that disbursements of funds by the National Science Foundation are largely disbursements of funds of all American citizens, who have an essential interest—too often disregarded, one might add—in the manner in which such funds are used. But the most publicized defections from the Western world are those of people with access to scientific knowledge which could be useful to unfriendly powers. Every citizen should reasonably require that his funds be disbursed in such a manner as to bring maximum benefit to his country. The loyalty oath is certainly a means of *trying* to ensure this. The average citizen might well feel that it should be required of the scientist above all, in view of past happenings and of the scientific revolution which he is told he is witnessing.

If one were to require every person in the country who is to benefit from federally financed programs of any kind to take a loyalty oath—a course suggested by Hailman as being less objectionable—we would require it of every citizen from womb to tomb. That might indeed be desirable, but would it be practicable? In naturalization proceedings, for example, the courts normally absolve those of tender years from taking the loyalty oath. Perhaps it should be regarded as acknowledgment of maturity that graduate students are required to take the oath. They are, naturally, free to decide whether national funds available to them are worth a moral commitment.

One might echo President Kennedy's rhetoric, "Ask *not* what your country can do for you. Ask what you can do for your country!" The need for good scientists is freely acknowledged, but "good" has many connotations. All of them are implied in this context.

I should not like to think that the requirement of a loyalty oath for National Science Foundation fellowships is, through individual decision or the counsel of others, depriving us of sound scientists. I cannot feel that the requirement is depriving us of *good* sound scientists.

I hope that, if time permits, Hailman will reconsider his decision and take the oath, which would not deprive him of any rights but which would, in some eyes at least, enhance his stature as a *good* sound American scientist.

A. J. HAWORTH

30 Glendale Drive,
Glenbrook, Connecticut

UNESCO Statements on Race

If there is anything less profitable than replying to a hostile reviewer [see *Science* **133**, 873 (24 Mar. 1961)], it is to consume the valuable space of a journal devoted to more edifying matters. On one matter of fact, however, since it concerns others in addition to myself, may I beg the courtesy of a few words.

The first UNESCO Statement on Race was not, as your reviewer states, written largely by myself. It was written by the committee appointed to draft it. As *rapporteur* of the committee it fell to me to act as secretary. At the request of the committee I wrote the first draft, and after this was hammered into shape by the committee, I can by no stretch of the imagination conceive how I could be said to have been largely responsible for writing it. As for your re-

viewer's remark that the statement was so unacceptable that it had to be rewritten, the truth may be ascertained by any reader who cares to compare the first statement, mainly written by social scientists, with the second statement, mainly written by physical anthropologists and geneticists. The difference is as between Tweedledum and Tweedledee.

ASHLEY MONTAGU

321 Cherry Hill Road,
Princeton, New Jersey

I am sorry if I misrepresented Montagu's role in the preparation of the first UNESCO Statement on Race; I was reflecting what I feel to be the opinion of many physical anthropologists. Since it is only natural that he would be modest on this score, perhaps others connected with the project will set the record straight.

In the final sentence of his letter Montagu uses a literary reference to say that the second UNESCO Statement on Race differs only insignificantly from the first. This alleged equality is supposed to prove that the first statement was acceptable and did not need rewriting. Why then was it necessary to go to all the trouble of preparing a second statement? And why does Montagu take up space in his textbook with two "identical" statements? Here it is pertinent to point out that Comas, who was a member of the first committee, includes in his textbook *not* the first statement but the second. Why has he, a renowned physical anthropologist, abandoned his own committee's statement, unless he now feels that it is unacceptable?

T. D. STEWART

Department of Anthropology,
Smithsonian Institution,
Washington, D.C.

Authors as Indexers

John R. Clark's letter [*Science* **133**, 1040 (7 Apr. 1961)], suggesting that authors are best qualified to index their own books, misses the same point—of some moment for scientists and for scholarship as a whole—that is overlooked by the advocates of bibliographical machines.

Actually, of course, authors are seldom qualified to do indexing. Only occasionally can they do half as well as an experienced professional indexer. The fact that authors or publishers, or both, are frequently unwilling to pay a professional illustrates nicely their underestimation of the problems involved.

Aside from such general considerations as the special nature of indexing technique and the fact that some specialists cannot write intelligible prose without help, authors nearly always

have special slants or blind spots that make it difficult for them to do a good index. An individual may be the authority on the content of his particular book, but it does *not* follow that he has a good enough over-all, objective view of the whole field to which his book is a contribution to do a competent piece of indexing. This is particularly true in those fields where, for various reasons, terminology is not stabilized.

The point overlooked by both Clark and the gadgeteers is that research, writing, publishing, indexing, literature searching, and criticism are all parts of an extremely complex, ever-changing *social* process in which more judgments and decisions, involving values as well as subject-matter technicalities, must be made than any one person can handle.

HENRY BLACK
Bibliographical Services,
New York, New York

Statistical Evidence

Warren Weaver's statement ["The disparagement of statistical evidence," *Science* **132**, 1859 (1960)] that "statistical evidence is, in essentially all nontrivial cases, the only sort of evidence we can possibly have" seems to me to be as "wholly unwarranted" as is the practice he very properly condemns—namely, the "automatic discarding of evidence because it is statistical." Statistical evidence is usually very useful, sometimes essential, but there certainly are many kinds of useful evidence that are not statistical in the usual meaning of that term. Our belief that the earth is not flat, that it revolves once in 24 hours, and that it completes an orbit around the sun in a year is not based on statistical evidence. Neither does our acceptance of the theory of evolution, or of a dozen other theories that might be mentioned, depend to any great degree on statistical evidence, although these theories may be and often are supported by such evidence.

The difficulty is partly semantic. Some writers seem to regard as statistical practically any method of dealing with quantitative data, but usually the term implies frequency distributions, standard errors, analysis of variance, correlation coefficients, and so on. Presumably it is these latter that Weaver had in mind. If so, he certainly must realize, on second thought, that failure to use these techniques does not automatically negate the usefulness of quantitative data. Mendel, for example, did not use them, and yet he revolutionized our ideas of heredity.

Weaver is not the first to imply or state that statistical methods are essen-

tial for all really worth-while research. And if we enlarge the subject to include mathematics, of which statistics is a branch, several noted scientists could be added to the list. Yet Darwin had little use for mathematics, and Faraday's experience with mathematics is said to have been limited to turning the crank of a calculating machine. G. N. Lewis has deplored the tendency of many to overrate mathematics.

Both mathematics and statistics have made, and will no doubt continue to make, revolutionary contributions. But we might well ask whether the occasional reluctance to use them or to accept the conclusion derived from them may not be due in part to the tendency to overemphasize their value. Pasteur pointed out many years ago that overemphasis of any kind leads to reaction, which, again overshooting the mark, makes the search for truth ever more difficult.

S. C. SALMON
4103 Roanoke Road,
Hyattsville, Maryland

Warren Weaver's editorial has stirred up some discussion here. As a statistician in experimental work, I should like to present a statistician's view.

The simple phrase *statistical evidence* is, unfortunately, widely used to describe two very different kinds of evidence: (*a*) that provided by data gathered from uncontrolled events just as they happened to occur "in life," and (*b*) that provided by data gathered from a planned experiment in which every effort is made to prevent the effects being studied from being confounded with effects of irrelevant factors.

Weaver mentions this dichotomy, but his ensuing remarks seem to imply that he regards the difference as one of degree. I would go further and say that the two are entirely different, and that a major reason for the slow improvement of the quality of scientific inference has been the confusion of *a* with *b*. Scientists who call both *a* and *b* "statistical evidence" have properly rejected *a* but have then gone on to reject *b* merely because they have given it the same name; the result is that they do not know or use as much statistics as they should. Some of them might even be surprised to know that the statisticians are very much on their side and have even invented a distinguishing phrase, "historical evidence," for evidence of the kind described under *a*.

Evidence of type *a* is exemplified by the sports column I once read in which the writer decried the firing of football coaches and produced "statistical evidence" of the folly of this action by displaying data showing that colleges which keep their coaches for long periods of time have much better records on the field. Much of the "statistical evidence" relating to smoking and lung cancer may be in a similar category; we do not know.

Type *a* evidence is good only for suggesting experiments which may produce type *b* evidence. Unfortunately, there are fields in which type *b* evidence cannot be obtained. This may force us to use type *a* evidence, but it does not force us to assign to it the degree of confidence that is associated with type *b*. In fact, those fields wherein type *b* evidence cannot be produced might be better described by some word other than *science*.

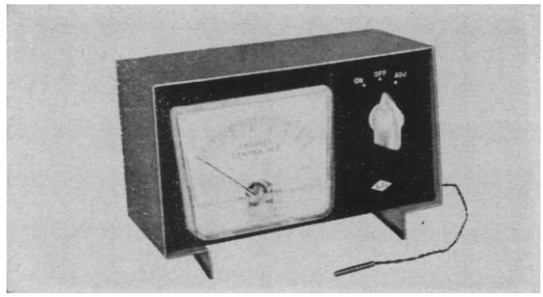
ROBERT HOOKE
Westinghouse Electric Corporation
Research Laboratories,
Pittsburgh, Pennsylvania

Warren Weaver's provocative comments on the power of statistical evidence merit repeating. They merit repeating in particular to scientists who are not mathematicians.

The disparagement of statistics on the part of the tobacco industry and of political parties, on which Weaver comments, is understandable (although not commendable) in view of the vested interests of these groups in the results. But the disparagement of statistics on the part of scientists—of men, presumably, with a vested interest only in seeking truth—is deplorable.

In a field of science, such as geology, where there are so many variables, the application of statistical methods is most appropriate. The usefulness of experimental design should be particularly obvious to any working geologist. Yet it is true that only the most prescient geologists are receptive not only to statistical evidence but to statistical methods as well.

I think the reasons for this reluctance are many. In part it reflects the general opposition people (and scientists among them) display toward new ideas that they cannot fully understand. Certainly this is true of geologists, who, as a group, know too little mathematics to comprehend even the general problem, and who are reluctant to accept new ideas that originate outside the ranks. There is also this factor: statistical methods have been taken up by a few zealots who, through a combination of ignorance and enthusiasm, have succeeded in overselling them to the scientific community. There is no doubt that the proper application of statistical theory requires a mathematical maturity not readily come by. Then, certainly, the wild use of statistics in the hands of the advertising agencies has not raised the reputation of statistical



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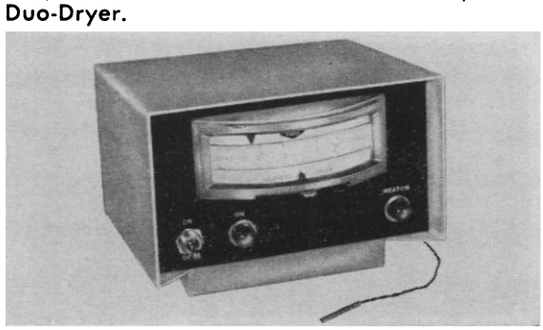

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methods with thoughtful segments of the public.

How to make the general scientific fraternity more receptive to these practical statistical methods poses a problem. It would seem to me that one effort which might help this situation would be to emphasize in education the importance of combining enough statistical theory with other scientific education to make scientists better informed with respect to its potential. And vice versa: urge the would-be statisticians to study enough of one other science so that they can comprehend the problem to which they may subsequently apply their methods. Perhaps, also, there should be more general articles written by qualified statistical experts, who could write for the essentially nonmathematical scientist in *his* technical journals. These articles should describe specific case studies where statistical methods apply.

In any event, I greet Weaver's editorial as welcome prose in *Science*.

M. MATHEZ
Standard Oil Company (New Jersey), New York, New York

Warren Weaver's editorial is trenchant and timely and deals effectively with a most urgent problem. In directing attention to the growing role of probabilistic processes, Weaver has done all of us a great service.

In my opinion, the effectiveness of Weaver's message is marred by an unfortunate usage of the phrase *statistical evidence*. It is important to distinguish between evidence from both experimental and nonexperimental observations, and between the statistical methods with which this evidence is analyzed. For example, the relationship between cigarette smoking and lung cancer, on the basis of all evidence and its analysis, is amply confirmed. The basis for this conviction, however, is not so much statistical as it is the convergence of multiple studies to support the same conclusions, and the finding that diminution in cigarette smoking leads to diminished risk of lung cancer and other diseases. The nub of this protest is that the "evidence" ought to be described by an adjective referring to the nature of the problem and to methods of collecting data—for example, genetic evidence, mortality evidence, and radiological evidence. The proper adjectival use of the word *statistical* seems to me to be restricted to the method by which the data are analyzed and to conclusions drawn concerning whether or not the data conform to random distributions or to other prescribed distributions.

This objection would be trivial were it not for the suspicion that the use of the phrase *statistical evidence* tends to obscure a more fundamental property of many of the sets of evidence which are cited in the editorial—namely, that the evidence is obtained from the observation of naturally occurring phenomena rather than from experiments. It is precisely in this realm of evidence that statistical analysis has provided some powerful insights; nevertheless, the nature of the evidence is often of greater importance to the interpretation of any conclusions than the fact that it was subjected to statistical analysis. As indicated by three out of the four examples given in the editorial, there is special likelihood that man or his reactions will be studied from sets of nonexperimental rather than experimental observations. It is a matter of some concern that, by placing the emphasis on statistical analysis or even upon the probabilistic character of data, the inherent problems associated with the collection and proper interpretation of nonexperimental data about man may readily be overlooked.

These problems are particularly serious in areas of chronic-disease epidemiology, in sociology, and in human genetics. One of these inherent problems is the recognition, measurement, and selection for study of important variables which may influence the as-

sociation which is being studied. A second problem is the collection of data under circumstances which prevent bias and which provide full freedom for the data collected to refute the hypothesis being tested. A third problem has to do with the relationship of evidence to conclusions. While statistical criteria are often important, the convergence of evidence from several independent studies, the demonstration of joint gradients in the magnitude of the supposedly related variables, and the demonstration of reversibility of the dependent variable by the selection and study of suitable sets of data are important guides for drawing conclusions from nonexperimental data. Despite the important contributions which statisticians have made to the management of the inherent problems of nonexperimental data, these are not solely statistical problems but problems of logic and method.

That there are other areas of science where comparable problems exist is shown in the same issue of *Science* by the article "Recent statistical studies in astronomy" [**132**, 1870 (1960)]. Here, also, the basic data are nonexperimental, but the use of systematic data collection and skilled statistical analysis permits some striking and very important conclusions. It is of interest that the use of statistics in the article about astronomy has not led the author to describe his data as "statistical" data. It is proper to describe them as "astronomical" data.

The word *evidence* is also used to designate certain facts presented in a trial; following this analogy, one thinks of statistics as a sort of jury or judge, which helps to decide, on the basis of law (accepted scientific criteria), whether the evidence presented supports the allegations of counsel (the scientists) with sufficient certainty to lead to a verdict. With naturally occurring data, particularly concerning man, we need to focus more attention on the adequacy of the criteria for drawing conclusions (the law, in the analogy of the trial).

In my opinion, use of the phrase *statistical evidence* should be discouraged, since it tends to obscure recognition of this problem.

JOHN R. GOLDSMITH
State of California Department of Public Health, Berkeley

I am delighted that my editorial aroused some interest, and I appreciate a chance to comment. I will not deal in detail with all four letters, for that would be tedious and repetitive. I think a few remarks may clarify my position.

1) An editorial in *Science* contains about 500 words. It is not feasible very fully to develop a subject—even a restricted and minor one—in so brief a statement, nor is there space to give qualifying refinements.

2) The phrase *statistical evidence* appears to have been assumed synonymous with statements made by professional statisticians and making explicit use of "frequency distributions, . . . analysis of variance, correlation coefficients, and so on." There even was some assumption (in other letters which I received) that I had argued in favor of *all* "statistical" reasoning, whether good or bad!

Not being a statistician, I used the phrase less professionally and much more broadly. By statistical evidence I meant evidence in the gathering *or* analysis of which probabilistic considerations enter. Since the entire universe is, as far as science now knows, made up of elementary particles all of whose ultimate laws are probabilistic in nature, it would, at least to me, seem rather difficult to produce any evidence which does not, at some stage of refinement, involve such considerations.

If Salmon will approach the subject from this point of view, he will, I am sure, recognize why I think his "nonstatistical" conclusions (about the nonflatness of the earth, and so on) are so inescapably statistical. His remark that

the earth "revolves once in 24 hours" is particularly vulnerable, partly because it is strictly untrue, but chiefly because one cannot possibly discuss the degree of accuracy of the statement or the reliability of the evidence without a detailed and explicitly statistical analysis.

3) One can, of course do a great deal of very useful and quantitative science without utilizing the procedures of the professional statistician, and certainly without bothering to remember that all evidence is (in the sense stated above) statistical. I had no slightest intention of implying otherwise.

4) Hooke states that the phrase *statistical evidence* is applied to data gathered by observation of uncontrolled events, and also to data gathered from planned experiments. Since these two together include, as far as I can see, all data, Hooke would appear to agree with me that all evidence is statistical evidence. But Hooke has a low opinion of type *a* evidence, this naturally resulting from the fact that by his type *a* he really means (as revealed by his next-to-last paragraph) badly argued conclusions from poorly observed data. Darwin used type *a* evidence and revolutionized man's thinking in the process.

5) I do not advocate turning all of science over to the statisticians, nor do I think that a small boy, when he is counting his marbles, need be reminded that the counting of electrons is a very queer and slippery business. But I do object to the snide implication that evidence which is "merely statistical" is, by virtue of that fact, silly and unreliable.

WARREN WEAVER
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Nature and Nurture

Questions about the effect of environment on intellectual potential [G. Allen, *Science* **133**, 378 (1961)] should be considered in light of Spitz's studies on "hospitalism" [R. A. Spitz, in *Psychoanalytic Study of the Child* (International Universities Press, New York, 1946), vol. 1, pp. 53–74]. Controlled studies of institutionalized infants showed a drop in developmental quotient from 124 to 72 in institution X during a given period; in institution Y there was no change, and infants at home in comparable socioeconomic areas showed no change. The significant variable was the presence of one mother or mother substitute for each infant in institution Y and of one mother-substitute for each eight infants in institution X.

Follow-up studies 2 years later

[R. A. Spitz, *ibid.* (1947), vol. 2, pp. 113–117] showed profound emotional and intellectual crippling among the surviving infants from the original study in institution X. In addition, in height and weight they were considerably below the expected levels for their age. I urge those interested in this subject to read Spitz's carefully documented and lucid report.

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Fallout

In their article "Atmospheric transport of artificial radioactivity," Martell and Drevinsky (*1*) undertake to demonstrate that the yield, for a given size of stratospheric source, from the Russian weapon test at about latitude 52°N in the autumn of 1955 was greater than that from the American equatorial tests (at 11°N), Castle in 1954 and Redwing in 1956, by factors of 60 and 10, respectively. Martell and Drevinsky calculated these factors from Sr^{90} and Sr^{89} data (*2*) from the rain-collecting station at Milford Haven, Wales, and from estimates of the stratospheric sources by Libby (*3*). We offer here an alternative interpretation of the same data and suggest that the relative yield of the high-latitude test has been overestimated.

Peirson *et al.* (*4*) calculated that not more than 13 percent of the Sr^{90} collected in rain at Milford Haven during the spring of 1956 was due to the Russian tests of 1955. The effect of the Russian tests was seen against a background of stratospheric debris from the 1954 Castle series. This percentage is derived from the values for the ratio Sr^{89}/Sr^{90} after correction for the radioactive decay to 22 November 1955, the date of the only high-yield test of this Russian series (*5*).

The amount of radioactivity injected into the stratosphere by these weapon tests was estimated by Libby (*3*) to be 20 "megatons of fission" for Castle and 1.8 megatons for the Russian 1955 series. Then, on the basis of Martell and Drevinsky's parameter (micromicrocuries of Sr^{90} per liter per megaton), the ratio of yields during the first half of 1956 is

$$\frac{\text{Russian 1955}}{\text{Castle 1954}} = \frac{13}{87} \times \frac{20}{1.8} = 1.7$$

The estimate of 13 percent for the Russian contribution is a subjective estimate of the upper limit, since a significant proportion of the *new* debris during this period could well have been of tropospheric origin. If, however, all the new debris is attributed

to the Russian 1955 test, then the proportion would be raised to 22 percent, and the ratio of yields, to 3.2. On the basis of this interpretation, Martell and Drevinsky's estimate is 20 to 30 times too high.

Where our approach differs from the foregoing interpretation is in the choice of (i) the appropriate reference date for the only reported high-yield explosion for the Russian test series of the autumn of 1955 and (ii) a period in which the Russian debris can be compared with contemporary Castle debris. A comparison in the same period avoids the error of the foregoing interpretation, caused by discounting the effect of the seasonal variation in Sr^{90} activity.

The relative yield from the Redwing 1956 test series may be derived in a similar manner. It has been estimated (*4*) that 78 percent of the Sr^{90} collected at Milford Haven during the autumn of 1956 could be attributed to Redwing. (For simplicity of analysis, the Redwing debris is considered in relation to a background consisting essentially of Castle debris, and the fraction due to the Russian 1955 tests is ignored.) On the basis of Libby's estimates (*3*) of the stratospheric source strengths, the ratio of yields during the autumn of 1956 is found to be

$$\frac{\text{Redwing 1956}}{\text{Castle 1954}} \approx \frac{78}{22} \times \frac{20}{6.7} \approx 11$$

This ratio is about twice that calculated by Martell and Drevinsky, who in this case have overestimated the contribution of Sr^{90} from Castle by selecting an inappropriate period of measurement.

The relevance of this type of calculation can be no greater than that of the stratospheric injection data. As Martell and Drevinsky suggest, the stratospheric component from these weapon tests is uncertain. Also, it is improbable that the selected data, from a single measuring station, would provide a truly comprehensive index of comparison for the relative global yields of these weapon tests.

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It is surprising that two interpretations of the same aspect of fallout can vary so widely. The Peirson and Stewart analysis gives a relative rate of stratospheric fallout from the 1955 Soviet tests and the 1954 Castle tests which differs from our result by a factor of 20 to 30. Part of the difference is due to a change in the time basis of comparison. The physical consequences of the Peirson-Stewart interpretation (discussed below) suggest that these authors have seriously underestimated the contribution of the 1955 Soviet tests to fallout during the first half of 1956. In every respect in which the assumptions and method of Peirson and Stewart differ from our own, they tend to reduce the contribution of the 1955 Soviet tests relative to that of the Castle tests. Since our interpretation of the Milford Haven rainfall data has been presented elsewhere (*1*, *2*), we address our attention to the several points of disagreement.

In our analysis of the Milford Haven data (*2*, Fig. 2), we compared the relative intensities of stratospheric fallout for the Castle, Redwing, and 1955 Soviet tests at corresponding early times after test injection. Since we have concluded from our Ba^{140}/Sr^{90} data (*2*) that most short-lived fission products in world-wide fallout are of stratospheric origin, the initial large differences in fallout rate acquire special significance. Furthermore, during these early periods the contribution of each test source can be assessed unequivocally from Sr^{89}/Sr^{90} data within the stated uncertainties of production ratio and production date. By contrast, Peirson and Stewart attempt to resolve the concurrent contributions of the 1955 Soviet and Castle tests during the first half of 1956 and of Castle and Redwing during the autumn of 1956, thus employing not only a different time scale of comparison but a far more subjective procedure. Any uncertainty in the estimation of one component affects the other component in the opposite sense, magnifying the uncertainty in the ratio. Peirson and Stewart's assignment of all unidentified Sr^{90} to Castle is a dubious procedure, particularly for the autumn of 1956, a period for which residual stratospheric debris from the 1955 Soviet tests cannot be ruled out.

The Peirson and Stewart analysis results in assignment to the Castle tests of 87 percent of the Sr^{90} fallout in the first half of 1956 and 22 percent of that in the autumn of 1956. Applying these percentages to the Milford Haven rainfall data (*3*) gives values for Castle components of 5.6 $\mu\mu$c/lit. for the pe-

riod 14 February to 9 July 1956 and of only 0.52 $\mu\mu$c/lit. for the period 2 August to 1 December 1956. Seasonal variation of this magnitude, with levels varying by a factor of nearly 11 during 1956, is a surprising result for Castle debris, which had been injected into the high equatorial stratosphere more than 2 years earlier. This remarkable variation is all the greater when only the spring peak and the fall minimum periods are compared, and also when the Castle component in rainfall in the autumn of 1956 is corrected for residual debris from the 1955 Soviet tests. By contrast, Stewart (*3*) has shown that levels of Sr^{90} in rains at Ohakea (latitude 40°12'S) varied seasonally by less than a factor of 2 during 1956, when Castle was unquestionably the only significant source of fallout in the Southern Hemisphere. Observed seasonal variations in ozone levels in tropospheric air in the Northern Hemisphere are similarly small. We suggest that the remarkable seasonal variation in Sr^{90} fallout from Castle which results from the Peirson-Stewart analysis is not real and is due to underestimation of the contribution of the 1955 Soviet tests during the first half of 1956.

Peirson's method (*4*) of estimating the Soviet-test component in fallout of Sr^{90} during the first half of 1956 differs from our own in two important respects, each of which results in an underestimation on Peirson's part, or an overestimation on ours, of the Soviet test contribution. First, Peirson arbitrarily assigns a substantial fraction of the Sr^{89}, and thus some of the Sr^{90}, to tropospheric sources. On the basis of the Ba^{140}/Sr^{90} data for New England rains (*2*) we have concluded that substantially all Sr^{89} in world-wide fallout is stratospheric in origin. The Milford Haven Sr^{89}/Sr^{90} data for the first half of 1956 are consistent with assignment of all Sr^{89} to the 1955 Soviet tests. Second, Peirson takes the high-yield 23 November shot alone as the source of stratospheric fallout from the 1955 Soviet tests. Other shots in that series took place on 4 August, 24 September, and 10 November. Although the yield and the cloud heights for these events have not been made public, they cannot be excluded from consideration. Surface shots of 100 kilotons and air shots of even lower yield would inject debris into the lower stratosphere at latitudes of Soviet testing (*5*). In our own analysis we assume stratospheric origin of the Sr^{89} and a Sr^{89}/Sr^{90} activity-production ratio of 170. For the Milford Haven data for the first half of 1956, this leads to an assignment to the stratosphere of 25 percent of the Sr^{90} fallout for the 23 November 1955 shot alone, or nearly 100 percent of the fallout for the 4 August 1955 shot

alone. We take the mid-point of the 1955 Soviet test series as the production date for which it can be assumed, with a factor of uncertainty of 2, that 50 percent of the debris was of 1955 Soviet test origin.

On this basis, we suggest that Peirson and Stewart have underestimated the 1955 Soviet contribution by a factor of between 2 and 8, the Soviet-Castle ratio being thus affected by a much larger factor. The physical consequences of their interpretation indicate that the higher factors must apply. The remaining difference can be explained on the basis of differences in rate of deposition of Sr^{90} fallout from Castle for the two quite different periods considered.

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On Reading Original Papers

The light-hearted editorial, "Electricity and personal magnetism," in your issue of 3 March [*Science* **133**, 611 (1961)] makes amusing reading, but it exhibits the lack of understanding that is at the root of C. P. Snow's "Two cultures." While I cannot claim to have read all 2.5 million words of the "Great Books of the Western World," or even the 642 pages of Faraday's *Experimental Researches in Electricity* (and am in no way connected with the publishers or endorsers), I am sure that your editorial view of what constitutes good reading about science is an extremely limited one.

As I understand it, the writer of the editorial proposes that a reader be told what parts of a scientific work are "really great," what terms are to be considered "right," and where a scientist of the caliber of Galileo, Newton, Faraday, or Darwin was "wrong." Apparently he feels that it is a waste of time to "make one's way" through lengthy, outdated material in the classic works of science when the confirmed results can be condensed to half a page

in a modern textbook. And many of yesterday's scientists agree with him. From his point of view, all that counts in science are the currently accepted *results*.

Of course, most of your readers, including myself, *are* interested in results, but we would be blind indeed if we thought there were nothing else of importance in science but results. The scientific mode of thought, through which these results can be said to have been arrived at, is relatively recent in the history of mankind and is far from being accepted by the majority today —even by the majority of intellectuals. The acceptance of scientific results is easy—too easy—and has led to a common view of science very similar to that of magic in primitive cultures (see, for example, Malinowski's *Magic, Science, and Religion*). Most educated people "know," for instance, that matter is composed of atoms, but how many of them have a clear idea of the complex reasoning from necessarily incomplete and confused data that led to this concept? When the incompleteness and confusion are eliminated in a textbook, this isn't presumptuous so much as simply misleading. How can such treatment give any insight into the development of science or the turbulent frontiers of science today?

I dare say many criticisms can be made of the "Great Books of the Western World," but the criticism leveled in the editorial is not the one to make if you are interested in better understanding of science on the part of intelligent nonscientists.

THORNTON PAGE

Wesleyan University,
Middletown, Connecticut

Your editorial concerning the "Great Books of the Western World" completely misses the point. Editors of *Science* cannot be blamed, perhaps, for being unfamiliar with the wacky world of sales promotion and "consumer motivation," and with the workings of the unscientific mind.

The truth is that most of the people who buy the "Great Books" have no intention of actually reading the material. The set is a prestige object, an exhibit for guests, filling for a handsome bookcase, or a source of pride for a booklover.

I know a person who, having just installed a set of bookshelves, went to a second-hand bookstore and bought a box full of books selected at random for their covers. Buying a set of the "Great Books" is a more sophisticated way of doing the same thing. At the other end of the scale, there are empty ornamental book covers on the market for the economy-minded.

I'm serious!

KIRBY WALKER

609 Wendy Lane,
New Orleans, Louisiana

Your editorial points up most effectively the weakness of the "Great Books" curriculum. The advocates of such an educational sequence seem to imply that to be educated one must repeat the experience of the race, at least as represented in the writing of the great minds of history. But the true curriculum has to be a *short cut* to the experience of the race. And that conception does not preclude all first-hand contact with the writings of bygone centuries; it means simply that we must be sufficiently selective so that within practical time limits we may help the learner to gain understanding and control of his present environment.

P. W. HUTSON

University of Pittsburgh,
Pittsburgh, Pennsylvania

I agree completely with Thornton Page that there is great value in reading original scientific papers. The point of my editorial, however, was that the approach followed in the "Great Books" befuddles the general reader unnecessarily, and hence should not be

endorsed. There are also unnecessary opportunities for befuddlement in some of the humanities selections. Let me cite an example from the Jowett translation of Plato's *Republic*, which dates from the Victorian era and which is the translation offered by the "Great Books." As noted by Cornford in the preface to his own more recent translation, the reader of Jowett, when he lights on "the statement . . . that the best guardian for a man's 'virtue' is 'philosophy tempered with music,' might run away with the idea that, in order to avoid irregular relations with women, he had better play the violin in the intervals of studying metaphysics." Not only is this idea false, as the violinist in the Tabu ad has learned to his peril, but this is not what Plato meant by describing (again to quote Cornford) "*logos*, combined with *musiké*, as the only sure safeguard of *areté*."—J.T.

Economics in the News

The tenor of your staff reporter's observations on the economic philosophies supposedly animating the Eisenhower and Kennedy Administrations [*Science* **133**, 367 (1961)] prompts me to register an objection and to offer a constructive proposal.

As a political independent who participated in the preparation of all eight of President Eisenhower's annual economic reports to Congress, I find this piece both superficial and intellectually offensive. To identify the Eisenhower position with "the dismal science" while characterizing "Kennedy's economics" as "the dismal science made cheery" may be good enough journalism and may be assumed to be consistent with the emotional commitment of a substantial fraction of the scientific community. But the Carlylean allusion is anachronistic, certainly since the passage of the Employment Act of 1946 with strong bipartisan support; and the equally Carlylean hero worship manifested by your reporter is inappropriate, not only in a scientific publication but also in a pluralistic democracy in which the economic roles of the President and of the federal government altogether are deliberately confined and in which the "declaration of policy" inserted into an employment act must be so burdened with qualification that it cannot provide an unambiguous standard for administration.

As a member (fellow) of the AAAS, I suggest that the same kind of criteria of objectivity, reliability, and high seriousness that presumably apply to the section of *Science* devoted to research reports be extended to contributions to "Science in the News."

Surely, the association has a much greater stake in teaching than in delighting. It can best upgrade the average level of literacy of its readers regarding other people's specialties by providing unbiased, straightforward, factual reports and balanced analyses. Above all, it must avoid the appearance of concurring in the bizarre proposition that the star of scientific advancement should be hitched to a particular political wagon.

IRVING H. SIEGEL
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Effects of Punishment on Behavior

In his report entitled "Punishment in the squirrel monkey *Saimiri sciurea*" (*1*), James B. Appel states "little is known about the effects of punishment on the behavior of higher organisms." He presents his observations in the evident belief that long-lasting effects have not previously been reported.

Evidence has for many years been available that punishment by electric shock can condition extremely persistent anxiety reactions to previously "neutral" stimuli, accompanied by inhibition of other responses in the presence of the conditioned stimuli, just as Appel observed in his monkeys. The existence of this evidence may have been obscured by its coming under the heading "experimental neurosis." The first reports, as usual, came from Pavlov's laboratories, where dogs were the subjects; subsequently, similar effects were reported in cats by Dimmick, Ludlow, and Whiteman (*2*) and by Masserman (*3*). The Russian workers believed that the persistent behavioral changes in their animals were the result of a "clash" between excitation and inhibition, and Dimmick *et al.* and Masserman attributed the changes in theirs to conflict between feeding and avoidance motivations, because all their animals were punished in circumstances in which food-approach behavior had been conditioned. However, a few years later I demonstrated (*4*) that in cats subjected, without any preliminary training with food in the experimental cage, to high-voltage, low-amperage shocks in the cage situation, the same persistent behavior, characterized by marked anxiety responses, is conditioned. Thus, despite the opinions of the earlier experimenters, the effects reported by them must be regarded as a straightforward conditioning of emotional and other reactions primarily evoked by electric shock.

Though these conditioned autonomic reactions do not disappear through

the ordinary process of extinction, it is relatively easy to eliminate them by counterposing other emotional responses that are antagonistic to the anxiety in the sense that their evocation is accompanied by reciprocal inhibition of anxiety responses (*4*). The extension of this principle to persistent unadaptive habits (neuroses) in human subjects has led to significant new methods of therapy (*5*).

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Wolpe's letter points toward two problems which have existed in the behavioral sciences for many years. The first concerns the categorization, communication, and (perhaps most unforunately) selection of evidence.

While the studies Wolpe mentions, and many others besides, such as those of Solomon and Wynne on "traumatic avoidance" (*1*), indicate that "persistent anxiety reactions" may develop after exposure to electric shock, there is an equally imposing number of investigations which seem to suggest that shock has generally temporary effects or, at best, may facilitate the acquisition of responses which are incompatible with that for which the subject is being punished. I chose to limit my brief remarks to the experiments of Estes (*2*) and Azrin (*3*), because the experimental procedures of these authors closely resembled my own. Moreover, I was interested, together with Estes and Azrin, in *punishment* rather than conflict, anxiety, or experimental neurosis.

It is unfortunate that similar or even identical bits of information are often overlooked, as such, or are confused in a mass of professional jargon even within the field of experimental psychology. This is, I believe, a result of the many different conceptualizations investigators impose upon their data when reporting their results. The confusion would be attenuated if events were described objectively in terms of the variables manipulated. It is obvious that a statement about the effects of electric shock upon the rate of response on a variable-interval schedule of reinforcement is much clearer than a similar account of the effects of conflict on the production of anxiety states.

A second and far more important problem arises indirectly from Wolpe's

comments. If it is true that contradictory conclusions have been drawn from the results of experiments concerned with the effects of electric shock, one might wonder why enough systematic studies have not been made to relate this variability to experimental parameters such as species, shock intensity, and previous history of the animal. Such a program has been largely neglected in favor of short, exciting theoretical studies which may be reinforcing to the experimenter but which seem to have added little to the store of communal knowledge about one of the great problems in contemporary behavioral science, that of aversive control.

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Quantum Mechanics and Biology

The molecular basis of energy transduction in biological systems has been excellently reviewed by Green and Hatefi *(1)*. However, quantum mechanical contributions make it possible to relate these energy changes to more fundamental submolecular processes. Indeed, the application of quantum theory offers the most promising approach to as yet unanswered problems. This viewpoint has been eloquently expressed by Szent-Györgyi *(2)*.

Green and Hatefi *(1)* quite correctly conclude that electron flow in transport systems is not a result of simple molecular collision. Quantum mechanics is more specific. The rapidity of electron flow stems from the redistribution—a practically instantaneous process—of molecular orbital energies. Energy transduction based on such redistributions need not contemplate any appreciable linear movement of the electrons. The quantum-mechanically derived π-electron system fully satisfies the requirements for a catalytically functioning mechanism. This is particularly true for the unsaturated conjugated structures in which the electrons are coupled resonators. This completely allows for both instantaneous and distant site transfer of energy. The known facts concerning biologically active compounds (purines and pyrimidines) are in accord with these concepts. They are predominantly unsaturated, conjugated structures.

As pointed out by Green and Hatefi *(1)*, the mitochondrial lipids are char-

acterized by fatty acid residues with a high degree of unsaturation. This unsaturation is quite consistent with the ability of lipids to provide a high degree of stereospecificity combined with participation in the π-electron system of associated proteins. Thus, the role of lipid in electron transport instead of being "far from clear," as indicated by these authors, is, on the contrary, a very distinct one when based on quantum-mechanical considerations.

The respiratory coenzymes have been shown by Green and Hatefi *(1)* to play a predominant role in electron transport. The redox mechanism of these coenzymes has also been specifically related to the molecular orbital energies of their π electrons. Employing LCAO (linear combination of atomic orbitals) calculations, Pullman and Pullman *(3)* have obtained resonance integral coefficients for the enzymes which explain their redox function. The coefficient for the homo (highest occupied molecular orbital) of reduced flavin mononucleotide ($FMNH_2$) turns out to be negative. Since this is quantum-mechanically indicative of antibonding character, the finding is in remarkable accord with the known fact that this compound is autoxidizable. While similar calculations have not as yet been reported for the cytochrome components of the transport system, evidence is not lacking on which to predict that some will be found with negative homo coefficients. Dixon *et al. (4)* have reported on an irreversible autoxidation of cytochrome *c* reductase. Armstrong *et al. (5)* have reported a similar autoxidation of cytochrome b_2 lactate dehydrogenase. Both autoxidations involve flavin dissociation.

The findings reported above are not without potential application to medical problems. Dehydrogenase deficiencies have been implicated in genetically transmitted and sex-linked transmitted diseases (see *6*). Theoretical quantum mechanical calculations involving π electrons have also been applied to the study of carcinogenesis (see *7*). The ability to theoretically predetermine those particular components of an enzyme system which may be the ultimate source of a distorted metabolism offers exciting frontiers for the biological scientist. A great deal of ground work has been done by the physicist, and if the quantum guideposts are read correctly, this ability could be within the realm of realization.

No doubt, as Szent-Györgyi (*2*) has emphasized, the mastering of a new discipline like quantum mechanics is a Herculean task for the biologist, but it is a road that must be traveled. Nor is it necessarily a one-way street. It is of interest to note the increasing application of biological phenomena to the solution of electronic and engineering problems. Simulation of neurons, memory tracks, and retinas has become the basis of the entirely new field of bionics (see *8*). It is not unlikely that new and revolutionary concepts for physics will have their origins in biology. Knowledge gained in the study of photosynthetic and bioluminescent systems is today providing answers to questions concerned with the storage of solar energy. Such answers may, in the future, provide the means of sustaining life in outer space. All of which points up the basic unity and universality of the various disciplines of science.

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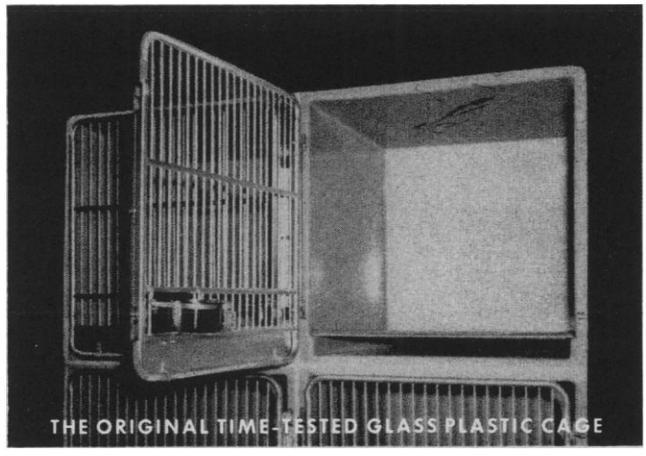

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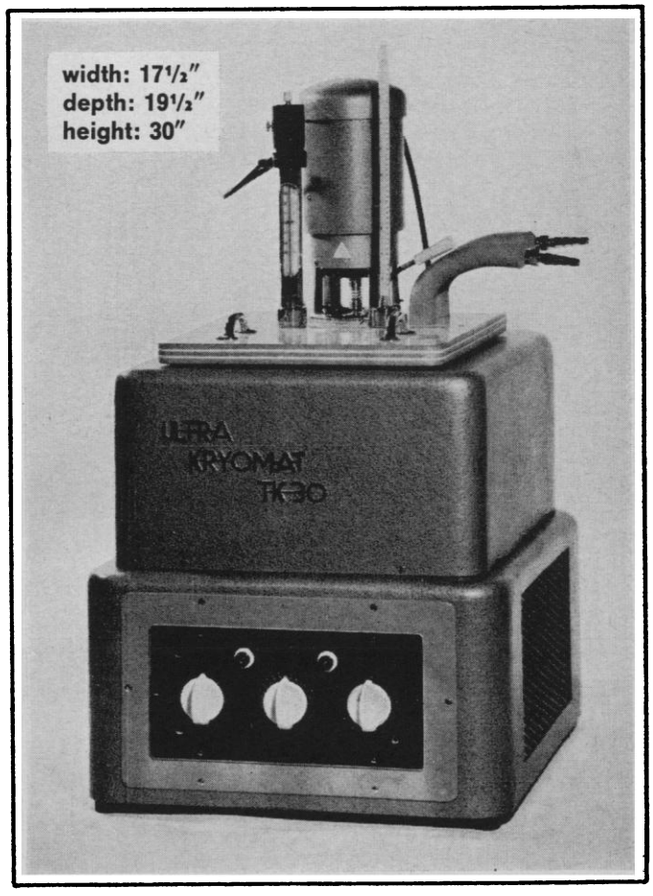

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Size and Productivity

Harmon's very interesting analysis of "High school backgrounds of science doctorates" [*Science* **133**, 679 (1961)] will surely be widely studied and quoted. One aspect invites immediate comment.

The correlation between the size of the graduating class and the productivity of doctorates in all fields was demonstrated unequivocally. However, the implication that large size is *responsible* for high productivity has been reflected in newspaper headlines based on this article. Surely the size of the graduating class is related to the population density of the area and therefore to at least two other important variables, the economic status of the parents and the cultural backgrounds of the families. There was no attempt in Harmon's study to equate such variables; indeed, this would require many more data from the schools and would even then be very difficult. But it seems wrong to assume that by combining small units into large ones these wide differences will disappear.

To give an extreme example, high schools with graduating class size "greater than 800" produced twice as many Ph.D. students as those of class size "600–800," and a simple first analysis would lead us to change all to the very large units. But the "600–800" unit is surely sufficiently large for purposes of organization to best serve the needs of talented students.

It is even difficult to tell from the results presented whether the striking variations of productivity with geographical area and with size are in fact independent, or whether these simply are the result of a predominance of large high schools in the New England and Middle Atlantic states and of small ones in the East South Central area.

Conant and others have argued on logical grounds that adequate size is a major requirement if a high school is to offer a good program. This is certainly confirmed by the experience of those who have tried to provide an enriched program in a small high school. But the statistics which support this conclusion must be analyzed and interpreted with care, lest the problem of providing a good education appear simpler than it really is.

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Harmon's analysis of the high school backgrounds of Ph.D.'s may indeed substantiate the suspicion of many that individuals with degrees in education are recruited from the lowest I.Q. level, but biologists can take little comfort from the finding that their field is populated by students of the next lowest