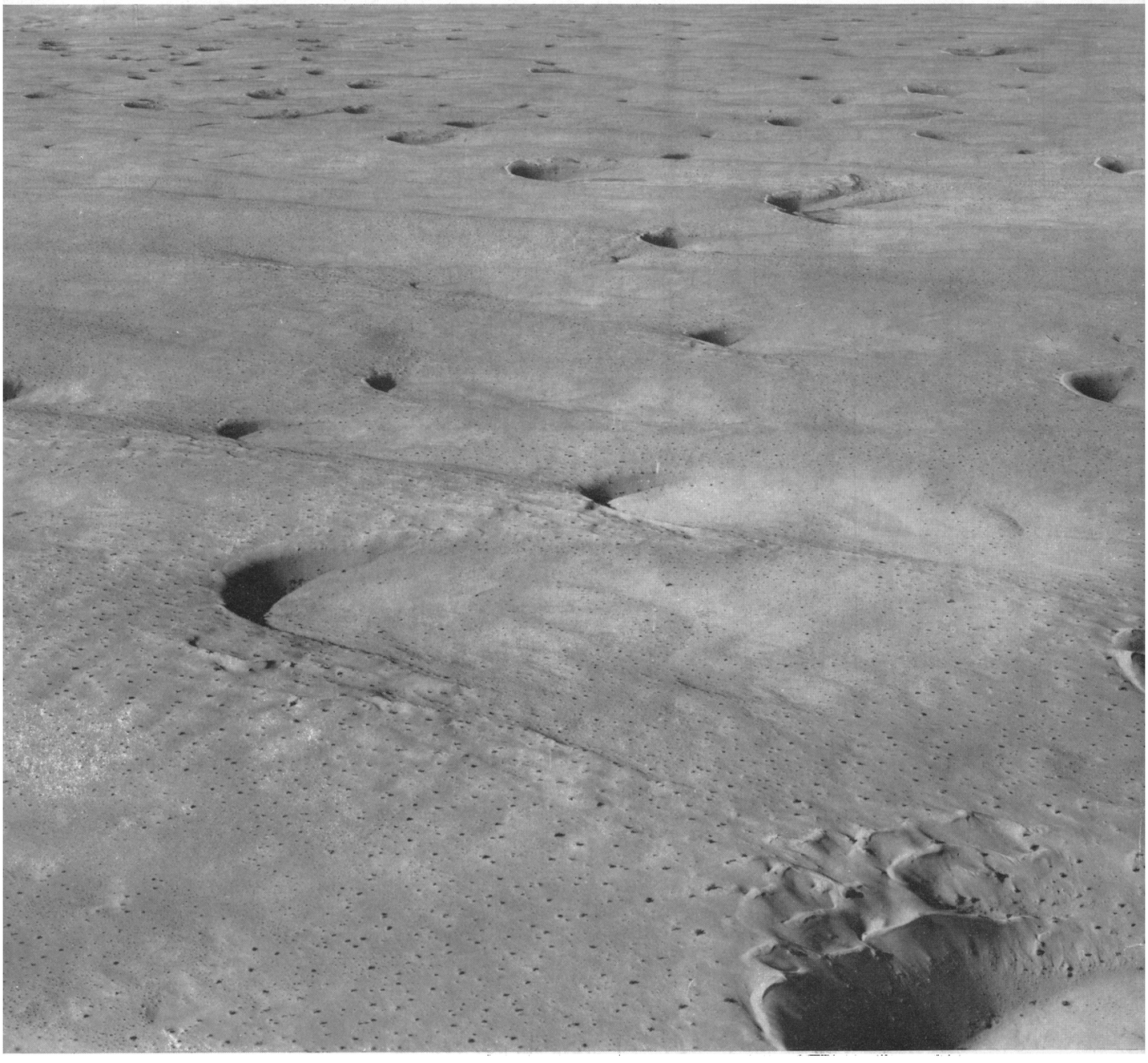
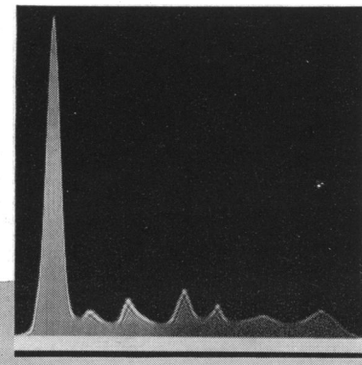
SCIENCE

11 November 1960

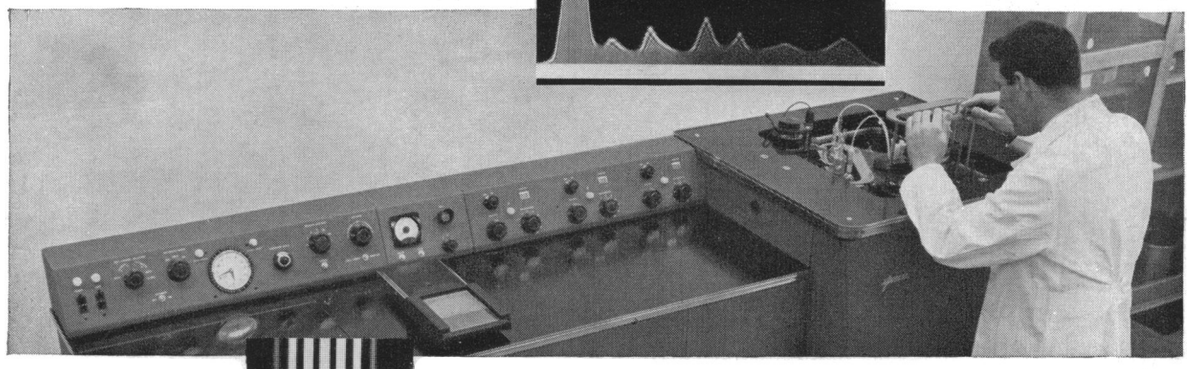
Vol. 132, No. 3437

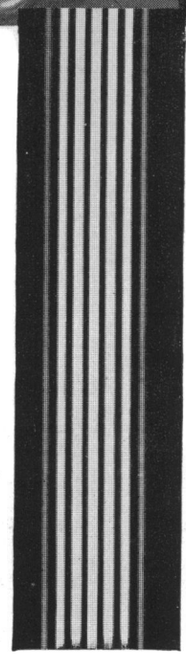
AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE





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





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

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in one precision instrument

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

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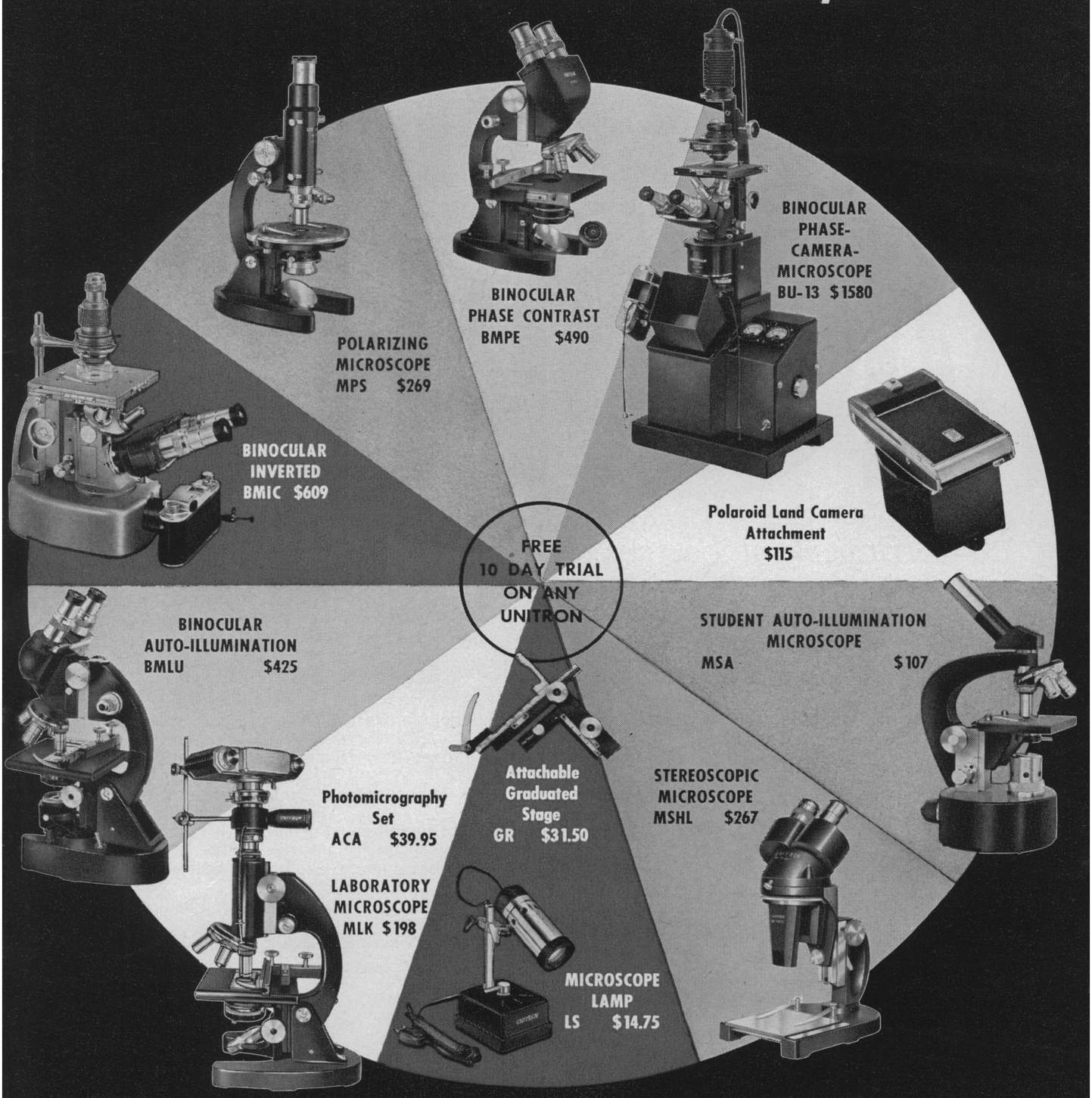

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11 November 1960, Volume 132, Number 3437

SCIENCE

Cover Giant crescentive hollows and slipfaces in the Great Nafud, looking northwest. See page 1369. [Arabian American Oil Co.]

of your next laboratory centrifuge

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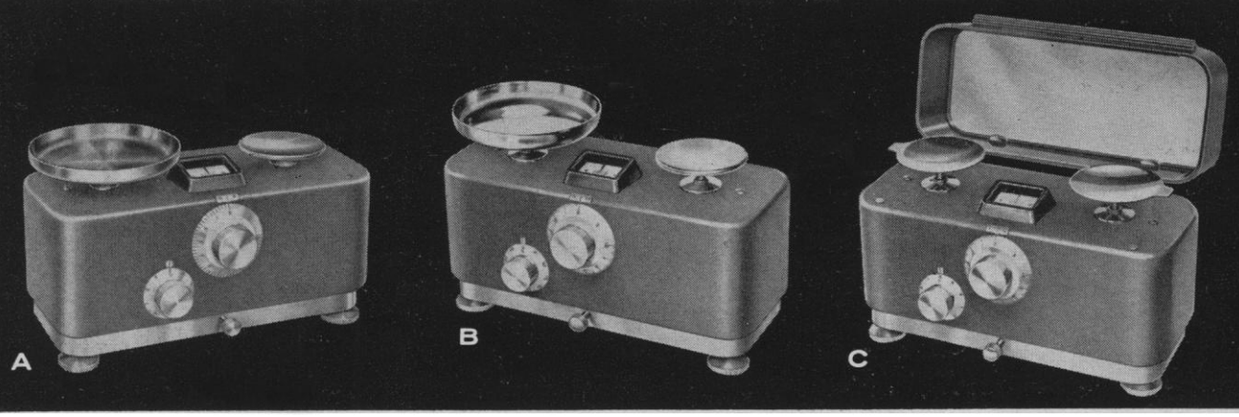

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Fine Weighing Dial: 10 grams by .1 gram graduations
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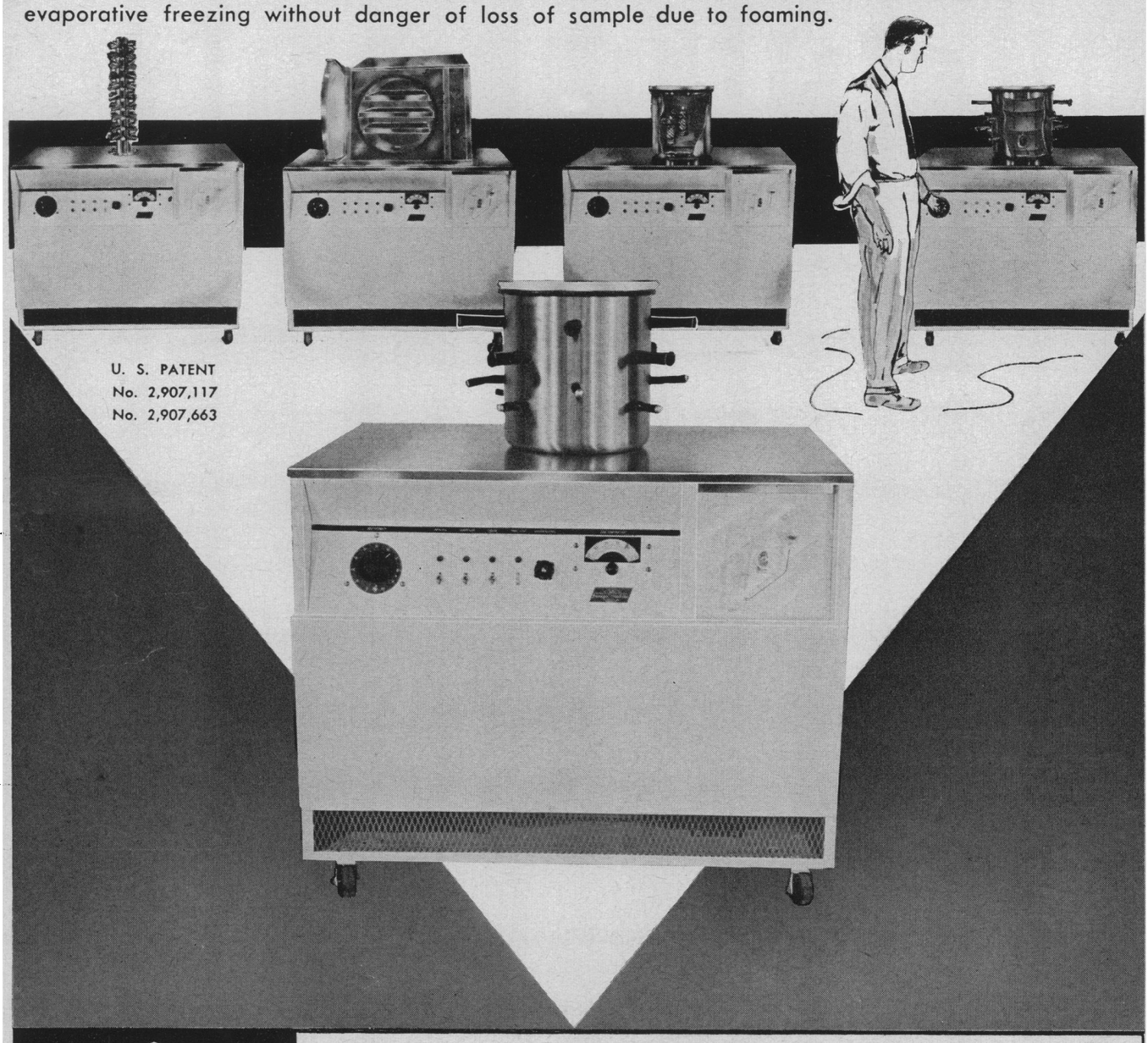

U. S. PATENT
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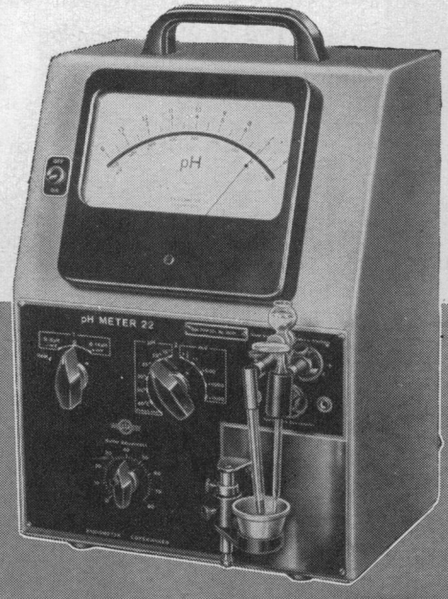
RADIOMETER pH Meter 22



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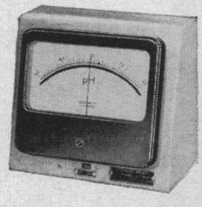

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Using linear programmed temperature gas chromatography, hydrogen, oxygen, nitrogen, methane, ethane, carbon dioxide, and ethylene separate, as shown in the chromatogram, on a single 4 ft. Molecular Sieve 5A column[7]. Mikelson[8] performed similar analyses and found reproducibility to be better than ± 2% relative for hydrogen, oxygen, nitrogen, carbon monoxide and methane and better than ± 5% relative for carbon dioxide. The programming technique also separates nitric oxide and nitrous oxide in the presence of other gases[4,8] and light hydrocarbon gases[3,5].

Work in our laboratory has shown that gas-solid chromatography and temperature programming is adaptable to the permanent and light hydrocarbon gases, fluorocarbon gases, rare gases, and other multicomponent mixtures.

LITERATURE CITED

(1.) Brenner, N., and Cieplinski, E., *Ann. N.Y. Acad. Sci. 72*, 705-713 (1959).

(2.) Davis, R. E., and Schreiber, R. A., in *Advances in Gas Chromatography*, Preprints, American Chemical Society, New York, September 1957, p. D 91.

(3.) Drew, C. M., private communications.

(4.) Graven, W. M., *Anal. Chem., 31*, 1197-1199 (1959).

(5.) Greene, S. A., and Pust, H., *Anal. Chem., 29*, 1055 (1957).

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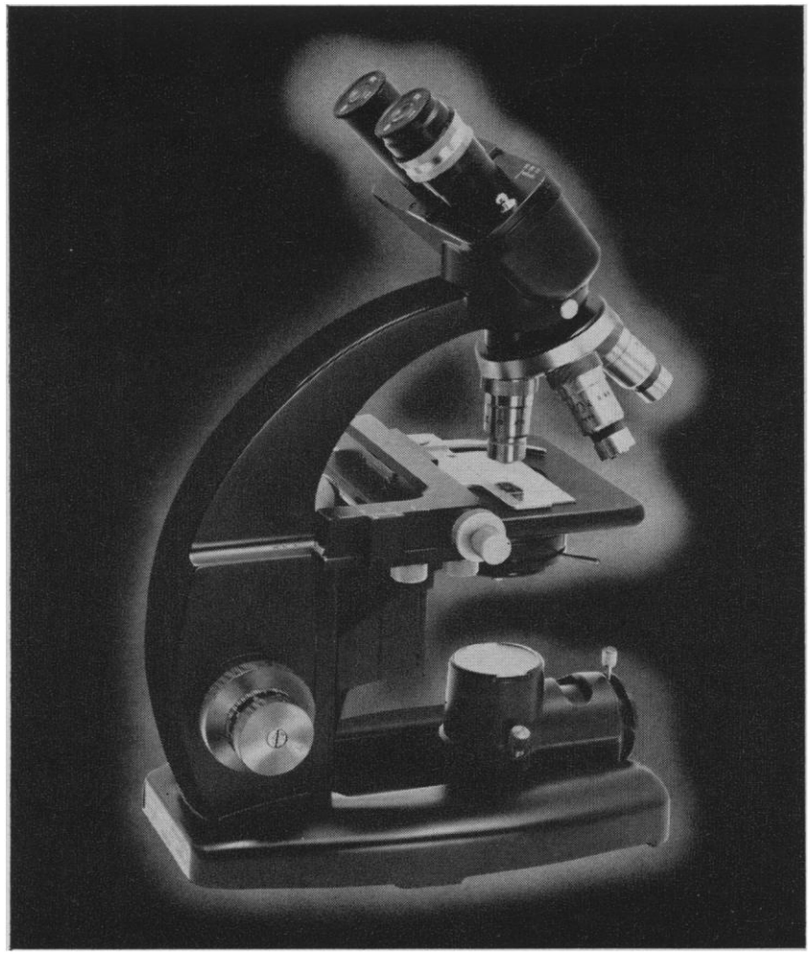
(7.) *Martin, A. J., Bennett, C. E., and Martinez, F. W., Jr., "Linear Programmed Temperature Gas Chromatography", presented at the ISA 2nd Biannual International Gas Chromatography Symposium, Michigan State University, June 1959.

(8.) Mikelson, L., private communications.

*Write **F & M** for free copies.



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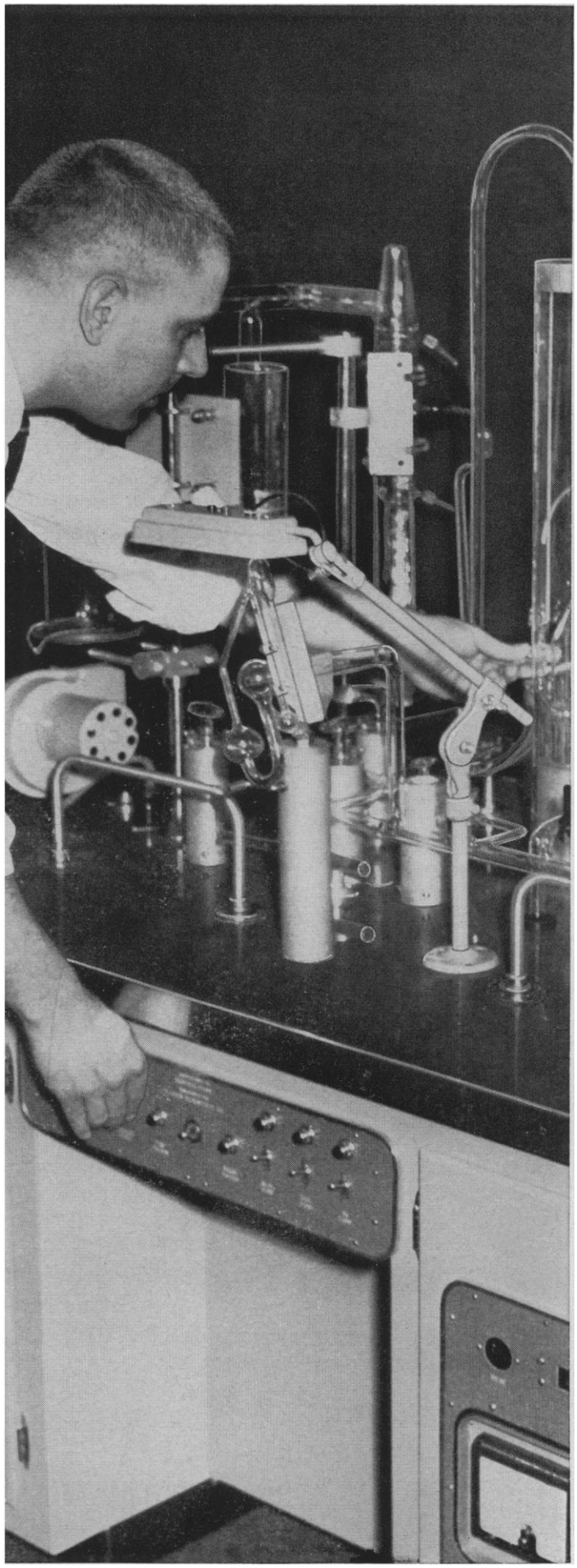

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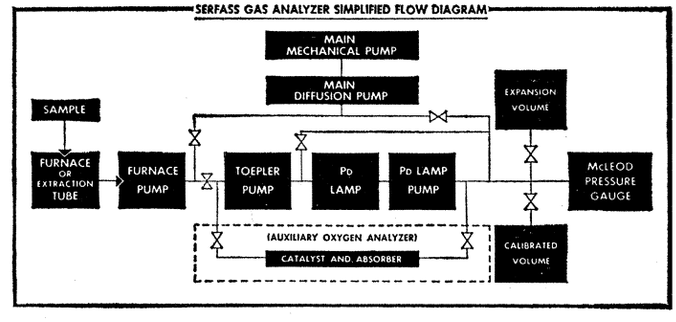

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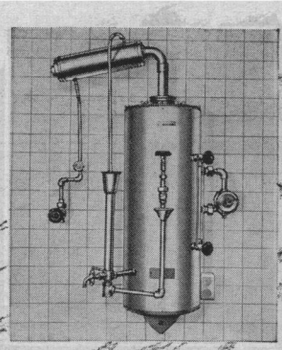

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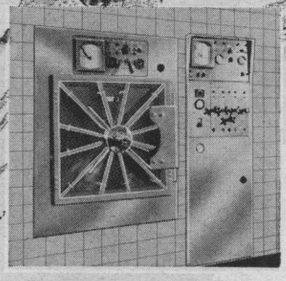

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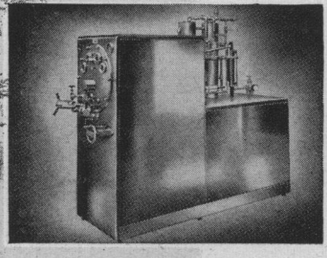

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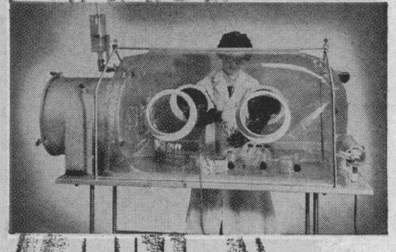

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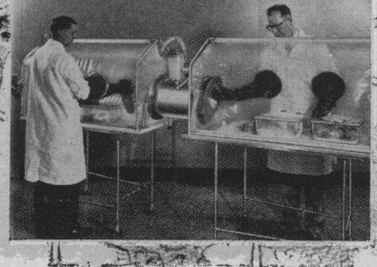

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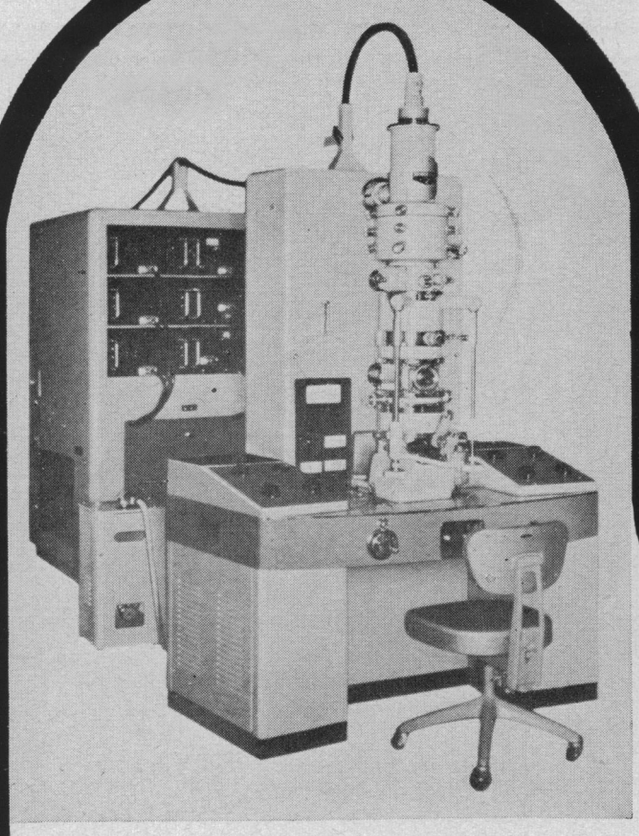
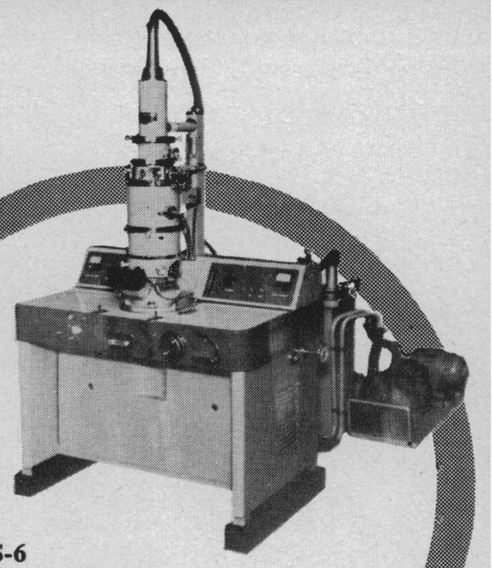


HU-11



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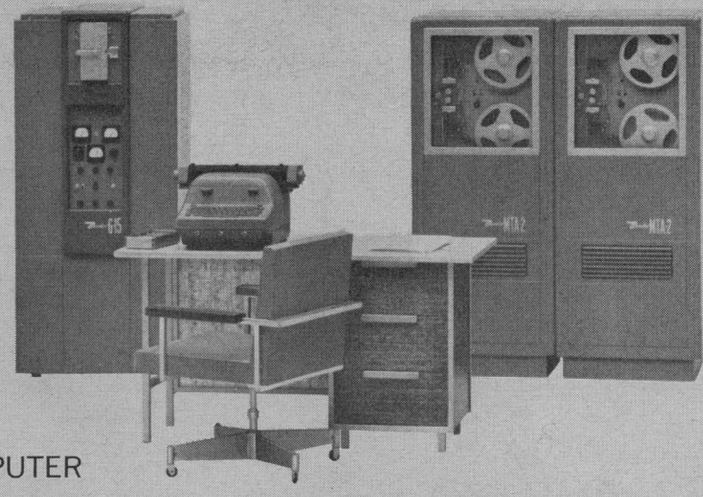

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(For values of R F & L as specified. For values of E ranging from 100 to 300 in increments of 50. For values of C ranging from .00002 to .000021 in increments of .0000001)

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```
BEGIN Ⓢ
R = 10 Ⓢ
F = 60 Ⓢ
L = ₀2 Ⓢ
FOR E = 100(50)300 BEGIN Ⓢ
FOR C = ₀00002(₀0000001)₀000021 BEGIN Ⓢ
I = E/SQRT(R ↑ 2 + (6₀2832 * F * L — (1/(6₀2832 * F * C))) ↑ 2) Ⓢ
PRINT (FL) = E Ⓢ
PRINT (FL) = C Ⓢ
PRINT (FL) = I Ⓢ
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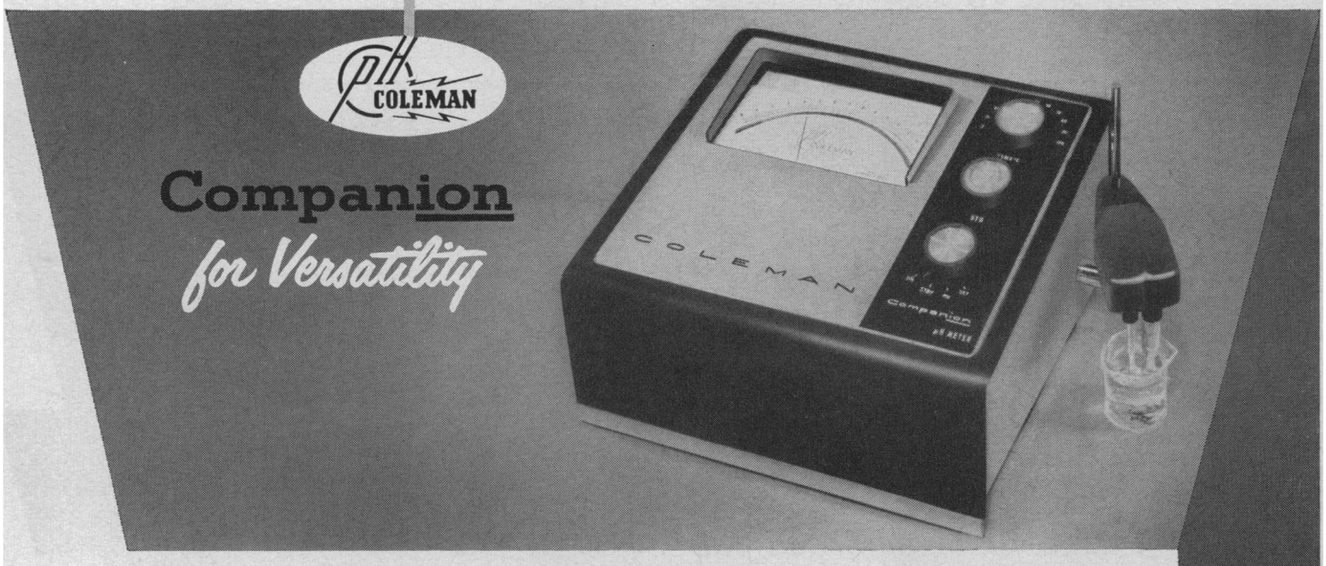

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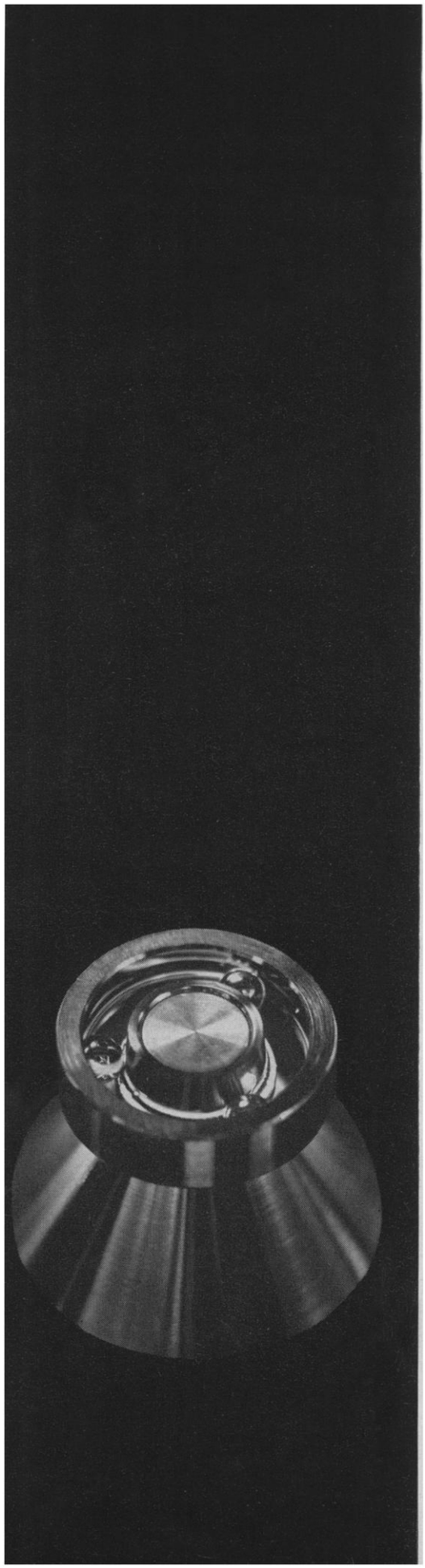

One of a series

Catching the Drift of Gyro Bearings

The fantastic accuracies needed by inertial guidance systems for space flight depend on the suppression of gyro drift, the tendency of a gyro to precess from minutely occurring internal torques. Particularly puzzling has been the problem of "jogs," or sudden axial shifts, within gyro spin-axis bearings. Shifts of but one ten-millionth of an inch can cause serious steering error.

Specialists at the GM Research Laboratories have found that the real key to drift lies in the thickness and distribution patterns of bearing lubricating films. Only a tenth of a milligram of oil — equivalent in volume to less than two-thousandths of a drop of water — is required in a gyro bearing, but even this amount unevenly distributed may cause jogs.

Conducting unique studies of single bearings apart from rotor assemblies, GM Researchers use a hydrostatic spindle and special instrumentation to take film-thickness measurements they compare with hydrodynamic theory. Jogs, due to excess oil supply, have been analyzed in relation to surface oil transfer and separator feed control, ball spin orientation, displacement, and differential heating and ball wander.

This experimental and analytical approach is achieving progress toward jog-free, stably distributed, and suitably thick oil films required in high-precision bearings. It is a further example of the critical and advanced research General Motors carries out in seeking "more and better things for more people."

General Motors Research Laboratories

Warren, Michigan

The fluoresced streaks show the disturbed "wake" of the lubricating film during bearing operation. The active part of the film, too thin to fluoresce visibly, averages ten-millionths of an inch in thickness.

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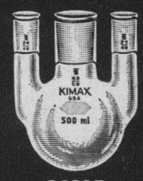

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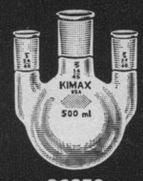

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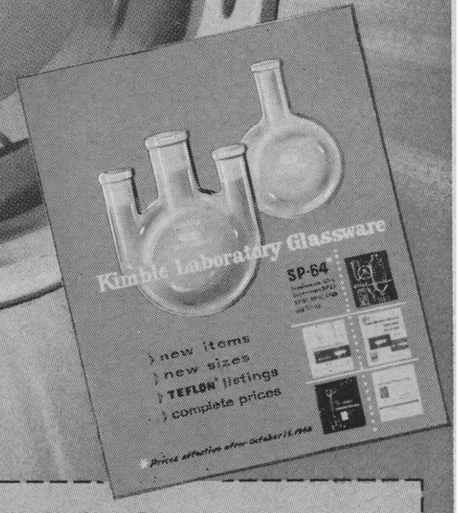

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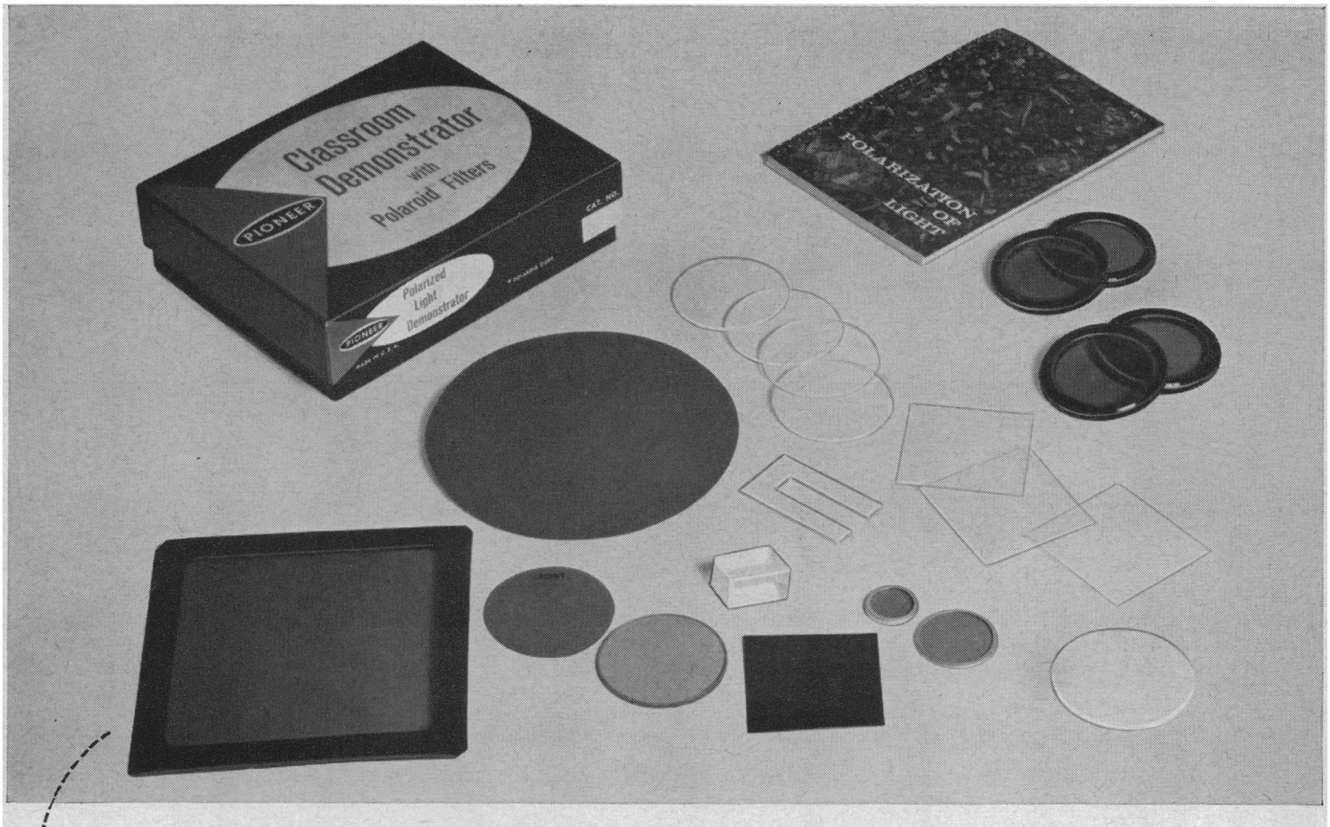

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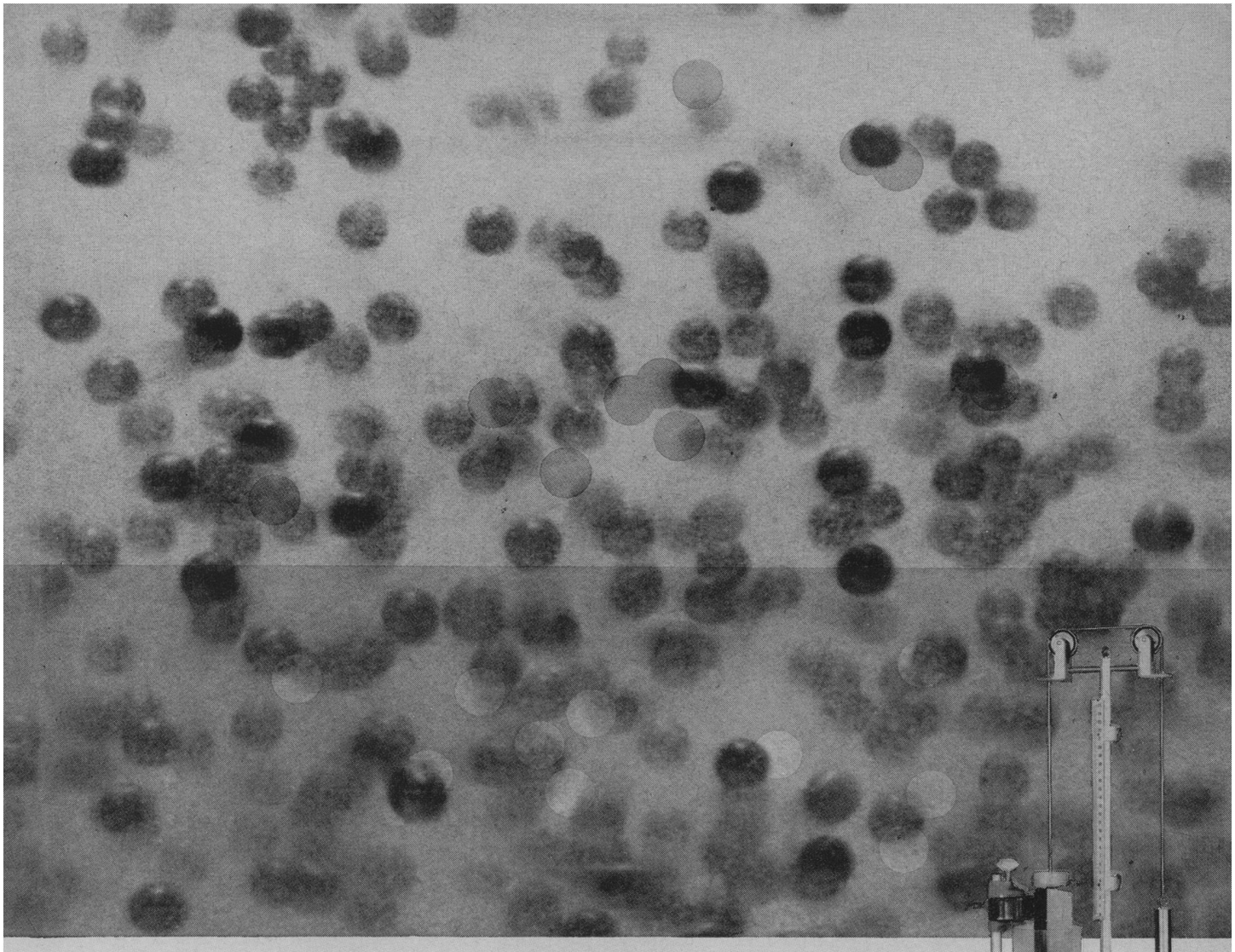

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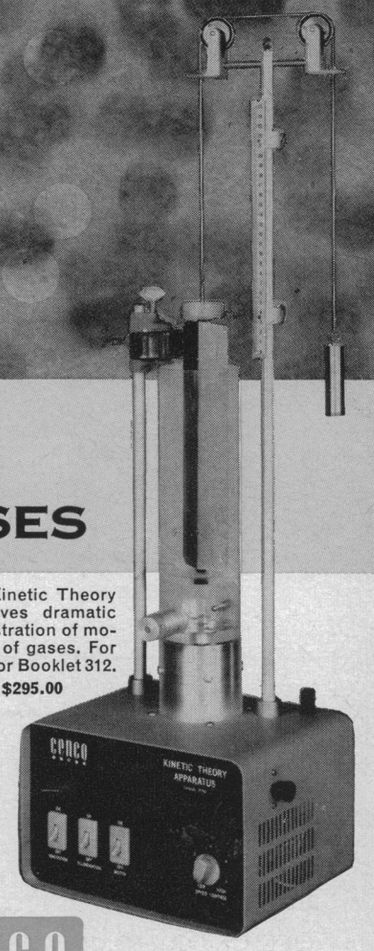

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Program Content

1. The two-session AAAS General Symposium, "Moving Frontiers of Science V"—Speakers: Edward Anders, H. W. Magoun, George Wald, and H. H. Goldstine; Thomas Park, presiding.
2. The "Challenge to Science" evening with Sir Charles P. Snow, Theodore M. Hesburgh, and W. O. Baker; Warren Weaver, presiding.
3. On "AAAS Day," the three broad, interdisciplinary symposia—Plasma: Fourth State of Matter; Life under Extreme Conditions; and Urban Renewal and Development, arranged by AAAS Sections jointly.
4. The Special Sessions: AAAS Presidential Address and Reception; Joint Address of Sigma Xi and Phi Beta Kappa by Polykarp Kusch; the Tau Beta Pi Address; National Geographic Society Illustrated Lecture; and the first George Sarton Memorial Address by René Dubos.
5. The programs of all 18 AAAS Sections (specialized symposia and contributed papers).
6. The programs of the national meetings of the American Astronomical Society, American Nature Study Society, American Society of Zoologists, History of Science Society, National Association of Biology Teachers, Scientific Research Society of America, Sigma Delta Epsilon, Society for General Systems Research, Society for the Study of Evolution, Society for the History of Technology, Society of Systematic Zoology, and the Society of the Sigma Xi.
7. The multi-sessioned special programs of the American Association of Clinical Chemists, American Astronautical Society, American Geophysical Union, American Physiological Society, American Psychiatric Association, American Society of Criminology, Association of American Geographers, Ecological Society of America, Mycological Society of America, National Science Teachers Association, New York Academy of Sciences—and still others, a total of some 90 participating organizations.
8. The four-session program of the Conference on Scientific Communication: The Sciences in Communist China, cosponsored by the AAAS, NSF, and ten societies.
9. The sessions of the Academy Conference, the Conference on Scientific Manpower, and the conference of the American Council on Women in Science.
10. The sessions of the AAAS Cooperative Committee on the Teaching of Science and Mathematics, and of the AAAS Committee on Science in the Promotion of Human Welfare.
11. Titles of the latest foreign and domestic scientific films to be shown in the AAAS Science Theatre.
12. Exhibitors in the 1960 Annual Exposition of Science and Industry—103 booths—and descriptions of their exhibits.

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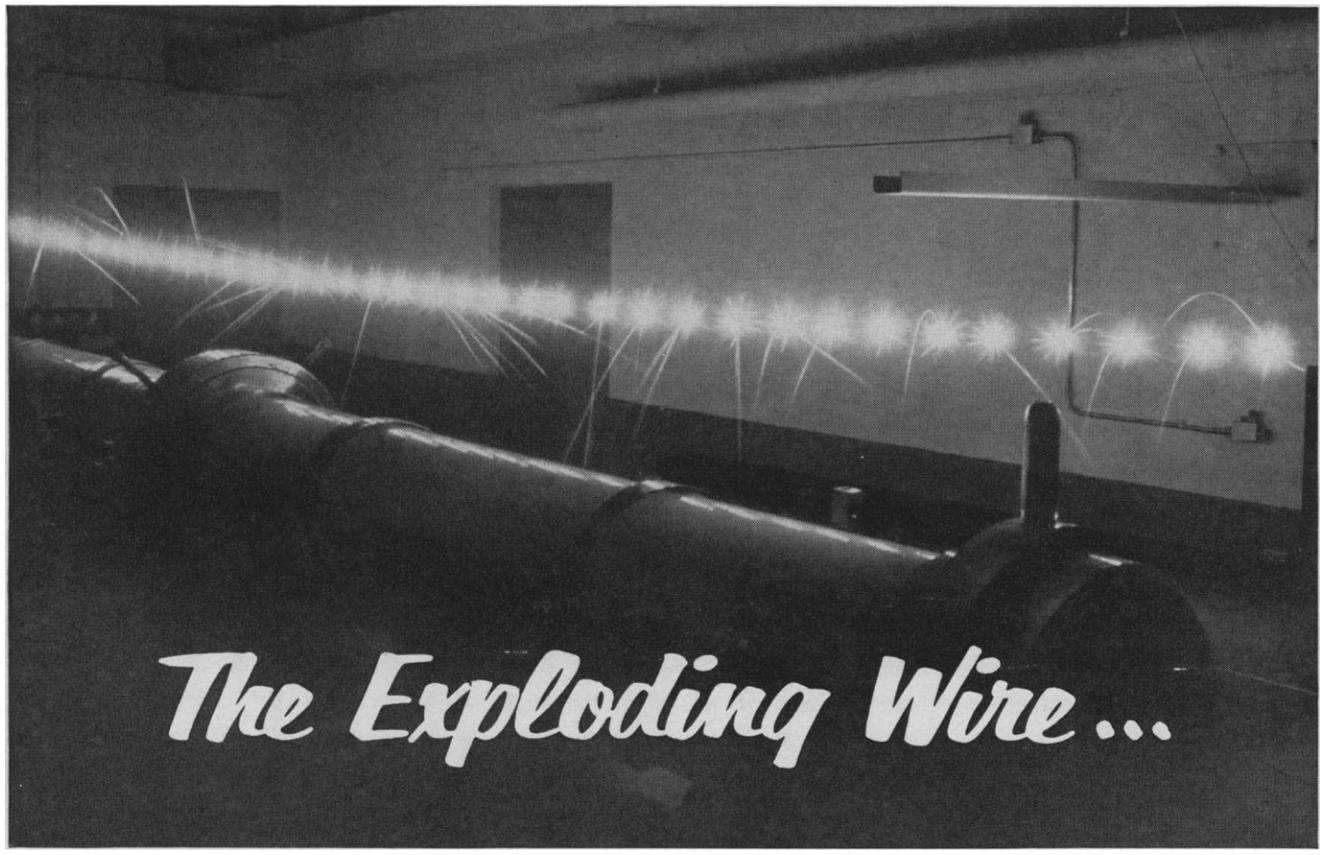

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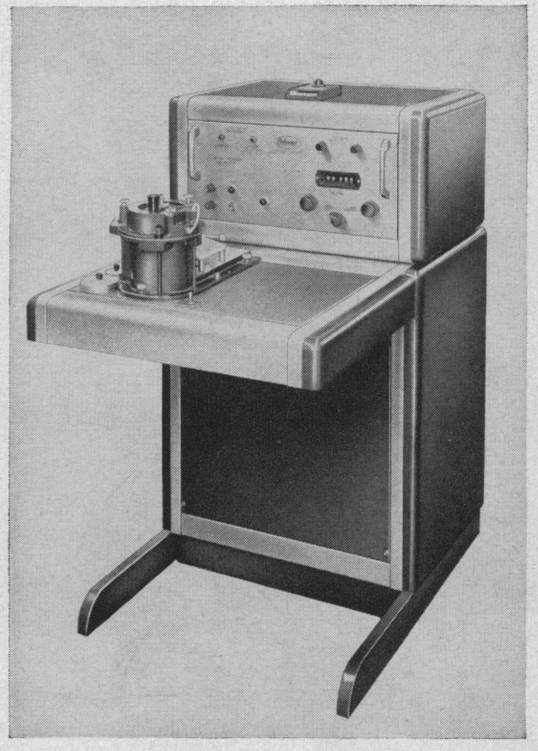

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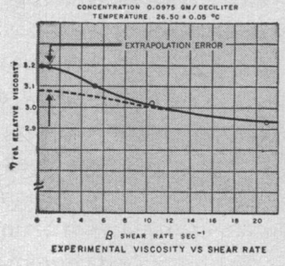

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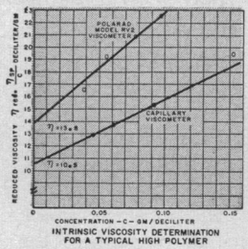

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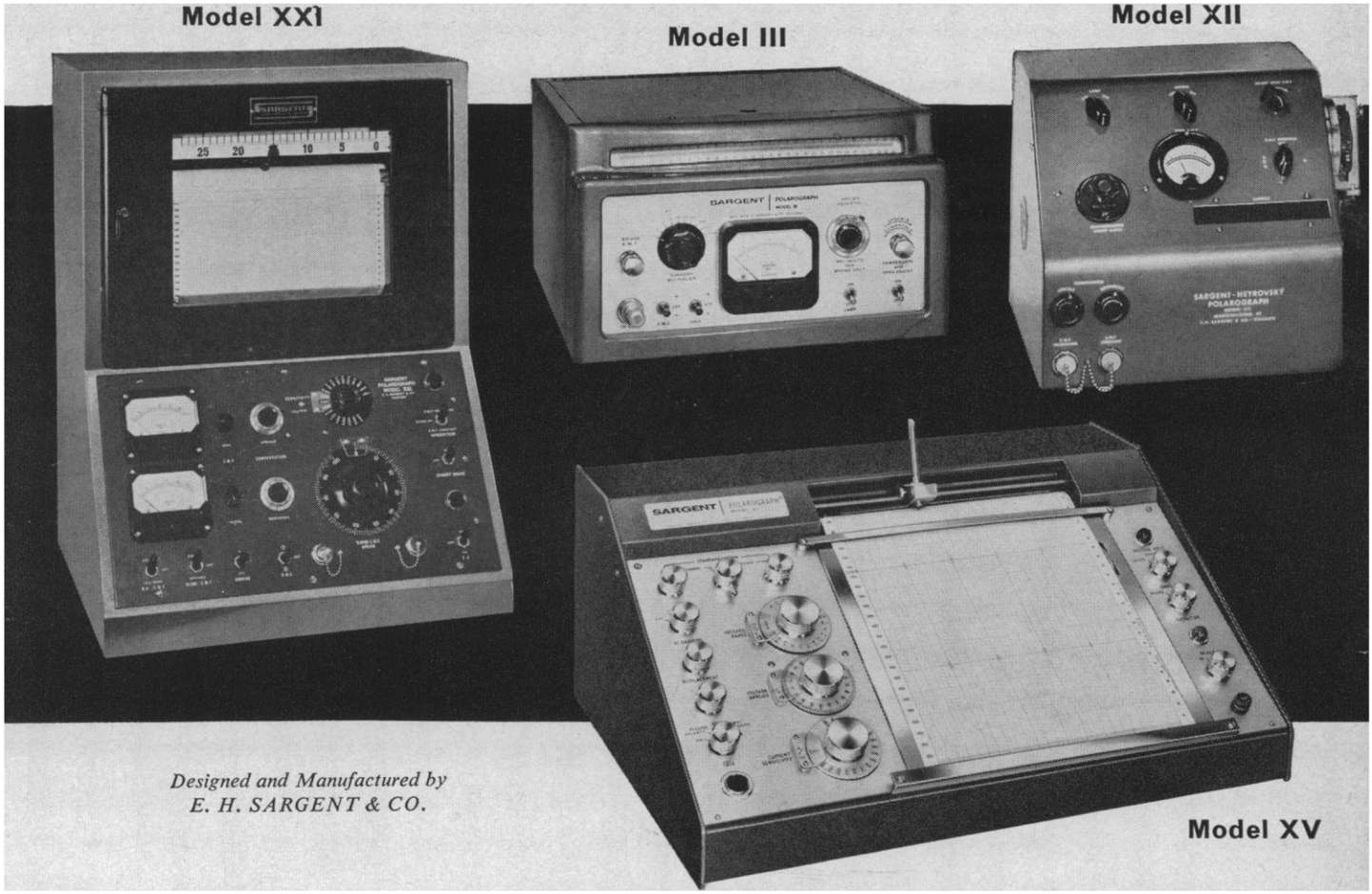

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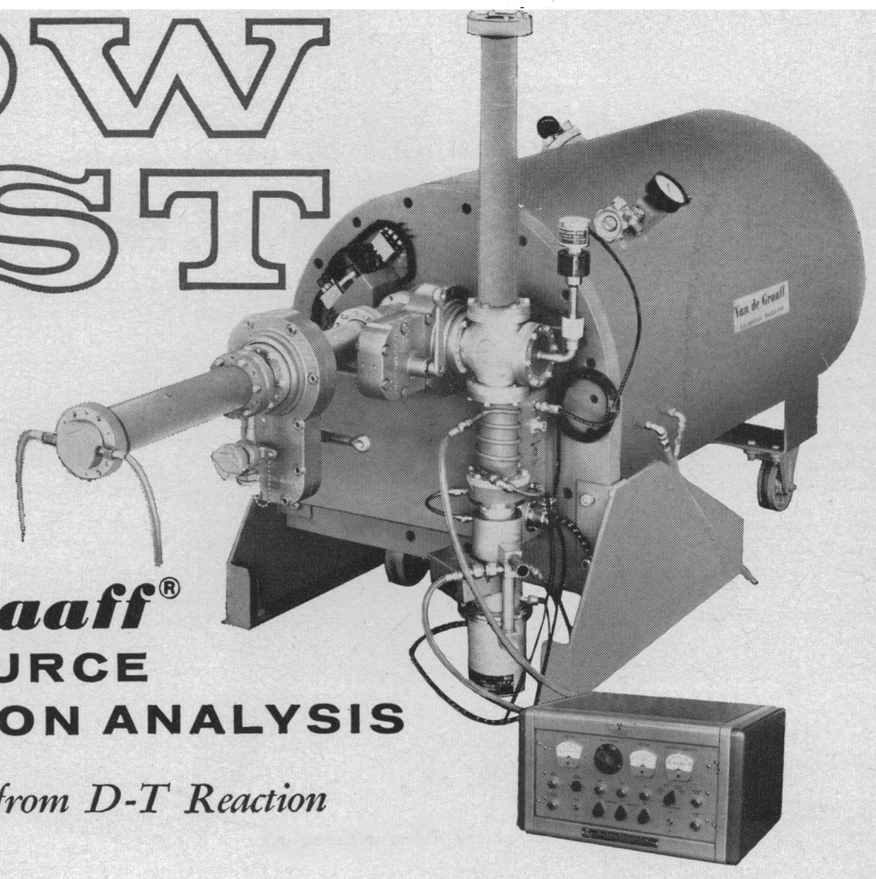

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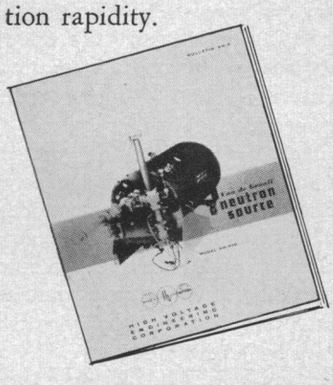

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11 November 1960, Volume 132, Number 3437 **SCIENCE**

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When Boys Will be Buys

John Hersey's new novel, *The Child Buyer* (Knopf), finds no cause for universal rejoicing in this country's growing interest in the nurture of high intelligence. A satirical comedy, in which unfolds a bud of horror, the novel pushes one aspect of this interest to its ultimate conclusion. A child buyer, Mr. Wissey Jones, comes to a small New England town to attempt to purchase for his corporation a ten-year-old boy, Barry Rudd, who gives promise of transcendent scientific ability. The purchase is to be part of the corporation's far-sighted efforts in behalf of national defense. The story is told in the form of testimony before a State Senate committee investigating the purchase, and this enables Hersey to get in some licks at the conduct of government hearings. In fact, many facets of present-day America, from TV to JD, come under scrutiny, but the focus of the attack is the education, and place in society, we accord the gifted.

That segment of America which is coming to regard intelligence as a national resource is only part of Hersey's grievance. The novel also has its quota of professional educators of the personal-adjustment philosophy, and examines what they have to say about Barry. In modern educational theory, as in those quotations that occasionally serve to fill out *The New Yorker's* columns, the straight doctrine is often its own satire. Only a punch line is needed. To the educationists, Barry Rudd, as a gifted child, falls into the same category as the mentally retarded and physically handicapped. He is exceptional, but modern theory has it that any child is exceptional who is not centrally located on the normal distribution curve. To miss the mark is to miss the mark, whether to the right or the left. A few persons, as Hersey's State specialist in deviants points out, are even exceptional twice over, like Lord Byron, who was both gifted *and* clubfooted.

Although the educationists regard Barry as exceptional, they do not believe he should be singled out for special attention. That would be undemocratic. His place is in the classroom with other future citizens of the same age. Wissey Jones, on the other hand, wants to give Barry special treatment, also in the name of democracy—its defense. But he does not see Barry as a person either, only as a piece of valuable property. And once in the hands of the corporation, Barry will be treated as property not only economically but physically as well. He will be engineered in such a way, including major surgical attention, that his innate abilities will be able to function unhampered by doubts or distractions. This kind of special preparation, of course, is expensive, but as testimony before the committee brings out, the facility where the work is done is classified as an institution of higher learning—Hack Sawyer University, named after the company's president—so that the program is tax-deductible.

Barry is not at the university yet, however. It is first necessary to get the consent of his parents and also of his teachers, a few of whom are sufficiently independent to regard the boy as exceptional in the old-fashioned sense. But Wissey Jones is something of a genius himself, at least in public relations, in persuading people to accept a point of view. For some of the opposition to the scheme, persuasion consists simply in presenting attractive but unusable gifts, like a matched set of lightweight luggage for a man with no occasion to travel. For others, making the reward fit the crime is more complex, and requires a little experimentation. Barry's consent also proves necessary, and the final note of horror hinges on Jones' efforts to get it. The book is funny, it is grim. It is a kind of antidote to that poison in our thinking which treats scientific activity—and persons—as components in a system geared to larger ends.—J.T.

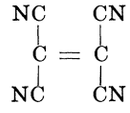
Kodak reports on:

a skyrocket from the Brandywine . . . a camera for a progressive Christmas . . . new black-and-white film speed numbers

Just carbon and nitrogen

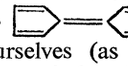
A family of Frenchmen ran a powder mill down on Brandywine Creek in Delaware. They attracted some smart chemists to work for them and branched out. The chemists attracted other chemists and they got quite a thing going.

On June 4, 1957, U. S. Patent No. 2,794,824 was granted to two of these chemists for the preparation of a compound called tetracyanoethylene, which contains only carbon and nitrogen.



Tetracyanoethylene stirred up attention from chemists all over, particularly after the inventors and their buddies let on that

1) though it survives 600° unchanged, when it catches fire in oxygen it burns hotter than does acetylene-oxygen;
2) it reacts with compounds like dimethylaniline to give a new class of intensely colored dyes, the tricyanovinyls, that might some day go commercial for hydrophobic fibers;
3) it can be converted to hexacyanoisobutylene, a dibasic acid so strong that in 0.4M solution it shows pH 1.7 (as compared with pH 1.8 for sulfuric at the same molarity), with a very stable ion containing nothing but carbon and nitrogen;
4) it is far speedier even than maleic anhydride at forming a ring with a diene, Diels-Alder-wise;
5) its solutions in various aromatic hydrocarbons are intensely and differently colored, each characteristic of the availability of electrons from the hydrocarbon.

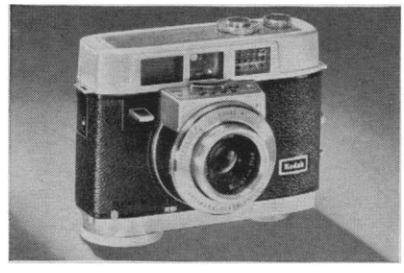
From across town here in Rochester came the idea of using that π-electron transfer to identify by color the spots on paper chromatograms of aromatic hydrocarbons instead of having to pull down the window shades for ultraviolet work. An oil company in Houston found that while these spots lost their color on heating, the spots from nitrogen-bearing aromatics after heating gave a new series of characteristic colors. At Yale, tetracyanoethylene captured and held for release at will the elusive compound fulvalene,  . So it went.

We ourselves (as this very page strives to remind you) were spawned by a snapshooting camera. One direction in which *we* branched out was the business of supplying organics for research and analytical use. Chemists asked us for tetracyanoethylene. We turned toward the Brandywine and asked permission to make a little under U. S. Patent No. 2,794,824. Back came a suggestion—that the Frenchmen's successors in interest make it and sell us some for resale in conveniently small quantities. Since this saved them and us and the rest of the chemical world a lot of bother, *Tetracyanoethylene* from Delaware is now obtainable as Eastman 7883. If you want to write them for general cyanocarbon information, we have their name and address around here somewhere.

Abstracts of the analytical procedures we can supply gratis. Likewise our latest catalog, Eastman Organic Chemicals, List No. 42. Tetracyanoethylene, *Eastman 7883, will cost you $2.65 for 1 gram. Any or all from Distillation Products Industries, Rochester 3, N. Y. (Division of Eastman Kodak Company).*

Inverse squares are square

Christmas is coming, a time when one can quit being obnoxiously logical about all things, a time of fun and acquisition for kin and self. Maybe the family needs a new camera. Cameras nowadays must be automatic. A non-automatic camera suggests lack of respect for progress.



The *Kodak Motormatic 35 Camera* is the most automatic of automatics. Energy for advancing the film and cocking the shutter for ten pictures is supplied from a spring wound in loading the camera. You can fire all ten in ten seconds, then rewind the spring and fire off ten more. Not only is the *f*/number photoelectrically set for shutter speeds from 1/40 to 1/250, but for flash the act of focusing adjusts the opening to the guide number. There is no need to trouble the little woman with the law of inverse squares.

Less than $110 at most Kodak dealers'. You can also get a generator flash holder for less than $14.

Too conservative

Only conservatives sit on committees that formulate standards for specifying film speeds. They set their lips in thin lines when they overhear enthusiasts trying to top each other with speed numbers. In the '40s they fixed the enthusiasts for fair. They defined the index so as to result in 2½ times the least exposure that even a stern judge would already have to accept as producing negatives of the highest pictorial quality.

They now confess that they leaned over backward too far for minimum grain and maximum sharpness from modern black-and-white films, modern practices, and modern equipment. The following speed numbers are derived from the revised American Standard:

KODAK PANCHROMATIC FILMS

Sheet Films	ASA
Panatomic-X	64
Portrait Panchromatic	125
Super-XX Panchromatic	200
Super Panchro-Press, Type B	250
Tri-X Panchromatic	400
Royal Pan	400
Royal-X Pan	1250
Roll Films	**ASA**
Panatomic-X	40
Verichrome Pan	125
Plus-X Pan Professional	160
Tri-X Pan	400
Royal-X Pan	1250
35mm Films	**ASA**
Panatomic-X	40
Plus-X Pan	160
Plus-X Portrait	160
Tri-X Pan	400

Note that only one figure is given for each film because most meter cells have about the same spectral response as panchromatic film. Two are needed for:

Kodak Blue-Sensitive and Orthochromatic Sheet Films	ASA Daylight	Tungsten
Commercial	50	16
Commercial Ortho	80	25
Super Speed Ortho Portrait	125	64
Royal Ortho	400	250

Prices subject to change without notice.

Kodak
TRADE MARK

This is another advertisement where Eastman Kodak Company probes at random for mutual interests and occasionally a little revenue from those whose work has something to do with science

sexual (Richard C. Robertiello, author of *Voyage from Lesbos*); psychopathology of the sex offender (Ralph Brancale, New Jersey State Diagnostic Center); the Fourth International Congress of Criminology—an analytic commentary (Hector Ritey, New York City).

Session II: "New Approaches to Continuing Problems in Crime Control," with Vernon Fox (Florida State University) as chairman, Clyde Vedder (Northern Illinois University) as discussion leader, and Lee Lawder (editor of *Law and Order*) as rapporteur; 26 Dec. Papers will be presented on problems in organizing and directing a citizens' crime commission (Alvin J. T. Zumbrun, Maryland Crime Investigating Committee); the nature of an interdisciplinary center approach to delinquency problems (Kenneth Kindelsperger, Youth Development Center, Syracuse University); justice through science—the role of the university in professionalizing law enforcement (Paul B. Weston, Sacramento State College); a killer without a motive (James A. Reinhardt, University of Nebraska); the White House Conference on Children and Youth—comment and criticism (Lois Higgins, Crime Prevention Bureau, Chicago; president, International Association of Women Police).

The American Society of Criminology will hold a business meeting on 26 Dec. with Marcel Frym as chairman. Reports will be given on the Second United Nations Congress on Crime and Delinquency (Marcel Frym) and on the Second International Meeting on Forensic Medicine and Pathology (Donal E. J. MacNamara, American Society of Criminology). Election of officers will follow these reports and a report of the planning chairman of the International Criminological Congress of 1961 (John Kenney, University of Southern California).

Session III: "Reaching the Anti-Social Gang," with Jacob Chwast (Educational Alliance) as chairman, Harris Peck (New York City) as discussion leader, and Canio L. Zarrilli (New York Institute of Criminology) as rapporteur; 27 Dec. Papers will be presented on social-work ethics and law enforcement—a dilemma (Russell Hogrefe, Chicago Youth Centers); the organization and behavior of violent gangs (Lewis Yablonsky, University of Massachusetts); reaching the fighting gang—New York's experience (Hugh Johnson, New York City Youth Board); a summer program in delinquency prevention and control (Arthur Clinton, New York City Board of Education); and the juvenile in detention—standards and problems (Sherwood Norman, National Institute of Crime and Delinquency).

Session IV: "Research and Theory in Criminology and Penology," with John Kenney as chairman, Herbert Block (Brooklyn College) as discussion leader, and Margaret Scott MacNamara (Brooklyn College) as rapporteur; 27 Dec. Papers will be presented on the Raiford study — alcohol and crime (Shaw Grigsby, University of Florida); conflicts and cooperation in criminology (Albert Morris, Boston University); containment theory — a new operational theory for criminologists (Walter Reckless, Ohio State University); criminal responsibility as a scientific concept—a crucial issue in criminology and corrections (Frank Hartung, Wayne State University); squirrel-cage penology—the rotary jail (Walter Lunden, Iowa State University of Science and Technology).

The Society will have its Annual Awards and Memorial Session on 27 Dec., with Donal E. J. MacNamara as chairman. The program will consist of a memorial address (Jerome Nathanson, Society for Ethical Culture); presentation of the 1960 Annual Award of the American Society of Criminology (Hon. James Bennett, U.S. Bureau of Prisons); an address by an award recipient (Thorsten Sellin, International Criminology Society); two presentations of the August Vollmer Award for Research in Criminology (Vernon Fox, Florida State University, and Marcel Frym); an address on patterns of criminal homicide (Marvin E. Wolfgang, University of Pennsylvania); an address on African homicide and suicide (Paul Bohannan, Northwestern University).

American Sociological Association. Invited papers, cosponsored by Section K: "Sociology of Science: Organization of Research," with Vincent H. Whitney, University of Pennsylvania, presiding; 28 Dec. Papers will be presented on organization and authority in the industrial research laboratory (Simon Marcson, Princeton University and Rutgers State University); organizational pressures and role strains in an industrial laboratory (William Evan, Bell Telephone Laboratories); research administration and the administrator—the U.S.S.R. and the U.S.A. (Norman Kaplan, Cornell University); industrial scientists and the values of science (David N. Solomon and Silvia Lamb, McGill University).

Invited papers, cosponsored by the Population Association of America and Section K: "Populations, Trends, and Policies in the Communist Countries," with Vincent H. Whitney presiding. Papers will be presented on population change and policy in Eastern Europe (Jerry W. Combs, Jr., Foreign Manpower Research Office, Bureau of the Census); demographic development of the Soviet Union (John F. Kantner,

Population Council); Communism, culture, and population—China as a case study (Irene Taeuber, Princeton University).

American Statistical Association. Invited papers, joint program of the Biometrics Section of the New York Chapter of the American Statistical Association, and Section K: "Hospital Statistics and Community Planning," arranged by Monroe Lerner, Health Information Foundation, with Paul M. Densen, Deputy Commissioner of Health, New York City, presiding; 29 Dec. Papers will be presented on the opportunities of operation (J. Douglas Colman, Associated Hospital Service of New York); the Massachusetts Hospital Study—collecting data through survey research (Paul Sheatsley, National Opinion Research Center, University of Chicago); hospital use in Indiana and in Saskatchewan by diagnosis—use of existing hospital data (Monroe Lerner); hospital data in community planning for the aged (Marta Frankel, New York City Department of Hospitals).

Invited papers, joint program of the Biometrics Section of the New York Chapter of the American Statistical Association and Section K: "Some Statistical Problems in Social Insurance Research," arranged by Abram J. Jaffe, Columbia University, and Nathan Morrison, New York State Department of Employment; 30 Dec.

Metric Association. Symposium: "The Current Interest in the Adoption of the Metric System," arranged by J. T. Johnson, president of the Metric Association; 27 Dec. After introductory remarks by J. T. Johnson, papers will be presented on the timeliness of the metric question (Hon. Lewis L. Strauss, former Secretary of Commerce); problems, progress and proposals for resolving some of the confusion on units in the U.S.A. (Carl F. Kayan, Columbia University); the evidences of current interests in the metric question here and abroad (Robert P. Fischelis, National Drug Trade Conference); what can and should be done for reviving activities in the Metric Association (K. E. Ettinger, Consulting Engineer, New York City); future plans for the Metric Association (Fred J. Helgren, Secretary-Treasurer, Metric Association). The symposium will be followed by an informal dutch-treat luncheon at which there will be continued discussion of the metric question. The luncheon is for those who are interested in the discussion, and especially for those interested in the Metric Association.

National Academy of Economics and Political Science. Invited papers, cosponsored by Pi Gamma Mu: "The Research Revolution and Public Policy," arranged by Amos E. Taylor, National Academy of Economics and Political Science, with John Green, U.S. Department of Commerce, presiding; 29 Dec. Papers will be presented on the research revolution and its economic implications (Leonard Silk, *Business Week,* McGraw-Hill Publishing Company) and on public policy and the intersectoral flow of funds for research and development in the United States (Jacob Perlman, National Science Foundation).

National Institute of Social and Behavioral Science. Cosponsor of the symposium of Section K, "Some Perspectives on Political Science and Science," 27 Dec. The Institute will also cosponsor Session V of the National Science Teachers Association, 30 Dec., in which there will be an address on biology of the mind by Irwin J. Kopin, National Institute of Mental Health, and a panel discussion (for details, see Section Q).

Social Science Research Council. Cosponsor of the AAAS symposium on the sciences in Communist China. Part IV, "Agriculture and the Social Sciences," will be of particular interest to social scientists. There will be a paper on scientists and the community (Theodore Chen, University of Southern California), a paper on organization and development of science (John M. H. Lindbeck, Harvard University), and a paper on anthropology, linguistics, and archeology (Francis L. K. Hsu, Northwestern University); 27 Dec.

Letters

Biological Organization

Ehret's interesting article [*Science* **132**, 115 (1960)] deserves some comment regarding the evidence for the evolution of cell structure. The classification of cells on the basis of envelope systems lacks phylogenetic soundness for it is necessary to place some viruses, bacteria, and actinomycetes in all envelope groups; this can certainly not be an accurate classification on the basis of their evolutionary position. The primary difficulty, however, lies in the proposition that bacteria and actinomycetes form a class of inner or single envelope organisms, the plasma membrane of which would correspond to the nuclear membrane of two envelope systems or advanced cell types. This would imply a mechanism of cellular evolution in which the cytoplasm and cell organelles form outside the protobiote (nucleus). It is well to note that all nuclear membranes, unlike the bacterial protoplast membrane, are double, being formed of sacs of endoplasmic reticulum which are organized and flattened against the phase which they surround. The bacterial protoplast contains those enzymes that are found in the cytoplasm but are absent from the nucleus of higher cell types.

Endoplasmic reticulum in higher cell types is frequently seen to be continuous with the plasma membrane, and the outer component of the perinuclear cisternae, with the endoplasmic reticulum. Indeed, in some fungi the membranes appear to be a continuous system all associated with the plasma membrane [*Exptl. Cell Research* **16**, 689 (1958)]. These continuities in some cases occur by means of connections which resemble circumferential, centripetal infoldings of the plasma membrane, which, instead of cutting the cell in two as in bacterial cell division, spread over the nucleoid as in the early stages of bacterial spore formation. This appears to be the only evidence for the evolutionary origins of the nuclear envelope at the present time, and it seems therefore to be the best. The envelope theory makes little sense when one attempts to apply it to this schema.

The formation of the endomembranes of higher cell types does, indeed, appear to have involved two steps: (i) membrane invagination as a modification of the cytocinetic mechanism, and (ii) a progression from obligate to facultative membrane continuity. If the envelope theory is applied to bacteria, then it would be the inner envelope rather than the outer which is missing. I would suggest abandoning

the envelope idea, because it is unrealistic from the phylogenetic point of view and relies upon assumptions which are unsupported.

I would submit that at present the best defensible hypothesis regarding the relationships among membranes is the following: "All cellular membranes are phylogenetically derived from the plasma membrane and share with it the capacity to become structurally differentiated by the specific association of proteins thereon in genetically determined patterns" [J. H. McAlear, thesis, Harvard University (1958)]. I would further submit that attempting to apply mathematical rigor to morphology at any level, at this time, must only result in a pseudosophisticated over-simplification. The study of the evolution of morphology is an inductive process and requires the accumulation of information of a comparative nature.

JAMES H. MCALEAR
Division of Laboratories and Research, New York State Department of Health, Albany

The article by C. F. Ehret entitled "Organelle systems and biological organization" expounds a view of the nature of living things that many of us thought had disappeared with the advent of evolutionary theory. It purports to be an account of certain entities obeying natural laws, termed "macromolecular aggregates" and "organelles." This is an inherently misleading approach to what are, in fact, biochemical and biophysical problems, because it deals with words from biological discourse as though they had the same kind of referents as similar words in physics.

There are, indeed, subatomic particles, atoms, and molecules which follow certain physical laws. A general class of larger things also exists—namely, the class of physical objects, obeying the laws of mechanics (relativistic or classical). The biological world does not contain classes of this variety, because its "components" have evolved according to their adaptive fitness to survive in various environments and not according to any internal "laws" of a paraphysical kind. Similarities between organelles, such as those mentioned by Ehret, demonstrate not a common law but a common origin. Likewise, the differences between bacteria and cellular organisms, about which Ehret makes such far-fetched topological play, demonstrate not two fundamentally opposed "patterns" derived from any natural law of cell construction but a more remote common origin.

One might also have thought that the "free-living nucleus" hypothesis—never a very likely one—would have

been scotched by recent work in bacterial biochemistry. But Ehret does not discuss biochemistry at all, because it does not fit into his formal framework. In his critique of the specification of organelles, by function, he claims (citing only a personal communication) that structurally typical but nonrespiratory mitochondria occur in a yeast mutant. He totally ignores all the most important work in this field since 1945, by such people as Chance, Lehninger, Green, Greville, Slater, Schneider, and many others, who show how close a relationship exists in the mitochondrion between structure and biochemical functioning. To say of such an organelle that its primary role is "niche-filling" or "space-filling" is either absurd or meaningless. In fact, the role of these structures seems to be to maintain the proper environments for certain reaction sequences, the structure being itself modified according to the reactions occurring within it [see, for example, L. Packer and A. L. Tappel, *J. Biol. Chem.* **234**, 525 (1960)]. Ehret's hypothesis of a role for such organelles concerned with "transmission, diffraction, oscillation [sic], and reception of electromagnetic energy" seems to be a deliberate attempt to confuse the issue as regards the well-founded chemical roles of such systems and also smacks of the aberrations of such pseudoscientific cults as "radionics."

In developing his peculiar *Weltanschauung*, Ehret makes various mistakes of fact and nomenclature. To refer to cilia from different organisms as "isotopes" is just wrong; a word exists to describe the relationship—namely, *homology*, which indicates a common evolutionary origin. To state that all cilia are homologous is to put forward an interesting theory about show. I find the meaning of his other statement on this issue, that "all particle sense. And what on earth are "non-atoms" (Fig. 7)?

His claim that "at the molecular level . . . the interacting particles . . . have relatively infinitesimal lifetimes" is untrue, as a glance at the half-lives for various types of cellular component, obtained by radioisotope labeling or other methods, would suffice to show. I find the meaning of his other statement on this issue, that "all particle concepts suffer from illusions of the reality of a static point," not entirely clear.

Ehret says that by his method, the "gap between the molecular and cellular levels is realistically bridged." It is difficult to see why the need for such a "bridge" was ever felt. The link between the physical and biological worlds is provided by the theory of evolution. "Nous n'avons pas besoin de ces autres hypothèses."

The editorial in the same issue of *Science* takes exception to a popular article about scientific things written in an anthropomorphic manner. In the circumstances, there is a temptation to cry "Tu quoque."

PETER NICHOLLS
Science Research Institute,
Oregon State College, Corvallis

McAlear's opening remarks regarding incongruity are based upon an erroneous notion about the relationships that are required between structural and phylogenetic categories. In taxonomy we frequently classify organisms with abbreviated life cycles on the basis of their larval stages, despite the absence of a terminal stage of development "normal" to their category; HeLa tissue cultures the world over remain generically *Homo* despite their structural simplicity and instability; each of us has arisen from a fertilized egg, structurally a microorganism but phylogenetically human; at the subcellular level, the transplanted frog nucleus remains phylogenetically an amphibian. But consider the interesting case of *Pneumococcus* transforming deoxyribonucleic acid, which retains only some of its host's phylogenetic

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character; these structural units are smaller than the minimal phylogenetic unit, which is sensibly an approximation of the organism's entire genome.

The well-known relationships between endoplasmic reticulum and nuclear membrane are not only consistent with but a part of my central thesis. We must, of course, eventually account for the specific structural differences between nuclear membranes and bacterial envelopes, as well as for those between different nuclear membranes and between different bacterial envelopes. The envelope-system method of classification simply places central emphasis upon the interesting topological properties (especially the inside-outside relationships) associated with the various known and possible forms of cellular organization. Numerous alternative speculations, including McAlear's, are compatible with it (no test of their soundness). From the evolutionary viewpoint, gene mutation and selection have produced the diversity of outer wrappers seen in bacteria and could easily account for such specific differences as are known between nuclear membranes and bacterial envelopes. Basic to this must be both gene-product differences and environmental phase differences: the nuclear membrane is at a karyoplasm-cytoplasm interface; the bacterial envelope, at a "karyoplasm"-substrate interface. In this regard, it should be interesting to compare the "karyoplasm"-substrate interface (normal envelope) with a "karyoplasm"-host cytoplasm interface in the case of facultative intracellular symbionts or parasites.

Regarding the evolution of cell structure, there is, of course, no direct evidence. The fossil record offers little help at the subcellular level, and all extant "living fossils," from coelacanths to protozoa, are 1960 models, most of which appear to be in an evolutionary cul-de-sac. From this point of view a word like *protozoa* is etymologically acceptable; in this sense and within the context of the discussion of envelope systems I have lumped bacteria and actinomycetes as "protobiotes." Whether they represent primitive or degenerative forms of life has been argued brilliantly, but also fruitlessly and *ad nauseam*, in a voluminous literature.

Irrespective of how any particular system has actually evolved, the presence in multienvelope systems of a unique ensemble of organelles unquestionably permits the development of cytoplasmic structures of greater diversity than is possible without them. This follows of logical necessity, as should be obvious if one considers the physical basis of diversity: for example, generically more structural forms are possible in a 100-atom than in a ten-atom system. The first appearance, then, of such a supramolecular unit as a cilium is surely to be regarded as a major innovation within the bios, and the subsequent capitalizations upon such units are indeed remarkable achievements in the evolution of metazoa.

In the history of science, logical rigor, though always difficult in virgin territory, has been indispensable for progress; it has invariably led to oversimplification, and as mortals the best we can hope for is an orderly progression from good oversimplifications to better ones.

It is commonplace to ridicule by unsupported generalizations that which one does not understand, and in both respects Nicholls has succeeded beautifully. In the spirit of substituting light for heat, I shall comment on the several issues he has raised.

1) The biological world certainly contains classes of components that obey the laws of mechanics. A finite number of sardines or of college students can be packed into a can or a telephone booth, respectively; gravity keeps most of us in our place, and modifications of the kinetic theory of gases have been applied meaningfully

to such component interactions as antigen-antibody agglutination, phage-bacterium adsorption, sperm-egg fertilization, and ciliate-ciliate mating.

2) With respect to similarities between organelles, there is no evidence that all cilia are derived from one common progenitor cilium. One of the important hypotheses expounded in my article is that of the *de novo* origin of such organelles by means of physically limited developmental processes from genetically limited sub-organellar-level macromolecular pools.

3) That the topological play is far-fetched is merely Nicholls' unsupported value judgment.

4) The reasons favoring the term *one-envelope system* over *free-living nucleus* are developed in the article. It is not clear to me from Nicholls' letter in what way he supposes them to be inconsistent with the recent work in bacterial biochemistry.

5) In addition to not citing some elegant works in biochemistry, I also elected to consider irrelevant to the crux of the argument much of the important contemporary work in sociology (people-people interrelations) and in nuclear physics (particle-strange particle interactions).

6) The concept of niche-filling is well understood in many professions. The tree cavity preempted by the starling will not be occupied at one and the same time by a flicker; an old French railway car will hold eight horses or 40 men; a simple keystone in an arch may be of marble or of brick or of wood, but not of all simultaneously. A more general concept of space-filling is appreciated by most physically minded people, including architects, city planners, and traffic managers. The important point is that, irrespective of the specific qualitative nature of the unit employed, fundamental geometrical and topological relationships are established between the units. It is at this level that we "intuitively" appreciate the shapes of things and many of the interrelationships in biology and society at large. In a rigorous analytical sense, we may evaluate a termite colony, a hardwood climax forest, an infantry regiment, or a political congress in session without once mentioning adenosine triphosphate or oxidative phosphorylation yet without underestimating their indispensability. In my article I have done this for the shapes of cells in terms of organelles. In other worlds, higher-level interrelationships might be formally similar to ours, but the molecular machinery might be quite different.

7) The possible role of organelles as receivers or transducers of electromagnetic energy was suggested from observations of their ultrastructure by

Fernández-Morán and seems inescapable from a knowledge of these objects. An important unanswered question is simply, "How much?"—a question provoked in the open mind by such knowledge.

8) The concept of organelle isotopy is structurally correct from both physical and etymological standpoints. Plant and animal, microorganismal and metazoan cilia have basically the same form. This similarity does not impose the limitation of common evolutionary origin on the structure itself in the developmental sense.

9) On or off the earth, "non-atoms" represent the subatomic particles. The term stems from the well-known philosophical device of *differentia and negative* and denotes a subset class that relates logically to the higher levels in the figure.

10) The lifetimes of such units as the enzyme-substrate complex are sufficiently short that ingenious instrumental measures, such as those conceived and brilliantly executed by Britton Chance, have been needed for their measurement. As pointed out in the article, the very organelles in or on which these fleeting events occur may be observed in vivo by cytologists at relative leisure. The qualification "relatively" was used by me deliberately and precisely to emphasize comparative lifetimes.

11) I cannot explain Nicholls' failure to find a need for a bridge or a link. A current key concern in physiological genetics and developmental embryology is the construction and testing of hypotheses that relate directly to the ascent from the macromolecular level that is experienced in the lifetime of every organism. If authoritarianism must be resorted to, it is well known that eminent experimentalists since Altmann and Hertwig have grappled with this problem along lines closely analogous to mine. That Nicholls' general accusation—that I have underestimated the contributions of biochemistry at the one extreme or of evolutionary theory at the other—is unfounded may be seen from a careful reading of my article and of the contextually relevant sections of the references cited.

CHARLES F. EHRET
Laboratoire de Biophysique, Université de Genève, Geneva, Switzerland

Dream Deprivation

William Dement tentatively concludes from an interesting study of "The effect of dream deprivation" [*Science* **131**, 1705 (1960)] that "a certain amount of dreaming each night is a necessity." A different interpretation of

the data, however, appears equally plausible. Dement's procedure, in brief, was as follows: to determine the subject's base-line total nightly dream time and sleep pattern, eye movements and brain waves were recorded during a variable number of undisturbed nights of sleep. During the subsequent nights the subjects were awakened each time the electromyographic (EMG) eye-leads indicated that they were dreaming (and the electroencephalographic [EEG] leads presumably indicated that they were in light sleep). Immediately after, when allowed a number of undisturbed nights of sleep ("recovery nights"), the EMG showed a significant elevation in the nightly dream time as compared with the base-line levels. This finding is open to the following interpretation: The psychological disturbance caused by the abrupt awakenings (as indicated by "anxiety, irritability . . . difficulty in concentrating . . . apparent panic") was closely correlated with a disturbance in the sleep pattern which included an increase in the amount of time spent in light sleep. Since evidence previously presented by Dement and Kleitman [*Electroencephalog. and Clin Neurophysiol.* **9**, 673 (1957)] strongly suggests that dreaming occurs during periods of light sleep, the "progressive increase in the number of attempts to dream" during the "dream deprivation" nights and the increased percentage of dream time during the "recovery nights" may have been a secondary consequence of the increase in light sleep time during these periods.

After a varying number of nights off, the same subjects returned to the experimental room and were awakened when the EMG indicated that they were *not* dreaming (and the EEG presumably indicated that they were in deep sleep). Subsequently, when permitted a number of undisturbed nights of sleep ("recovery nights"), the EMG did *not* show an increase in the amount of dream time over the base-line levels. This finding can be interpreted as follows: By this time the subjects had become adjusted to the procedure. Since they were now habituated and undisturbed, they slept normally; consequently, as compared with the base-line levels, they did not show a significant change in the percentage of light sleep time or the percentage of dream time during the "recovery nights" (or during the preceding nondream awakening nights).

It would be inappropriate to accept either Dement's interpretation or the above interpretation until a second experiment is completed in which subjects are *first* awakened when the EMG indicates that they are *not* dreaming and, after an intervening "recovery" period, are awakened when the EMG indicates that they are dreaming. Such an experiment may give the opposite results of those reported by Dement: Subjects may show an increased amount of dreaming during "recovery nights" which follow nondream awakenings and no significant increase in the amount of dreaming during "recovery nights" which follow "dream deprivation."

THEODORE XENOPHON BARBER
Worcester Foundation for Experimental Biology and *Medfield State Hospital, Harding, Massachusetts*

I wish to comment briefly on William Dement's tentative interpretations of the effects of his experimental studies in dream deprivation. Over two decades ago Nathaniel Kleitman put forth the notion of a subcortical wakefulness center which influenced the course of events in the sleep-wakefulness cycle. The more recent studies on the reticular activation system appear to have borne out some of these earlier ideas. When these data are considered in connection with the elucidation of the existence of the corticofugal fibers linking the cortex with the reticular system, and thereby creating a reverberating circuit, we have the beginnings of a neurophysiological model elucidating the dreaming process. We know as a result of the recent studies of Kleitman and his associates that repetitive bouts of partial arousal associated with dreaming occur universally as a nightly process. At certain times during the night it would appear as if the threshold in the reticular system is low enough for partial cortical arousal to occur. The events associated with this are experienced subjectively as the dream; but objectively these events further influence the threshold in this system. It is in this manner that it has been suggested that the cortex participates in its own arousal.

Freud had no neurophysiological model to serve as a guide in evolving his own dream theory. He was forced to see the dream as psychologically inspired by a disturbing unconscious impulse and as taking shape through the mechanism of dream work oriented towards disguising the impulse and maintaining sleep. The neurophysiological model, by comparison, suggests that the level of arousal varies during the night in a rhythmically patterned manner and that states of partial arousal are associated with more or less extended periods of dreaming.

Dement's study is concerned with the question of what happens when these events are interfered with in a specific way, namely, by eliminating that aspect of the cycle associated with dreaming. Both direct effects relating to dreaming and indirect, more distant effects relating to generalized behavioral disturbances were noted.

With the above considerations in mind, one can then move in two pos-

sible directions in evaluating the direct results of dream deprivation experiments. One can minimize all factors other than the occurrence of dreaming and thus focus mainly on the subjective aspects of the experience. One then begins to talk as Dement does: "It is as though a pressure to dream builds up . . ." and ". . . a certain amount of dreaming each night is a necessity."

A second approach would emphasize the fact that the experimenter is interfering with states of partial arousal which presumably may have significance for the organism apart from dreaming. If these states of repetitive partial arousal are, for example, associated with vigilance operations during sleep, then the direct effects noted —namely, the increased frequency of dream attempts on the experimental nights and the increased nightly dream time on the recovery nights—may represent states of heightened vigilance of which dreaming is but one manifestation.

MONTAGUE ULLMAN
46 East 73 Street, New York

Theodore Barber's alternative interpretation of the results is based mainly on his perception of a methodological weakness in the experimental procedure. He feels that the multiple awakenings, in and of themselves, could have been responsible for the rise in dream time seen on recovery nights after all, because, as he points out, the control awakening series always followed the dream deprivation series and "by this time the subjects had become adjusted to the procedure." He feels that if the control awakening series had been carried out *before* the dream deprivation awakening series, one might have seen a dream time rise following the control awakenings and little or none following dream deprivation.

Although we did not consider such a possibility "equally plausible" at the time the article was in preparation, it was obvious that it could not be entirely ruled out. Happily, there is no need now for an exhaustive documentation of the reasons for our original judgment. We have recently concluded experiments along the line independently suggested by Barber in his letter; and we have found that a second dream deprivation series in the same subject is as effective as the first, that no habituation is apparent, and that when the control series is carried out first, a dream time rise still follows the later dream deprivation series.

There are, however, several points in Barber's letter that warrant further comment. First, one of his contentions seems to be that abrupt awakenings caused the observed psychological disturbances. In all our work in this area,

one thing that has continued to impress us is how well subjects tolerate various disturbances of their sleep. When we were waking subjects many times a night to elicit dream recall in our early studies, we never observed any sort of psychological disturbance. To be sure, until the dream deprivation experiments were made, we had never done 20 to 30 awakenings in a single night. However, with the support of our more recent experiments, we are still inclined to view the psychological disturbances reported in the article as a *secondary* consequence of the reduction in dream time and not as a primary result of awakenings per se.

Second, taking his cue no doubt from my own statement in the article which referred to earlier work, "the eye movement periods were observed to occur regularly throughout the night in association with the lightest phases of a cyclic variation in depth of sleep as measured by the electroencephalogram," Barber uses the term, *light sleep*, saying, "the increased percentage of dream time during the recovery nights may have been a secondary consequence of the increase in light sleep time during these periods." It should be made very clear that what is meant by "light sleep" in this context can only be the so-called rapid eye movement period. The term, *eye movement period*, refers to a discrete interval of sleep during which rapid eye movements are seen in association with a characteristic low voltage, nonspindling EEG pattern (called stage 1 by us) which persists unchanged throughout the entire period although the amount of the eye movement may vary widely. Dreaming, as shown by earlier work, very likely occurs throughout the entire eye movement period, its presence being specifically indicated by the appearance of the stage 1 EEG pattern. The rapid eye movements are also related to dreaming, but more precisely to the visual imagery involved, and accordingly may vary in quantity from virtually none to many depending on what the dreamer is doing in the dream. Thus, for all practical purposes, the terms *eye movement period, stage 1 period, dream period*, and Barber's *light sleep* are synonymous. It should now be apparent that one cannot say, as Barber does, that increased dream time is a secondary consequence of an increase in light sleep time. The light sleep (stage 1 EEG) stage is simply the physiological concomitant of dreaming. Until more is known about mind-brain interaction, it might just as well be said that the increase in light sleep time was a secondary consequence of the increase in dream time.

Perhaps Barber had some other attributes in mind when speaking of light sleep which made his proposition seem

more reasonable. However, these should be specified because light sleep (or deep sleep) is a concept that can have objective meaning only in terms of the variable or variables used to describe it. One may speak of light sleep in terms of a low voltage EEG (as we have), or a low arousal threshold, or a high incidence of body movement, or a high heart rate, and so forth. Unfortunately for the concept, the various measures of lightness of sleep do not correlate well with each other. For example, the incidence of gross body movement is lower during stage 1 (dream) sleep than during adjacent periods of stage 2 (deeper) sleep. I personally feel that if depth of sleep must be measured, the most meaningful criterion is the arousal threshold. Even in this instance dreaming fails to qualify as light sleep. Current work in several laboratories seems to indicate that subjects are often more difficult to arouse while dreaming than at certain other times. The use of the EEG, or any other variable, as *the* criterion of depth of sleep is merely a matter of taste or convenience, and probably depends to some extent on how well the chosen variable fits the experimenter's personal subjective notion of this elusive characteristic.

A final point is that the eye movement potentials are not muscle potentials and hence the eye movement record is not an EMG (electromyogram). There is a more or less steady potential difference existing across the eyeball (cornea positive–retina negative). The eye movement potential is a consequence of the spatial change of the electrical field of the eyeball dipoles with reference to the fixed periorbital electrodes. Passive movement of the eyes will elicit such a potential with reference to fixed recording points, and retinal destruction will abolish it. There is no evidence of EMG potentials in the recordings that we routinely obtain, presumably because the extraocular muscles are too far from the recording electrodes.

Some of the above considerations may also apply to Ullman's comments. He refers to the dream periods as states of "partial arousal" perhaps "associated with vigilance operations during sleep." It seems to me that the term, *partial arousal*, is as ambiguous as the term *light sleep*. If by "partial arousal" is meant only the occurrence of the stage 1 EEG pattern, then the term is redundant. If the state of partial arousal is thought to have some other attribute (for example, we might logically assume it would include a lowered arousal threshold), again, we must ask, is this additional attribute in fact associated with the stage 1 EEG period and dreaming?

The periodic occurrence of a low voltage, fast EEG stage during normal sleep has also been observed in cats by myself [*Electroencephalog. and Clin. Neurophysiol.* **10**, 291 (1958)] and Jouvet *et al.* [*Compt. rend. soc. biol.* **153**, 1024 (1959)]. Since we can do no more than speculate about dreaming in cats, and since it seems unlikely that cats dream in the manner of humans, we might assume that the occurrence of a low voltage, fast EEG stage in this animal is a pure example of a vigilance operation. It might follow that during these periods the cat is more alert to various stimuli, and that the frequent occurrence of such periods during sleep greatly enhance his chances for survival. However, even here, where the issue of dreaming does not complicate the picture, the cat is, if anything, less vigilant. Jouvet reports a definite *increase* in arousal threshold during these low voltage periods as opposed to the slow wave stage of sleep, and, in a less refined study, I found that it was difficult to differentiate the two phases in terms of arousal threshold. I think that Ullman must recast his definition of vigilance with an eye to some of the more recent findings regarding the functional significance of the various EEG patterns during sleep. I think that he might also ask himself how much of the current neurophysiology of the brain stem really applies to the physiology of sleep in a more than speculative way, especially since there are still large gaps in a purely descriptive picture of sleep physiology.

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Craftsmen and Physicists

In his paper "Dons or crooners?" [*Science* **131**, 1165 (1960)], Eric Ashby gives some examples of "craftsmanship in science" to illustrate how popularization of science for adults can be furthered. Taking the development of microscopes and microscopy as an example, he states: "Finally, in 1886 technicians succeeded in making lenses out of new sorts of glass. . . . About the same time it was discovered that if the light rays between the object and the lens do not pass through air but pass, instead, through a drop of liquid which has optical properties similar to those of glass, then larger and clearer magnifications can be obtained. These lenses were called apochromatic. . . ."

This statement contains several errors. First, it may give people the incorrect impression that apochromats were produced empirically (by craftsmen), whereas it ought to be well known that they were designed entirely mathematically by the famous professor

Ernst Abbe after he had induced the firm of Schott, in Jena, to produce the new sorts of glass which he had asked for on theoretical grounds. The grinding and mounting of the lenses is indeed craftsmanship; the design of the apochromats is not craftsmanship but applied science. Further, Ashby confuses the principle of the immersion objective with that of apochromatic correction. These are, of course, entirely independent. The principle of oil immersion objectives dates back to Amici (1784–1863), although his *Papaver* oil immersion system was not a really homogeneous immersion like Abbe's (in fact most modern oil immersion objectives are not truly homogeneous immersions either).

Later on in the article it is suggested that the detection of chromosomes was made possible by the invention of the apochromats, whereas in reality this was due entirely to improvements in microscope technique. In properly prepared sections or squash preparations chromosomes can be detected easily with objectives of considerably older design than the first apochromats.

While agreeing completely as to the need for popularization of science in the way Ashby proposes, I think it should be emphasized that the facts presented must be true.

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I am familiar with the facts van Duijn mentions, but I do not follow his reasoning when he implies that because of these facts the statement of mine which he quotes "contains several errors." Without craftsmen there would have been no apochromatic lenses. It is a matter of judgment whether the improvement of microscopes in the 19th century owed more to craftsmen than to physicists. I think it did. Van Duijn thinks otherwise. But our differences are matters of the interpretation of history. They do not justify either of us in accusing the other of error.

Similarly with homogeneous immersion lenses: I believe that the fundamental advance was made by Abbe. And as for van Duijn's assertion that the observation of chromosomes was due "entirely" to improvements in techniques for staining and mounting cells, I readily agree that these techniques were of critical importance, but I wonder whether he has tried to draw the morphology of a dividing nucleus under a pre-apochromatic microscope. I have, and I stick to my statement.

ERIC ASHBY
Clare College, Cambridge, England

Meetings

Friends of the Pleistocene

The 23rd field conference of the eastern division of the Friends of the Pleistocene was held in western New York on 20–22 May. Approximately 125 geomorphologists, glacial geologists, ground-water geologists, pedologists, and palynologists from Ontario, Quebec, the New England and Middle Atlantic states, Ohio, Indiana, Michigan, South Carolina, and Louisiana participated in this sixth reunion to be held in New York state. Leader of the conference was Ernest H. Muller of Syracuse University, whose investigations in Chautauqua, Cattaraugus, and adjacent counties have been sponsored by the New York State Museum and Science Service since 1956. Clair Brown of Louisiana State University, William S. Benninghoff of the University of Michigan, and Edward Ketchledge of the New York State College of Forestry at Syracuse University contributed paleobotanical data.

The group gathered at the Dunkirk Conference Grounds, Dunkirk, N.Y., on 20 May. The next day they traveled by automobile caravan southeast from Dunkirk through Gowanda and Otto to Olean. Leaving the Erie lake plain in the morning, the convoy traversed progressively older terrain from the Warren and Whittlesey strands, across late Cary moraines of the plateau margin, to early Wisconsin features of the Olean drift border. The second day's travel was westward from Olean to Salamanca along the drift border northeast of the Salamanca re-entrant.

Much of the first day was devoted to problems of glacial stratigraphy. Applicability of mechanical analysis as a criterion for till correlation was discussed at the first stop, on the outermost of the late Cary moraines southwest of Dayton. This moraine is composed of Hiram till and tentatively correlated with the Defiance moraine of Ohio and northwestern Pennsylvania. Eastward in New York the matrices of correlative upland and valley bottom till samples may differ more than the tills of contrasting moraines, and the criterion so far is not applicable. The question of how to distinguish between clay derived from comminution of shale and that from assimilation of lake sediments was discussed, without satisfactory conclusion.

At the Gowanda high bluff and Gowanda Hospital sites, four very similar tills are separated by lake silts. Discussion centered around criteria for evaluating the deglaciation represented by each set of nonglacial deposits of an oscillatory ice margin fronting on deep pro-glacial lake waters.

Deposits of post-Sangamon, pre-Farmdale age were studied at the Gowanda Hospital site and at the classic Otto site described previously by MacClintock and Apfel. At Gowanda a distinctive pink silt till, similar in color to till of the Ontario lake plain but unlike anything else in the immediate vicinity, underlies crumpled and deformed flood-plain deposits containing sparse invertebrate tests, *Picea* and *Pinus* pollen, and wood dated as older than 38,000 years (sample W-866, U.S. Geological Survey). At Otto, more abundant organic remains dated as older than 52,000 years (sample Gro 2565, Groningen Laboratory) represent the southern aspect of the boreal forest and are suggestive of climatic conditions slightly more rigorous than present conditions rather than of the mild conditions of the Sangamon to which they had previously been assigned.

Considerable attention was devoted also to periglacial and drift border conditions. At The Narrows, the postulated divergence of the Binghamton and Olean drift borders was considered. Discussion arose as to whether stone nets near the Wisconsin glacial border and the Olean Rock City in the unglaci-

ated Salamanca re-entrant represent periglacial conditions or are in process of active development under the present environment. The upland peneplain was introduced as evidence of the relative ineffectiveness of glacial scour on summits near the limits of glaciation.

ERNEST H. MULLER
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Forthcoming Events

November

29–30. Air Research and Development Command, Science and Engineering Symp., 7th annual, Boston, Mass. (Office of information, Headquarters, ARDC (Attention: RDEP), Andrews AFB, Washington 25)

30–2. Steels in Reactor Pressure Circuits, symp., London, England. (Secretary, Iron and Steel Inst., 4 Grosvenor Gardens, London, S.W.1)

December

1–16. Commission for Climatology, 3rd session, London, England. (World Meteorological Organization, Campagne Rigot, 1, avenue de la Paix, Geneva, Switzerland)

2–5. Central American Medical Conf., 8th, Panama City. (A. Bissot, Departamento de Saud Publica, Ministerio de Trabajo, Prevision Social y Salud Publica, Panama)

3–6. Visual Communications, 4th annual intern. cong., Chicago, Ill. (Visual Communications Cong., 10600 Puritan Ave., Detroit 38, Mich.)

3–8. American Acad. of Dermatology and Syphilology, Chicago, Ill. (R. R. Kierland, First National Bank Building, Rochester, Minn.)

4–6. Spectroscopy, annual southern seminar, Gainesville, Va. (Annual Seminar on Spectroscopy, Univ. of Florida, Gainesville)

4–7. American Inst. of Chemical Engineers, annual, Washington, D.C. (F. J. Van Antwerpen, AICE, 25 W. 45 St., New York 36)

4–9. Radiological Soc. of North America, Cincinnati, Ohio. (D. S. Childs, 713 E. Genesee St., Syracuse 2, N.Y.)

5–7. American Soc. of Agricultural Engineers, winter, Memphis, Tenn. (J. L. Butt, 420 Main St., St. Joseph, Mich.)

5–7. Electronic Industries Assoc., 3rd conf. on maintainability of electronic equipment, San Antonio, Tex. (E. B. Harwood, Office of the Secretary of Defense, Room 3D1018, Pentagon, Washington 25)

5–8. American Rocket Soc., 15th annual, Washington, D.C. (R. L. Hohl, ARS, 500 Fifth Ave., New York 36)

5–8. American Soc. of Agronomy, annual, Chicago, Ill. (L. G. Monthey, ASA, 2702 Monroe St., Madison 5, Wis.)

7–13. American Acad. of Optometry, San Francisco, Calif. (C. C. Koch, 1506–08 Foshay Tower, Minneapolis 2, Minn.)

9–10. The Myocardium—Its Biochemistry and Biophysics, New York, N.Y. (A. P. Fishman, New York Heart Assoc., 10 Columbus Circle, New York 19)

9–11. American Psychoanalytic Assoc., New York, N.Y. (D. Beres, 151 Central Park West, New York 23)

10–11. Academy of Psychoanalysis, New York, N.Y. (J. H. Merin, 125 E. 65 St., New York 21)

11–14. Hot Laboratory and Equipment Conf., 8th, San Francisco, Calif. (J. R. Lilienthal, Los Alamos Scientific Laboratory, P.O. Box 1663, Los Alamos, N.M.)

12–14. American Nuclear Soc. (Isotopes and Radiation Div.), San Francisco, Calif. (O. J. Du Temple, ANS, 86 E. Randolph St., Chicago 1, Ill.)

12–14. Water Pollution, natl. conf., Washington, D.C. (F. A. Butrico, Office of Engineering Resources, Div. of Engineering Services, U.S. Public Health Service, Washington 25)

12–16. Atomic Industrial Forum, conf., San Francisco, Calif. (D. J. Scherer, 3 E. 54 St., New York 22)

13–15. Eastern Joint Computer Conf., New York, N.Y. (E. C. Kubie, EJCC, Computer Usage Co., Inc., 18 E. 41 St., New York 17)

19–20. Statistical Mechanics, conf., London, England. (Organizing Secretary, Physical Soc., 1, Lowther Gardens, London)

22–2. Panamerican Diabetic Congress, 1st, British Honduras. (B. R. Hearst, Director, Diabetic Inst. of America, 55 E. Washington St., Suite 1646, Chicago 2, Ill.)

26–30. Inter-American Cong. of Psychology, 7th, Havana, Cuba. (G. M. Gilbert, Psychology Dept., Long Island Univ., Brooklyn 1, N.Y.)

26–31. American Assoc. for the Advancement of Science, annual, New York, N.Y. (R. L. Taylor, AAAS, 1515 Massachusetts Ave., NW, Washington 5)

The following 52 meetings are being held in conjunction with the AAAS annual meeting.

AAAS Committee on Science and the Promotion of Human Welfare (B. Commoner, Shaw School of Botany, Washington Univ., St. Louis 5, Mo.). 26, 28, 29 Dec.

AAAS Cooperative Committee on the Teaching of Science and Mathematics (J. R. Mayor, Director of Education, AAAS, Washington, D.C.). 28, 29 Dec.

Academy Conference (J. G. Arnold, Jr., Loyola Univ., New Orleans, La.). 26–27 Dec.

Alpha Epsilon Delta (M. L. Moore, 7 Brookside Circle, Bronxville, N.Y.). 29 Dec.

American Assoc. of Clinical Chemists (H. Goldenberg, Dept. of Biochemistry, Hillside Hospital, P.O. Box 38, Glen Oaks, N.Y.). 26–27 Dec.

American Assoc. of Scientific Workers (Miss M. Yevick, 214 Western Way, Princeton, N.J.). 27 Dec.

American Astronautical Soc. (R. Fleisig, 58 Kilburn Rd., Garden City, N.Y.). 27 Dec.

American Astronomical Soc. (J. A. Hynek, Dearborn Observatory, Northwestern Univ., Evanston, Ill.). 28–31 Dec.

American Council on Women in Science (Miss E. B. Thurmann, Div. of Research Grants, National Insts. of Health, Bethesda 14, Md.). 27 Dec.

American Economic Assoc. (K. E. Boulding, Dept. of Economics, Univ. of Michigan, Ann Arbor). 26 Dec.

American Geophysical Union (R. Jastrow, NASA Theoretical Div., 8719 Colesville Rd., Silver Spring, Md.). 26 Dec.

American Nature Study Soc. (R. E. Hopson, 4138 S.W. Fourth Ave., Portland 1, Ore.). 27–30 Dec.

American Psychiatric Assoc. (P. H. Knapp, Boston Univ. School of Medicine, Boston, Mass.). 29, 30 Dec.

American Soc. of Criminology (D. E. J. MacNamara, New York Inst. of Criminology, 115–117 W. 42 St., New York 36). 26, 27 Dec.

American Soc. of Naturalists (R. C. Rollins, Gray Herbarium, Harvard Univ., 22 Divinity Ave., Cambridge 38, Mass.). 27 Dec.

American Soc. of Zoologists (R. L. Watterson, Dept. of Zoology, Northwestern Univ., Evanston, Ill.). 28–30 Dec.

American Sociological Assoc. (V. H. Whitney, Dept. of Sociology, Wharton School of Finance, Univ. of Pennsylvania, Philadelphia, Pa.). 28, 29 Dec.

American Statistical Assoc. (R. E. Lewis, New York Area Chapter, 55 Wall St., New York 15). 29 Dec.

Association of American Geographers (C. Morrison, Jr., American Geographical Soc., Broadway at 156 St., New York 32). 27–30 Dec.

Association for Computing Machinery (W. F. Cahill, NASA, 8719 Colesville Rd., Silver Spring, Md.). 29 Dec.

Astronomical League (Miss A. A. Pindar, Amateur Astronomers Assoc., Inc., 223 W. 79 St., New York 24). 28 Dec.

Beta Beta Beta Biological Soc. (Mrs. F. G. Brooks, P.O. Box 515, Ansonia Station, New York 23). 27 Dec.

Biomedical Information Processing Organization (R. S. Ledley, Natl. Biomedical Research Foundation, Silver Spring, Md.). 30 Dec.

Committee on Cosmetics, American Medical Assoc. (J. B. Jerome, 535 N. Dearborn St., Chicago 10, Ill.). 29 Dec.

Conference on Scientific Communication Problems (G. L. Seielstad, Technical Reports Group, Applied Physics Laboratory, Johns Hopkins Univ., Silver Spring, Md.). 26, 27 Dec.

Conference on Scientific Manpower (T. J. Mills, Natl. Science Foundation, 1951 Constitution Ave., NW, Washington 25). 27 Dec.

Conference on Scientific Manuscripts (N. Reingold, Dept. of History of Science and Medicine, Yale Univ., New Haven, Conn.). 29 Dec.

Ecological Soc. of America (R. S. Miller, Dept. of Biology, Univ. of Saskatchewan, Saskatoon, Saskatchewan, Canada). 26–31 Dec.

History of Science Soc. (D. J. de Solla Price, Dept. of History of Science and Medicine, Yale Univ., New Haven, Conn.). 27–29 Dec.

Institute of Management Sciences (M. M. Flood, Mental Health Research Inst., 205 N. Forest Ave., Ann Arbor, Mich.). 30 Dec.

Metric Assoc. (J. T. Johnson, 694 W. 11 St., Claremont, Calif.). 27 Dec.

Mountain Lake Biological Station (H. H. Hobbs, Jr., Univ. of Virginia, Charlottesville, Va.). 29 Dec.

National Acad. of Economics and Political Science (A. E. Taylor, Parkton, Md.). 27 Dec.

National Assoc. of Biology Teachers (P. Webster, Bryan City Schools, Bryan, Ohio). 27–30 Dec.

National Assoc. for Research in Science

Teaching (G. Mallinson, School of Graduate Studies, Western Michigan Univ., Kalamazoo). 27 Dec.

National Assoc. of Science Writers (E. Ubell, Herald Tribune, New York, N.Y.). 27 Dec.

National Geographic Soc. (W. R. Gray, NGS, 16th and M Sts., NW, Washington 6). 30 Dec.

National Speleological Soc. (Brother Nicholas, FSC, Dept. of Biology, Univ. of Notre Dame, Notre Dame, Ind.). 27 Dec.

Nature Conservancy (J. W. Brainerd, Springfield College, Springfield, Mass.). 27 Dec.

New York Acad. of Sciences (D. Purpura, College of Physicians and Surgeons, Columbia Univ., New York, N.Y.). 30 Dec.

Science Clubs of America (Miss L. V. Watkins, Science Service, 1719 N Street, NW, Washington 6). 30 Dec.

Scientific Research Soc. of America (D. B. Prentice, 56 Hillhouse Ave., New Haven 11, Conn.). 29 Dec.

Sigma Delta Epsilon (Mrs. E. Cortelyou, Aeroprojects Inc., W. Chester, Pa.). 27–29 Dec.

Society for General Systems Research (C. A. McClelland, Dept. of History, San Francisco State College, 1600 Holloway Ave., San Francisco, Calif.). 29 Dec.

Society for the History of Technology (C. W. Condit, Dept. of English, Northwestern Univ., Evanston, Ill.). 27–29 Dec.

Society for Industrial and Applied Mathematics (J. Griesmer, IBM Research Center, Box 218, Yorktown Heights, N.Y.). 28 Dec.

Society for Industrial Microbiology (J. A. Ramp, 11 Van Dyke Rd., Waldwick, N.J.).

Society of the Sigma Xi (T. T. Holme, 56 Hillhouse Ave., New Haven 11, Conn.). 29 Dec.

Society for the Study of Evolution (H. H. Ross, State Natural History Survey, Urbana, Ill.). 27–29 Dec.

Society of Systematic Zoology (C. F. Lytle, Dept. of Zoology, Tulane Univ., New Orleans 18, La.). 27–29 Dec.

Tau Beta Pi Assoc. (R. H. Nagel, Tau Beta Pi Assoc., Univ. of Tennessee, Knoxville). 29 Dec.

Torrey Botanical Club (Miss A. Hervey, New York Botanical Garden, Bronx Park 56, N.Y.). 27 Dec.

27–14. Bahamas Surgical Conf., Nassau. (B. L. Frank, P.O. Box 4037, Fort Lauderdale, Fla.)

27–29. Conference on Strong Interactions, Berkeley, Calif. (A. C. Helmholz, Dept. of Physics, Univ. of California, Berkeley.)

27–29. Northwest Scientific Assoc. and Idaho Acad. of Science, joint meeting, Moscow. (E. J. Larrison, Dept. of Biological Sciences, Univ. of Idaho, Moscow.)

28. Association for Education in International Business, St. Louis, Mo. (J. N. Behrman, Univ. of Delaware, Newark, Delaware)

28–30. American Economic Assoc., St. Louis, Mo. (J. W. Bell, Northwestern Univ., Evanston, Ill.)

28–30. Econometric Soc., St. Louis, Mo. (R. Ruggles, Dept. of Economics, Yale Univ., New Haven, Conn.)

(See issue of 21 October for comprehensive list)