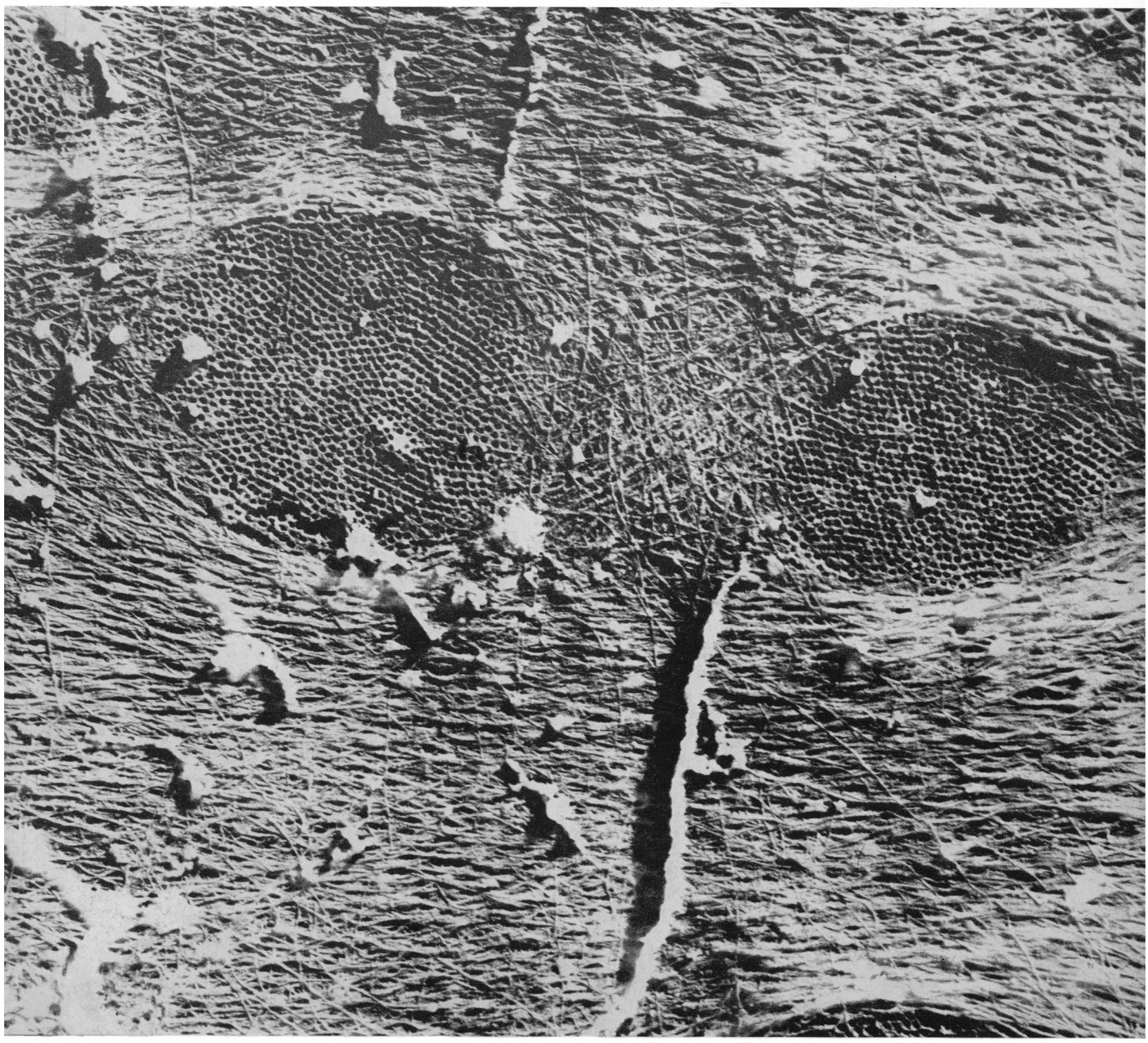
SCIENCE

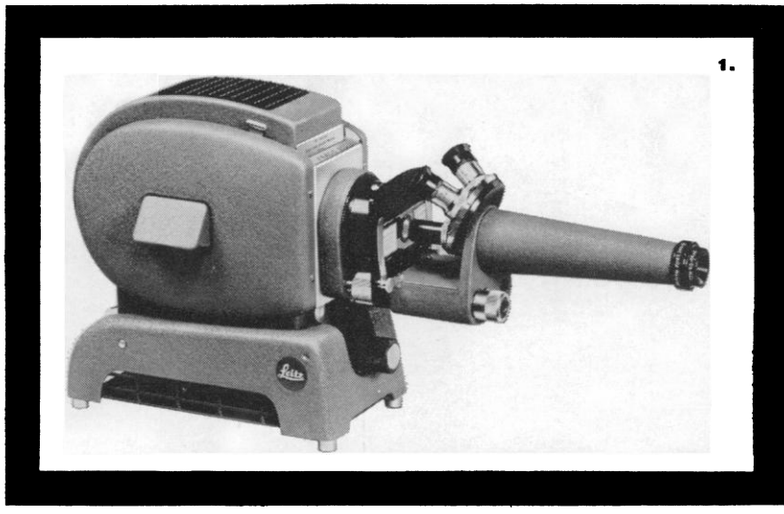
2 December 1960

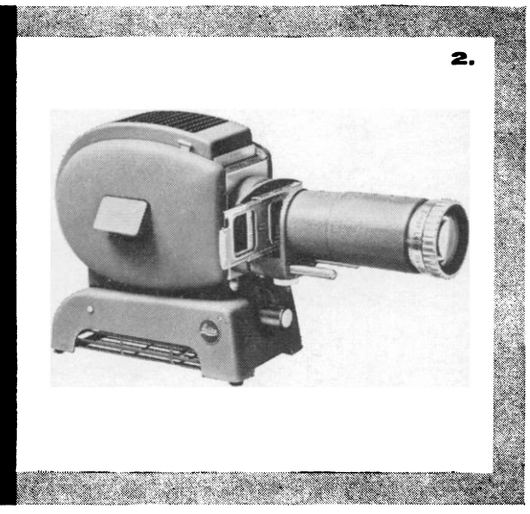
Vol. 132, No. 3440

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE



Preconvention Issue





switch in seconds { *from projection of microscope slides to projection of transparencies with the*

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The ultimate in convenience for use in classroom, conference room or lecture hall, the PRADO Micro-Projector delivers screen images of unrivalled brilliance, clarity and definition. With Micro Attachment it produces magnifications up to 2400x on screens at a distance up to 40 feet; with a film slide carrier and lens inserted, the PRADO projects 2″ x 2″ or of 2¾″ x 2¾″ transparencies. And you can switch from micro to film slides in seconds —easily. The revolving nosepiece of the Micro Attachment holds three objectives: 3.5x, 10x and 25x. The high power objective is equipped with spring-loaded mount. Micro attachments are available which allow the stage to be placed in a horizontal position to accommodate wet mounts.

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The PRADO Micro-Projector is portable, and may be carried easily from room to room and used wherever there is an electrical outlet.

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1. PRADO as Micro-Projector for microscopic slides **2.** PRADO as standard projector for film slides **3.** Attachable reflecting prism for tracings **4.** Polarizing Attachment for polarization demonstration **5.** Large Vertical Micro Attachment for wet mounts

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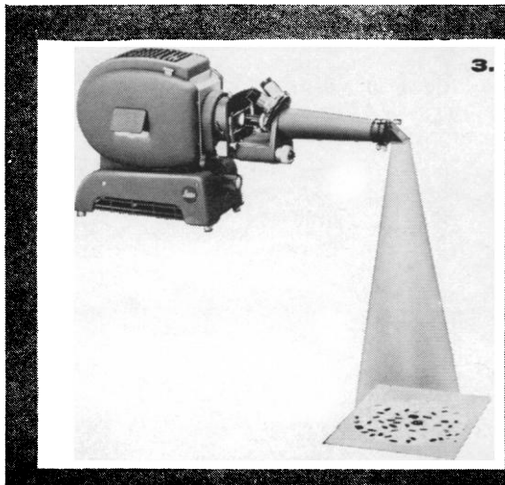

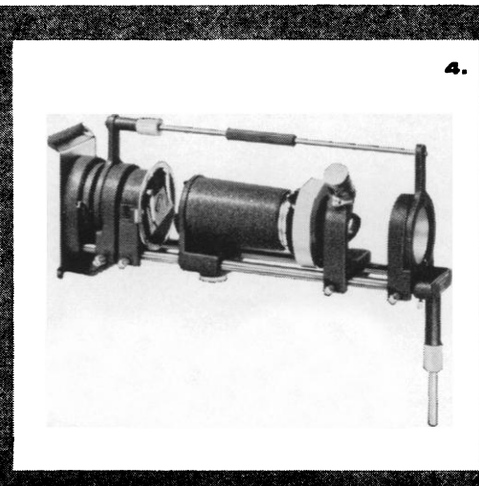

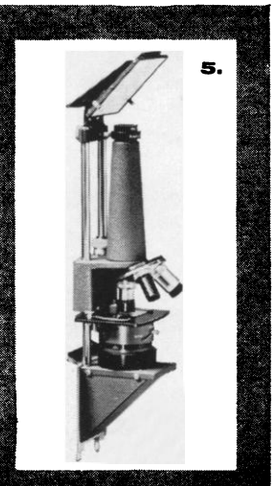

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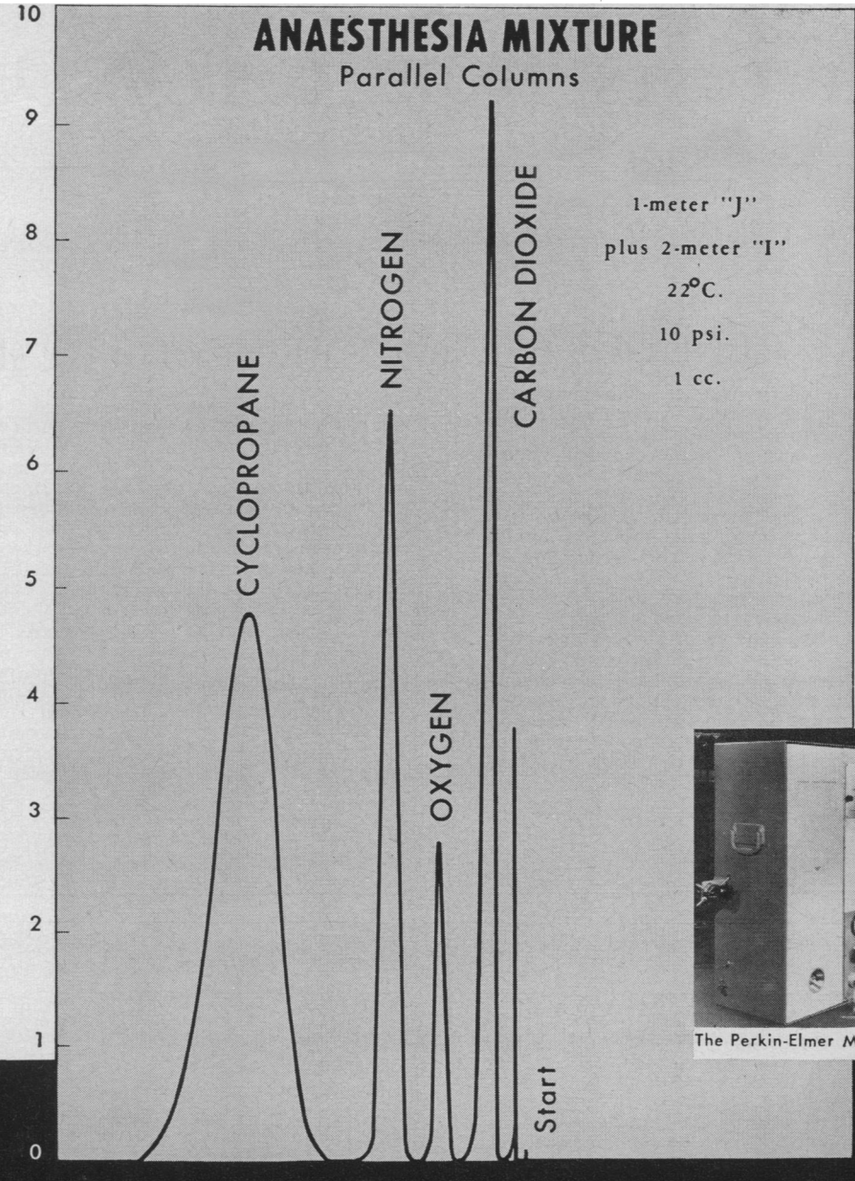

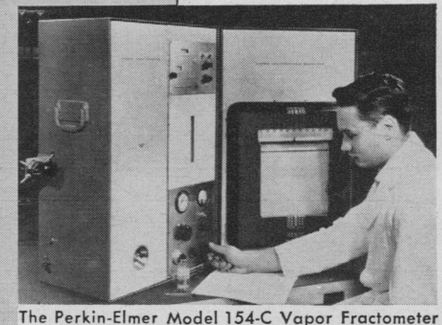

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Such examples are just part of the wide applicability of gas chromatography to biological and medical problems. Perkin-Elmer Model 154-C Vapor Fractometers are being used with outstanding success in the analysis of blood gases, and at the top of the GC temperature scale for the separation of high-boiling methyl esters of fatty acids.

To find out more about the versatility of the Model 154-C Vapor Fractometer and its application to your analytical work, write us for a new and definitive bibliography of gas chromatography in the medical field. Address us at 910 Main Avenue, Norwalk, Conn.

Ask about our instrument leasing program.

INSTRUMENT DIVISION
Perkin-Elmer *Corporation*
NORWALK, CONNECTICUT

2 December 1960, Volume 132, Number 3440

SCIENCE

Cover

Cell wall of a brown alga, *Dictyota flabellata.* Two large pits are flanked by microfibrils with a dominantly parallel orientation and are separated by an area in which the fibrils retain the reticulate pattern. Electron microscope, about × 26,500. See page 1663. [Edwin Bowler, University of California, Los Angeles]

*American Journal of Clinical Pathology Vol. 33. No. 2. February 1960, pp 144-151 "Application of Refrigerated Microtome in Surgical Pathology" by Bernard Klionsky, M.D. and Othello D. Smith, M.D.

The Journal of Histochemistry and Cytochemistry Vol. 8, No. 5. September, 1960, pp 310 "A Frozen Section Freeze Substitutions Technique and an Improved Cryostat" by Jeffrey P. Chang and Samuel H. Hori.

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COMPLETE FROZEN SECTIONING PACKAGE includes: Model CT Microtome Cryostat; rust-proof International Minot Rotary Microtome; Quick Freeze Attachment for freezing fresh tissue directly to Minot chuck; Anti-Roll Attachment for keeping sections flat; Suction Pickup for transferring sections.

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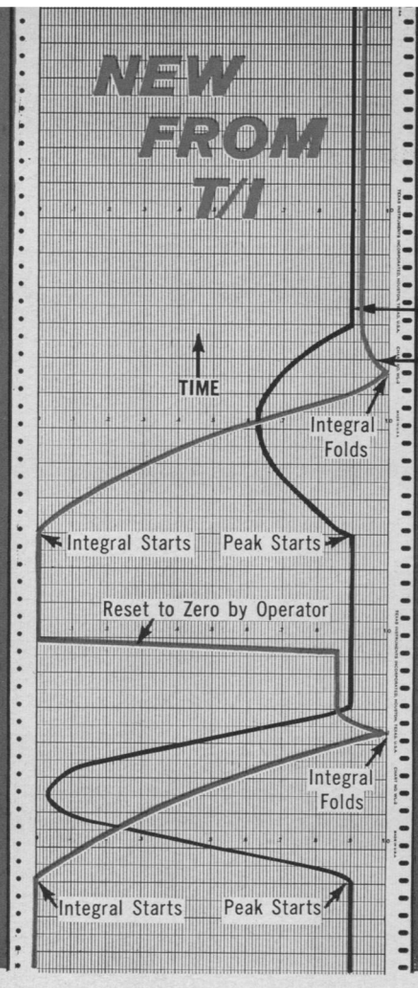
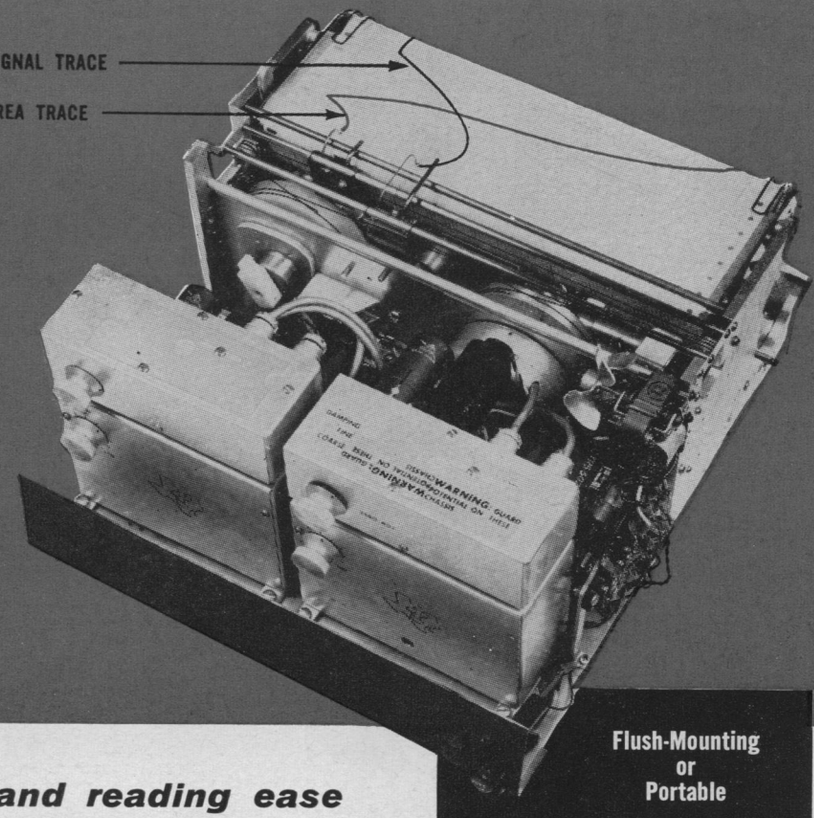


servo/riter*

INTEGRATING RECORDER



More speed, flexibility and reading ease than any other integrating recorder

Accurate, quantitative analysis of *any variable* that depends on the precise measurement and integration of the curve may be obtained with the TI Integrating Recorder. The integrator channel is installed as a second channel in a standard wide-grid, single-channel *servo/riter* recorder. It consists of a special potentiometer-amplifier-servo arrangement and activates an overlapping "area trace" automatically and simultaneously with the signal trace.

Three different maximum count rates are readily obtained through an adjustable gear system up to a maximum of 40,000 area counts per minute. The counting rate linearity is ± 0.5% of full scale.

Note that the integrating signal utilizes the full chart scale to provide faster, easier reading. Traverse of the full scale represents 1,000 counts. If the integral exceeds 1,000 counts, the integral "folds" and completes its excursion in the opposite direction. You may reset the pen to either margin between each peak or at the start of each integration for easier interpretation. The integrating circuit may be set to any assumed zero point in the span of the recorder signal.

The Integrator Recorder is offered with either portable or flush-mounting *servo/riter* recorders. Or, the Integrator Unit may be factory installed in your existing wide or dual chart *servo/riter* recorder. In fact, many flexible arrangements are possible . . . let TI's recorder engineers work with you on specific applications.

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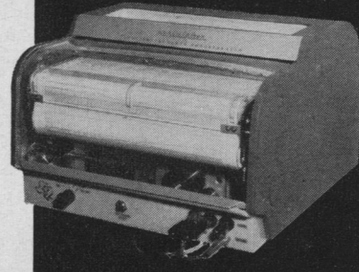

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- Gaseous examination applications . . . gas industry; mining; rubber and linoleum factories; incandescent lamp manufacture; hospitals and tuberculosis clinics; physical, physico-chemical and chemical institutes

TECHNICAL DATA

Range of measurement —
5 to 5000 units of the fifth decimal of the refractive difference Δn, subdivided according to the length of chamber

Limit of error —
± 2 units of the eighth decimal to 2 units of the fifth decimal of the refractive difference Δn, likewise in accordance with the length of the chamber

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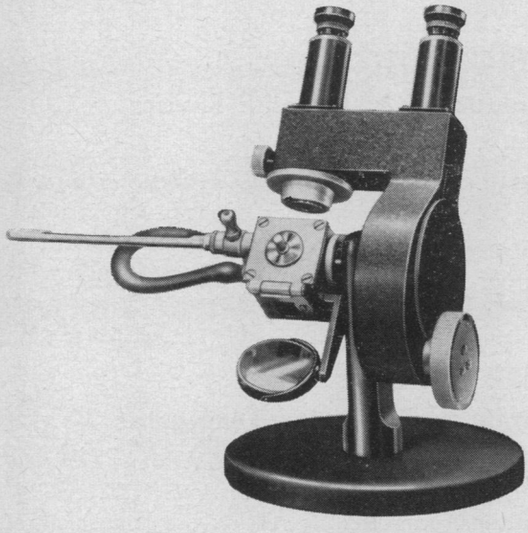

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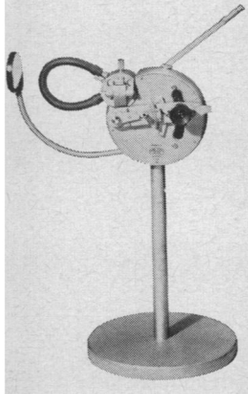
This newly designed instrument measures the refractive index of liquids, fats and oils as well as plastic and solid materials.

- Interchangeable prism casing with insulated lock
- Easier reading and increased measurement accuracy
- Safe, dust-proof scale housing
- Pinion head conveniently positioned for easy manipulation of border line setting
- Adjoining scales for refractive index and dry substance
- Measurements with higher or lower temperatures

TECHNICAL DATA

Range of measurement —
Refractive index $n_D = 1.3$ to $n_D = 1.7$
Dry substance 0 to 85%

Limit of error —
Refractive index ± 1 to 2 units of the fourth decimal
Dry substance ± 0.1 to 0.2%



FOOD PRODUCTS (SUGAR AND OIL) REFRACTOMETER

This precision production control and laboratory instrument is designed for rapid tests in series to determine dry substance or water content.

- Surface of double prism opens horizontally to facilitate filling with sample
- Easy to manipulate
- Fast, accurate measurements
- Economical use of test material

TECHNICAL DATA

Range of measurement —
Dry substance 0 to 95%
Refractive index $n_D = 1.300$ to 1.540

Limit of error —
Dry substance ± 0.1 to 0.2%
Refractive index ± 1 to 2 units of the fourth decimal

DIPPING REFRACTOMETER

This is the most accurate of all refractometer instruments. A complete range of ten heatable and ten non-heatable prisms permits rapid analysis, even at high temperatures. Boundary line position is read from scale containing 110 divisions. Readings can be made to one-tenth of a scale division by means of a micrometer screw.

TECHNICAL DATA

Range of measurement — 1.3254 to 1.6470
Limit of error — ± 2 units of the fifth decimal of the refractive index

Complete information may be obtained from your local Carl Zeiss Jena instrument dealer or by writing Dept. S12/60

ERCONA **Ercona Corporation, Scientific Instrument Division, 16 West 46th Street, New York 36, N.Y.**
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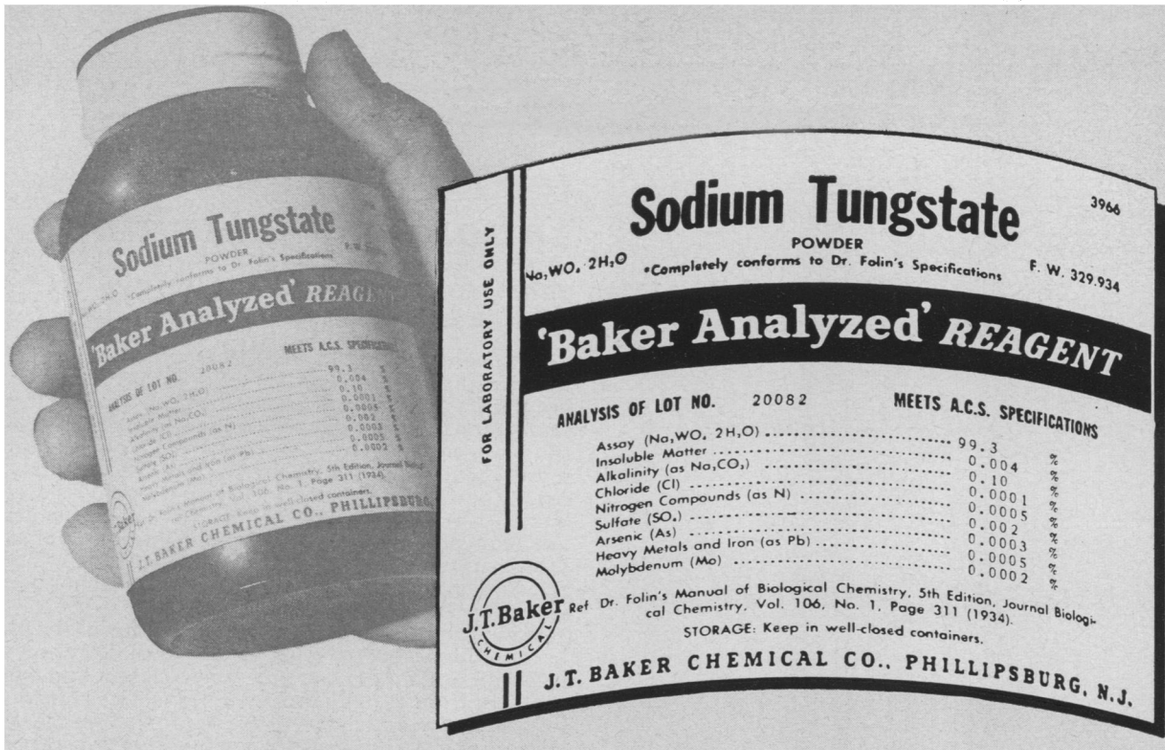

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In hospital work, your clinical laboratory team needs dependable purity, accurately defined purity—more than just purity within a range

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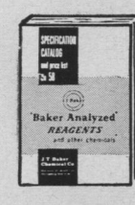
...contains complete information about 'Baker Analyzed' Reagents and other chemicals, including pricing and packaging data, formulas, formula weights and specifications. May we send you a copy?



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A precise constant delivery device primarily intended for titration with recorders and comprising precision ground Pyrex Brand Glass solution barrels and plungers, operating on a simple displacement principle with the plunger driven at a constant rate by a synchronous motor. Sealing is effected by replaceable Neoprene "O" rings and Quad rings held in special stainless steel collars with spring loading. Motor drive is transmitted to the plunger by a precision ground stainless steel lead screw running in a Nylon following member. Drive is self-limiting at both ends of the lead screw and automatically resumes on reversal.

A separate motor provides rapid motion of the plunger in both directions for refilling from a directly connected reservoir and for discharging excess titrant and flushing.

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Barrel and plunger assemblies are completely interchangeable permitting accommodation of a total volume of either 10 ml or 50 ml. The rate of drive delivers either volume in 10 minutes. Drive and control components are mounted in a cabinet with a protecting stainless steel cover on which the burette assembly is supported. The panel of this case provides pilot light, fuse, rapid drive reversing switch, and starting button which may be locked in for continuous delivery and for automatic applications. The rear panel provides connecting facilities for using the burette with second derivative automatic titrators with automatic shut-off at the end point and for remote operation by direct connection to Sargent recorders S-72150 and S-72151 for fully recorded titrations.

The three-way glass header connects to the burette barrels by a 10/18 ground joint.

Accuracy, 0.1% of burette capacity; readability and reproducibility, 0.01 ml with 50 ml capacity, 0.002 ml with 10 ml capacity. Height of base, 5 inches; depth of base, 8¾ inches; width of base, 10 inches; height over header, 23 inches.

S-11120-1 BURETTE—Motor Driven, Constant Rate, Precision Grade, 10 ml, Sargent. With S-11121-1 burette equipment for 10 ml capacity only..... **$280.00**

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S-11121-1 Burette Equipment Unit, Only, 10 ml. For converting S-11120-5 to 10 ml volume **$27.00**

S-11121-5 Burette Equipment Unit, Only, 50 ml. For converting S-11120-1 to 50 ml volume **$37.00**

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SPERMATOZOA FROM MACROGLOSSUM STELLATARUM (INSECTA, LEPIDOPTERA)

Transversal section through the middle pieces. The dense core is the mitochondrial derivative flanked on one side by the flagellum and on the other by a series of laciniate appendages of the head. 60,000 X. Reproduced by courtesy of Dr. Jean André, Institut de Recherches sur le Cancer, Villejuif (S.), France.

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GET YOUR ADVANCE COPY

of the General Program of the AAAS New York Meeting by first class mail – early in December

The General Program of the 127th Meeting of the AAAS in New York, 26–31 December 1960, will be available to you, at cost, within the first week in December—whether you can attend the Meeting or not.

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.

Advance Registration

Advance registration has these decided advantages: (1) You avoid delay at the Registration Center upon arrival; (2) You receive the General Program in ample time to decide, unhurriedly, which events and sessions you particularly wish to attend; (3) Your name is posted in the Visible Directory as the Meeting opens.

The following coupon may be used both by advance registrants and by those who wish only the advance copy of the General Program.

——————THIS IS YOUR COUPON FOR AN ADVANCE COPY OF THE GENERAL PROGRAM——————

1a. ☐ Enclosed is $3.50 for my advance Registration Fee which brings me the General Program, Convention Badge, and all privileges of the Meeting (50¢ is for first-class postage and handling).

1b. ☐ Enclosed is $2.50 for only the General Program. (It is understood that, if I should attend the Meeting later, the Badge–necessary for the privileges of the Meeting—will be secured for $1.00 more.)
(check 1a or 1b)

2. FULL NAME (Dr., Miss, etc.) .....
(Please print or typewrite) (Last) (First) (Initial)

3. ACADEMIC, PROFESSIONAL, OR BUSINESS CONNECTION .....

4. OFFICE OR HOME ADDRESS .....
(For receipt of General Program)
CITY ..... ZONE ..... STATE .....

5. FIELD OF INTEREST .....

6. CONVENTION ADDRESS .....
(May be added later, after arrival)

Please mail this Coupon and your check or money order for the total amount to the
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1515 Massachusetts Avenue, NW, Washington 5, D.C.

APPLICATION FOR HOTEL RESERVATIONS
127th AAAS MEETING
New York, 26-31 December 1960

The five hotels for the AAAS New York meeting have established special, **low, flat rates** and have reserved appropriately large blocks of rooms for this meeting. Thus everyone making room reservations for the AAAS meeting is assured substantial savings.

The list of hotels and the reservation coupons below are for your convenience in making your hotel reservation in New York. Please send your application, *not* to any hotel directly, but to the AAAS Housing Bureau in New York and thereby avoid delay and confusion. The experienced Housing Bureau will make assignments promptly; a confirmation will be sent you in two weeks or less.

If requested, the hotels will add a comfortable rollaway bed to any room, at $3.00 per night. Mail your application now to secure your first choice of desired accommodations. All requests for reservations must give a definite date and estimated hour of arrival, and also probable date of departure.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

For a list of the headquarters of each participating society and section, see page 230, *Science*, 22 July. Both the Commodore and the Biltmore are AAAS headquarters hotels.

Flat Rates for Rooms with Bath*

Hotel	Single	Double Bed	Twin Beds	Suites
Commodore	$ 8.50	$14.00	$15.50	$21.00 to $52.50
Biltmore	8.50	14.00	15.50	45.00 and up
Roosevelt	8.50	14.00	15.50	39.00 to 43.00
Belmont Plaza	8.50	14.00	15.50	30.00 and up
Waldorf-Astoria	10.00	16.00	18.00	45.00 and up

* All rates are subject to a 5% New York City tax on hotel room occupancy.

THIS IS YOUR HOUSING RESERVATION COUPON

AAAS Housing Bureau
90 East 42nd Street
New York 17, N.Y.

Date of Application .....

Please reserve the following accommodations for the 127th Meeting of the AAAS in New York, 26–31 December 1960:

TYPE OF ACCOMMODATION DESIRED

Single Room ..... Double-Bedded Room ..... Twin-Bedded Room .....

Suite ..... Desired Rate ..... Maximum Rate .....
(Desired rate and maximum rate apply only to suites)

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

(Attach list if this space is insufficient. The name and address of each person, including yourself, must be listed.)

.....

.....

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

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

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

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

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

versatile new laboratory recorder

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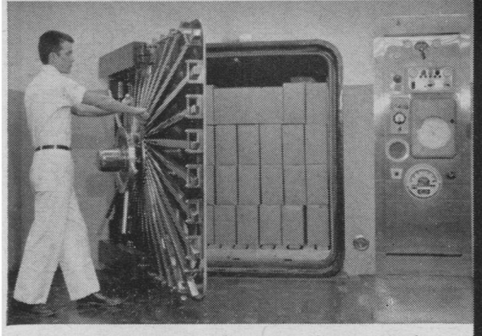

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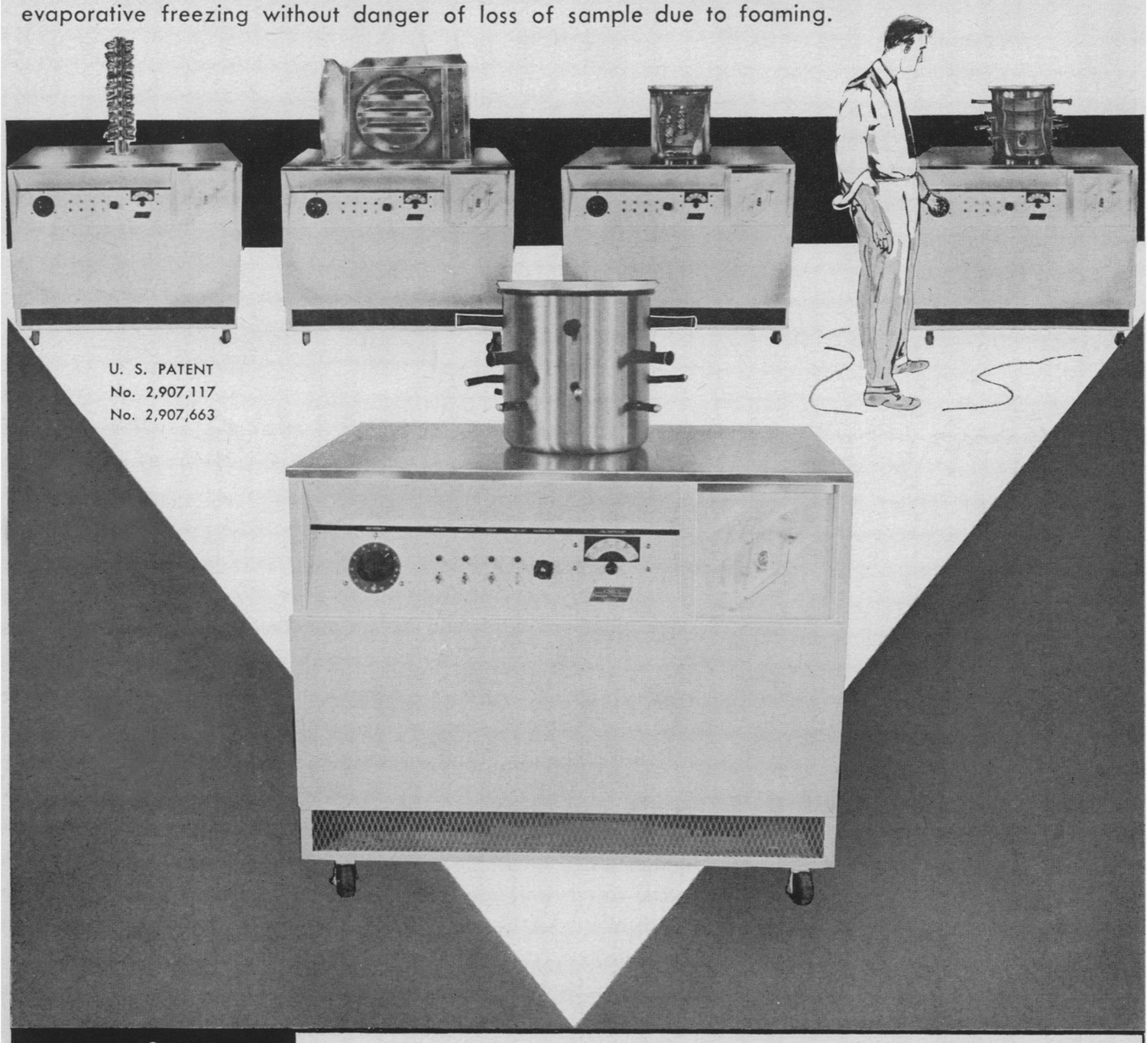

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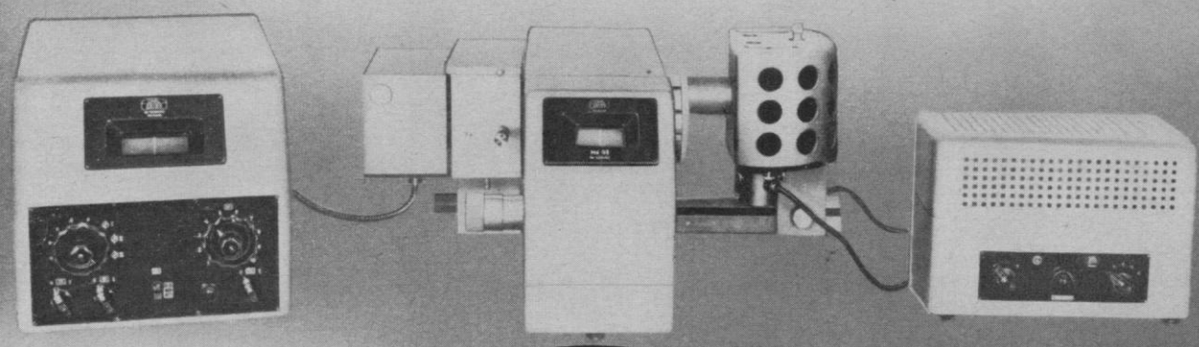

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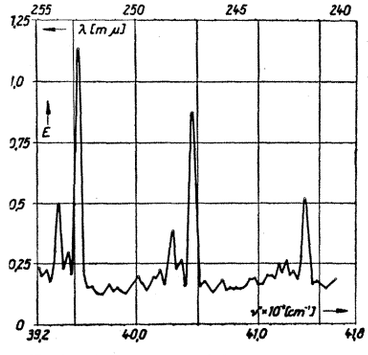

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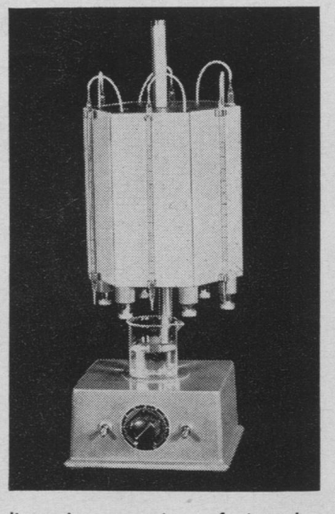

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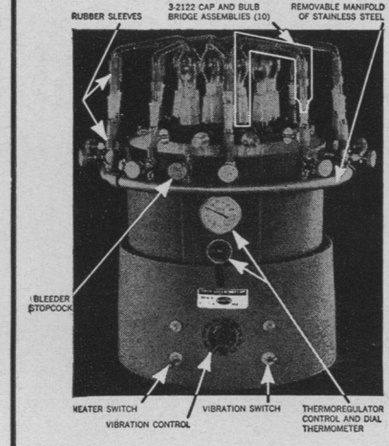

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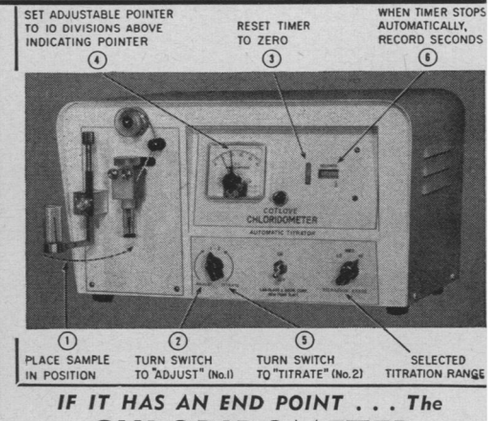

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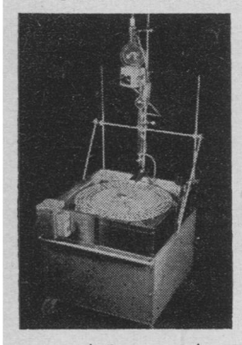

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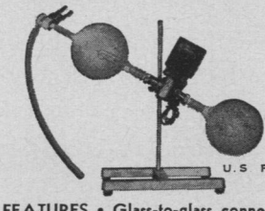

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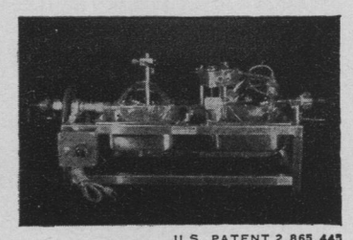

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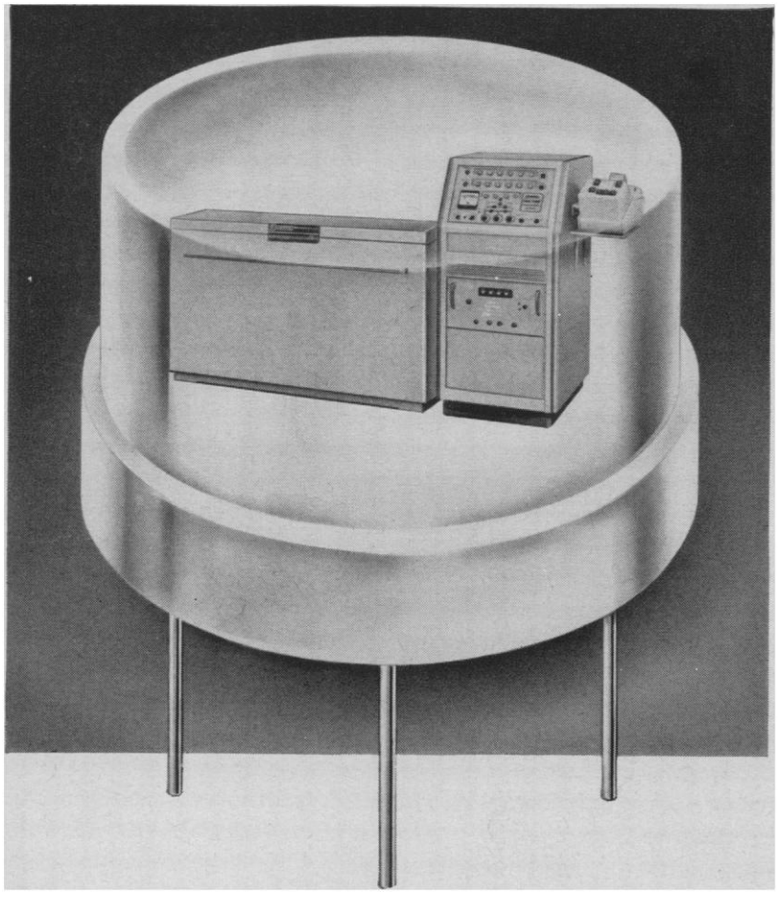

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LITERATURE CITED

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(6.) *Martin, A. J., Euston, C. B., and Martinez, F. W., Jr., "The Design and Evaluation of a Linear Programmed Temperature Gas Chromatograph Featuring a Hydrogen Flame Ionization Detector," presented at the Detroit Anachem Conference, October 1960.

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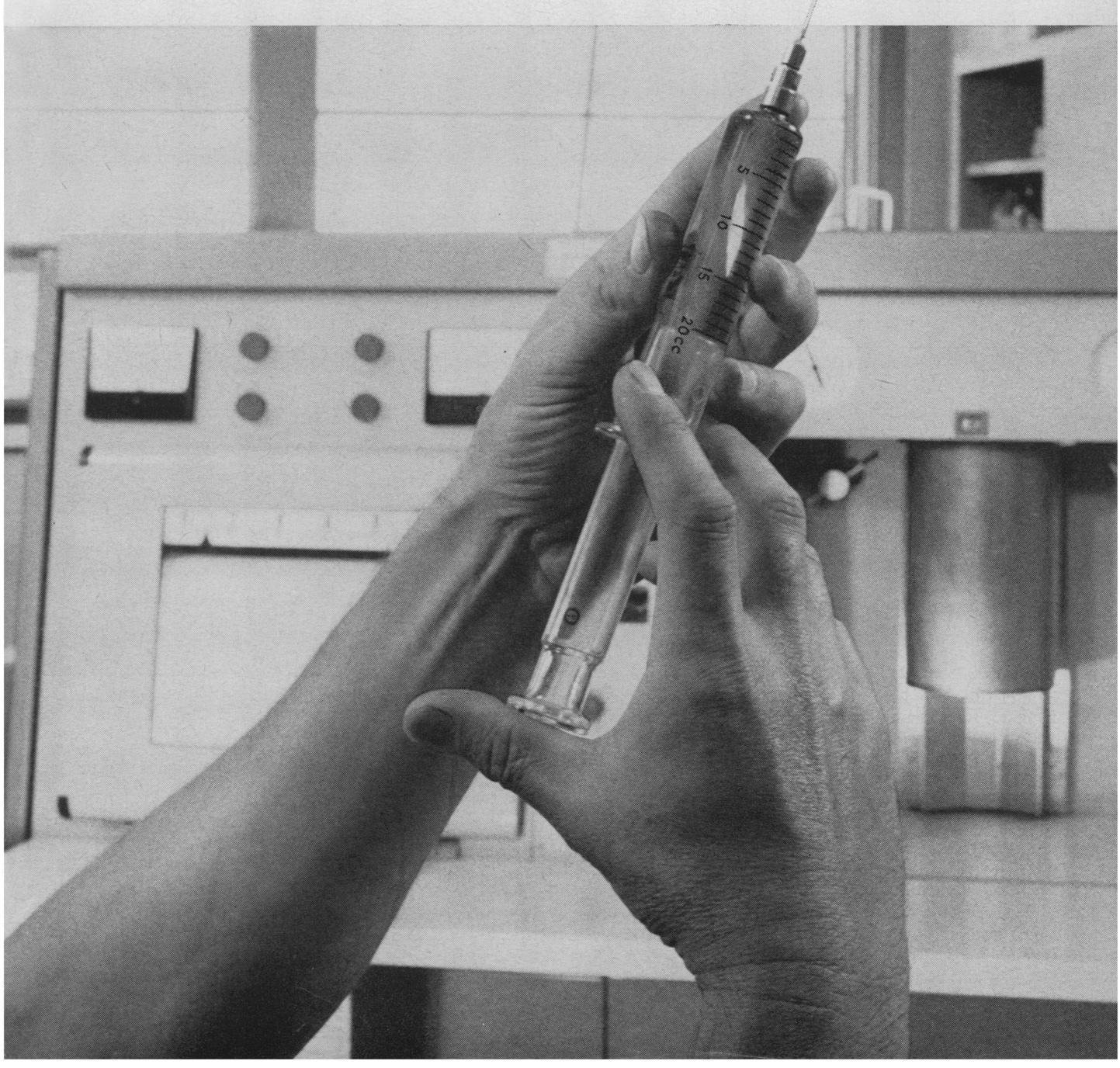

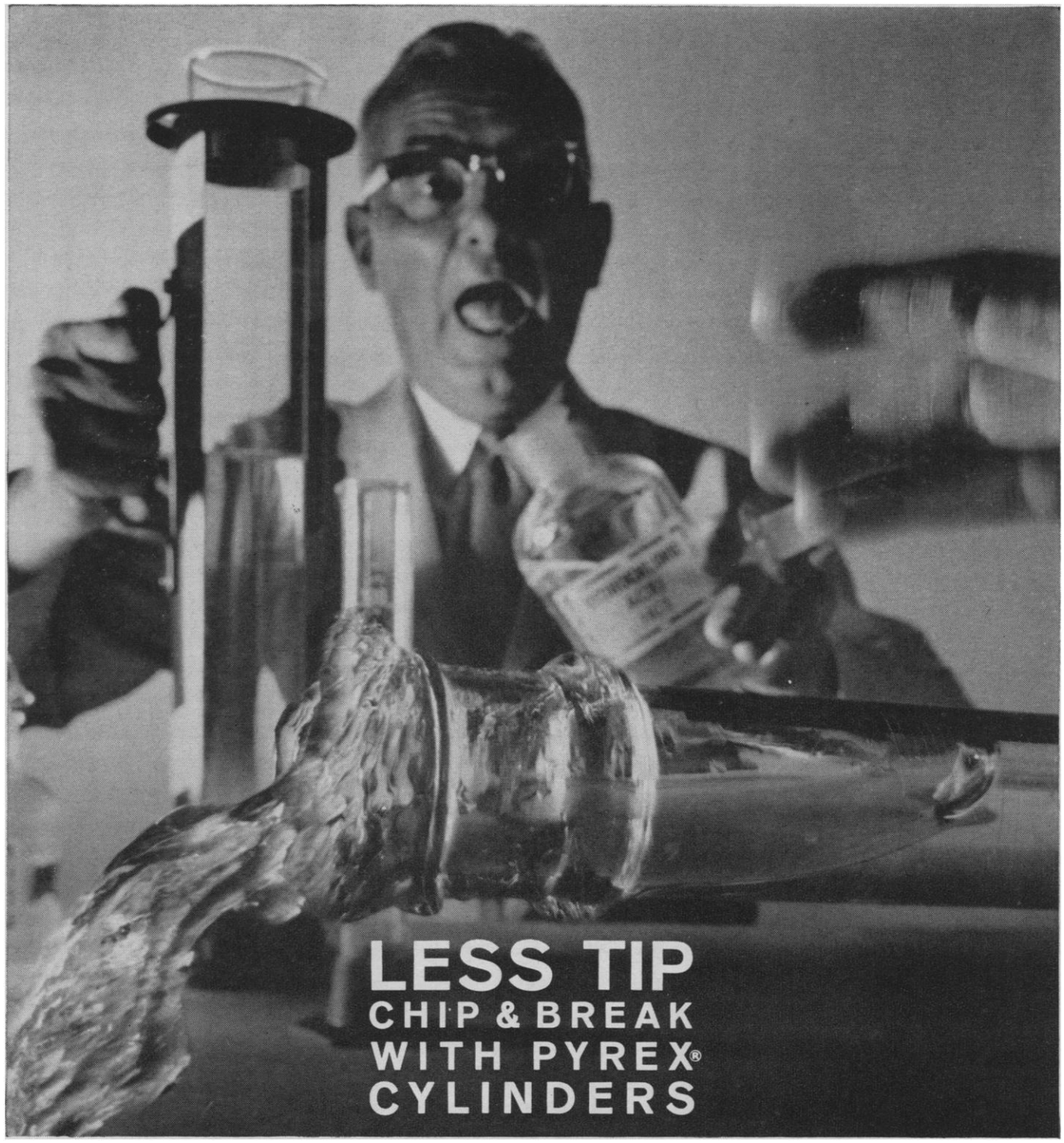

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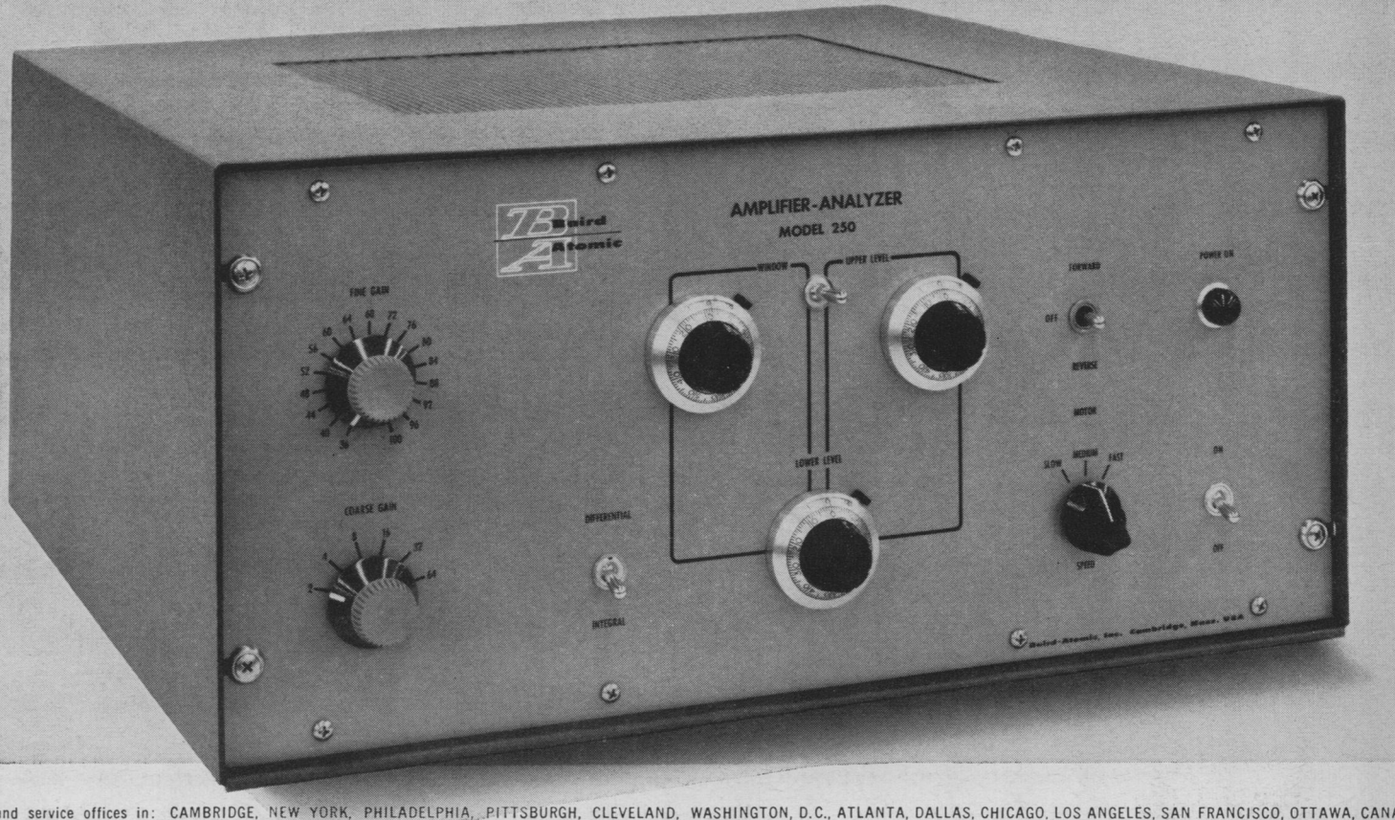

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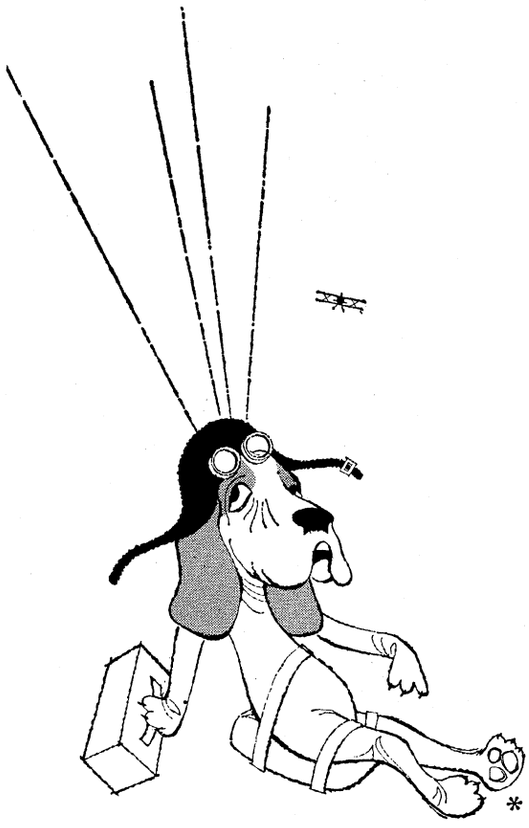

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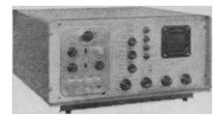

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2 December 1960, Volume 132, Number 3440

SCIENCE

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Two Weeks B.C.

Since this issue of *Science* is largely dedicated to the 1960 AAAS meeting, some comment here on this traditional event has become a pleasant practice. This time we are thinking about the care and feeding of program copy—feeding it daily, that is, to an unusually understanding printer. As an omelet is impossible without fractured eggs, so a printed program is nothing without reworked copy.

Converting the raw copy of the 18 sections, of several AAAS committees, and of some 50 societies into a program of 300 pages seems a bit of a chore on some days, on others, a monumental task. We must meet a tight printing schedule—and on some days the copy is rawer than on others!

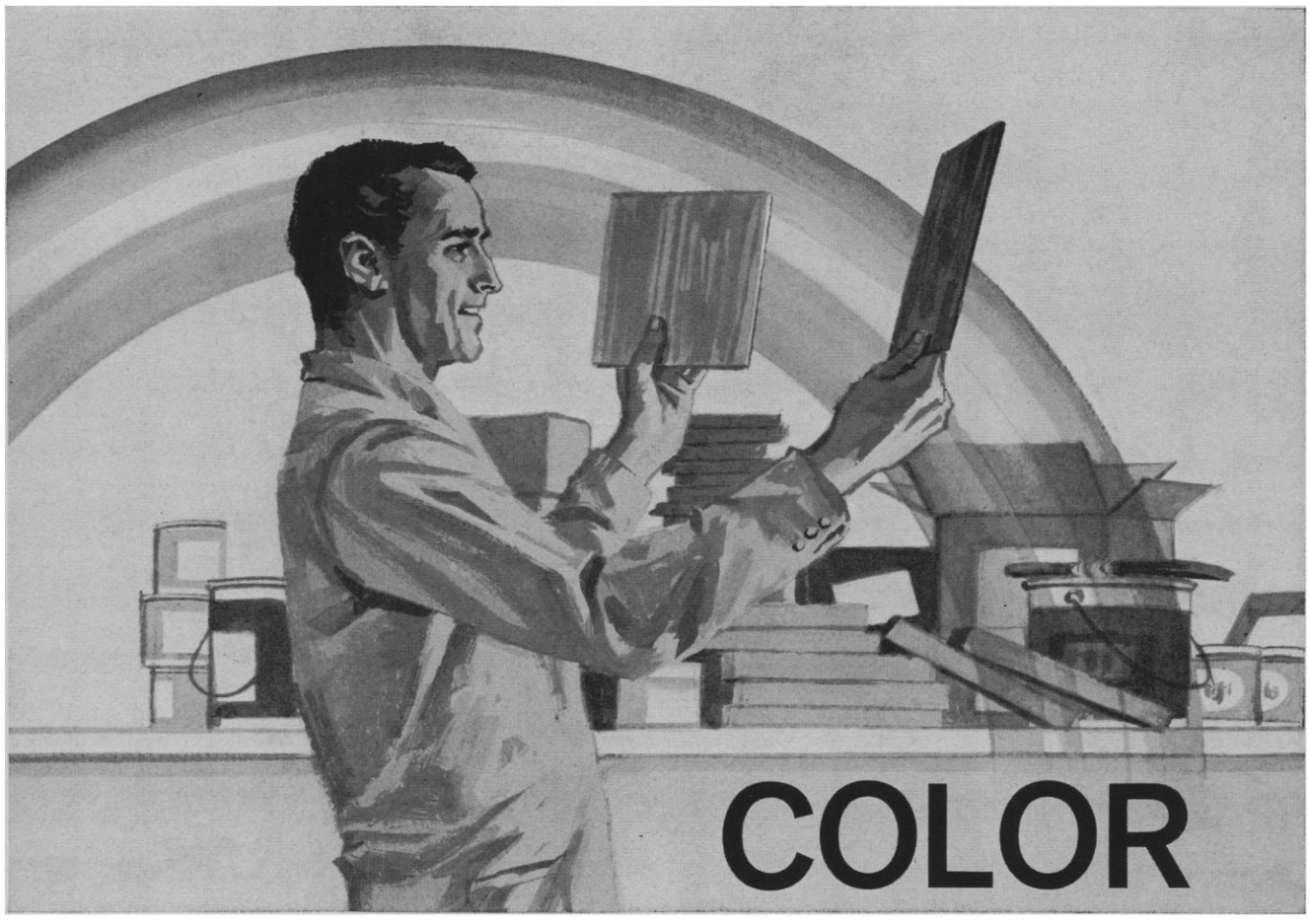
On deadline day, 1 October, the small office staff at once begins to quarry the avalanche of copy. Each one of us knows that the program must be produced in 8 short weeks. Most observers marvel that it can be done. Precious time would be gained if no program copy had to be retyped, but too often this step is necessary. Despite advance suggestions, many manuscripts arrive single-spaced, full of solid caps, heavily underlined, in pale ditto with letters missing, or laid out like an ad—all difficult for an editor and far too rugged for the tired eyes of linotypists.

As October's golden days merge into sere November, the pace becomes still more frenetic. Sheaves of galley proof deck the desk tops and bury all but the most urgent correspondence. When 125 arrangers return their proof, anguished moments may ensue. Perhaps scheduled days have been changed or larger rooms requested: room assignments must be reshuffled and innocent participants affected. Authors' alterations of the usual type—added papers, renumbered papers, new titles for old, authors' names respelled, forgotten initials inserted—all take time. One missing institutional connection for a participant may necessitate several phone calls and hours of research.

In mid-November, "Pagination Day" brings page proof. Now, would-be authors are shocked to be too late; exhibitors still want booth space; banquet advice is given; and weekly releases on "The Meeting" are prepared for *Science* and other journals. As the news gets out, three telephones interrupt ceaselessly. These are reasonable requests for information, but how one wishes for a single recorded speech that politely could summarize programs in response to: "Can you tell me something about the meeting?" We're in a spot where automation can't help, but our morale wavers not.

In the last two weeks, we are tempted to work after hours, on Saturdays, or even around-the-clock—especially when a 2500-name index looms—but we do resist. And, in D.C., our ladies may not labor longer than 40 hours in any week!

At long last, our annual travail attains its goal, and blissful euphoria prevails. We tilt back, relax slightly, and sincerely rejoice that all advance registrants will get their programs about two weeks B.C.—i.e., before Christmas!—RAYMOND L. TAYLOR, AAAS.



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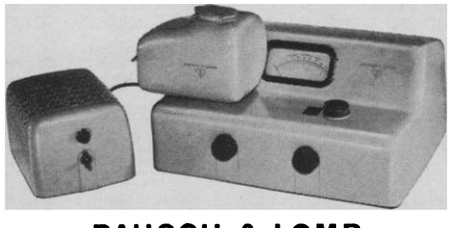

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Letters

Further Competitive Exclusion

The perennial battle on what is now known as the "competitive exclusion principle" has again been joined by LaMont Cole [*Science* **132**, 348 (1960)], who minimizes its importance. I will follow L. C. Birch and define competition as occurring "when a number of (organisms) . . . utilize common resources the supply of which is short; or if the resources are not in short supply competition occurs when the (organisms) seeking the resource nevertheless harm one another in the process" [*Am. Naturalist* **101**, 5 (1957)].

The environment of an individual can be partitioned into several areas, as has been done perhaps most clearly by H. G. Andrewartha and L. C. Birch [*Distribution and Abundance of Animals* (Univ. of Chicago Press, Chicago, Ill., 1954)]. It is well known that related allopatric species are often indistinguishable in their requirements for food or "a place in which to live." If only one of these two aspects, which may be called subniches, or an essential part of either of them, is in short supply relative to the needs and behavior of the species, they cannot indefinitely coexist unless (i) they are equally fit in this environment, (ii) immigration replenishes the less fit species, or (iii) the species are prevented by some extrinsic or intrinsic cause from ever reaching the population size where they would compete. It does not matter that the species may differ in many other respects; the possession of identical requirements in one or a part of one of the two subniches, if this is at any season or stage a limiting factor to population size, is enough for the elimination of one species. This restatement of the principle is thus stronger and more testable than the usual one.

Cole cites an example from Skellam purporting to show a case where the principle is false. But they assume "that the species are equally good competitors," so that it is no wonder that both are present indefinitely, and "competitive ability" is used so narrowly that it excludes differences in fertility, which are then brought in to balance the viability component of fitness when the latter differs. But it is certainly rare, and perhaps nonexistent, that two species would have precisely the same total fitness in the same range of environments, although in closely balanced situations either might be eliminated because of, for example, unpredictable individual interactions, which could lead in nature to a fluctuating and patchy distribution of largely pure areas of each species, as in Skellam's model.

Cole's alternative maxim may be rephrased to read, "Species cannot *exist* indefinitely because of the inevitability of random extinction," but, except for individual demes and some rarer (and therefore less fit) species, this is unimportant even over geological time. Most individual species, not to mention life itself, are not likely ever to become extinct by "random" fluctuations, by which I mean simply those for which the causal complex is not adequately known, and which in some cases seem actually to be self-damping.

Competition (in the sense defined) is rare or absent in nature at any one time and place, because of the short time before the elimination of one of the species. Mainly its importance is supported by (i) allopatry of species with apparently identical subniches, (ii) waves of replacement (as in Brown's studies on the ants of the Pacific islands), and (iii) the few nonequilibrium situations now known. Apart from plant successions, which demonstrate the principle beautifully, the latter mainly involve artificially introduced species, such as the gray squirrel in England. Although the importance of competition in these latter cases has been questioned, perhaps justifiably in some, it is usually not clear why the native species should decrease immediately upon the arrival and expansion of the newcomer, except by some form of competition (which may not be aggressive but in the form of higher fertility or some other advantage). Even if exceptions to the principle as now stated could be proved, they would merely add further qualifications to its use and not remove its wide applicability in explaining the distribution of related organisms.

LEIGH VAN VALEN
Department of Zoology,
Columbia University, New York

Leigh Van Valen seems determined to remain entangled in what I referred to as "the semantic difficulties surrounding competitive exclusion." I think I know what he means by a "subniche," but what factors make two species "equally fit" and under what conditions do they "harm one another?" I will concede that the individual organism is "harmed" by the predator that totally consumes it or by the competitor that causes it to starve to death, but this does not necessarily harm the population to which the individual belongs. At this level the activities of other species in holding down numbers may be important influences favoring survival. I do not find myself enlightened by dogmatic assertions containing ambiguous words but, if *Science* is going to print Van Valen's letter, I suppose it merits an answer.

Van Valen's "restatement of the (exclusion) principle" in a "stronger" form is an unsupported assertion that, in the absence of immigration, two species actually limited by competition for the same "subniche" or "part of one" cannot coexist unless "they are equally fit in this environment." Now, most proponents of competitive exclusion believe that species can coexist if there are differences between them and my aim was to present a model in which they can coexist without such differences. In my boiled down version of Skellam's model I (not "they") undoubtedly made the species "equally fit" by assuming no differences whatsoever. This limitation is not an essential part of Skellam's model.

If Van Valen will go to the original he will learn that, with no immigration, two species limited by the same "subniche" ("a place in which to live") can contribute different numbers of potential offspring per individual to the next generation (does this not make them unequally fit?) and can still coexist indefinitely—provided that we neglect the possibility of random extinction, which Van Valen asserts to be unimportant.

My report was not designed to advocate any particular definitions or models of competition but to warn against uncritical acceptance of competitive exclusion as an axiom. I am confident that there remain great possibilities for contributing to our understanding by investigating species interactions under specified conditions in the field, in the laboratory, and in theory. For example, M. H. Williamson [*Nature* **180**, 422 (1957)] has given objective definitions of "controlling factors" and "competition" and has investigated objectively the circumstances under which competing species can and cannot coexist. I doubt that Van Valen will find much comfort in Williamson's conclusions but I commend the paper to him as an example where it is possible to debate the reality of the assumptions [H. G. Andrewartha and T. O. Browning, *Nature* **181**, 1415 (1958); M. H. Williamson, *ibid.*] and where we are left in no doubt about the nature of the conclusions or how they were reached.

LAMONT C. COLE
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Social Responsibility

The report of the AAAS Committee on Science in the Promotion of Human Welfare, as published in the 8 July issue [*Science* **132**, 68 (1960)], is most interesting; I am writing with regard to three items in this report.

The report mentions, under the heading "Scientists' approaches to their social responsibilities," a third group, "typified by the Society for Social Responsibility in Science, which takes the view that scientists have a moral responsibility to try to limit to ethical uses the applications of science and technology." This statement, while completely correct, may yet give a wrong impression on one point: the members of the society believe that such limitation can be achieved only by a personal commitment. Thus, the members try to decide for themselves what an ethical use is; they try to foresee the applications of their work (and in part, of course, scientists today work directly on applications in any case) and limit their work to tasks which appear ethical to them in the light of the above criteria. They do so for the dual reason that they feel (i) that only so can they fulfill their social responsibilities and (ii) that such personal commitment is the best way of educating the national and international community to the awareness of moral and social implications. In the brief text of the report, the words "try to limit," might be taken to imply such means as strikes which would bring pressure to bear on scientists of opposite views. Such pres-

sure is not part of the goal of the Society for Social Responsibility in Science.

With regard to item 5 (page 72) "How can scientists best meet their social responsibilities?" the authors of the report suggest "discussion among scientists." It is hoped that the AAAS may arrange, at an annual meeting, a forum to discuss the several means to achieve this end. It seems to me that there is no single means, either presently known or probable in the near future. Instead, a number of means will have to be utilized simultaneously. Such means include education of the public with regard to social issues; they include work with the legislature; they include, also, the personal commitment mentioned above.

Finally, with regard to the code of ethics mentioned in item 6, I recognize that there are scientists in several fields who put great emphasis on such a code. This is not the place to debate the usefulness of such codes per se; rather, I would enter a plea that if such a code is developed it should state clearly its basis: Does it deal only with what one might call the "production" end—namely, truthfulness, honesty, and so on in developing scientific insights—or does it deal also with the "consumption" end—the application of science to technological ends and to humanity in general?

VICTOR PASCHKIS
School of Engineering, Columbia University, New York

American Men of Science

As editor of *American Men of Science*, I would like to call to the attention of the readers of *Science* the confusion caused by the titles which have been adopted by others in the biographical publishing field.

There has been published for several years a small directory called *Leaders in American Science*. This has been confusing to many busy scientists because of the similarity of the above title to *American Men of Science* and *Leaders in Education*.

Recently, the Institution for Research and Biography changed the name of its directory from *Who's Important in Medicine* to *American Men of Medicine*. The fact that both the name and the price of the volume are similar to *American Men of Science* has caused us to receive hundreds of questionnaires from persons believing it to be under the same editorship and publisher as *American Men of Science*. We wish to say that *Leaders in American Science* and *American Men of Medicine* are in no way connected with *American Men of Science* or *Leaders in Education*.

American Men of Science for the past 35 years has been under my editorship. I have always placed great importance in the care exercised to assure that financial support and subscriptions have no relationship whatsoever to the inclusion of individuals in the directory.

The 10th edition of *American Men of Science*, including both the Physical and Biological Sciences and the Social and Behavioral Sciences, will come to around 130,000 names. It is probably the most complete directory of its kind in the world.

JAQUES CATTELL
Arizona State University, Tempe

Open Literature

An article in the Russian newspaper *Ekonomicheskaya gazeta* (Economic Gazette) of 24 August 1960 shows that an influential part of Soviet public opinion desires now a stricter control over the Russian technical press in order to prevent publication of data that may benefit the government of the United States or industry in this country. This newspaper is published by the central committee of the Communist party, and it may be assumed that nothing would appear there which is contrary to the prevailing views of the leaders of the Communist party.

After referring to the Francis Powers case and citing a number of cases of Soviet engineers who carelessly talked to American and other spies and unwittingly revealed various secrets, the article, entitled "Guard closely the State's secrets," continues, "Many valuable secret information items find their way abroad also through our scientific-technical journals and [other] publications. The American political expert, Harry H. Ransom, the author of the book *Central Intelligence and National Security*, published by Harvard University Press, touches upon the methods of openly collecting information and writes, 'The U.S., in attempting to obtain important information from behind the Iron Curtain, does not rely completely or even principally upon secret operations. They utilize open information pertaining to achievements of Soviet science and technology which appears in the Soviet press." A case of a secret apparatus described in an article by the chief engineer of a plant manufacturing radio appliances is then quoted; also given is the case of a chemist who described a method of preparation of a chemical element in high purity, after which, a year later, an American manufacturer visiting the U.S.S.R. thanked Soviet specialists for revealing their method which helped in the development of an economical process for the same purpose in the U.S.A.

"Our successes in science and technology achieved in recent years cause their [capitalist countries'] envy and anger. They attempt by all means to obtain the keys to our scientific and technical discoveries," continues the author of the article.

J. G. TOLPIN
Standard Oil Company, Whiting, Indiana

Stilbestrol

In his recent letter to *Science* [**132**, 156 (15 July 1960)] concerning W. J. Darby's review of his book *The Poisons in Your Food*, W. F. Longgood made some statements for which I would like to see supporting evidence. These concerned (i) stilbestrol residue in the meat of cattle fed this material and (ii) increased water content of the flesh of cattle fed stilbestrol.

Under the conditions of its current use, stilbestrol has not resulted in either of these conditions (*1*).

R. L. PRESTON
Department of Animal Husbandry, College of Agriculture, University of Missouri, Columbia

References

1. G. M. Briggs, *J. Am. Med. Assoc.* **164**, 1473 (1957); E. J. Umberger *et al.*, *Endocrinology* **63**, 806 (1958); R. L. Preston *et al.*, *J. Animal Sci.* **15**, 3 (1956); R. L. Preston and W. Burroughs, *ibid.* **17**, 140 (1958).

The evidence for both statements is in *The Poisons in Your Food* (pp. 141–146). Jack M. Curtis of the *Food and Drug Administration* stated that "meat from steers fed 10 mg of stilbestrol per day contained approximately 0.6 parts per billion estrogenic activity when ready for market." The cumulative effect of carcinogens has been established.

A group of physicians headed by cancer researcher William E. Smith pointed out that meat from a steer fed the prescribed 10 mg of stilbestrol had shown about 14 times the amount of stilbestrol needed as a daily dose to produce cancer in mice. The physicians also said that the testing method had limited sensitivity, and that meat certified as being stilbestrol-free could contain traces of the drug.

Clive McCay of Cornell said that rodents used in research must be fed special diets to avoid reproductive failure due to stilbestrol. He said special mixtures are prepared without meat scrap, "because this product [meat] is the carrier of . . . stilbestrol. No one is certain how this stilbestrol gets into the meat meals, but it is there and has been during the past several years when steers have been fed stilbestrol."

Wilhelm C. Hueper, cancer researcher at the National Institutes of

Health, has raised the possibility that stilbestrol may remain in treated meat in some changed form. He advocates banning the drug from animal feed.

Robert K. Enders and Carl G. Hartman, U.S. Department of Agriculture consultants, testified before the Delaney committee about the deleterious effects of stilbestrol and its ability to make meat retain water. Enders called the practice of using it for this purpose "an economic fraud."

The Livestock Reporter reported that cattle buyers had down-graded by as much as 5 cents a pound cattle fed with stilbestrol. These cattle were described by buyers as deformed, covered with fat, and "undesirable."

WILLIAM LONGGOOD
New York, New York

Perception of Apparent Motion

Walter and Francis Kaess have shown [*Science* **132**, 953 (1960)] in their exemplary experiments that toads have perception of apparent motion. One could also say it this way: that experiments can be so devised that conditions of movement-perception required for the feeding of a toad can be fulfilled without the actual motion of either toad or food.

These experiments also bring additional evidence for something else. When the toad is placed on a 1-, 2-, and 3-day food deprivation schedule, it will not feed on food in front of it unless movement of food, or at least the conditions of food movement-perception, are fulfilled. Thus the drive of hunger, like other familiar drives, can be satisfied only within a distinct, particular configurational frame. As Tinbergen has shown [N. Tinbergen, *The Study of Instincts* (Oxford Univ. Press, London, 1951)], drives are not amorphous vague impulsions in living things but specific tension systems in search of specific configurations.

GEORGE G. HAYDU
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Life Shortening and Production of Tumors by Strontium-90

The recent report by V. E. Archer and B. E. Carroll [*Science* **131**, 1808 (17 June 1960)] includes two figures that are intended to demonstrate that the degree of life shortening and the production of tumors increase linearly with increasing absorbed dose of radiation from strontium-90. Since the data they used were those I had published in *Science* and elsewhere, I am obliged to call attention to several features of their analysis that may influence the acceptability of their conclusions.

The basic alteration applied by Archer and Carroll in their analysis concerns time, and by this alteration they changed *injected millicuries per kilogram* to *millicurie-days per kilogram*. Their approach was in the proper direction, but they oversimplified by using average survival time, and their values would have been more accurate if they had employed the power function for retention. The necessary data and formula have been published in an Argonne National Laboratory Report by S. A. Tyler (No. 5841, p. 132, 1958).

There is no question but that a correction for the time during which the radiation dose accumulates is required for a complete evaluation of the long-term toxicity of any internal emitter. With the present state of knowledge, however, we do not know over what period of time the dose should be integrated. One major problem concerns the length of the latent period between injection and neoplastic change since any radiation received after a tumor has been induced is wasted as far as that tumor is concerned. Another concerns the relative contributions of dose-rate and total accumulated dose to the response, whether it be tumor induction or life shortening or any other effect. But this is not the place to discuss the variety of complications that stand in the way of accurately assessing the absorbed dose that is responsible for a particular response. Nor is this the place to discuss the series of studies now in progress that should help resolve these complications. Archer and Carroll state: "It is hoped that Finkel will calculate an accurate dosage for the different groups in rads." That is my hope as well. However, until this can be done, I feel that we add very little by playing with numbers. Actually, the survival data uncorrected for continuing exposure fit a linear dose-response curve just about as well, or as poorly, as Fig. 1 in Archer and Carroll's report.

What is true for the survival data, however, is not true for the osteogenic sarcoma data. The incidence of malignant bone tumors increases approximately as the square of the injected dose. Since the higher incidences are associated with shorter survival times, correction for continuing exposure makes the curve even steeper and, consequently, more nonlinear. Archer and Carroll's Fig. 2, however, presents an apparently linear relationship between tumor incidence and millicurie-days. This result was obtained by a combination of two fundamental errors.

The first error was the inclusion with the osteogenic sarcomas of a variety of

tumors, some of which have not been observed to change in frequency after Sr^{90} administration. As a result the baseline of tumor incidence was raised substantially. For example, the values for reticular tumors used by Archer and Carroll, which were the incidences observed 625 days after injection, varied in a dose-independent fashion between 25 percent and 38 percent among the control mice and those that had received up to 200 μc/kg (Archer and Carroll's 11.7 millicurie-days). On the other hand, the incidence of malignant bone tumors ranged in a dose-dependent fashion from 0 to 13 percent. Consequently, the reticular tumors masked the true relationship between osteogenic sarcomas and these lower doses.

The second error concerns the higher doses, and it involved failure to recognize the fact that, as the dose of radiation increased, the osteogenic sarcoma response approached 100 percent but could not exceed it because the unit of response was the tumor-bearing animal, although the number of tumors per animal continued to rise steeply. Therefore, there is no justification for drawing the line between the values at 18.8 millicurie-days and 29.3 millicurie-days. The terminal point might better have been the former. Saturation at 100 percent could have been avoided by using the tumor rather than the tumor-bearing animal as the statistical unit. The required data appear in Argonne National Laboratory Report No. 5841, to which Archer and Carroll referred. This, in fact, was done in part when the various tumor types were combined. If this procedure had been followed through properly, the tumor incidence in Fig. 2 would have been 2.4 per mouse at 18.8 millicurie-days and 4.4 per mouse at 29.3 millicurie-days. No manipulation of these points can produce a linear dose-response curve.

There is a growing trend in radiobiology to develop a theory and then to search for the published data that with a little treatment will support it. Other hypotheses that may equally well be satisfied by the data are no longer given any consideration. If we are to discover how ionizing radiation in general, and Sr^{90} in particular, influences a mammalian population, we should attempt to sharpen our understanding of the mechanisms involved and not simply pass over them with broad a priori assumptions and generalizations.

My original paper did not prove that the dose-response curve was not linear, or that there was a threshold. It did, however, point out that such an interpretation is entirely possible. For reasons pointed out above, the type of analysis presented by Archer and Carroll adds very little to our understanding of the subject.

MIRIAM FINKEL
Argonne National Laboratory, Box 299, Lemont, Illinois

Miriam Finkel's letter correctly points out some of the defects in our knowledge of the effects of internal emitters on animals. We fully agree with her that a better understanding of the mechanisms involved is needed. When new facts are elicited, they should be checked for compatibility with all current theories. Our article supplemented that of Finkel with regard to this checking.

The first of two "fundamental errors" cited by Finkel is really a difference of opinion as to suitable analytical approaches. She rejects our grouping of several different tumors, "some of which have not been observed to change in frequency after Sr^{90} administration." We included all of the "probably malignant" tumors of the specified kinds which occurred in both control and experimental groups. If the frequency had been the same in the control as in the experimental groups, the resulting curve would not have been affected by such grouping. When more tumors of a specific type (or types) occur in the experimental group than in the controls (greater than statistical limits of randomness), it may fairly be assumed that the treatment has been instrumental in producing the extra tumors—and in changing the frequency. The average frequency of each of the tumor types used by us in our Fig. 2 is greater among

the animals to which Sr^{90} was administered than it was among the controls. Since Sr^{90} was the only known tumor-inducing agent administered, these extra tumors of varied types must then be attributed to Sr^{90}. Why then, is it not perfectly legitimate to group them together when assessing the malignant tumor-inducing activity of Sr^{90}?

The primary reason that we grouped several tumor types was that the number of mice used was too small in several of the groups to provide a sufficient number of tumors of a given type to be of statistical significance. When grouped, they appear to become meaningful, as illustrated in our Fig. 2. A secondary reason for grouping them is that once an animal develops one malignant tumor its chances of living long enough to develop a second are decreased. It is reasonable to suppose that when total malignant tumors reach a certain frequency, the relative frequency of individual types may be changed automatically. It was felt that grouping of tumor types might minimize this effect, even though it cannot be eliminated. An example of this interaction between tumor types may be seen in Finkel's work (Argonne National Laboratory Report No. 5597) in the 200, 440, and 880 μc/kg groups. As the incidence of osteogenic sarcomas rises from 18 to 73 and 91 percent, the incidence of reticular tissue tumors falls from 38 to 34 and 15 percent. One might therefore be misled if he confines his attention to a single tumor type. One might also be led to underestimate the total tumor-inducing potency of an agent if this interaction is not considered.

Finkel's example of reticular tumors purporting to illustrate a lack of change after Sr^{90} administration may be misleading. Her data show a small but definite increase of reticular tissue tumors among Sr^{90} treated animals. Among 150 control animals the frequency was 25 percent. Among the 690 Sr^{90} treated animals represented in our Fig. 2, the frequency averaged 30 percent. This increase occurred in spite of the shortened life span and in spite of the increased frequency of sarcomas among the Sr^{90} groups—both of which factors appear to decrease the observed frequency of reticular tissue tumors.

We concede that the second "fundamental error" pointed out by Finkel is a minor error, but one which has no material effect on the results. The terminal point in our Fig. 2 at 29.3 millicurie days, as Finkel suggests, is probably too low. It is low not only because of the saturated response which she details, but because of the depressing effect of a high sarcoma rate on the frequency of other tumors. The

graph in Fig. 2 is only slightly affected by omission of this point.

We agree, from a theoretical standpoint, with Finkel when she states that it might be better to use the tumor rather than the tumor-bearing animal as the statistical unit. However, we must also agree with Wollman [*J. Natl. Cancer Inst.* **16,** 198 (1955)] that this is a very difficult unit to use for the following reasons: (i) new tumors may appear over such a wide range of time intervals after treatment that the experimental animals may die from the first tumor before all potential tumors are detected; (ii) if the tumor metastasizes, it may be difficult to distinguish between a metastasis and a tumor of independent origin; and (iii) the early tumors may coalesce, resulting in an underestimate when scoring late. The first of these two objections would be especially applicable to the sarcomas, and the second would be especially applicable to the reticular tissue tumors.

VICTOR E. ARCHER
BENJAMIN E. CARROLL
National Cancer Institute, National Institutes of Health, Hagerstown, Maryland

Reprint Exchange Center

All of us who consider the scientific reprint as a valuable research and teaching tool, and as a necessary part of our personal reference libraries, have experienced the frustration which accompanies the reply to a specific request indicating that the author's supply of reprints has been exhausted. At the same time, we are all largely guilty of harboring reprints which are no longer pertinent or useful. In addition, we frequently shelve, indefinitely, reprints which were never of any real interest to us and which came into our possession by means unknown or forgotten. In short, many thousands of reprints are taken out of circulation permanently every year, their purpose for existence defeated, when they should be available to those who could make serious and profitable use of them.

It would seem that a free reprint exchange center could be created with the cooperation of interested scientists. Such a center would accept the voluntary contributions of reprints from individual and institutional files, catalog them, and, in turn, make them available to other scientists in response to general or specific requests without charge.

Although I am certain that there are agencies, institutions, and individuals better equipped by virtue of experience, finances, and facilities to undertake such a program. I would, nevertheless, be happy to attempt to establish such a cen-

ter at Duquesne University. In that regard, I welcome the comments, suggestions, and cooperation of scientists everywhere who share these views.

JULIUS S. GREENSTEIN
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Village Solidarity

Peter Suzuki [*Science* **132**, 891 (1960)] makes a most important point and one which I had almost despaired of seeing in a "neutral" journal—that of "village solidarity." While he may overstate the case by saying that what "is generally taking place in many of the underdeveloped countries is a ruralization of the cities. . ." (since the majority of these cities were hardly urban within the meaning this term has assumed in recent years), yet it must be stated repeatedly that, with few exceptions, aid administrators continue to err in failing to recognize that this group attitude is a prerequisite if their rural programs are to meet with success. The opposite view, held in the past and still the principal method of assistance—that this sense of common identity and common purpose must be altered or eradicated before change can take place—has resulted in the almost unqualified failure of rural development programs carried on by the International Cooperation Administration and its predecessors throughout the so-called "underdeveloped nations." I am therefore quite happy that Suzuki has supplied us with an additional case study to substantiate my criticism of the direction these programs have taken. ["*Social and political aspects of Philippine economic development*," Philippine Council, Institute of Pacific Relations (Kyoto Conference, Pacific Council, Institute of Pacific Relations, 1954) (mimeographed)].

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Developmental Selection of Mutations

We should like to comment on the interesting and provocative report by L. L. Whyte entitled "Developmental selection of mutations" [*Science* **132**, 954 (7 Oct. 1960)]. Whyte is well known as a perspicacious and imaginative thinker and, as always, his writings are worthy of consideration. If we understand his report correctly, however, the problem to which he alludes does not really exist.

Whyte's main point seems to be that there is a class of mutations whose role in evolution has not been appreciated by students of the evolutionary process. These are the mutations which prevent "internal organizational efficiency permitting continued growth." In other words, he is referring to lethal and detrimental mutations whose effect is manifest during morphogenesis, as opposed to those genes whose morphogenetic effect is to produce an adult ill-adapted to the adult environment. We are rather aghast to learn that a perusal of the literature has left Whyte with the impression that such genes have been neglected. The existence of a very large class of embryonic lethals is very well known to evolutionists and geneticists in general. Elementary textbooks of population genetics always begin by a discussion of gene-frequency changes in populations, in which unconditional lethals are segregating, before going on to the discussion of more subtle forms of natural selection [see, for example, C. C. Li's textbook, *Introduction to Population Genetics*]. If general works on evolution fail to deal explicitly with such lethal genes, it is largely because they *are* so well known that it hardly seems worthwhile calling further attention to them. As a matter of fact, Lewontin and Dunn have recently published [*Genetics* **45**, 706 (1960)] a report on the evolutionary dynamics of a series of unconditional embryonic lethals in wild populations of *Mus musculus*.

There is, however, a more subtle and more important point here. Students of evolution have emphasized the interaction of environment with genotype because they have for some time realized that there is no real distinction between "developmental selection," as Whyte so aptly calls it, and postembryonic adaptation. There is no sharply defined boundary between unconditional embryonic lethals and those whose effect is intimately bound up with environment. There is, rather, a continuous spectrum of gene effects, from those genes whose action seems virtually independent of any environmental modification, to those whose sensitivity to environment encompasses every slight change of physical and biotic milieu.

We would venture so far as to say that no gene is totally independent of environment in its expression, and therefore the fitness of every genotype is in some measure a function of environment. It is, of course, true that early embryonic lethals whose effect is to completely disrupt the normal morphogenetic pattern to the point of death are less susceptible of modification. But they are not wholly insensitive to it. In general, the more protected

the embryonic tissue is against changes in the milieu, the more unconditional will be the lethality. No one has yet found an environment in which the *t* alleles in the mouse are viable as homozygotes, but in *Drosophila* there are many genotypes that are complete preimaginal lethals at one temperature yet viable at another.

Human disorders such as erythroblastosis foetalis and diabetes mellitus trace back to primary metabolic processes, yet man has created environments in which such genotypes survive. Even for early embryonic lethals in mammals it is reasonably certain that these lethal effects can be traced to some enzymatic imbalances. Presumably the molecules that are missing could be supplied exogenously, perhaps simply by ingestion of food of the right kind. We know very well that even very large protein molecules such as antibodies can pass across the placental membranes. In general, the degree to which the fitness of various genotypes will depend upon the environment is related to the time of action of the genes in development and the proximity of the phenotypic result to the primary gene action. Yet, even when the effect is proximate to the primary gene action, environmental modification must be reckoned with. Thus, there are mutants in *Neurospora* which block single steps in known enzymatic reactions, but whose effects are sensitive to temperature.

Finally, it should be pointed out that geneticists in fields other than evolutionary study have quite deliberately chosen to work with genes that are insensitive to environmental modification. Investigations of the physicochemical nature of the genes are made very much easier by eliminating the confusing effects of environment, just as the early elucidation of the fundamental laws of classical genetics was made possible by the deliberate choice of environmentally insensitive traits.

What Whyte's report suggests is that nongeneticists have not yet fully comprehended the concept of the "norm of reaction." A careful rereading of the first chapter of Schmalhausen's *Factors of Evolution* might be in order. Organisms at every stage of their development are the products of heredity and environment. To maintain any other position is to ignore the overwhelming weight of fact and reason.

R. C. LEWONTIN
ERNST W. CASPARI
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R. C. Lewontin and E. W. Caspari's courteous and informative letter neglects the main issue and is on one point misleading.

The developmental elimination of

harmful mutations has long been studied. This may be called "negative developmental selection"; it produces no evolutionary change, and was discussed only to provide the background to "positive developmental selection." The constructive thread of my argument concerned evolutionary change, this term or an equivalent being used five times. Such change involves positive developmental selection, the preservation of neutral or favorable mutations which, because they conform to the necessary conditions of biological organization in the particular species and therefore survive developmental selection, may determine the direction of evolutionary change—for example, in periods when there are no significant changes in the environment. This is now being studied in a few special fields, but no general inferences regarding these still unknown conditions are yet possible.

The authors, occupied with my assumed ignorance of work on lethals (though deliberately mentioned), seem momentarily to forget that "selection" does not merely mean "elimination" and do not mention evolutionary change! I am grateful; this proves there was a flaw in my exposition; the term "positive developmental selection" is necessary, much current experimental work being on elimination. N. B. Schmalhausen (1949) does not mention positive developmental selection.

The concept of positive developmental selection, already expressed by Dobzhansky [*Genetics and the Origin of the Species* (Columbia Univ. Press, New York, 1937)], Haldane [in *Darwin's Biological Work*, P. R. Bell, Ed. (Cambridge Univ. Press, Cambridge, 1959), p. 147], and possibly others, merits attention. Hence my report. If genetic systems or developmental processes or, more broadly, the conditions of biological organization in the species ever determine the actual, or restrict the possible, direction of evolutionary change, then the now prevailing form of the theory of evolution by adaptive selection and its mathematical expressions are likely to require modification —that is, generalization to include the very different effects of developmental selection.

If the term "developmental selection" is "apt," and the work cited by the authors is relevant, then the problem of developmental selection must "really exist."

This inconsistency arises from a *non sequitur*. It is not correct, as Lewontin and Caspari seem to suggest at one point, that the principles, (i) that internal and external factors interact in some degree at all levels and (ii) that organisms at every stage are products of heredity and environment, make the

problem unreal by preventing the influence of the two classes of factors ever being separated. As they well know, it is normal scientific method in this cosmos of pervasive interactions to restrict the variables so that one thing can be studied at a time; indeed they give an example of this. If there is a continuum of effects the extreme cases are the most important, because simpler. To study positive developmental selection, stimulate mutations but keep the environment constant and observe what evolutionary changes occur, or examine corresponding situations in the past. And on the theoretical side it may one day be possible to predict the class of mutations which is capable of surviving developmental selection in a given species in a constant environment.

Other geneticists, mainly interested in negative developmental selection in microorganisms, have been surprised to discover that ideas vaguely taken for granted for some time have seldom been made explicit in the literature, and that their implications, being radical, have never been developed. [However, see J. Marquand Smith, *Theory of Evolution* (Penguin, Harmondsworth, 1959), for one of the first statements of these ideas in the literature on evolution reaching a wider audience.]

Developmental selection is of great importance, not only for evolutionary theory, but because it may hold clues to the nature of biological organization in general, at each level and in all species, which is the main problem of structural biology. The present condensed analysis is certainly imperfect; not only are the known facts inadequate, but even as a speculative theoretical statement it requires further development.

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Grants and Scientific Freedom

Norman Kaplan's recent article on "Research overhead and the universities" [*Science* **132**, 400 (12 Aug. 1960)] is certainly timely. It seems to me, however, that he has skirted one of the major aspects of the problem.

He gives as one of the three major functions of a university the "extension of knowledge." Traditionally this extension is to be directed exclusively by the university and especially by the individual investigator. Any erosion of this freedom of direction constitutes external "control" and, if it is to be acceptable at all, must be compensated by some very real gain to the welfare of the public. Kaplan has, I think, recognized this in restricting his discussion

to "basic" research. Research which is not "basic" has no proper place on a university campus; if it is admitted at all it should be paid for in full; in fact, not only should the "donor" (purchaser) pay all overhead and administrative costs but he should recompense the university over and above these costs for permitting itself to be diverted from its proper function.

But leaving aside developmental research as an improper function, what about basic research? I would agree with Kaplan that in so far as the university is left in complete control of such research it has no right to expect inclusion of overhead costs. It seems to me, however, that the only way that a university can possibly be left in complete control of its research program is to have it accept only such funds as are without strings. The mere fact of having to spend money on predesignated programs, to make reports of such expenditures, and to be limited in the reallocation of funds constitutes a very real control. The university, in order to obtain such funds, is forced to choose as immediate research objectives not what are the most pressing intellectual and cultural objectives within the total framework of knowledge but, rather, the objectives which will bring the greatest price in the market place. Whether we wish it or not, the whole structure of present-day research financing is contrary to the traditional university function of untrammeled search for truth.

If I am right in this, and I think it is a view tacitly supported by the quotation from DuBridge which Kaplan cites with approval (p. 403), then all grant-sponsored research is "controlled" and should pay full overhead *plus* a subsidy in compensation for this control. Kaplan is wrong in wishing to eliminate such payments.

However, I think he is right in opening up the larger question of whether such research belongs properly in a university. Does not grant research belong more properly in private institutions outside the university framework? I suggest that although the universities have not come out and said so openly they have partly recognized this in setting up semiautonomous agencies which are segregated from the teaching function. The Space Research Institute of Johns Hopkins is a frankly externally oriented agency. The Brookhaven Laboratories constitute another. The Oriental Institute of the University of Chicago is a semiautonomous agency which is *not* externally oriented and as such, I suspect, gets very little financial support from the sources to which Kaplan was referring and expects very little in the way of agreed-upon "overhead" cuts.

I think that funds donated to a university for predesignated projects and programs, by whatever agency, should

be looked upon as constituting a purchase of partial control that should be paid for. I hope that someday someone, perhaps even the government, may recognize the need for truly "free" subsidy of the university's function of the pursuit of knowledge, or really *basic* research. When it does, and funds are given without strings, the university itself will decide what portion is to be allocated to overhead and the question Kaplan is discussing will cease to have pertinence. A note on National Science Foundation grants in the same issue of *Science* [**132**, 405 (1960)] suggests a welcome trend in this direction. Until this trend becomes more definite—that is, so long as Kaplan's question *is* pertinent—I believe the granting agencies are remiss in providing such niggardly funds for overhead.

PHILIP R. WHITE
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I cannot agree with White's contention that all grant-supported research—whether basic or not—is, in effect, "controlled" research. There are significant variations among the granting agencies with respect to the elements of control listed by White. The National Institutes of Health, for example, allow the investigator considerable liberty in reallocating grant funds. Moreover, they extend scientific freedom quite explicitly. In the statement accompanying the notification of the grant award, they inform the investigator that he "is not required to follow the specific details of the project submitted for review, particularly if he finds promising leads that in his opinion are likely to be more productive than the project proposal itself." Not all granting agencies extend these kinds of freedom, to be sure. But I feel it would be unfair to classify all grant research as necessarily controlled.

However, White's main point is an extremely important one, and I would certainly agree that we would do well to re-examine the heavy emphasis on the project-grant system as the best possible mechanism for the support of basic research in the universities.

Despite the title of my article, I tried to make it clear that I do not consider overhead the major issue. I deliberately chose it as a central focus and made the rather drastic suggestion that all overhead payments for basic research in the universities be eliminated as a dramatic way of drawing attention to some of the larger questions involved. I also wanted to show that piecemeal decisions even on relatively trivial issues have consequences for the development of our national policies on the organization and support of scientific research. The drifting tendency toward full reimbursement would, as I indicated in the article, solve few of the basic long-range problems. Once institutionalized, this tendency could result in a situation favored by few and difficult to reverse.

It is precisely for this reason that I would disagree with White's suggestion that the university be recompensed over and above full costs for permitting itself to be diverted from its proper function. This, it seems to me, would "reward" the university for abdicating one of its major responsibilities. And in the present financial crisis this might prove all too tempting to far too many universities which have rarely appreciated fully the significance of the research function. If I understand White's argument, I find this a particularly precarious means of achieving the objective on which we are in complete agreement—namely, a truly "free" subsidy of really basic research.

I should like to see the overhead problem viewed in the context of such unanswered questions as these: Should we continue to rely on the universities as the major producers of basic research? What about those universities which are doing little to establish the conditions considered essential for the effective conduct of basic research? Should we consider encouraging the formation of many more independent research institutes (like the Jackson Memorial Laboratory or perhaps more generally modeled on the Max Planck Institutes) with the specific objective of pursuing basic research?

I might also note that the block grant, favored by many as a superior device for supporting research while providing greater freedom, would probably work out quite differently in independent institutes and in universities. The block grant may be more effective in the institute, where it would go directly to the people committed to, and involved in, research. A block grant to the president of the typical university, removed as he is usually from the research people and faced with conflicting departmental demands and rivalries only partly related to research criteria, may be far less effective than we might typically expect.

I hope I have not given the impression that I think I have some of the answers to the pressing issues raised in either White's letter or my article. As I continue my research in this area, especially through comparative analyses of research organization in different countries and in different institutional frameworks, I hope to find out more about the kinds of questions which must be asked long before any "final" answers can be sought. I appreciate this opportunity to comment on White's very stimulating letter.

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Meetings

McCollum-Pratt Symposium

A symposium, "Light and Life," was held at the McCollum-Pratt Institute of the Johns Hopkins University 28–31 March 1960. The meeting brought together researchers in biology, biochemistry, physical chemistry, and physics, all concerned with some aspect of the interaction of visible radiation with organic chemicals and, ultimately, with living organisms.

There were five sessions, which started with the quantum mechanical description of excited states of molecules, narrowed down to excited states of molecules of biological interest, and then focused on the specific and complementary reactions of chemiluminescence (including bioluminescence) and photosynthesis. The final session was devoted to gross physiological aspects of light—in particular, phototropism and photoperiodism.

W. Robinson discussed the nature of electronic excited states of simple molecules such as formaldehyde using the molecular orbital approximation and the differences in molecular geometry obtained upon excitation in various environments. These differences can be observed as spectral shifts due to changes in charge density. The polarizability of a molecule increases in the excited state, giving rise to attractive forces. However, repulsion also increases since the orbitals become larger. The difference between these two determines the nature of the shift.

M. Kasha discussed "solvent shifts" in terms of lone-pair electrons in molecules and $n \rightarrow \pi^*$ transitions. In the nitrogen heterocyclics, which are nonfluorescent, the $n \rightarrow \pi^*$ transitions can be identified by several criteria: (i) comparison with $\pi \rightarrow \pi^*$ transitions of parent hydrocarbons; (ii) blue-shift on hydrogen-bond formation and disappearance upon the addition of H^+; (iii) comparison of fluorescence versus phosphorescence; (iv) $n \rightarrow \pi^*$ transitions are usually of low intensity, with molecular extinction coefficients of less than 2000; and (v) $\pi \rightarrow \pi^*$ transitions are polarized parallel to the plane of the molecule, while $n \rightarrow \pi^*$ transitions should be polarized perpendicular to the plane of the molecule. These criteria were discussed in relation to pyridine, quinoline, and phenazine, the latter being nonfluorescent but exhibiting a strong infrared phosphorescence at low temperature. The polarization of the $n \rightarrow \pi^*$ transitions may be observable in the oriented fibers of deoxyribonucleic acid. The $\pi \rightarrow \pi^*$ transitions should be in the plane perpendicular to the helical axis, and conversely, $n \rightarrow \pi^*$ transitions should be polarized parallel to the axis of the helix. The long-wavelength shoulder around 3000 A in the absorption spectrum of deoxyribonucleic acid is characteristic of $n \rightarrow \pi^*$ transitions.

J. Platt discussed qualitative methods of assigning electron density distributions to ground and excited states of simple organic molecules and to the geometry of chain molecules such as stilbene in its cis- and trans- configuration and the cyanine dyes. The interesting speculation was made that in the latter case a donor molecule could attach at one end and an acceptor molecule at the other end, the chain serving in effect as a connector in a charge-transfer type of reaction.

Next, G. Porter presented the results of a beautifully conceived series of experiments in oxygen-free solutions, giving direct experimental evidence of triplet-triplet energy transfer. Briefly, the experiments consisted in using solutes where ${}^1\Gamma_B > {}^1\Gamma_A$ and ${}^3\Gamma_B < {}^3\Gamma_A$. In this case, addition of B to a phosphorescent solution of A quenched the ${}^3\Gamma_A$ phosphorescence and resulted in ${}^3\Gamma_B$ phosphorescence. These experiments could have important biological significance as a means of energy transfer. The slightest trace of oxygen quenches all phosphorescence.

G. Weber presented data on the absolute fluorescence yield of the aromatic amino acids as related to their pK values and of the depolarization of their fluorescence at low temperatures as a function of concentration. There is a divergence from the results expected on the basis of the Förster theory of energy transfer.

S. Velick described experiments on the depolarization of the fluorescence of the flavin coenzymes where it can be inferred that oxidation and reduction are accompanied by changes in molecular configuration. Energy transfer from adenine to pyridine in reduced diphosphopyridine nucleotide implies a folding of the nucleotide. The binding of these coenzymes onto the lactic dehydrogenase enzyme also affects the molecular configuration. In the discussion period, H. Beinert presented data on the flavin mononucleotide absorption in various states of reduction, and B. Commoner discussed electron spin resonance data on succinic dehydrogenase and pig-heart mitochondria, showing the existence of free radicals in enzyme complexes.

In the session on chemiluminescence, H. Linschitz presented data on the chemiluminescence of tetralin hydroperoxide catalyzed by porphyrin molecules. The mechanism is interpreted in

terms of a bimolecular reaction, and the catalytic action of the porphyrin molecule was discussed. E. White summarized the available data on hydrazide chemiluminescence and presented several possible mechanisms for the general case of chemiluminescence. Experiments by White and his co-workers and by H. H. Seliger indicate that the luminol chemiluminescence in aqueous solution is the fluorescence of the excited oxidized product molecule. Seliger found that the quantum yield, fluorescence yield, and spectral emission support this hypothesis. W. D. McElroy and Seliger presented a summary of present information on the physics and biochemistry of the chemiluminescence reaction in biological systems where catalysis is by enzymes. In the firefly the chemiluminescence efficiency is practically 100 percent. Spectral data, quantum-yield data, and a series of ingenious biochemical investigations on both firefly and bacterial bioluminescence have brought the subject at least to the place where meaningful questions concerning the detailed mechanism of oxidation can be put to experimental test. F. McCapra reported on the partial success in elucidating the structure of firefly luciferin as a hydrocarbon-linked thiazole and benzthiazole ring.

The subject of conversion of light to chemical energy in photosynthesis provided some of the most active discussions of the session. M. Calvin described model solid-state experiments in which light energy absorbed by phthalocyanine led to the reduction of *o*-chloranil in an adjoining layer. These reactions in the solid state were contrasted with reactions of chlorophyll or of other dyes in solution. Some evidence was presented suggesting that the photophysical chemistry occurring in chloroplasts might be more closely related to the solid-state model than to the reactions occurring in solution.

The seed-yeast provided by the late R. Emerson, in the form of experiments showing that photosynthesis may require the cooperation of light quanta absorbed by two different pigments, showed signs of increasingly vigorous fermentation at the meeting. While J. Franck had proposed earlier, and discussed further at this symposium, an explanation for the effect in terms of photochemistry and energy transfer, the alternative possibility has remained open—that different excited pigments might be performing different biochemical tasks. Some such differential function was strongly suggested in a number of kinds of experiments described by C. S. French. In particular, the time course of photosynthesis immediately after turning the light on, and the time course of respiration immediately after

turning the light off, show very significant differences, depending on the pigment which is illuminated (the "chromatic transients" discovered by Blinks). A highly significant finding is that the cooperation between pigments can occur over time intervals of the order of seconds, indicating that ephemeral excited states cannot be the meeting ground for light energy coming from the two pigments. Another new finding of considerable interest (supported in papers by Rabinowitch and Govindjee, and by Mary Belle Allen) is the existence of different in vivo forms of chlorophyll *a*, some of which evidently perform the same function that the accessory pigments do.

B. Kok and G. Hoch reported on various experiments with both whole cells and isolated chloroplasts, in which illumination at different wavelengths led to specific changes in the absorption spectra of the photosynthetic material. This work may be at the point of bridging the gap between photochemistry and biochemistry, in that a number of reagents with known effects on electron-transport processes in chloroplasts apparently had a consistent series of effects on the absorption-spectrum shifts. Kok and Hoch have also apparently found that light absorption by chlorophyll *a* by itself is basically able to carry out photosynthetic phosphorylation with isolated chloroplasts, even though light absorption by two pigments is needed for over-all photosynthesis.

R. Hill presented a speculative working hypothesis for the mechanism of a two-quantum process in photosynthesis. The first quantum should oxidize cytochrome *f*; this, in turn, should (in a dark reaction) oxidize cytochrome *b*, generating adenosine triphosphate, and the oxidized cytochrome *b* should be reduced in a second light-requiring step which also liberates oxygen. W. D. Bonner described the more recent evidence for cytochromes in chloroplasts, and Lucile Smith reviewed their function in bacterial (*Rhodospirillum rubrum*) photosynthesis. M. D. Kamen suggested, as a possible first step in photosynthesis, the simultaneous production of a reductant (perhaps partially reduced chlorophyll) at a potential of −0.6 volt and an oxidant (his candidate being a heme in a valence state of + 4) with a potential of + 0.9 volt.

Arnon reviewed photosynthetic phosphorylation, with special emphasis on the possibilities for different electron transport pathways in chloroplasts. A. San Pietro described the reduction of triphosphopyridine nucleotide—a special case with its own special enzymes. Evidence relating to molecular oxygen as a terminal electron acceptor from the

photoreductant was discussed by Vennesland, Nakomoto, and Stern, by D. W. Krogmann, and by Jagendorf and Forti. In addition, Vennesland and his colleagues described an apparent confirmation of Warburg's recent discovery that carbon dioxide is required (but not consumed) in the Hill reactions.

The existence of adenosine triphosphatase reaction in chloroplasts under rather specialized conditions was described by Barbara Petrack and F. Lipmann. A lower level of adenosine triphosphatase activity was described by Strehler and Hendley.

K. V. Thimann reviewed the phototropic phenomena in plants and suggested a correlation between the orientation of plastids with respect to light and the phenomenon of lateral transport of growth substances under the influence of light. Further data on phototropism were presented by A. W. Galston and R. Kaur. C. Pittendrigh discussed photoperiodic phenomena, including both plant and animal mechanisms, and W. S. Hillman described experimental modifications of photoperiodic reactions in *Lemna minor*.

In the final session, on vision, discussion ranged from the biochemical basis, described by George Wald, to the electrophysiological responses, discussed by W. A. H. Rushton, T. Goldsmith, and E. F. MacNichol, Jr. The latter, in a very interesting paper, was able to show that the electrical discharges in the optic nerve fibers of the goldfish are differentially affected by changes in wavelength in such a manner as to provide a mechanism for color vision. Color vision in insects was reviewed by T. Goldsmith, with special emphasis on evidence for its occurrence and the spectral sensitivities found.

W. A. H. Rushton considered especially the problem of how nerve signals are generated after bleaching of the visual pigments. This question represents one of the major gaps in our understanding of the mechanism of vision at the present time and was not answered at this symposium. However, various characteristics of the system were defined by Rushton from the relationships between light threshold and the amount of pigment bleached, and between the light intensity and the kind of nerve signal generated.

The symposium volume, to be published shortly by the Johns Hopkins Press, should serve as a useful and up-to-date guide in the range of topics covered.

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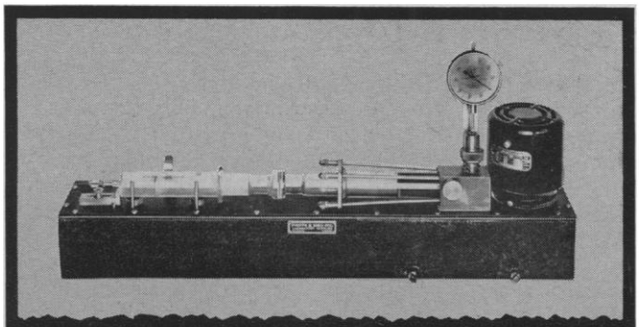
Physicomathematical Aspects of Biology

A 3-week postgraduate course on physicomathematical aspects of biology was given in Varenna, Italy, from 11 to 30 July, under the sponsorship of the Italian Physical Society. It was the 16th of the series of postgraduate summer courses that were sponsored by the society.

N. Rashevsky, chairman of the committee on mathematical biology at the University of Chicago, was invited to organize and direct the course. The objective of the course was to present to the students a balanced program of theoretical and experimental research in selected subjects. This objective determined to a large extent the selection of the eight lecturers. Three of the lecturers—A. Bartholomay (Harvard), H. D. Landahl (Chicago), and N. Rashevsky—represented the theoreticians. The other five lecturers—E. Boeri (Ferrara, Italy), M. A. Bouman (Soesterberg, Holand), J. Defares (Leiden, Holland), M. Polissar (San Francisco), and M. Wise (Belmont-



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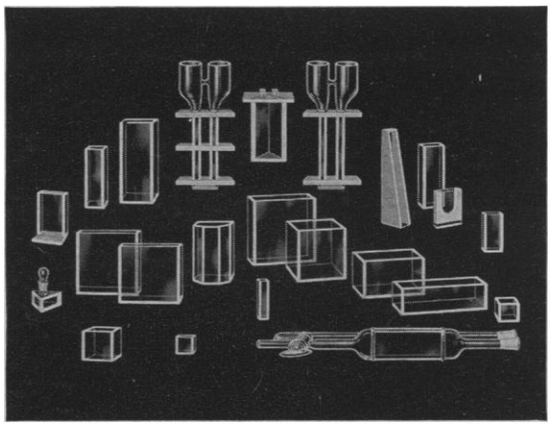

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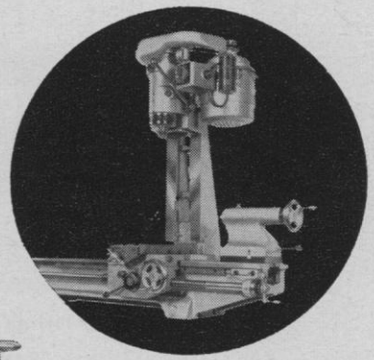

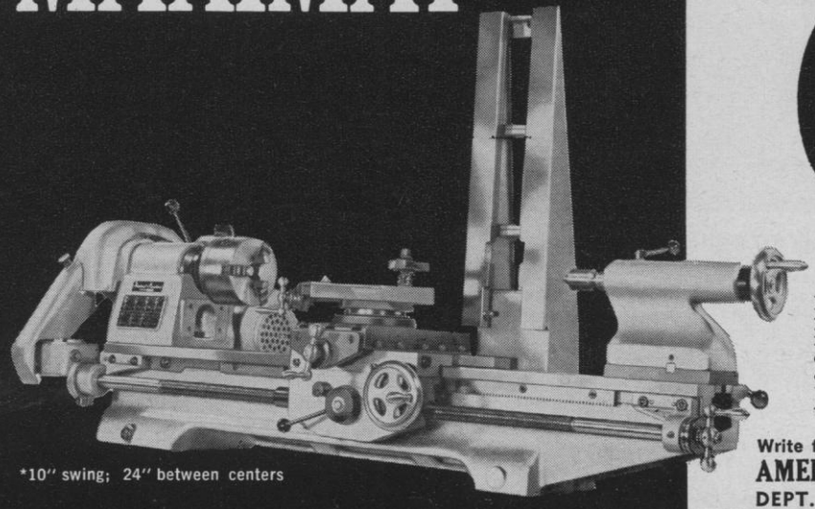

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Sutton, England)—represented both experimental and theoretical research.

Altogether, 53 1-hour lectures were given. There were two lectures in the morning and one or sometimes two in the afternoon, with ample time left for discussions. Except for the four lectures by Rashevsky, which were given at the end of the course, the lectures of all speakers were spread over the whole period. All lectures were given in English.

Of the 26 students, two were Americans, one was German, one was a Swede, one was a Dane, and the others were Italians. The physical and the biological sciences were about equally represented among the students. In academic attainment the students ranged from full professor to young Ph.D.'s.

The same topics were sometimes discussed by two different lecturers, one discussing from a theoretical, the other from an experimental, point of view.

Bartholomay lectured on the general subject of reaction rates. The specific topics covered were the classical deterministic model of reaction rates, the collision theory, the modern transition state theory, and the Michaelis-Menton model of enzymatic reactions.

Boeri discussed the general subject of enzyme reactions. He spoke on reactions between proteins and ligands, reaction of hemoglobin with gases, electrotitration of proteins, antigen-antibody reactions, extension of the Michaelis-Menton theory, specificity of reactions, multienzyme states, and the appearance of enzyme activity. His lectures represented a blend of experimental and theoretical considerations. They were closely correlated with the lectures of Bartholomay, and numerous references to the presentation of the other were given by both lecturers.

Bouman spoke on sensory phenomena, discussing the quantum theory of vision (particularly such topics as the dependence of visual threshold on the time and on the stimulated area), color vision, adaptation, discrimination of intensities, visual acuity, color discrimination, and directional color effects in the retina. His lectures, which also represented a blend of the mathematical and the experimental approach, were illustrated by numerous slides.

The general topic of Defares's lectures was the physics of respiration. He discussed, both from a theoretical and experimental point of view, the respiratory system as a feedback mechanism, giving a cybernetic analysis of the respiratory "chemostat." Defares also discussed the problems of the rise of carbon dioxide tension in the lung during rebreathing and his joint work with Wise (presented by the latter) on a mathematical method of estimating the statistical distribution of inspired gases from experimental mixing curves.

Landahl lectured on four different subjects. The first was the mathematical theory of the central nervous system and its applications to numerous phenomena, such as reaction times, flicker phenomena, apparent movement, discrimination and learning, conditioning, and color vision. Several of these topics were closely related to topics discussed by Bouman, and a useful exchange of ideas took place.

The second subject of Landahl's lectures was the mathematical theory of some pharmacological phenomena. He discussed such problems as interaction of drugs, a mathematical model of response to Pitressin, and a mathematical model of the effect of aldosterone on salt excretion.

His third topic was the theory of diffusion phenomena. He discussed steady states and transient phenomena in biological systems and presented a mathematical theory of the diffusion of population with simultaneous growth. He then discussed the application of diffusion theory to spreads of rumors.

Landahl's final subject was the theory of removal of airborne particulate matter from the respiratory tract and the practical application of the theory.

Polissar spoke on mathematical models of the human heart, discussing, in terms of the proposed models, a number of experimental results, such as the analysis of indicator curves. Several alternative models and their experimental implications were discussed.

Wise gave three lectures on human radiation hazards, discussing such problems as the assessment of a small risk in a very large population and the estimation of elimination rates for radioactive particles within the body. He also gave a discussion of radiation-induced leukemia.

Rashevsky gave four lectures on general mathematical principles in biology. In the first two lectures he discussed his own work and the work of his associates on the problem of organic form from the point of view of the "principle of optimal design." In the last two lectures he spoke of the "principle of biological epimorphism," which emphasizes the relational aspects of biology and leads to a number of conclusions, some of which may be of clinical importance.

The course will be published as a book.

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Forthcoming Events

December

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.

27–28. Council for the Improvement of Scientific Communication—Cong. on Educology and Role Growth Theory of Communication and Learning, New York, N.Y. (R. L. Switzen, 840 Grand Concourse, New York 51.)

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.)

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

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

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.)

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

28–30. National Council of Teachers of Mathematics, Tempe, Arizona. (M. H. Ahrendt, 1201 16 St., NW, Washington 6, D.C.)