The New Brunswick Scientific Company Presents The PsycroTherm™

A CONTROLLED-ENVIRONMENT INCUBATOR-SHAKER

U.S. Patent No. 3,002,895

For the Growth of Microorganisms under Controlled Conditions of Temperature, Atmosphere, and Agitation

The PsycroTherm is a rigidly controlled environmental incubator with a continuous-duty shaking mechanism. Though it occupies comparatively little floor area, it has 10½ cubic feet of usable work space in the incubation chamber, where static and shake cultures can be incubated simultaneously or separately.

A VERSATILE UNIT
With fully integrated heating and refrigeration systems the unit is ideal for work with psychrophilic, mesophilic, and thermophilic systems. Temperatures can be accurately regulated from 0° C to 60° C with a control tolerance and temperature gradient both within ± 0.5° C. In non-refrigerated units, the temperature range is from ambient to 60° C, with the same tolerance and gradient as above.

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

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

Continuous Duty Shaking
The shaking mechanisms are precision built for continuous operation, long life, and for smooth, quiet, reproducible agitation. Speed is adjusted mechanically and will never drift or vary when workloads or voltages change. The rotary shaker mechanism has a range of speeds between 50 rpm and 400 rpm. The reciprocating shaker mechanism has an adjustable stroke from 0 to 3½” and a speed range between 40 and 285 oscillations per minute.

Overall Dimensions: Width 40”, Depth 29”, Height 65”
Chamber Dimensions: Width 32½”, Depth 21”, Height 26”

Unconditional One-Year Warranty
Write for Catalog G26S/4132

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Inaccuracies in measurements made under the microscope are nearly always due to uncertainty as to the precise location of the reference line used (either the wire of a filar micrometer eyepiece or the graduations on a graticule). Location of the reference line at the very edge of an object is inherently difficult, time-consuming and rarely is the location precisely the same from operator to operator.

With the new Cooke-A.E.I. Image Splitting Microscope measuring settings of a type to be described can be made easily, with extreme precision and unequalled operator-to-operator repeatability. Here, briefly stated, are some of the characteristics of the instrument:

1. Measuring accuracy is as high as 0.125μ (0.000005"), depending upon the Numerical Aperture of the objective in use.

2. Comparative measurements can be made without actually measuring. Operator can determine at a glance which particles in a field are larger than, smaller than or equal to a particular dimension.

3. Rigidity of the microscope, or a lack thereof, does not affect accuracy. Measurements can even be made on slowly moving objects.

The Image Splitting Microscope consists essentially of a special prism assembly mounted in a conventional compound microscope system. Prisms are linked to a micrometer screw by means of which their angular relation to each other can be varied. When the prism faces are parallel to each other, two images of the object, exactly superimposed and appearing as one, will be visible in the eyepiece. As the micrometer screw is turned the images move (or shear) across each other. Four different relations of the two images are possible:

A

Double images of object, exactly superimposed. Prism faces parallel — zero shear.

B

Images of object overlapping. Amount of shear less than object dimension.

Images of object just touching. Amount of shear equal to object dimension.

Images of object apart. Amount of shear greater than object dimension.

The edge-to-edge setting (as in C) is made with great precision, since both images are of identical appearance and sharpness and the transition from bright to dark in the area between the images is very distinct. To avoid confusion in a crowded field of view color filters can be introduced, coloring the two images distinctively.

To make an exact measurement, setting is made to the relation C, then to the reversed relation C and the total amount of micrometer run read off. Calibration of this value for the various magnifications produced by the microscope system is routine. With calibration the amount of micrometer screw shear is readily converted into an absolute measurement. Figures B, C and D show how the comparative "measurements", mentioned under 2. above and of such value in particle size analyses, are made.

<table>
<thead>
<tr>
<th>Objective Power</th>
<th>Reading Accuracy</th>
<th>Maximum size object which can be completely sheared (10X eyepiece in use)</th>
</tr>
</thead>
<tbody>
<tr>
<td>3X (N.A. 0.1)</td>
<td>0.0001&quot;</td>
<td>2.5μ</td>
</tr>
<tr>
<td>5X (N.A. 0.15)</td>
<td>0.00008&quot;</td>
<td>2.0μ</td>
</tr>
<tr>
<td>10X (N.A. 0.28)</td>
<td>0.00004&quot;</td>
<td>1.0μ</td>
</tr>
<tr>
<td>20X (N.A. 0.50)</td>
<td>0.000026&quot;</td>
<td>0.6μ</td>
</tr>
<tr>
<td>40X (N.A. 0.65)</td>
<td>0.0000128&quot;</td>
<td>0.325μ</td>
</tr>
<tr>
<td>100X (N.A. 1.30)</td>
<td>0.000005&quot;</td>
<td>0.125μ</td>
</tr>
</tbody>
</table>

Note 1. Under very favorable conditions estimations can be made to twice the above accuracies.

Note 2. It is possible to detect conditions of image overlap indicating values so small that they cannot be measured with the micrometer. (For instance, small variations in diameter of fine wires or rods.)

References


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Scientists in the News

Pierre Gloor, assistant professor, and chief of the laboratory of electroencephalography at McGill University's Montreal (Canada) Neurological Institute, has received the Swiss Academy of Medical Sciences' Robert Bing prize.

V. B. Wigglesworth, of the University of Cambridge (England), is lecturing on insect physiology at the University of Chicago during the month of April.

At Carnegie Institution of Washington:
Ronan O'Rahilly, special fellow in the public health service department of embryology, will become director of the department of anatomy at St. Louis (Mo.) University School of Medicine, effective in September.
Sir Harold Jeffreys, fellow of Saint John's College in Cambridge, England, has received the institute's $6000 Gilbert award for research in seismic geology.

Robert B. Woodward, Donner professor of science at Harvard, has received the 1962 Priestley award for his work in organic chemistry. The $1000 award is presented annually by Dickinson College in Carlisle, Pa.

George J. Huebner, Jr., executive research engineer for Chrysler Corporation, has received the turbine award of the American Society of Mechanical Engineers for his development of the gas turbine engine for passenger cars.

William A. Sodeman, dean of the Jefferson Medical College of Philadelphia, will receive the Clarence E. Shaffrey award, presented annually by Saint Joseph's College medical alumni.

Taivo Laevastu, fisheries oceanographer with the U.N. Food and Agriculture Organization, has joined the staff of the University of Hawaii as associate professor of oceanography.

Raymond B. Allen, former director of the U.S. economic mission to Indonesia, has been appointed chief of the Pan American Sanitary Bureau's office of research coordination.

John C. Duncan, emeritus professor of astronomy at Wellesley College, has retired after a 12-year term as visiting professor at the University of Arizona.

Eduardo Eidelberg, former associate research anatomist at University of California (Los Angeles) School of Medicine, has been appointed chairman of the division of experimental neurology at Barrow Neurological Institute, Phoenix, Ariz.

Allan L. Forbes, clinical director of the Veterans Administration hospital in Richmond, Va., has joined the National Institute of Arthritis and Metabolic Diseases as assistant medical program director for the interdepartmental committee on nutrition for national defense.

Ralph L. McCready, former director of research at Collins Radio Company in Cedar Rapids, Iowa, has been appointed manager of the Motorola, Inc., Systems Research Laboratory in Riverside, Calif.

Recent staff appointments at the University of Cincinnati:
Doris T. Allen, associate professor of psychology, has been designated a Chevalier of the Order of Palmes Académiques—the academic counterpart of the French Legion of Honor—for founding the Children's International Summer Villages.

Ting Yi Li, professor of aeronautical engineering and astronautics at Rensselaer Polytechnic Institute, has been appointed professor of aerospace engineering in the university's College of Engineering.

Hayden C. Nicholson, executive director of the Hospital Council of Greater New York, has been appointed dean of the University of Miami School of Medicine.

Richard S. Berk, senior research biochemist for Magna Products in Anaheim, Calif., has been named associate professor of microbiology at Wayne State University College of Medicine.

Calvin W. Tribiano, pediatrician formerly with Ciba Pharmaceutical Company, has been appointed medical director of Walker Laboratories, Inc., in Mount Vernon, N.Y.

Raymond R. Suskind, associate professor of industrial medicine and dermatology at the University of Cincinnati College of Medicine, will head the newly formed division of environmental medicine at the University of Oregon Medical School.

Walter E. Loomis, professor of plant physiology at Iowa State University, is spending the spring term as visiting professor of botany at Duke University.

Eugene Eichler, of Oak Ridge (Tenn.) National Laboratory's chemistry division, is serving a year as an adviser on nuclear chemistry at the Israel Atomic Energy Commission, Rehovoth, under an International Atomic Energy Agency exchange program.

Francis J. Michelini, recently appointed professor of biology at Wilkes College in Wilkes-Barre, Pa., will be on leave during 1962-63, serving as assistant director of the National Science Foundation's institutes section.

Recent staff appointments at Lockheed Missiles & Space Company in Sunnyvale, Calif.:
Carmine M. Bertone, former research psychologist in the Federal Aviation Agency's human factors research branch, has been named engineering psychologist.
Frank Herman, theoretical physicist formerly with RCA Laboratories in Princeton, N.J., has been named consulting scientist for electronics research.

Recent Deaths

Albert E. von Doenhoff, 51; staff scientist at National Aeronautics and Space Administration's office of space sciences; 23 Mar.

Andrew E. Douglass, 94; emeritus professor of astronomy and dendrochronology, and director emeritus of the Steward Observatory and the Laboratory of Tree-Ring Research at the University of Arizona; 20 Mar.

Thomas R. Goethals, 71; emeritus clinical professor of obstetrics at Harvard Medical School; 23 Mar.

Mary T. Harman, 84; emeritus professor of zoology at Kansas State College; 16 July.

Wendell A. Parker, 54; retired chief of surgery in the Veterans Administration's benefit office; 20 Mar.

George E. Potter, 63; professor in the zoology department at Agricultural and Mechanical College of Texas; 24 Mar.

Erratum: In the announcement of the films "The world within" and "The development of bacteriophage plaques" (Science 135, 1053 (23 Mar. 1962)), the prices quoted were not rental fees, but purchase prices. Rental fees are $10 and $1.50, respectively.
activation enzymes, transfer RNA's, guanosine triphosphate, and ribosomes are reviewed. The final chapter deals with those aspects of enzymatic adaptation which throw light on the mechanisms and control of protein synthesis. The author considers successively induction and repression of enzyme synthesis in microorganisms, non-Mendelian hereditary factors, changes in protein synthesis during differentiation, and the synthesis of antibodies.

Although there are a number of typographical errors, this book, on the whole, is well written. The experimental basis for theories of protein biosynthesis is extensively recorded, and the book should be of value to research workers even after some of the current theories have become obsolete.

H. S. LORING
Department of Chemistry, Stanford University

Concise, Clear, Utilitarian


Publication of this second edition of Minerals for the Chemical and Allied Industries again brings to our attention the valuable contribution that Sydney J. Johnstone and Margery J. Johnstone made in assembling a comprehensive volume based largely on the industrial applications and specifications of minerals and metals. The first edition, so well received in 1954, has been carefully brought up-to-date; six chapters have been added, and parts of the volume have been completely rewritten to make available descriptions of recent technological advances and data, previously classified. Although the book was prepared in Great Britain and draws heavily upon British publications for source material, its coverage is world-wide in scope, and many other sources have been utilized, including the principal U.S. agencies concerned with material specifications as well as numerous previously unpublished reports.

The format is nearly identical with that of the first edition. Each of the 77 chapters covers a commodity or group of commodities, arranged alphabetically. The typical chapter contains an introductory section, which briefly covers natural occurrences and includes general observations on the factors that control uses and marketing. Material on world production, extraction, and use is presented in the sections that follow. The bibliographies were selectively assembled, and they provide separate lists of publications exclusively devoted to standard specifications.

The emphasis placed upon utilization and specifications makes this book unique among the large, commodity-oriented volumes, such as Industrial Minerals and Rocks and Mineral Facts and Problems, and a valuable companion to them. Unlike some other commodity volumes the Johnstones' volume is industrially oriented, and includes both metallic and nonmetallic materials, thereby providing more complete coverage and ignoring the commonly arbitrary distinction between the two.

Because this book has only two authors rather than many contributors, its coverage is very well balanced and the ease of reference is at once obvious. Moreover, the text is refreshingly concise and readable, strongly suggesting that its preparation was a labor of love for the authors. Although both editions are primarily intended for use by mineral producers and consumers, the second edition especially should appeal to all persons concerned with mineral economics and economic geology.

LAUREN A. WRIGHT
Department of Geology, Pennsylvania State University, University Park

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17-20. Sector-Focused Cyclotrons, conf., Los Angeles, Calif. (B. T. Wright, Dept. of Physics, Univ. of California, Los Angeles 24)
18-20. Information Retrieval in Action, conf., Cleveland, Ohio. (Center for Documentation and Communication, Western Reserve Univ., 10831 Magnolia Dr., Cleveland 6)
19. Southern California Acad. of Sciences, Los Angeles. (G. Sibley, Los Angeles County Museum, 900 Exposition Blvd., Los Angeles 7)
19-20. Southern Municipal and Industrial Waste Conf., Chapel Hill, N.C. (Dept. of Sanitary Engineering, Univ. of North Carolina, Box 899, Chapel Hill)
20-22. Czechoslovak Soc. of Arts and Sciences in America, 1st natl. congr., Washington, D.C. (K. Rechcigl, Jr., 1703 Mark Lane, Rockville, Md.)
21. Pennsylvania Acad. of Science, Pittsburgh. (K. B. Hoover, Messiah College, Grantham, Pa.)
23-25. Canadian Inst. of Mining and Metallurgy, annual, Ottawa, Ont. (C. Gerow, CIMM, 1117 St. Catherine St., W. Montreal 2, Quebec, Canada)
23-27. Problems in Education and Re-
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search in Tropical Biology, conf., San Jose, Costa Rica. (J. M. Savage, Dept. of Biology, Univ. of Southern Calif., Los Angeles 7)

23-5. Television Arts and Sciences, intern. symp. and festival, Montreux, Switzerland. (Intern. Television Symp., 8, Grand-Rue, Montreux)


25-27. International Federation of Associations of Textile Chemists and Colorists, annual, Amsterdam, Netherlands. (J. Boulton, Dean House, 19, Piccadilly, Bradford 1, Yorks, England)

25-27. Present Status and Future Prospects of Television and Motion Pictures as Media for Medical Education, intern. conf., Milan, Italy. (L. L. Leveridge, Medical Television Unit, New York Univ. Medical Center, 550 First Ave., New York 16)


27-28. Idaho Acad. of Science, annual, Moscow. (L. M. Stanford, College of Idaho, Caldwell)

27-29. Oklahoma Acad. of Science, Woodward. (A. D. Buck, Northern Oklahoma Junior College, Tonkawa)

27-29. West Virginia Acad. of Science, Bethany. (J. D. Draper, Dept. of Chemistry, West Virginia Univ., Morgantown)


29-2. International Acad. of Pathology–American Assoc. of Pathologists and Bacteriologists, Montreal, Canada. (F. K. Mosoff, c/o Armed Forces Inst. of Pathology, Washington 25)


29-3. American Ceramic Soc., annual, New York, N.Y. (C. S. Pearce, ACS, 4055 N. High St., Columbus 14, Ohio)

29-4. Society of Motion Picture and Television Engineers, annual, Los Angeles, Calif. (H. Teitelbaum, SMPTE, 55 W. 42 St., New York 36)

30-1. International Acad. of Pathology, annual, Montreal, Canada. (M. Davis, International Committee on Pathology Information, 1785 Massachusetts Ave., NW, Washington 6)

30-1. International Acetylene Assoc., annual, Toronto, Canada. (L. Matthews, 30 E. 42 St., New York 17)

30-2. American Soc. of Mechanical Engineers, Design Engineering Div., Phil-
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2-6. Film Techniques, conf., Budapest, Hungary. (Hungarian Soc. of Optics, Acoustics and Film Techniques, Szabad-ság tér 17, Budapest V)
2-8. Space Science Symp., intern., Washington, D.C. (Secretary, Committee on Space Research, 28 Nieuwe Schoolstraat, The Hague, Netherlands)
3-5. Kansas Acad. of Science, Pittsburgh. (G. A. Leisman, Dept. of Biology, Kansas State Teachers College, Emporia)
3-5. Ohio Acad. of Science, Toledo. (G. W. Burns, 505 King Ave., Columbus 1, Ohio)
3-5. Society for American Archaeology, Tucson, Ariz. (J. B. Wheat, Univ. of Colorado Museum, Boulder)
3-5. University Computing Centres, intern. conf., Mexico, D.F.. Mexico. (Centro Electronico de Calculo, Universidad Nacional Autónoma de México, Mexico, D.F.)
3-7. German Soc. of Metallurgy and Mining, Berlin. (Gesellschaft Deutscher Metallhütten und Bergleute, Schlesisfach 51, Clausthal-Zellerfeld, Germany)
4-5. Minnesota Acad. of Science, annual, Winona. (J. P. Emanuel, 206 E. Howard, Winona)
4-5. North Dakota Acad. of Science, Fargo. (B. G. Gustafson, Box 573, Union Station, Grand Forks, N.D.)
4-5. Population Assoc. of America, Madison, Wis. (K. B. Mayer, Dept. of Sociology and Anthropology, Brown Univ., Providence, R.I.)
4-5. South Dakota Acad. of Science, Vermillion. (T. Van Bruggen, Dept. of Botany, Univ. of South Dakota, Vermillion)
4-6. Protides of the Biological Fluids, colloquium, Bruges, Belgium. (H. Peeters, Sint Jans Hospital, Bruges)
4-6. Wisconsin Acad. of Sciences, Arts and Letters, La Crosse. (T. J. McLaughlin, Univ. of Wisconsin, 3203 N. Downer Ave., Milwaukee 11)
5-6. Academy of Psychoanalysis, annual, Toronto, Canada. (J. H. Merin, 125 E. 63 St., New York 21)
5-6. Society of Biological Psychiatry, annual, Toronto, Canada. (G. N. Thompson, 2010 Wilshire Blvd., Los Angeles 57, Calif.)
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13 APRIL 1962


6–12. World Congr. of Gastroenterology, Munich, Germany. (G. A. Martini, Martinstr., 52, Hamburg 20, Germany)

7. League against Trachoma, annual, Paris, France. (J. Sédan, Ligue contre le Trachoma, 94, rue Sylvabelle, Marselles, France)


7–9. Implications of Organic Peroxides in Radiobiology, intern. symp., Argonne, Ill. (F. F. Feinsten, Div. of Biological and Medical Research, Argonne Natl. Laboratory, Argonne)


7–11. American Psychiatric Assoc., Toronto, Canada. (C. H. Branch, 156 Westminster Ave., Salt Lake City, Utah)

7–11. American Soc. of Tool and Manufacturing Engineers, annual convention and tool exposition, Cleveland, Ohio. (A. Cervenka, Vanderbilt Blvd., Oakdale, N.Y.)


7–11. Society of Photographic Scientists and Engineers, annual, Boston, Mass. (E. S. Cobb, Box 1609, Main Post Office, Washington, D.C.)


8. American Soc. of Safety Engineers, Chicago, Ill. (A. C. Blackman, ASSE, 5 N. Wabash Ave., Chicago 2)

8. World Health Assembly, annual, Geneva, Switzerland. (World Health Organization, Palais des Nations, Geneva)

8–10. American Soc. of Lubrication Engineers, annual, St. Louis, Mo. (A. E. Cicheelli, Bethlehem Steel Co., 701 E. Third St., Bethlehem, Pa.)


8–19. Latin American Meeting on Higher Agricultural Education, Medellin, Colombia. (Intern. Agency Liaison Branch, Office of Director General, U.N. Food and Agriculture Organization, Viale delle Terme di Caracalla, Rome, Italy)


9–12. Glass Technology Conf., Baden-Baden, Germany. (Deutsche Glashandelsgesellschaft, Bockenheimerlandstr. 126, Frankfurt am Main)


9–12. Virginia Acad. of Science, Norfolk. (P. M. Patterson, Hollins College Branch, Roanoke)

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168 SCIENCE, VOL. 136
9-19. Prediction of Volcanic Eruptions and the Relationship between Magmas and the Nature of Volcanic Eruptions, symp., Tokyo, Japan. (Secretary, Organizing Committee, c/o Science Council of Japan, Ueno Park, Tokyo)

10-12. Meetings on Diabetes, University of Paris, Paris, France. (M. Rathey, Hotel-Dieu, Paris)


10-11. American Inst. of Chemists, Inc., Chicago, Ill. (J. Kotrady, c/o AIC, 60 E. 42 St., New York 17)


12. International College of Surgeons, clinical meeting, London, England. (Secretary, ICS, 1516 Lake Shore Dr., Chicago 10, Ill.)

13-16. American Acad. of Dental Medicine, annual, Baltimore, Md. (P. Block, 36 N. Luzerne Ave., Baltimore)

13. Transfer of Calcium and Strontium across Biological Membranes, conf., Ithaca, N.Y. (R. H. Wasserman, Dept. of Physical Biology, New York State Veterinary College, Cornell Univ., Ithaca)


14-18. American Soc. of Civil Engineers, convention, Omaha, Neb. (W. H. Wisely, 345 E. 47 St., New York 17)


16-17. Navy Medical-Dental TV Workshop, Bethesda, Md. (Inst. for Advancement of Medical Communication, 33 E. 68 St., New York 21)

16-18. Conference on Dust, Scheveningen, Netherlands. (Fachgruppe Staubtechnik, Prinz-Georg-Str. 17/79, Düsseldorf 10, Germany)

16-18. Noise Abatement, intern. congr., Salzburg, Austria. (Österreichischer Arbeitsring für Lärmbekämpfung, Stubenring 1, Vienna 1, Austria)


17-19. American Inst. of Industrial Engineers, annual, Atlantic City, N.J. (W. J. Jaffe, Newark College of Engineering, Newark, N.J.)

17-19. Eccrine, Apocrine, and Holocrine Glands, symp., Madison, Wis. (Div. of Postgraduate Medical Education, University of Wisconsin Medical School, Madison 6)

17-19. Nephrology, intern. conf., Catania, Sicily. (S. Rapisardi, Via Mavilla 37, Catania)


17-20. International Medical Soc. of Endoscopic Photocinematography, Television and Radiocinematography, Louvain, Belgium. (J. M. Dubois de Monrey, Société Médicale Internationale d'Endoscopie et de Radiocinématographie, 4, rue du Général-Baratier, Rheims, France)
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22–26. Disposal and Utilization of Solid Domestic and Industrial Wastes, intern. congr., Essen, Germany. (Haus der Technik, Schliessfach 668, Essen)

22–26. International Medico-Athletic Federation, congr., Santiago, Chile. (G. La Cava, Via A. Serra, 104, Rome, Italy)


23–25. American Soc. for Quality Control, annual, Cincinnati, Ohio. (A. W. Worthington, Texas Instruments, Inc., P.O. Box 5474, Dallas 22)

24–26. Institute of Radio Engineers, conf. on space communications, Seattle, Wash. (IRE, 1 E. 79 St., New York 21)


26–30. International Federation for Hygiene and Preventive Medicine, intern. congr., Vienna, Austria. (E. Musil, IFHPM, Marihilfer Strasse 177, Vienna XV)

27–30. Chemical Inst. of Canada, annual conf. and exhibition, Edmonton. (CIC, 48 Rideau St., Ottawa 2, Ont.)

27–2. International Federation of Pre-stressing 4th congr. Rome, Italy. (IFP, 6, rue Paul Valéry, Paris, 16th)

28–29. World Heavy Water Reactor Technology, Canadian Nuclear Assoc., annual conf., Ottawa. (Manager, CNA, 19 Richmond St. West, Toronto 1, Ont.)


28–30. Heavy Water Reactors, Canadian Nuclear Assoc., annual conf., Ottawa, Ont. (CNA, 19 Richmond St. West, Toronto 1)

28–30. International Discussion on Heat Treating, Lausanne, Switzerland. (Institut für Härterei-Technik, Postfach 13, Bremen-St. Magnus, Germany)


29–31. International Congress of cardiology, Sao Paulo, Brazil. (Dr. C. A. Re Orient, Sao Paulo, Brazil)

29–31. International Congress of cardiology, Sao Paulo, Brazil. (Dr. C. A. Rein, Sao Paulo, Brazil)

29–31. International Congress of cardiology, Sao Paulo, Brazil. (Dr. C. A. Rein, Sao Paulo, Brazil)

29–31. International Congress of cardiology, Sao Paulo, Brazil. (Dr. C. A. Rein, Sao Paulo, Brazil)

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can be quickly hand assembled without special tools for three-dimensional study of stereochemical and conformational factors and reaction mechanisms. Designed to overcome many shortcomings of earlier model systems, they
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- present precise angles of distortion (to 30°).
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the past! This simple procedure is, alas, unacceptable to the "analyticist," to whom the past is, for unexplainable reasons, tabu.

In spite of these disagreements in method we are in full agreement with Dorn's conclusions that "man's ability to control his environment" can avert a population catastrophe "provided he rapidly develops cultural substitutes for those harsh but effective governors of his high reproductive potential," because his suggestion is precisely our thesis. We observed that the growth phenomenon of the human population in the past is typical of an open-loop system that is composed of cooperative elements following a superadditive composition rule. An intrinsic instability of such systems, which manifests itself in a pathologically rapid growth, can be avoided by converting the open-loop system into a closed-loop system. Hence, we suggested a "population servo," which, first of all, has to provide a feedback that informs the system of its present state. Dorn's paper serves our purpose admirably.

HEINZ VON FÖRSTER
PATRICIA M. MORA
LAWRENCE W. AMIOT
Department of Electrical Engineering,
University of Illinois, Urbana

References
2. H. von Förster, P. M. Mora, L. W. Amiot,
   ibid. 132, 1291 (1960).
3. ibid. 133, 943 (1961); 133, 1932 (1961).
4. Webster's New World Dictionary of the
   American Language (World Publishing Co.,
5. U.N. Publ. No. 31/304/3, Ser. A.17 (1953);
   U.N. Preview 4 (July 1957); U.N. Population
   No. 15 (1959).
6. N. Wiener, Extrapolation, Interpolation, and
   Smoothing of Stationary Time Series (Wiley,
   New York, 1950).

Girdles and Griddles

With much delight I read the following comment by Bancroft W. Sitterly, in his review of Man's Conquest of the Stars, by Pierre Rousseau [Science 135, 35 (5 Jan. 1962)]: "The translation seems to convey well the spirit of the French original. One grotesque slip in this English edition is the statement, repeated on a number of pages, that the galactic system has the form of a girdle-cake! But I found no other."

A light sponge-cake batter, perhaps, baked in a fine corset with steel stays—the comparison does seem laughable (although it is not entirely disagreeable to imagine the grace of a wasp-waisted

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belle, or even the fine abundance of a
generous matriarch, repeated on a cos-
mic scale: das Ewig-Weibliche . . .

In justice to Michael Bullock, who
appears to be the translator in ques-
tion, I should like to point out that ac-
cording to the New English Dictionary,
girdle is a respectable variant form of
gridle "by metathesis of t." It is defined
as a "circular plate of iron which is
suspended over the fire and upon which
cakes are baked or toasted." The first
element of this usage is dated about
1400. There is an entry for girdle in
this sense as a combining form, "as
girdle-cake. . . ."

According to Nancy Mitford, a rec-
ognized authority on U diction, "girdle-
cake" might be mentioned casually in
a conversation in London. Fanny Win-
cham (whose husband, pastoral theolo-
gian at Oxford, has been named ambas-
sador to Paris) and Uncle Matthew are
having tea:

"Delicious girdlecake."

"Comes from the Shelter—they've
got a Scotch cook there now" [Don't
Tell Alfred (Harper, New York, 1961)]

An American edition of Man's Con-
quest of the Stars might well explain
that the galactic system has the form
of a griddle cake, a hot cake, a pan-
cake; but it seems that the English edi-
tion is within its rights.

Mary Ann Harrell
4607 Connecticut Avenue, NW,
Washington, D.C.

Electroplax and Nerve Activity

In his article "Chemical factors con-
trolling nerve activity" (1), D. Nach-
mansohn refers to a protein which I
have isolated from the electric organ
of the electric eel as the "physiological
acetylcholine receptor." Although initial
results suggested such a role for the
protein (2), recent studies on its prop-
erties led to a change in my interpta-
tion of the nature of the material. This
new interpretation was presented at the
1st International Pharmacology Meet-
ing, held in Stockholm in August 1961;
since the paper has not yet appeared in
print (3), I am writing to restate my
views.

The following points must be con-
sidered in assessing the possible func-
tion of the protein.

1) Binding of acetylcholine (ACh)
and some depolarizing agents to the pro-
tein is very weak as compared to their
high activity in physiological processes.

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RUGGED
time-saving
EASY
in
capacity)
course.
COUNTLESS LABORATORY
injection
and
normal
Shadograph
almost
cision
to
our
laboratory.
We
will
designed
and
instrument, sturdily
constructed
and
nonconducting
membranes
of
electroplax
(5). The
ACh
receptor
would
be
expected
for
the
physiological
receptor
is
well
documented.

3) Immunohistochemical
studies
reveal
that
the
protein
is
localized
in
or
near
theconducting
and
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4) The
amount
of
purified
protein
obtained
from
electric
tissue
is
much
greater
than
would
be
expected
for
the
ACh
receptor
substance
of
the
end-plate
region.

From
these
and
other
considerations
it
is
now
concluded
that
the
protein
is
a
membrane
component
which
is
distinct
from
the
physiological
ACh
receptor
substance.
Moreover,
the
protein
may
have
a
role
in
the
electrical
activity
of
conducting
membranes.
Thus,
the
effectiveness
with
which
a
series
of
drugs
blocked
axon
dependence
paralleled
quite
closely
their
affinity
for
the
protein
(6).

Compounds
which
combine
weakly
with
the
protein
(for
example,
ACh,
choline,
decamethonium,
neostigmine,
and
dimethylaminoethylacetate)
in
0.1M
concentration,
did
not
affect
squid-axon
activity,
even
after
of
nerve
with
cobra
venom.

Acetylcholine
in
10^4M
concentration
acts
at
the
nerve
membrane.
These
compounds
do
not
block
activity
in
detergent-treated
carcinoma,
but
only
after
of
nerve
with
cobra
venom.

However,
other
depolarizing
agents—for
example,
noracetylcholine-12,
norcho-
line-12,
and
pyridinealdoxime
dodeciodide—have
a
high
affinity
for
the
protein,
yet
these
compounds
in
vivo
are
considerably
less
active
than
ACh
(4).
Moreover,
the
binding
of
noracetylcholine-12
and
norcho-
line-12
is
about
equal,
though
as
analogues
of
ACh
and
choline,
respectively,
large
differences
in
affinity
would
be
expected.

2) Schoffeniels
and
I
recently
found
that
binding
of
d-tubocurarine
(0.02
mg/ml)
to
the
purified
protein
is
not
affected
by
carbamylcholine
(1
mg/ml).

Competition
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these
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3) Immunohistochemical
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with
cobra
venom.

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component
which
is
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from
the
physiological
ACh
receptor
substance.
where the active membrane is covered by a thin and porous structure.

These findings are consistent with a newer concept (3)—that the purified electrophylax protein may be identical with, or may closely resemble, the component of conducting membranes with which drugs combine when they produce their effects. It differs from the physiological ACh receptor, particularly with respect to its affinity for ACh, carbamylcholine, decamethonium, neostigmine, and dimethylaminoethyl acetate. On this basis one might explain the weak action of these compounds on nerve conduction, in contradistinction to their high potency at the end-plate.

Seymour Ehrenpreis
Department of Pharmacology,
Georgetown University Medical Center,
Washington, D.C.

References
9. ——, personal communication.

Ehrenpreis raises the question whether the protein which he isolated from electric tissue is indeed the acetylcholine (ACh) receptor protein. He explains why he now disagrees with the interpretation accepted by him when he worked in my laboratory.

Isolation of proteins has become a commonly used procedure, and in the case of enzymes no problem exists as to identification. In the case of the receptor protein the main difficulty confronting us has indeed been the question whether the protein isolated is identical with the physiological receptor postulated to react with ACh in the elementary process of conduction. This identification is impossible solely on the basis of test-tube studies. It has been achieved essentially in the studies on intact cells—in particular, in the studies on the monocellular electrophylax preparation carried out by Henry Higman, Philip Rosenberg, and Eva Bartels and developed during the last 2 years to a high degree of sensitivity for evaluating structure-activity relationships (1, 2). Ehrenpreis was not directly associated with these investigations; his

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- **PHARMACEUTICALS**: Quality control of formulations; research into new chemical structures
- **AGRICULTURE**: Research on new compounds that show promise as herbicides, weedicides, or pesticides
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opinion is, therefore, based on second-hand and, unfortunately, incomplete information.

When my associates and I first offered evidence, in experiments on the electroplax, for the existence of a cell constituent distinctly different from ACh-esterase, we were struck by the observation that the compounds, reacting with the receptor only and not affecting the esterase, blocked electrical activity in two different ways: one type blocked with, the other without, depolarization. The former type had the biological action on the receptor postulated for ACh, the latter had only an inhibitory effect. In analogy with enzyme chemistry we referred to the first type as receptor activators, to the second as receptor inhibitors (3). Acetylcholine, carbamylcholine, dacamethonium, and others belong to the first type, procaine, tetracaine, curare, and others to the second type.

1) When the protein in question was isolated by Ehrenpreis, he tested by equilibrium dialysis the binding strength of a series of tertiary, monoquaternary, and diquaternary compounds, known from the electroplax studies to interact with the receptor. The protein studies were still quite preliminary and are being carried out only now on a quantitative basis, by S. Beychok and H. B. Higman.

Nevertheless, it became indeed apparent, as I mentioned in my Herter lecture (4), that a marked difference exists between the binding strength in vitro and the potency of action on the living cell when receptor activators and inhibitors are compared. When several series of receptor inhibitors were tested, there appeared a parallelism between their binding strength and their efficiency in blocking electrical activity. A striking example of this parallelism is offered by the local anesthetics procaine, tetracaine, and dibucaine—compounds closely related in structure to ACh, but tertiary nitrogen derivatives and receptor inhibitors (1, 5). It has been, moreover, shown in experiments on the electroplax that tetracaine and ACh compete for the same cell constituent (2).

According to the quite preliminary studies in vitro it seems that the binding strength of receptor activators, such as ACh, carbamylcholine, prostigmine, and so forth, the protein may be weak when compared with their high potency on the cell. However, it must be stressed that the available information is tentative and far from being quantitative. No binding constants have been determined thus far. Binding between micro- and macromolecules is necessary for interaction. For the proposed physiological role of the receptor protein it is necessary to postulate a large rate constant for the combination of receptor and ACh. This, of course, yields no information about the equilibrium (binding) constant. Clearly, if the rate of dissociation is correspondingly great, the equilibrium constant for binding, by definition, will be low. Moreover, we do not know how many of the receptors and how many active sites on each receptor must be activated for a maximum response. In view of all these uncertainties, no statement is at present justified as to the quantitative relationships until extensive further studies will have clarified the situation.

But binding forms only part of biological activity. It is a prerequisite, permitting something additional to happen. We know from enzyme chemistry that binding of competitive inhibitors may be very strong but that the complex is nevertheless inactive. The dis-
Association constant of the prostigmine-
ACh-esterase complex is $10^4$, that of
acetylcholine–ACh-esterase is $10^8$, but
the activity in the latter case is extra-
ordinary. The enzyme has a very high
turnover number with ACh; it is one of
the fastest acting enzymes known.
Moreover, if one compares ethanol-
mine with the mono- and dimethyl ami-
noethanol, one finds that each methyl
group increases the binding by a factor
of 7. Addition of a third methyl group
does not contribute to binding at all.
But if we compare the enzyme activity
toward aminoethyl acetate with that
toward the ester containing one, two,
and three methyl groups, we find that
the trimethyl ester (ACh) has a ten-
fold higher rate of acetyl enzyme for-
mation than the dimethyl derivative.
In the hydrolytic process there is a
large increase of $\Delta S^*$, the entropy
of activation, in going from the dimethyl
to the trimethyl ester, indicating that
in the active phase some molecular
rearrangement of the enzyme must take
place in the activated complex (6).
Butyrylcholine is more strongly bound
to the enzyme than is ACh, but its $V_{\text{max}}$
is $1/150$ as large.

A difference similar to that found
with ACh-esterase has been observed in
the reaction of mono-, di-, and tri-
methylaminoethyl acetate with the re-
ceptor of the electroplax (7). The di-
methyl compound has a potency 10 to 20
times that of the monomethyl ana-
logue. This is a factor not too different
from that found in the interaction with
the enzyme and may conceivably be at-
tributed to increased binding by the
methyl group. But the trimethyl ana-
logue, ACh, has a potency 200 times
that of the dimethyl analogue. It would
be extremely difficult to attribute such
an enormous increase in potency to
the contribution to binding of one ad-
tional methyl group, especially since
there is good reason to believe, on the
basis of the esterase studies, that this
third methyl group does not contribute
to all binding to the protein. Clearly,
factors other than binding help to de-
termine the potency of action.

The long-chain analogues of ACh
offer a special problem. Owing to strong
van der Waals forces they react quite
strongly with a great number of pro-
teins in addition to those of the ACh
system, and also with other macro-
molecules; their mode of action is quite
complex and still under investigation.
They cannot be readily classified in
either of the two categories.

2) It is true that carbamylcholine in
a concentration of 1 mg/ml did not
show competitive action with curare
in a concentration of 0.02 mg/ml; but
Ehrenpreis failed to mention that at
a higher ratio curare binding was al-
most completely suppressed by car-
bamylcholine. The former finding just
means, again, that the binding of the
activator (carbamylcholine) is very
much poorer than that of the inhibitor
(curare). The binding of an activator
in equilibrium may easily be 1000 times
poorer than that of an inhibitor.

3) The immunochemical studies re-
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associated with ion flux regulation, even though the membrane has not the prerequisites for impulse conduction.

4) The amount of receptor protein seems to represent approximately 0.1 to 0.2 percent of the total protein; this percentage is far from surprising for an organ so highly specialized in its function. Acetylcholine esterase forms about the same fraction of the total protein. The evaluation of the amount of the receptor in the end-plate region is completely speculative and can easily be off by 1 to 2 orders of magnitude. If it were true that there was not more receptor protein in the electroplax than was expected to be present at the synapse, then this fact would form a serious difficulty for the assumption that the ACh system plays an essential role in the generation of bioelectric potentials.

Surprisingly, however, Ehrenpreis in his new interpretation admits that the protein may have some still-undefined function in the electrical activity of the conducting membrane. He argues that curare, but not ACh, affects axonal conduction either on special preparations (Ranvier node (8)) or after chemical pretreatment with detergent or cobra venom (9, 10). Therefore, he concludes, the protein isolated is not the ACh receptor protein, which is present only at the synapse. He thus implicitly accepts the notion of a fundamental difference between synaptic transmission and axonal conduction.

The striking demonstration that curare blocks axonal conduction, and not only synaptic transmission as was believed for a century, should be reason enough for reevaluating the role of ACh in nerve activity, since it is generally accepted that curare acts as an antimetabolite of ACh. Tertiary, lipid-soluble analogues of ACh, such as local anesthetics and diphenhydramine, act in similar concentrations and in a similar way on the synaptic junction of the electroplax and on the giant axon of the squid (10). These new developments support rather than invalidate the explanation offered many years ago—namely, that the failure of ACh to act on axonal conduction must be attributable to the presence of structural barriers surrounding axonal conducting membranes and preventing lipid-insoluble compounds, such as curare and ACh, from reacting with the ACh receptor protein. Cobra venom only reduces, but does not remove, the barriers, as is clear from the experiments reported (10). After ex-
Exposure to the venom of the cottonmouth moccasin, which is much more potent in reducing the barriers than is cobra venom, curare acts in much lower concentrations, and in this case ACh, too, is effective (11). At the Ranvier node the conducting membrane is covered not by heavy myelin but by a complex structure seen in the electron microscope; this structure apparently permits curare and tertiary analogues of ACh, the latter in extremely low concentrations, to act on the membrane, although ACh itself does not act. There are amazingly great variations in permeability of different axons with respect to very closely related nitrogen derivatives (12). In addition to the direct action of ACh after treatment with moccasin venom, direct effects have been observed on desheathed vagus (13) and even on sheathed somatic fibers (14). In these preparations the structural barriers seem to be insufficient to protect the membrane. Obviously, ACh must act in all these places on a macromolecule endowed with special properties and associated with axonal electrical activity; otherwise it could not produce its effects. Nevertheless, the physiological significance of these pharmacological actions, considered by themselves, may appear open to question. However, in conjunction with the huge amount of biochemical evidence accumulated in support of the interpretation that ACh has a role in the generation and propagation of electric currents—such evidence as the presence of ACh, ACh-esterase, and choline acetylase in the conducting fiber; the inseparable association of electrical and enzyme activity; and the effect of local anesthetics—the evidence of a direct action of ACh becomes pertinent and indicates the presence of an ACh receptor in the axon. Both the biological and the chemical arguments offered by Ehrenpreis fail to support his new interpretation.

David Nachmansohn
College of Physicians and Surgeons,
Columbia University, New York

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A Two-Way Affair

Edmund W. Sinnott [Science 135, 
278 (26 Jan. 1962)] assumes that such 
letters as he suggests [to individuals in 
the U.S.S.R.] would reach the person 
dressed. I have serious doubts about 
that. I know from experience that let-
ters from the United States are not 
exactly welcomed behind the iron cur-
tain. This includes letters to relatives.

The spread of good will must be a 
two-way effort to have any value. I 
wonder if Sinnott can show us a letter, 
similar in content to his, published in a 
leading journal of science of the 
U.S.S.R.

WILLIAM EISENMAN

160 West 77 Street, New York

As Eisenman points out, good will 
is a two-way affair, but this does not 
mean that we should wait for someone 
on the other side to make the first 
overture. There is a wide interchange of 
friendly correspondence between Amer-
ican and Soviet scientists, and my sug-
gestion simply is that in connection 
with this, or as an extension of it, there 
be more formally expressed the desire 
for sincere good will between our 
peoples.

EDMUND W. SINNOTT

Yale University,
New Haven, Connecticut

Science Curriculum in Argentina

Garrett Hardin's review entitled
"The 'two cultures' within biology" 
[Science 134, 548 (1961)] has some-
what belatedly come to my attention.

With reference to his query and 
comment, "What is to be done? Pos-
sibly planning within universities can 
put a brake on the speciation process 
by requiring physical scientists to take 
at least one biology course . . . ," I

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would like to inform your readers that this same problem has been overcome in the School of Exact and Natural Sciences of the University of Buenos Aires. This School, one of the ten comprising the University of Buenos Aires, one of the largest universities of the Latin world (with an enrollment of over 50,000), deals with teaching and research in the basic sciences and includes at present six large departments (physics; inorganic, analytical, and physical chemistry; biological chemistry; meteorology; biology; and geology) plus partnership in the School of Engineering of the Department of Industries.

The impact of science and technology in the development of the huge natural resources of Latin America within an imperatively short period is obvious. The leaders in overhauling methods in the School of Sciences have been aware of this, as well as of the problem raised by Hardin. Hence, all students of the School (including, of course, the ever more numerous students of mathematics and physics) are required to take a short but intensive course in biology. One of the arguments for establishing this requirement was that no student of the sciences can utterly ignore one of the main fields of science. Insofar as the "other" sciences are concerned, the curriculum establishes also at least two full semesters of physics and two of chemistry for all students of the School.

Notwithstanding the fact that the overhaul mentioned has been partially based on the university system that has found great favor in the United States, the obligatory courses that I have pointed out give students of all branches a broader view of science in toto.

Jorge E. Wright
Department of Biology,
University of Buenos Aires,
Buenos Aires, Argentina

Statue of Claude Bernard

C. D. Leake [Science 134, 2069 (1961)] was quite right in his statement, which appeared under the excellent photograph of a bronze memorial of the celebrated physician Claude Bernard (1813–78), that this statue was melted by Germans who occupied Paris during World War II. It may interest readers to know that in 1946 a new monument, in stone, was erected on the same spot in front of the Collège de France (Fig. 1). Bernard's pose is different in this second statue, although the memorial is approximately of the same size.

John O. Corliss
University of Exeter,
Devonshire, England

Sparing Action of Folic Acid by Thymidine

The article by Grossowicz and Mandelbaum (1) was commented on by Bolinder (2). In reply, Grossowicz (3) stated that thymidine produced growth in our experiments but not in his system. However, we pointed out (4) that our results showed that although thymidine by itself was ineffective in promoting growth, it significantly reduced the requirement of Leuconostoc citrovorum for "citrovorum factor."

The current state of biochemical research in folic acid coenzymes has reached a degree of sophistication not discussed or expressed in the article by Grossowicz and Mandelbaum. Studies by Friedkin (5) indicate that the thymidylate synthetase reaction includes transfer of the 5,10-CH2- group and dehydrogenation of 5,10-methylene tetrahydrofolic acid to dihydrofolic acid. The reduction of dihydrofolic acid...
to tetrahydrofolic acid is a step in the renewal of the supply of 5,10-methylene tetrahydrofolic acid. This reduction is presumably not carried out readily by \textit{L. citrovorum}, as shown by its defective response to folic acid and dihydrofolic acid as contrasted with its ready response to tetrahydrofolic acid (6). The addition of preformed thymidine would lessen the requirement for 5,10-methylene tetrahydrofolic acid. Although this evidence is inferential, it could account for the well-established sparing action of thymidine on the “citrovorum factor” requirement of \textit{L. citrovorum} (4, 7).

THOMAS H. JUKES
American Cyanamid Company, Princeton, New Jersey

HARRY P. BROQUIST
University of Illinois, Urbana

References

Messages from Other Worlds

The article by S. von Hoernner [Science 134, 1839 (1961)] gave us very interesting estimates of the probabilities of radio communication with other civilizations in space.

A small thought of my own is that notice of the existence, location, and communication system of such a civilization might reach us by means other than direct radio transmission. If an earlier civilization was as egocentric as our own, it would probably want to leave a record of its existence and communicate its knowledge to successor civilizations. It would need, for this purpose, objects that would be maintenance-free, would attract attention, and would carry much information in a small space for millions of years.

If such a speculation has any merit, one might want to take a new look at meteorites, comets, and other space travelers for possible messages. Might the organic compounds in meteorites contain coded information? Also, could one intercept comets to obtain material for analysis?

LESLIE C. EDIE
700 Nassau Street, Bellmore, New York
Studies of Starvation

As referee editors of several physiological and nutritional journals, we are perturbed by the continuing submission of mediocre papers dealing with starvation. Usually the only experimental method used consists in starving rats to a near-terminal condition and then determining one of the many parameters which can be affected by starvation. Sometimes determination of the duration of survival on a given diet is the only "technique" employed. Previously obtained information, available in the U.S. and foreign literature—often admittedly older literature because of the very simplicity of the techniques used—is generally ignored.

The application of the "findings" to problems of human survival involves questionable extrapolations. What little new information, if any, is obtained in these studies does not seem to us to be commensurate with the suffering inflicted on the animals. There is little doubt that we need to know more about the physiological mechanisms involved in resisting starvation and about the pathological consequences of prolonged undernutrition, whether continuous or intermittent. We would certainly approve for publication papers in which a great many pertinent correlations are studied in order to close the book, at least for a while, on death by starvation. But we find it difficult to approve of these piecemeal dissections which have resulted, and which will continue to result in perhaps dozens of papers, none of them definitive.

John R. Brobeck
Department of Physiology,
University of Pennsylvania School of Medicine, Philadelphia

Jean Mayer
Department of Nutrition,
Harvard School of Public Health,
Boston, Massachusetts

Electronics and the Life Sciences

In their article "Biomedical electronics: potentialities and problems" [Science 135, 198 (1962)], Robert Ledley and Lee Lusted argue the need for conditions which are more favorable to the development of "biomedical electronics." They review the highly important role which electronics has played in the development of devices and systems of major contemporary importance and foresee a similar role for electronics in...
the life sciences. It would be most difficult to disagree in toto with this prediction.

Nevertheless, the totality of what these writers would seem to offer to medicine and to the biological sciences is only in part the subject matter normally associated with electronics. Rather, the entire field of engineering and those branches of science now largely pursued by those who have engineering training (the so-called engineering sciences) seem to be involved. Let us follow the authors' list, in part. "Measurement and analysis of small electric potentials" is an electrochemical problem to which important contributions have been made by physical chemists and chemical engineers. "Electronic flowmeters" present serious problems in non-Newtonian fluid mechanics as well as in electronics. Governing the design of many "artificial organs" is the approach instituted by chemical engineers to the problems of interfluid transport through membranes and the fluid mechanics of the adjacent liquids—not primarily, as Ledley and Lusted state, "the extensive use of electronics in medicine." The investigation of "infrared detectors" and "ultrasonic receptors" in animals lies in areas of applied physics to which engineers of many types have contributed.

No engineer in any of these areas hesitates to acknowledge the great assistance he derives from the largely electronic instrumentation which he uses. Nonetheless, the application of this instrumentation to problems in the life sciences is a far less worthy objective than the application of the whole of engineering technique, analysis, and science to the goals of biological research.

Electronic instrumentation is primarily a tool, useful to those trained in its use and in utilization of the measurements it produces. Most of the important biological problems arising today cannot be solved simply by increasing budgets so that more sophisticated instruments can be brought to bear on these problems. Before the quantitative output can be analyzed—even, given a certain instrumentation capability, before one can decide what to measure—some analysis must be conducted. The biological scientist frequently needs assistance in these analyses, and this assistance can often best be obtained from a problem-oriented individual with training in one of the several branches of engineering.

Alternatively, the life scientist may wish to acquire knowledge that is available in any of the several branches of engineering in which similar problems have been attacked quantitatively. In either case the primary requirement appears to be for a biomedical engineering function (1) or even for a broader, "bioengineering" function (2) from which will follow naturally the demand for instrumentation. "Biomedical electronics" is an important but subservient function, reflecting the broad cooperation which is developing between life scientists and engineers.

Concerning specific proposals made or analyzed by Ledley and Lusted, a few comments are perhaps in order. I agree that a basic deterrent to biomedical electronics is a lack of financing. But in proposing more support for biomedical electronics per se, the authors are stressing the means with insufficient concern for the end. They attribute the reluctance of industry to participate in the development of electronic devices for medicine to "the prevalent opinion that there is a small market." They cite three other problems: (i) "finding what devices need to be made," (ii) "getting the devices tested," and (iii) "selling the instruments." All of these are facets of one problem: a shallow dialogue between the biological scientist, who knows the subtle morphology of the problem, and the engineer, with his sometimes naive but often useful ca-
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Leonard quite correctly points out that the different engineering disciplines can contribute greatly to the advancement of biomedical research. If he feels our article implies that only electrical engineering or biomedical electronics can contribute to the progress of biomedical research, to the exclusion of other areas of engineering, then he has misinterpreted our intention. The main theme of our article is expressed in the first sentence: "The full application of electronic engineering technology to biomedical science is long overdue." We believe that such application, in many areas, is necessary (though clearly not sufficient in itself) to the general advance of biomedical science.

We should like to point out that in our article we considered mainly the tool—electronic engineering technology
—not electronic engineering science. Leonard’s letter is apparently concerned more with the engineering scientific disciplines themselves than with the tools of engineering science. He says that “the biological scientist frequently needs assistance in these analyses, and this assistance can often best be obtained from a problem-oriented individual with training in one of the several branches of engineering.” In a previous article, “Digital electronic computers in biomedical science” [Science 130, 1225 (1959)], one of us (R.S.L.) stated, “I strongly believe that ‘team’ approaches, where the biologist has no . . . [engineering] training and the engineer . . . has no biological training, are foredoomed to failure. For the full significance of the extensively detailed and often subtle . . . use of . . . [engineering science] in biomedical science can be understood only by those well grounded in both fields.” Whether or not any of the various engineering sciences—such as chemical, hydraulic, and mechanical, as well as electronic, engineering—is a necessary ingredient in biomedical research depends upon the particular problem being approached.

However, we believe that electronics as a tool is (or should be) in many cases a necessary ingredient of research. In order to lend weight to this point in our recent article, we described such use of electronic equipment as a necessary ingredient (but obviously not the only one) in many biomedical research activities, and we attempted to discuss the reason why fuller use of this important tool has not yet been made in biomedical research. We do place this electronic tool in a rather exalted position, because it is well known that advances in science closely parallel advances in instrumentation. However, we refute Leonard’s accusation that we have attempted “to fix the boundaries of this new and exciting area of cooperative research so as to make them in any way coincident with the boundaries of [our] own field [of electronics].” As a matter of fact, we would like to call attention to a symposium on educational frontiers in biomedical engineering [IRE Trans. on Bio-Medical Electronics BME-8, No. 4 (1961)] in which the very broad field of biomedical engineering is discussed.

In conclusion, we would like to reiterate the view, stated in our article, that the “use of electronics in biomedical science [as a tool] holds promise of tremendous advances in the study of the origins of the life processes; it may
result in spectacular advances in medical science, which could have a definite effect on individual health and longevity; it might pave the way for the discovery and development of whole new technologies based on intimate knowledge of biological processes.”

Robert S. Ledley
National Biomedical Research Foundation, Silver Spring, Maryland

Lee B. Lusted
University of Rochester School of Medicine, Rochester, New York

A Concerted Attempt To Improve Relations with the Communists

Recent commentary concerning what scientists can do to help resolve the arms race seem to overlook one unique contribution that scientists as a group can make. This is to focus attention on treatment of the fundamental disease—the almost total ignorance of the problems and intentions of the “other side” evident on each side—as well as the symptoms and their treatment.

This is not to say that the symptoms can be or should be ignored. But it does seem preposterous to budget over $50 billion on military defense and a pitance, if that, on long-range defense measures aimed at resolving the underlying tensions by improving the reciprocal understanding and appreciation of strengths, as well as weaknesses, of the American and Russian societies. To attempt any such program openly will be attacked as treasonable by many citizens who have closed their minds to the problem and see issues only in clear-cut blacks and whites. Scientific objectivity certainly needs to be applied here, and in generous dosage.

The risks of such a procedure may seem enormous to those among us who are strongly and often vehemently against communism (and perhaps also democracy, or some particular religion, and the like) but who more and more fail to emphasize the values that they feel we should be for—except in what appear to be vacuous, emotion-laden shibboleths. The corresponding erosion of those freedoms for which our country has long stood is especially disturbing and is aggravated most dangerously, if not initiated, by many of these people. Surely it is time for scientists to take a more forthright and uncompromising position in support of our freedoms, and to emphasize the strengths
of our own political system which permit us to explore the path to more open communications and interchange, even of ideas, with the Iron Curtain countries. Are scientists to be frightened into political apathy by vociferous men with a frenzied fear of ideas and little faith in our political strengths?

Once we have achieved a breakthrough on the present reluctance to recognize some degree of mutual culpability in the nuclear armament impasse, regardless of the extent to which either side is more or less culpable, then the really difficult task of implementing improved understanding can be attacked. Scientific ingenuity, now so largely mobilized in the interests of short-range, symptomatic efforts, can be turned at least partly to the fundamentally more important long-range objectives. Even if we were completely innocent and the Soviets were 100 percent responsible for the present impasse, nevertheless it would still be in our own best interest to make a serious and concerted attempt to improve relations and thus get at the fundamental difficulty.

The overwhelming political need of our time is an opening of the channels of communication between the Western countries and the Communist countries—China even more than Russia, if present portents are reliable. Let the scientists use up some of their recently accumulated prestige, if need be, by taking the lead here. Who else is in a position to do so?

Critics of this point of view will of course point with great alarm to the past difficulties encountered in dealing with Communist personnel. That there have been difficulties cannot be denied. But surely the situation is not hopeless. Past rebuffs, even if they are seen as all on one side, cannot be allowed to dissuade us. The most important social role of the scientist in our time, in my opinion, is to seize upon this opportunity to utilize the spirit of free inquiry in science as a base from which we can develop an increasing amount of social and cultural interaction. A good example of the kinds of mutual benefit, in the form of enhanced cultural understanding, that can be achieved is given in the article by Konrad B. Krauskopf [Science 134, 539 (1961)], in which he recounts his experiences on a scientific trip to the Soviet Union. It should be clear that I am not proposing merely an increase in the kind of "understanding" that seeks to assay Soviet motives, political or military, but rather a broad
attack on the general problem of getting to know the Russians, people and officials, in an atmosphere of genuine interest and tolerance rather than one of chronic suspicion and competition.

The objective outlined in this letter will require a redoubling of our efforts to open up lines of scientific communication, especially in regard to meetings and exchange visits, and such a move can be effectively spearheaded only by organizations like the AAAS. Advantage should be taken of the apparent readiness of the present Administration to act in a conciliatory manner from a position of military strength. Expression of support for AAAS officials in such endeavors can be offered by individual members, both in general and on specific issues that arise, and should be communicated to congressmen and other public officials.

If scientific opinion in this country can concentrate on this one major objective I feel that there will be some promise of our making at least a small crack in the Iron Curtain. And the social and political voice of the scientist will certainly become more potent when such common objective is widely accepted and promoted within the scientific community.

Melvin H. Marx
Department of Psychology,
University of Missouri, Columbia

Science, Linguistics, Lexicography

The editorial "Say it ain't so" [Science 134, 1493 (1961)] is a fair and reasonable commentary. Whether or not one agrees with the editor's views, no exception can be taken to the manner in which they are presented. It is to be regretted that the same cannot be said of the comments of Max S. Marshall [ibid. 135, 739 (1962)] on this editorial. One scarcely expects to meet in the pages of Science techniques more commonly reserved to propaganda and the polemical diatribe.

It is unfortunate that a scientist of some professional stature sees fit to employ, in discussing a matter outside his specialty, an intemperance of language and inaccuracy of reporting that he assuredly neither would nor could employ in professional communication in his own field. The use of such pejorative terms as ringleaders, proselytizing, self-styled, and make a mess of seems as misplaced in scholarly comment as does the quoting of fragments out of context in such a manner as to completely obscure the intent of the original author; for example, "the advocates of 'observing precisely what happens when native speakers speak.' These are the self-styled structural linguists. . . ." (Marshall), versus Gove's actual statement, "The fundamental step in setting down postulates for descriptive linguistics is observing precisely what happens when native speakers speak. This is the essential first step required by scientific method" (italics mine). Does Marshall deny linguists alone the right to accumulate data, or does he hold the familiar, the accepted, the authoritarian to be the proper bases for all scholarly activity? No, this seems hardly likely in an adherent of one of the newer biological disciplines and one whose founder answered the criticisms of academicians of his day with clear-cut data.

To turn to the content as distinct from the manner, there are a few points in Marshall's letter that require special notice, if only because they appear as misconceptions in several reviews. The idea that the Merriam-Webster editorial process was in any fundamental way a clerical or mechanical tabulation is wholly false. On the contrary, this process involved the accumulation of data (citations) on a carefully planned basis designed to assemble from varied sources and levels of usage word samples adequate for analysis. The resultant material was then sub-
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FORWARDED TO TECHNOLOGICAL QUALIFIED EDITORS who, in the light of their knowledge and experience, weighed the items included not solely for meanings exemplified but also with due attention to place of origin, character (as technical, scholarly, classical, popular) of publication, kind of writing and evidence of editing, and other indications of status. Another misconception in holding which Marshall is not unique is the assumption that matter in angle brackets in a Webster's Third New International definition is a quotation of authority. As is specifically stated in the initial explanatory notes (p. 19a), "The matter enclosed in a pair of angle brackets illustrates an appropriate use of the word in context" (italics mine). The purely illustrative status of such material should be patent, even in the absence of such a statement, from the fact that much of it is anonymous. That an editor has occasionally out of abundant material chosen to illustrate a usage by an apt quotation from a ballplayer, a disorderly-housekeeper, or a politician is rather indicative of his awareness of the living, democratic character of our language than of an attempt to establish false authorities. As one whose training is fundamentally in "science" in the narrow sense apparently recognized by Marshall, I am perhaps no better qualified to defend than he to criticize the scientific status of linguistics. Yet I think the question is no more than one of definition. If science is equated with what is often distinguished as the natural sciences, then by definition linguistics is excluded. If we recognize that the esoteric mystique of science is as dated as alchemy or the benefit of clergy and go along with much of modern thought in stipulating that science is not any one discipline or any group of disciplines but is knowledge obtained by the scientific method, then a scholarly field that is subject to objective analysis is at least potentially a science. On this basis we may doubt the possibility of a science of religion or of literature or, in traditional terms, of grammar, but there can be as readily a science of linguistics as of bacteriology or genetics. The crux is the methods and aims of the practicing scholars. Individually, linguist, bacteriologist, or geneticist may be scientist or nonscientist, but each field lends itself to objective investigation and, insofar as its adherents practice this, it cannot in any normal sense be denied status as a science. Personally, I doubt that any scholarly
or scientific linguist would call the classicists reactionaries on the basis of their belief in "a standard of quality in English" or their respect for "the accepted great in literature." While one might quibble over whose standard and whose acceptance, there are few indeed unwilling to acknowledge both the import of the classics as part of our cultural heritage and the desirability of nicety of expression. It is not to these the modern linguist objects but to attempts to restrain, by an authoritarian dominance, the normal evolution of language. If I write "Che cou'd not i' honor passe your worde vnchallenged" I am using 17th-century verbiage sanctioned by notable literary sources, but I would be more generally intelligible if I said "I could not honorably allow your remarks to pass unchallenged," and this, as a member of the Merriam-Webster editorial staff, I do say to Marshall.

Maire Weir Kay
47 Federal Street,
Springfield, Massachusetts

Though the challenge is somewhat personal, Kay does help to clarify the side he takes. Try J. Donald Adams [New York Times Book Review (11 Feb. 1962)] and Dwight Macdonald in his searching analysis [New Yorker (11 Mar. 1962)], or almost any other earlier comment on Webster 3, for more detail on points to which Kay and his associates have become hypersensitized.

Max S. Marshall
Department of Microbiology,
University of California Medical Center, San Francisco

Simulation of Cognitive Processes

The computer simulation of human thinking presumably described by Newell and Simon [Science 134, 2011 (1961)] is questionable on a number of counts, general and specific.

In the first place, the simulation is made to seem plausible because the authors first "postulate" that human beings behave exactly like computers. Then they "discover" that they can imitate on a computer the computer-like characteristics of man they have already postulated. The human use of symbols implies that, on some occasions at least, a human being considers both a symbol and what it symbolizes. Newell and Simon restrict their subjects
to symbol manipulations without meaning or understanding and then "find" that computers which do not understand the meanings of the symbols they manipulate behave exactly like human beings. It is unnecessary to say any more on this point, since there already exists in the literature a scathing indictment by O. H. Schmitt, the distinguished biophysicist, of this type of vicious-circle reasoning [IRE National Convention, 1955 (1955), pp. 240-255].

In the second place, there is internal evidence that the experiments with the computer and subjects were not actually carried out as reported in the article. It is stated that "the subjects read the first expression, for example as, "(r) dot (tilde-p horseshoe q)." They made no use of the meanings of the expressions in their usual interpretation but simply manipulated them as organized collections of symbols." There are no parentheses around R in the original expression. Furthermore, unless the subjects were coached or understood the function of parentheses in symbolic logic, why didn't they read the expression as "R dot curve tilde p horseshoe q curve"? Without an understanding of grouping in symbolic logic, why should the subject say there are "two things"? Why not eight (number of symbols)? On this point, see the section on "Formality" in Quine's Mathematical Logic. The rules of grouping, of association, and of distribution are an important part of symbolic logic. Either the subject understood these rules or the report of the solution by symbol manipulation is contrived and not a description of an actual experiment.

A similar difficulty arises in the description of the computer program. In a program based on an algorithm, the phrase "not desirable" would be a colorful description of "reject" or "not applicable." But a so-called heuristic program would require a sharp distinction between rules which could be applied but wouldn't lead to anything ("not desirable") and rules which couldn't be applied because they were simply not applicable—that is, would lead to invalidity. Rules 3 and 4, although stated in the program to be "not desirable," are in fact "not applicable." This fact, together with the absence of formal rules for grouping and distribution, suggests strongly that the program as given in the article was never actually run on a computer.

The basic difficulty in articles of this type is that they involve what the editor of Scientific American has called "fraud by computer." Certainly a computer can simulate human thinking if the word simulation is defined as Webster has defined it: "1. Act of simulating or assuming an appearance which is feigned, or not true; pretense or profession meant to deceive. 2. Assumption of a superficial semblance, a counterfeit display."

Mortimer Taube
Documentation Incorporated, Bethesda, Maryland

Taubé, in his letter as in his book, Computers and Common Sense, appears to be unable to discuss the simulation of cognitive processes without words like fraud, insinuations about the honesty of those with whom he disagrees, and bad jokes about the word simulation. In view of his abusive tone, we think it fruitless to enter into discussion with him. We will limit ourselves to clarifying for readers some technical points related to his comments.

Were the logic expressions manipulated as meaningless symbols? The parenthesis signs were interpreted by both the General Problem Solver (GPS) program and the subjects—that is, treated as punctuation marks for identifying phrase structure in the logic expressions. This interpretation was built into the GPS program; the subjects had ac-
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\textsuperscript{a} Ref. Analytical Chemistry, 33, 1138 (August 1961).

Recording Animal Activity

In a recent issue of Science [134, 730 (1961)], Kavanau and Norris describe an excellent application of the "capacitance-sensing" activity technique in behavior studies of burrowing animals. However, they state that "although the method is highly versatile, it apparently has not been used heretofore to study animal movements."

This method of recording animal activity was used and reported by Backlund and Ekeroot 11 years ago in a paper entitled "An actograph for small terrestrial animals" [Oikos 2, 213 (1950)]. These authors used the technique to record the activity of blowflies (Calliphora erythrocephala), and their paper has the advantage of including

required it previously, presumably from their acquaintance with algebra. To this extent the expressions were meaningful to both the human subjects and the interpreter of the program.

Does the program distinguish between "undesirability" and "inapplicability" of operators? The GPS contains both tests of desirability (involving comparison of the effects of an operator with current goals) and tests of applicability (involving comparisons of the operator with the input expression). \textit{Either} test can be applied first. If, as in the case of rule 3 or 4, an operator is neither desirable nor applicable, it will be rejected by whichever test is applied first. At the time the simulation in the \textit{Science} article was made, the version of GPS running on the IBM 7090 gave priority to the applicability test. To fit the behavior of subject 9, a hand-simulated variant was employed that altered the relation between the two tests, producing the result shown in the trace. This is a good example of the kinds of changes in GPS that are required to adapt it to individual differences among our subjects.

We might mention that our traces of runs on the IBM 7090 (about 800 of them), our hand simulations (several dozen), our recordings of human subjects (about 30 hours), and decks or tapes of our GPS program, written in IPL-V, can be made available to fellow scientists who wish to work with them and arrive at their own interpretations.

\textit{Allen Newell, Karl Simon}

\textit{Carnegie Institute of Technology, Pittsburgh, Pennsylvania}
the circuit diagram of the oscillator apparatus, which can be built in the laboratory. Unfortunately, they were not able to pursue their studies with this technique, but it is to be hoped that Kavanau and Norris will be able to use the method of Backlund and Ederoot to its fullest extent.

DONALD K. EDWARDS
Department of Forestry, Victoria, British Columbia

We are indebted to D. K. Edwards for calling attention to the note of Backlund and Ederoot. It substantiates our belief that the method has great potential for other applications.

Our use of the technique is quite different from that of the Swedish workers. They sought to record activity periods of blowflies confined under a petri dish, while our objective is to follow both the periodicities and the gross locomotory displacements of burrowing animals whose activities cannot be seen. We hope that others will apply the technique to new situations. Most biologists will find it more practical to use commercially available Hartley oscillators than to build their own.

J. LEE KAVANAU
KENNETH S. NORRIS
Department of Zoology, University of California, Los Angeles

An Unfortunate Event

I am writing with reference to your note entitled "An unfortunate event" [Science 134, 945 (1961)] concerning the report by Pande, Shukla, and Sekariah. Indian scientists are equally shocked over this disgraceful affair. This was brought to the notice of M. S. Thacker, director general of the Council of Scientific and Industrial Research, who is also the president of the Association of Scientific Workers of India. He, along with this body of Indian scientists, would like to inform you of our deep regret that such an unfortunate thing has happened.

We also take this opportunity to inform you that, at the initiative of Thacker, disciplinary action has been taken against the authors; one has been retired and the other two suspended. The matter is under investigation by an inquiry committee for determination of final punishment.

D. N. MISRA
Department of Mathematics, Lucknow University, Lucknow, India