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**Chemistry and Physics of Space**

Fred L. Whipple and Hans E. Suess are chairman and vice chairman, respectively.

1–5 July. The lunar surface (Eugene Shoemaker, moderator); Thomas Gold; Ewen A. Whitaker. **Abundances of the elements** (Hans E. Suess, moderator); Harold C. Urey. **Pressure history of meteorites** (Herbert C. Uhlig, moderator); Edward Anders; A. E. Ringwood. **Theoretical aspects of cosmic rays** (John A. Simpson, moderator). **Cosmic ray ages of meteorites** (Oliver Schaffer, moderator). **Radiogenic ages of meteorites** (Harrison Brown, moderator): Joseph Zähringer. **Isotopic anomalies**. Peter Signer: Robert O. Pepin; George Weatherill. **Interplanetary gas and dust** (Eugene N. Parker, moderator); Conway W. Snyder; Richard B. Southworth. **Early evolution of the solar system** (Lawrence H. Aller, moderator); William A. Fowler; A. G. W. Cameron. Other moderators and speakers will be announced.

**Chemistry and Metallurgy of Semiconductors**

Paul H. Egli and Peter Pollak will be chairman and vice chairman, respectively.

8 July. H. Krebs, "Structure and bonding in semiconductors"; E. Mooser and W. B. Pearson, "Basic concepts in semiconductor binding"; Georg Ruppecht, "Chemical binding in ferromagnetic and ferroelectric materials."


10 July. Gunther Wolff, "Crystallography and bonding of semiconductor surfaces"; Peter Pollak, "Rationalization and experiments in multicomponent semiconductor systems"; Frank Herman, "Selected topics in energy band theory."

transport in polymeric molecular solids”; Albert Szent-Gyorgyi, (subject to be announced).


Organic Coatings

Harold L. Jaffe and Joseph Gaynor are chairman and vice chairman, respectively.


Organic Reactions and Processes

Ellis K. Fields and William E. Truce are chairman and vice chairman, respectively.


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15 March 1963

Energy Coupling Mechanisms

D. R. Sanadi and W. Kielley are chairman and vice chairman, respectively.


Chance, “Conversion of energy into electron flow, with the formation of ‘high energy’ intermediates”; J. Gergely and A. Martonosi, “Control mechanisms in muscle contraction and relaxation.”


Photonuclear Reactions

D. J. Zaffarano and Evan Hayward are chairman and vice chairman, respectively.

5-9 August. (Speakers to be announced), “The photodisintegration of light elements”; “Fluorescence of nuclear levels below the particle threshold”; “Experimental evidence for higher order multiple excitations”; “Electron induced reactions and collective excitations”; “Excitation of nuclear states by heavy particle bombardment”; “Review of particle-hole calculations and shell model theory”; “Contributions to nuclear structure from high energy experiments”; “Photonuclear cross-sections above the giant resonance”; “New experimental techniques applied to particle emission cross-sections.”

Fluorine Chemistry

Leo A. Wall and Murray Hauptschein are chairman and vice chairman, respectively.


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**Geochemistry—Origin of Petroleum**

Frederick D. Rossini and Harold M. Smith are co-chairmen.


20 August. Organic compounds formed as a result of and subsequent to bacterial action (J. G. Erdman, discussion leader). Hydrocarbons in petroleum (B. J. Mair, discussion leader). Sulfur, nitrogen and oxygen compounds in petroleum (J. S. Ball, discussion leader).


22 August. Compaction and migra-
Glass

Norbert J. Kreidl and Martin Goldstein are chairman and vice chairman, respectively, for these sessions on defect structures.


30 August. (N. J. Kreidl, chairman) Continued brief papers.

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Meetings

Forthcoming Events

April

8-10. Selzsmological Soc. of America, Berkeley, Calif. (K. V. Steinbrugge, 465 California St., San Francisco 4, Calif.)


9-11. American Assoc. of Anatomists, Washington, D.C. (L. B. Flexner, Dept. of Anatomy, School of Medicine, Univ. of Pennsylvania, Philadelphia)


11-13. Natural Radiation Environment, intern. symp., Houston, Tex. (J. A. Adams, Dept. Geology, Rice Univ., P.O. Box 1892, Houston 1)

11-13. Eastern Psychological Assoc., 34th annual, New York, N.Y. (M. A. Iverson, Dept. of Psychology, Queens College of the City University of New York, Flushing 67)


11-13. Southern Soc. for Philosophy and Psychology, Miami Beach, Fla. (E. A. Alluisi, Human Factors Research Lab., Eckerd College, St. Petersburg, Fla.)

11-13. Pennsylvania Acad. of Science, East Stroudsburg, Pa. (K. B. Hoover, Messiah College, Grantham, Pa.)


15-20. Association for Research into Periodontal Diseases, 17th intern., Athens, Greece. (O. Louridis, ARPA, 8 rue Hippocrates, Athens)


16-20. American Physiological Soc., Atlantic City, N.J. (H. Rahn, Dept. of Physiology, Univ. of Buffalo, Buffalo 14, N.Y.)


16-20. Federation of American Societies for Experimental Biology, annual, Atlantic City, N.J. (M. O. Lerner, 9650 Wisconsin Ave., NW, Washington 14)

16-21. American Soc. for Experimental Pathology, Atlantic City, N.J. (K. M. Brinkhous, Dept. of Pathology, Univ. of North Carolina, Chapel Hill)

16-21. American Inst. of Nutrition, Atlantic City, N.J. (A. E. Schaefer, Bldg. 16, Rm. 207, NIH, Bethesda 14, Md.)

16-24. Forensic Immunology, Medicine, Pathology, and Toxicology, 3rd intern. meeting, London, England. (I. Sunshine, 2121 Adelbert Rd., Cleveland, Ohio)

17-19. Institute of Environmental Sciences, technical meeting and equipment exposition, Los Angeles, Calif. (Nat'l. Office, P.O. Box 191, Mt. Prospect, Ill.)


17-19. Plastics, joint congr. of West Germany, Switzerland, and Austria, Vienna. (Wirtschaftsförderungsinstitut der Bundeskammer der gewerblichen Wirtschaft, 3 Hoher Markt, Vienna 1, Austria)


17-20. German Soc. of Surgery, 80th meeting, Munich. (E. Derra, Chirurgische Klinik der Medizinischen Akademie, Moorenstr. 5, Düsseldorf, Germany)


18-20. Stereology, 1st intern. congr., Vienna, Austria. (Vienna Medical Acad., Alserstrasse 4, Vienna 9)

18-21. Radiology in Otolaryngology, intern. symp., Bordeaux, France. (G. Guil- len, 45, cours du Marechal Foch, Bordeaux)


21-24. Rare Earth, conf., Grand Bahama Island. (K. S. Vorres, Dept. of Chemistry, Purdue Univ., Lafayette, Ind.)

21-25. International College of Surgeons, North American Federation, annual, Los Angeles, Calif. (W. F. James, 1516 Lake Shore Dr., Chicago 10, Ill.)

22. Institute of the Aerospace Sciences, Dallas, Tex. (R. R. Dexter, 2 E. 64 St., New York 21)


22-24. Biomedical Engineering, 3rd symp., San Diego, Calif. (J. H. McLeod, Program Committee, 8484 La Jolla Shores Dr., La Jolla, Calif.)


22-26. Radiosotopes and Radiation in Plant and Animal Insect Control, intern. symp., Athens, Greece. (I. H. Kane, In-
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tern. Conferences Branch, Div. of Special Projects, U.S. Atomic Energy Commis-
ision, Washington 25)
22–27. American Acad. of Neurology, Minneapolis, Minn. (C. A. Kane, 80 E. 
Concord St., Boston, Mass.)
23–25. Electronic Processes in Di-
electric Liquids, Durham, England. (Ad-
ministration Assistant, Inst. of Physics 
and the Physical Soc., 47 Belgrave Sq., 
London, S.W.1, England)
24–26. German Soc. of Hygiene and 
Microbiology, Würzburg. (W. Herrmann, 
Städtischen Krankenanstalten, Robert 
Koch-Haus, Essen, Germany)
24–26. Institute of Radio Engineers, re-
regional conf., San Diego, Calif. (E. Herz, 
4444 Mt. Castle Ave., San Diego 17)
24–28. German Roentgen Congr., 44th, 
Baden-Baden, Germany. (H. Lossen, GRC,
Universitäts-Strahleninstitut, Langenbeck-
str. 1, Mainz, Germany)
25–27. Mississippi Acad. of Sciences, 
University. (C. Q. Sheely, Dept. of Chem-
istry, Mississippi State College, State Col-
lege)
25–27. Ohio Acad. of Science, Wilber-
force. (G. W. Burns, 505 King Ave., Co-
lumbus 1, Ohio)
25–27. Population Assoc. of America, 
Philadelphia, Pa. (P. C. Glick, Bureau of 
the Census, Washington 25)
25–27. West Virginia Acad. of Science, 
Buckhannon. (J. A. Duke, S.J., Dept. of 
Chemistry, Wheeling College, Wheeling, 
W. Va.)
25–28. Association of Clinical Scienc-
ists, Louisville, Ky. (R. P. MacFate, 54 
W. Hubbard St., Chicago 10, Ill.)
26–27. American Mathematical So-
ciety, University Park, N.M. (AMS, 190 
Hope St., Providence 6, R.I.)
26–27. American Assoc. of University 
Professors, San Francisco, Calif. (W. P. 
Fidler, AAAP, 1783 Massachusetts Ave., 
NW, Washington 6).
26–27. Illinois State Acad. of Science, 
Carbondale. (C. L. Kanatzar, MacMurray 
College, Jacksonville, Ill.)
26–27. South Dakota Acad. of Science, 
Rapid City. (T. Van Bruggen, State Univ. of 
South Dakota, Vermillion)
27. American Soc. for Experimental 
Pathology, Atlantic City, N.J. (K. M. 
Brinkhous, Dept. of Pathology, Univ. of 
North Carolina, Chapel Hill)
27. Clinical and Diagnostic Aspects of 
Enzyme Multiplicity, colloquium, Ghent, 
Belgium. (R. J. Wieme, Laboratory of the 
Medical Clinic, Pasteurduide 2, Ghent)
20th, Atlantic City, N.J. (APS, 265 Nassau 
Rd., Roosevelt, N.Y.)
27–2. American Ceramic Soc., Pitts-
burgh, Pa. (C. S. Pearce, ACS, 4055 N. 
High St., Columbus 14, Ohio)
28–3. American Assoc. of Cereal Chem-
ists, Minneapolis, Minn. (C. L. Brooke, 
Merck & Co., Rahway, N.J.)
28–29. Electron Beam Technology, 5th 
intern. symp., Boston, Mass. (J. R. Morley, 
Alloy Electronics Corp., 35 Cambridge 
Pkwy., Cambridge 42, Mass.)
29–30. Combustion Inst., Western States 
Div., San Diego, Calif. (G. S. Bahn, 16902 
Bollinger Dr., Pacific Palisades, Calif.)
29–1. International Acad. of Pathology, 
52nd, Cincinnati, Ohio. (F. K. Mostofi, 
Armed Forces Inst. of Pathology, Wash-
ington 25)
Scientific Radio Union, annual, Wash-
ington, D.C. (Miss J. Hannam, Natl. Acad. 
of Sciences, 2101 Constitution Ave., NW, 
Washington 25)
29–3. Society of Photographic Scientists 
and Engineers, annual, Atlantic City, N.J. 
(D. L. Castellini, 98 Leland Terrace, New 
Shrewsbury, N.J.)

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May

1–2. Polymer Science and Technology, conf., London, England. (J. N. Radcliffe, 
Plastics Inst., 6 Mandeville Pl., London, W.1)
1–2. American Assoc. for Contamina-
tion Control, natl., Boston, Mass. (AACC, 
6 Beacon St., Suite 626, Boston 8)
1–4. American Film Festival, New 
York, N.Y. (Educational Film Library 
Assoc., 250 W. 57 St., New York 19)
2–4. Human Factors in Electronics, 4th 
anual symp., Washington, D.C. (F. 
Chernikoff, U.S. Naval Research Labora-
tory, Code 5124, Washington 25)
2–4. Kansas Acad. of Science, Law-
rence. (G. A. Leisman, Dept. of Biology, 
Kansas State Teachers College, Emporia)
2–4. American Philosophical Assoc., 
Western Div., Columbus, Ohio. (L. E. 
Hahn, Washington Univ., St. Louis 30, 
Mo.)
2–4. Virginia Acad. of Science, Roa-
deke. (P. M. Patterson, Hollins College, 
Hollins College, Va.)
2–5. Council of Long Island Technical 
Societies, exposition of technology and 
industry, West Hempstead, N.Y. (CLITS, 
Route 110, Farmingdale, N.Y.)
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of these causes is the belief, in the minds of most people, that when it is considered totally, science is a relevant and important aspect of our society and individual lives.

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**Memory, Enzyme Induction, and Porphyrins**

C. E. Smith speculates shrewdly [Science 138, 889 (1962)] on the question of whether memory may be associated with enzyme induction. He indicates at least three aspects of experimental evidence suggesting that the basis of a form of biological "memory" lies in an increase in enzyme concentrations associated with transmitter substances "as a long-lasting effect of stimulation."

From the viewpoint of enzyme induction it may be in order to consider the two different components in the enzyme—namely, the template protein and the prosthetic porphyrin. Chemically, these parts of the molecule are different in structure and action. While the bonding compound has not yet been identified, in the scheme suggested by Smith, the question arises whether the substance might be a porphyrin, either metal-containing or metal-free?

There seems to be another factor which deserves investigation. This is the increase in the concentration of porphyrins in the central nervous system with evolutionary development. This has been clearly shown by H. Klüver [Science 99, 482 (1944); J. Psychol. 17, 209 (1944); Biochemistry of the Developing Nervous System (Academic Press, New York, 1944), pp. 137–144]. Porphyrins have not been isolated from the peripheral nervous system. There appears to be an "ascending porphyrinization" in the postnatal development of the central nervous system in birds and mammals.

It has been found that porphyrins exist in both the metal-containing and the metal-free conditions. In the enzyme the porphyrin is associated with a metal; the metal-free derivatives are located.
in the nerve system. How the sodium and the potassium ions pass through the nerve tissue, and whether the porphyrin residues are involved in allowing the ions to diffuse, is worthy of study. The holes which are known to exist in membranes and the open ring in porphyrins are of the same size.

Conductivity measurements have been made on closely allied chemical species of the porphyrins—namely, the phthalocyanines and the benzazoporphyrins. At the meeting of the Organic Crystal Symposium in Ottawa on 10-12 October 1962, Harrison and Heilmeir demonstrated the Hall effect in metal-free phthalocyanines.

In view of the increasingly provocative relationships that are becoming evident between computers and brains, it would seem that any factor involving semiconduction might be an important aspect of "memory" phenomena. Memory may reside in the arrangement, stacking and interplay of the prosthetic moieties of the enzyme. Currently, we are engaged in the synthesis and the preparation of single crystals of porphyrins which may be examined from the viewpoint of conductivity.

E. L. KROPA
Batelle Memorial Institute,
Columbus, Ohio

CHAUNCEY D. LEAKE
University of California School of Medicine, San Francisco

In his recent article "Is memory a matter of enzyme induction?" (1), Smith develops a theory which states essentially that memory may be a function of changes in the synaptic concentration of the neuroenzyme acetylcholinesterase. These variations in enzymatic activity are considered to be the result of alterations in the rate of neurotransmission which cause, momentarily, changes in the concentration of "free" substrate (transmitter substance). The amount of "free" substrate is considered to be the factor responsible for altered enzyme activity, since the transmitter substance induces the formation of the neuroenzyme. Therefore, the concentration of substrate present at any time regulates the rate of enzyme production.

In our laboratory we have come to these same conclusions independently, on the basis of results of several experiments in which changes in the level of acetylcholinesterase activity in the brain of the killifish Fundulus heteroclitus in response to thermal stress (2) were measured. It was found that a
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homeostatic mechanism exists in the brain of this fish which regulates the activity of brain acetylcholinesterase and maintains it as a constant, after a period of acclimation, regardless of any modification in ambient temperature. We have considered this to be a case of alteration in the rate of enzyme induction due to temporal changes in the concentration of the neurohumor acetylcholine. Thermal stress changes the substrate-enzyme relationship by modifying enzymatic activity. It is clear from studies in which enzymatic activity has been depressed by treatment with irreversible cholinesterase inhibitors (3) and in which additional transmitter substance has been added to biological systems (4) that there is a dynamic balance between enzyme concentration and substrate liberation at all times. In normal neural function this balance is probably upset by the liberation of varying amounts of acetylcholine per unit of time, due to changes in the rate of neuronal firing. Thus, it is not necessary to postulate, as Smith does, "that each nerve impulse would release from the bound form more acetylcholine than an equivalent impulse released before induction" to account for changes in the rate of enzyme induction.

We have expanded this theory to consider the role of a feedback mechanism for neuroenzyme induction in terms of neuro-integration, and also to consider the method by which the individual neurons can reliably retain their independent and integrated functions (memory) over long periods of time in the apparent absence of repetitive input.

If the enzyme-induction hypothesis is correct, we may postulate a dual role for synaptic cholinesterase. First, this enzyme must, within a very short time, hydrolyze all of the transmitter substance liberated, to prepare the nerve for further transmission. Second, since the presence of acetylcholine at the presynaptic membrane depolarizes the nerve and prevents further impulse transmission, the synaptic concentration of this enzyme apparently must also function to regulate the time interval before a second action potential can be transmitted. Cells containing more enzyme will hydrolyze acetylcholine at a higher rate, with resultant shortening of the "no-impulse" period, and will therefore be capable of transmitting impulses at a higher frequency. Cells having a lower concentration of enzyme will have a longer "no-impulse" period, and therefore the maximum
rate of impulse transmission will be lower.

Since much information is passed through the nervous system in terms of impulse frequency alone (5), and since it appears possible for neurons to modify their frequency response, "learning," on the biochemical level, may be considered to be the result of altered synaptic enzyme concentrations, mediated by changes in the rate of enzyme induction. Thus, repeated stimulation of a nerve with, initially, low-frequency transmission capability should result in attenuation of the nerve for the passage of coded information of high as well as low frequency.

Finally, we have postulated that long-term stability of synaptic enzyme concentrations and "memory" may be a function of rhythmic excitatory waves that are found in the brain and also in individual nerve cells (6). It is possible that a single periodic pulse through a nerve or a neuronal relay system may be sufficient to maintain synaptic enzyme concentrations that allow for passage of impulses at a preset frequency.

The hypothesis outlined certainly cannot in itself account for the complex nature of neuro-integration, but, in connection with other information about coding, may clarify the picture of brain function. The question of whether there are similar compensatory mechanisms in other neurohormonal systems remains to be answered.

**Morris H. Baslow**
Department of Marine Biochemistry and Ecology, New York Aquarium, Brooklyn, New York

**References**

Baslow's data do suggest both enzyme induction and a feedback system, but I feel that he and I are not talking about the same thing when we refer to learning. He states that it is not necessary to postulate an increase in the amount of transmitter liberated by any one impulse in order to account for induction. My point is quite the opposite: After induction has occurred — the induction of enzymes essential to synthesis of a transmitter — the consequence would be the liberation of

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more transmitter per impulse. Briggs and Kitto [Psychol. Revs. 69, 537 (1962)] reached essentially similar conclusions in a paper I wish I had written. Although they published the hypothesis at the time I did, they have clear priority because their manuscript was accepted some 7 months before mine was. Their work, like Baslow's, deserves the attention of readers interested in this problem.

C. E. Smith
San Jose State College,
San Jose, California

Smoking, Arteriosclerosis, and Age

The excellence of the report "Cigarette smoking and arteriosclerosis" [Science 138, 975 (1962)] is lessened by the fact that the statistics were not adjusted for age. In large epidemiological studies the average age of nonsmokers is usually 4 or more years higher than that of smokers. In this particular study the age difference of the two groups might have been greater or it might have been insignificant. It is not possible to correctly interpret the author's conclusion without this information because of the association of arteriosclerosis with age.

George E. Moore
Roswell Park Memorial Institute,
Buffalo, New York

In the report "Cigarette smoking and arteriosclerosis," Sigmund L. Wilens and Cassius M. Plair state, "There is no proof that sclerosis of coronary arteries develops more rapidly in cigarette smokers than in nonsmokers." However, they fail to present the distribution, in tabular or statistical form, of their subjects' ages, and their most sophisticated evidence (Table 3) seems to contradict their statement. They do say that the cigar and pipe smokers (for whom they have no criterion for determining intensity of smoking) tend to be older than the other groups, and that the light and moderate smokers of cigarettes are "a somewhat younger group on the whole, than... the other groups." No comparison of the age of the nonsmokers and the heavy smokers is presented, though such a comparison is essential if we are to interpret the findings objectively. The authors are analyzing for evidence of degenerative diseases (or the effects of aging), such as myocardial infarcts, vascular scars of kidneys, cerebral infarcts, and...
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cerebral hemorrhage. Surely the age of the people at death is a critical, pivotal characteristic in this type of research, for if the smokers of cigarettes are generally younger than the non-smokers, one should naturally expect them to show fewer of the degenerative diseases (other things being equal)—not more, as is generally the case in the evidence presented for the "diseases" listed above.

In the one case where they make specific use of the subject's age in relating smoking habits to the severity of aortic sclerosis at necropsy (see their Table 3), they gave each aorta an "arteriosclerotic age" through comparison with a set of previously prepared photographic transparencies of aortas which represented the standard or average degree of sclerotic change observed in each half-decade of adult life." As a result of this method of analysis, Wilens and Plair found highly significant differences ($P = .001$) in the degree of aging, favoring 40 percent of the non-smokers as compared to the heavy smokers.

I was confused by the fact that the probability figure in their Table 1 was given as $P = .028$, while they claimed that this was not significant, until I discovered that this was a typographical error and that the probability should actually have read .28.

We need more information before it can be so firmly stated that cigarette smoking does not contribute to the development of arteriosclerosis.

**Carl L. Johannessen**

*College of Liberal Arts, University of Oregon, Eugene*

The mean and median ages of the various categories of smokers discussed in our report are shown here in Table 1. Undoubtedly, if heavy smokers of cigarettes had survived, on the average, 3 years longer, as the nonsmokers did, the incidence of myocardial infarcts among them would have been somewhat but not greatly increased. The reports on the clinical incidence of myocardial infarcts suggest that the increased susceptibility in heavy smokers of cigarettes is so pronounced that it is not concealed by their tendency to develop fatal cancer of the lung.

The development of one fatal condition unquestionably influences the incidence at necropsy of other fatal conditions. In other words, if a high percentage of heavy smokers of cigarettes did not die of cancer of the lung, they might die a few years later of myocardial infarction. Our observation that the severity of aortic sclerosis is significantly increased in such smokers suggests that this might be the case. Whether or not this is so, the fact remains that in our series there was no significant increase in the number of myocardial infarcts at necropsy in the heavy smokers of cigarettes, contrary to what the clinical reports would have led one to expect.

We did not intend to state "firmly" that "cigarette smoking does not contribute to the development of arteriosclerosis" but meant to suggest that the connection between the two, if it exists, is much more tenuous than has been claimed and that, as a practical matter, the heavy smoker of cigarettes may not be much more likely to develop a myocardial infarct than a non-smoker.

**S. L. Wilens**

*New York Veterans Administration Hospital, New York*

**Science Reporting in Journals and Newspapers**

The editorial of 18 January, "Science reporting," treats fairly neither the problem nor the proposed solution. The policy of the American Institute of Physics, I believe, is to deny publication to papers the main contents of which have been released by the authors or their organizations to the daily press, but to encourage the publication and the release to the public of such information after it has appeared in the scientific journals. Thus, reporters who wish to verify the relevance of a release may consult their scientific advisers, who will have details at hand in a form guaranteed by the editorial standards of the scientific journal to be reasonably lucid, complete, and of some novelty. In my opinion, the results of a scientific investigation do not exist until
WAVES IN ANISOTROPIC PLASMAS

By William P. Allis, Solomon J. Buchbbaum, and Abraham Lias. The many published researches on the propagation of waves in plasmas have resulted in a confusion of terminology and a variety of special assumptions applicable only to limited domains. This book brings order out of that diversity, using phase velocity diagrams and carefully indicating the resonances and cutoffs that bound the areas wherein each diagram is applicable. The authors deal first with the general theory of plane waves in an unbounded plasma in a magnetic field; magnetioionic, acoustic, and magnetohydrodynamic regimes are given a unified treatment. The second section deals with guided wave propagation in magnetized wave guides and exhibits in detail the interplay between electromagnetic and kinetic energy flows.

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VARACTOR APPLICATIONS

By Paul Penfield, Jr. and Robert P. Rafuse. This most-comprehensive analysis of varactor capabilities examines the effect of parasitic series resistance upon varactor operation in their many circuit applications, and shows the circuit conditions which must be met to achieve optimum performance. “An excellent theoretical treatment... a very useful addition to the engineering tools in this area. The authors deserve high praise for the excellent bibliography... the most up-to-date and exhaustive on the subject.”—Microwave Journal.

1962 6 x 91/4  xiv + 623 pp.  $15.00

HIGH MAGNETIC FIELDS

Edited by Henry Kolm, Benjamin Lax, Francis Bitter, and Robert Mills. These papers on one of the fastest-developing areas of physical research, today, describe elegant experiments now in progress at such leading centers as the Clarendon, Cavendish, Kamerlingh Onnes, and National Magnet laboratories and present theoretical and experimental work by such distinguished physicists as Kurlt, Lehnert, Bloembergen, Bitter, Kunzler, Parkinson, and Sugawara. “... Give an excellent picture of the actual state of this extensive field... and includes complete lists of references on previous work.”—Nuclear Science and Engineering.

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1961 5½ x 9  xiv + 493 pp., illus.  $10.75

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they have been published in a form sufficiently complete for the scientific community to judge the value and the validity of the work.

It is hard to believe that the general public loses anything by waiting for scientific news. If the work is of real interest, it gains because the informed opinion of the scientific community is made available to the reporters. Too often flamboyant scientific results appear in the press and are later found to be incorrect. The correction is not newsworthy and does not reach the general reader.

A man or organization can make a public reputation by releasing his results to the general press without fear of effective criticism. Often it is the fault of his organization and not on the initiative of the individual scientist that such releases are made. The policy of the American Institute of Physics thus gives scientists incentive to police their own organizations and to eschew publicity at the expense of other scientists who publish in scientific journals.

RICHARD L. GARWIN
I.B.M. Watson Research Laboratory,
Columbia University, New York

For Physical Review Letters and Applied Physics Letters to refuse reports of research because there has already been a report in the public press is to debase scientific integrity by applying personal or moral standards, or sometimes even adventitious ones, to the dissemination of research findings.

Such a discomfited author may indeed have been seeking a cheap publicity, or he may sincerely have believed that he was helping the public to understand science and thus ultimately to support it. With a good science reporter he could have been right. Maybe his enthusiastic colleagues or a hopeful committee on public relations pushed him into prominence reluctantly or even unwittingly. Suppose, moreover, that the implicated author is really at bottom a publicity hound; what business is it of the scientific fanatic to deny his research an equal chance to be judged along with the work of more modest investigators? The weight of the history of science is on the side of personal vanity's favoring discovery.

It is also true that even good science reporting in the public press is unlikely to become a proper substitute for the scientist's own report in a standard journal. . . . No scientist goes to the New York Times for such information and his abstract journals do not cover

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SCIENCE, VOL. 139
it. Exclusion of investigators from the normal channels of scientific communication because they have consorted with the press seems to me to be partly a moral issue and an uncertain one at that.

EDWIN G. BORING
Harvard Psychological Laboratories, Cambridge, Massachusetts

... Any effort to restrict or confine coverage of scientific news seems to me incredibly shortsighted. I cannot see how it serves scientists' interests. Or those of citizens generally.

Some science reporting is irresponsible. So is some reporting of government news. But shall we wait for the history books, a generation or more from now, to know in what directions our world has gone? ...

ROBERT SOHNGEN
No address given

... I do not believe a scientist's behavior can be divorced from his standards of work in the laboratory. Restraint in stating conclusions in Science presupposes restraint in headline-seeking in the New York Times...

The public is, undoubtedly, entitled to know what is being done with the tax funds it spends on research. The way to keep it informed is not by competitive advertising on the part of individual investigators. The responsibility lies with the granting organizations which deal out the money.

B. RAYMOND FINK
College of Physicians and Surgeons, Columbia University, New York

... The tragedy of the situation is that the two publications of the American Institute of Physics, Physical Review Letters and Applied Physics Letters, are both financially endowed by a public agency, the National Science Foundation. ... So taxpayers are heavily subsidizing, on the basis of speed of announcement, two more or less private publications which inhibit, and in practice prohibit, the speedy announcement of scientific findings through any other media.

ARTHUR KRANISH
Washington Science Trends, Washington 4, D.C.

... An article in Organic Gardening (December 1962) mentioned results of some research on activities of earthworms which I did not know about. After some correspondence I have been informed that the material mentioned

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G. E. GATES
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... An item noticeably missing, which might also cause concern on "prepublication," is the technical report. Much research is performed under federal support, and research progress is published in report form, or in manuscripts incorporating their contents in part, which are also submitted to the scientific journals.

These reports have, at best, limited circulation. A given report presents only a single facet and includes either data and details too voluminous for inclusion in journals at current page costs, or information of a progress nature which needs integration with future results before journal publication would be appropriate.

R. W. SWANSON
505 Seward Square, SE,
Washington, D.C.

... The policy of the American Institute of Physics makes the barrier between scientists and the public higher and thicker. ... It will not improve the quality of science news published in mass media. In fact, it may dissuade competent and honest scientists from cooperating with science writers.

H. P. LEIGHTY, JR.
Department of Metallurgical Engineering, University of Missouri, Rolla

The timely release to the press of scientific news involves a deeper question than how much is owed the public. ... Who owns science? Whom does science serve? Is the scientific fraternity truly a type of Greek-letter society, whose mysteries are to be witheld from the barbarian non-fraternity men? In recent years a widened chasm between scientists and the public, resulting from the increasing complexity of maintaining meaningful communication, has created the non-scientists' reaction against "eggheads."

Accurate reporting of science news is the reporter's responsibility. The scientist has discharged his responsibility in providing information fairly and accurately and in as simple language as the complexity of the subject matter will allow. If the reporter then errs, it is the public he fails, not the scientist; his disservice is to journalism more than to science. But to withhold science news from the press as a whole because of the malfeasances of some of its members is illogical.

The gee-whiz approach to science journalism flourishes not entirely because science writers "tend to seek the more glamorous items." The public is interested in such items more than the others, but also shows interest in what will, or can, affect it directly. It is more interested in reading of the possibility of controlling weather by means of black asphalt ground coatings than it is in the effect of estradiol on the hypothalamic neurons of rats. The latter may eventually have more direct effect on the lives of humans than the former, but this is difficult for the public to see immediately.

Both of these items are taken from the same issue of Science in which the editorial now under discussion appears; the item on weather control did appear in the daily press. If the other appeared, I missed it.

The American Institute of Physics has a legal right to buttress the walls of its tower with more ivory, and to its opinion that "scientific discoveries are not the proper subject for newspaper scoops." But it only makes itself, and by extension all scientists, look foolish and petty.

E. E. SLOMAN
1100 Armada Drive, Pasadena, California

I was somewhat surprised because you apparently misunderstood the raison d'etre of the two express journals, Physical Review Letters and Applied Physics Letters, and chagrined because you call their respective editors on the carpet for obstructing rapid release of scientific information to the public.

The original intention leading to the establishment of an express journal such as APL was to expedite, rather than to delay, the appearance of urgent results in a form useful to the research physicist. Urgency is not necessarily synonymous with importance. There are many research papers of immediate and lasting importance which are not urgent. On the other hand, urgent "letters" may not have appreciable durability. A communication is judged to be urgent primarily because in the editor's or reviewer's considered opin-
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ion it may exert a strong influence on the direction of future effort and the present expenditure of resources in an active field of physical research. Only a small fraction of urgent communications are of immediate interest to the general public, at least judging by the treatment given the various topics covered by APL.

The space available for these urgent letters is premium space. It is both costly and limited, as we can carry only about 20 letters each month. Why then should this premium space be used to announce to the scientific community research results already disclosed in the public press, or, for that matter, in the Bulletin of the American Physical Society in abstract form? The purpose of early announcement has already been fulfilled and the urgency in large measure has been removed. Further airing of this information would only reduce the available space in APL for the not so “newsworthy” but, nevertheless, urgent letters.

There is another point which the gentlemen of the press do not seem to have grasped. The APL is only one activity in our editorial office. It is, in fact, auxiliary to the Journal of Applied Physics, which carries full length research papers as well as short scientific communications. There is no restriction relating to prior, brief disclosure on papers submitted to JAP. Therefore, we have left the choice with the author: (i) He may submit his contribution in abbreviated form to APL, agreeing to withhold public announcement until after the letter’s appearance. (Acceptance, of course, is contingent upon the recommendation of a qualified reviewer. Failing acceptance on the basis of urgency, the contribution may still be considered as a communication to JAP.) (ii) He may announce his results to the news media immediately and submit either a full length paper or a short communication to the JAP. The reviewing procedures are equally rigorous for the two publications, but the minimum publication delay is 4 months for the Journal and 15 days for the Letters.

Our contributions are unsolicited (except for an occasional review paper) and there is no attempt to tell the author which course to follow. Our prime consideration is to serve the scientific community and it is to this end that our policies are formulated.

However, a deeper issue seems to be involved: Is the public interest better
served by rapid reporting or by accurate reporting? Toth infers that speed should be the overriding consideration. He is impatient with the reviewing system that the scientific community has voluntarily adopted in the interest of preserving the integrity of its record. If he had to wait 4 to 6 months to check his story, one might be sympathetic with his impatience. However, when the period required for publication is only 3 to 4 weeks, during which time the article has been critically reviewed by a qualified individual, one wonders, "Why all the fuss?" Indeed, it seems that express-letters journals offer a service to the general public as well as to the research community. At the cost of only a small temporal inconvenience, the public may now obtain through news media information which has been reviewed and which therefore bears a much stronger claim to objectivity and accuracy. In the same sense, express publication can be considered a service to the science news writer himself: He has reasonable assurance that the facts in his story have been checked at a level of competence higher than he can muster without a considerable degree of inconvenience.

These remarks should in no way be construed as disparaging of the valuable service rendered by science reporters. Theirs is a difficult calling and their efforts deserve the respect of both scientists and the general public. Moreover, there should be no fear that scientific journals will ever compete with the daily press or that their editors will as a matter of deliberate policy obstruct the reporter's access to information. Rather, I am convinced that the innovation of express journals will assist the science reporter in his important function of linking the scientist with the general public.

J. H. Crawford, Jr.
Journal of Applied Physics,
Oak Ridge, Tennessee

Consumption of Water

The article, "Human water needs and water use in America," by Charles C. Bradley [Science 138, 489 (1962)] appears to be based on erroneous assumptions. It is conceivable that Malthusian limits will be imposed upon the population of North America, but it will not be in the foreseeable future and least of all will it be because of lack of water resources.

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If we accept Bradley's argument that "3300 billion gallons per day are productive of crops or surplus water," and that this figure less unconsumed runoff gives "a remainder of about 2500 billion gallons per day which we are consuming, though perhaps wastefully, to produce our crops," then we can agree with him that, after "metered consumption" is added in, the total of 15,200 gallons per day per person is reasonable.

In the literature, however, consumption is generally taken to refer to the loss of water as a result of its withdrawal from streams, lakes, reservoirs, and ground-water storage by man. In effect we ask how much the "loss" due to natural causes has been increased by human development and use of the resource. In toto man's activities have had a number of consequences, but the impact has been small. Evapotranspiration in the humid East, the area with which Bradley is mainly concerned, has probably not changed greatly from the days of the pristine forests.

Bradley indicates that the figure for consumption is to be obtained "by subtracting the water which we are not using [runoff] from the total water available." This appears to correspond to the familiar formula: evapotranspiration = precipitation - runoff. Consumption so defined is relatively constant through time, if no great climatic change occurs. Application of the formula for an earlier year—1910, for example—yields results as follows. In 1910, when the population of the United States was half what it is now, the per capita consumption would have been 28,470 gallons daily, if we assume the withdrawal to have been only half that cited by Bradley. Hence, it appears that per capita use of water has been rapidly diminishing, and that this trend will continue with rising population.

Bradley proceeds to multiply the figure for present per capita "consumption" by that for anticipated population and concludes that "young Americans alive today will see a significant deterioration in their standard of living before they are much past middle age." To determine the validity of this prediction let us find whether a prediction made 50 years ago on the basis of per capita "consumption" and the anticipated population for 1960 would have been borne out, had the population been correctly predicted.

Using the 1910 per capita figure of 28,470 gallons per day in this man-
ner, we find that by 1960 the use of water should have been 5125 billion gallons per day, or more than the total daily supply of 5000 billion gallons which Bradley claims is now available in the United States (exclusive of Hawaii and Alaska). The limit should already have been reached, and Americans should now be experiencing a significant decline in their standard of living.

Readers may wish to consult the most comprehensive estimates, to date, of present and future water use—those prepared by Nathaniel Wollman for the recent Senate Select Committee ["Water Resources Activities in the United States: Water Supply and Demand," Select Committee on National Water Resources, U.S. Senate, 86th Congress, 2nd Session, Print No. 32 (Government Printing Office, Washington, D.C., 1960)]. These estimates indicate that consumption (in the widely accepted use of the term) in 1954 was 109 billion gallons per day, estimates for the years 1980 and 2000 being put at 190 and 253 billion gallons per day, respectively.

IAN BURTON
University of Toronto,
Toronto, Canada

ROBERT W. KATES
Clark University,
Worcester, Massachusetts

I share Bradley's concern over the need to manage our water supplies wisely, and in general I accept his figures on water supply. But my interpretation of those figures leads me to more optimistic conclusions for the future.

For the portion of the country accounting for the bulk of our crop production, Bradley gives a figure of 2500 billion gallons per day for evapotranspiration—the return of water to the atmosphere through transpiration from plants and by evaporation. This may be divided into two parts. (i) Evapotranspiration from cropland and nonforested pastureland. This was estimated for 1959 at about 1000 billions of gallons per day for the United States, by Ackerman and Lof in their book Technology in American Water Development. (ii) Evapotranspiration primarily from forest land, of which about a third is grazed, and from other rural land not used for farming.

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1962 by the U.S. Department of Agriculture, we are expected to provide for 261 million Americans in 1980 from a smaller acreage of cropland and pasture than was needed to support 180 million in 1959. Certainly, the continued yield increases required to make this possible will result in materially increased evapotranspiration per acre, but at a much lower rate of increase in water use than in population growth. As yield increases, evapotranspiration does not rise in proportion.

This argument applies even more strongly to the share of evapotranspiration from forest land and other rural lands. With increased population these lands will require more intensive management for increased timber harvests, recreation, and other uses. But more intensive use does not always imply much higher evapotranspiration. Conceivably it may even mean less evapotranspiration in some cases.

Therefore it seems to me that lack of water need not be a bar to a rise in population and a sustained high standard of living, provided, as Bradley points out, that we make full use of human ingenuity to prepare for the future.

KARL GERTEL
Economic Research Service,
U.S. Department of Agriculture,
Washington, D.C.

It is probably true, as Burton and Kates say, that man’s activities have not altered very significantly the total evapotranspiration figure since 1910, although I strongly suspect these activities have shifted the balance significantly from transpiration to evaporation. However, in 1910 Americans were still a long way from complete utilization of America’s arable land. Transpiration was not yet working directly for us on a full-time annual basis. In 1910 we were at the peak of our exploitation of renewable resources—we were “mining” our forests, grasslands, and natural reservoirs. This amounts to living mainly off water capital instead of income. Those days are almost gone, and as a nation we are about down to income and are using our land (and the water that falls on it) far more fully than we did in 1910. I suggested in my article that we may have about 10 percent of highly productive land left to put into use. Gains beyond this will probably be marginal. While I could be wrong in this assumption, I do not believe the error would have any long-run significance when
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15 MARCH 1963
we are considering a population that is now doubling every 40 years. It is one thing to double the 1910 population of 60 million, quite another to double the 1960 population of 180 million.

My remarks on the American standard of living were, to a certain extent, made tongue-in-cheek. The rich flavor of chlorine in my drinking water, an open sewer named Clearwater Creek, the green scum and aroma of dead fish coming from my old swimming hole all tell me that something obscene has long since affected the quality of my “water standard of living.” More important, perhaps, is the fact that, in the last 10 to 15 years, quantity of water has become an expanding problem in more and larger areas of the United States. There are very few major areas left where no water problem exists. Some places are in deep trouble. It therefore really takes no sophisticated mathematical insight to see that the limits of water supply in the nation as a whole, for the ways in which we are now using it, are practically at hand. In other words, I don’t argue too much with Burton and Kates’s manipulation of my figures since to me they merely suggest that we already have passed the peak in our water standard of living.

Regarding Gertel’s comments, is he saying that increased yield of crops (forest and pasture included) per acre will not require a linear increase in transpired water, or does he perhaps mean that by more thorough plant cover and management a larger proportion of the rainfall can be shifted from evaporation to transpiration? If he means the former the statement should be documented. If he means the latter I agree. In fact I hinted in my article that herein lies our biggest opportunity to effect water conservation.

Gertel suggests that the “people versus water” picture is not as bleak as I have painted it. Would he care to apply his own figures toward answering the question posed in my article: How many more years can we sustain our present water standard of living with the projected population curve?

I realize that the timetable for Malthusian limits to be imposed on the population of the United States is not really foreseeable. I merely indicated that present population trends and present rate of rainfall would, in 200 years, bring us to the point of using all our rainfall to raise our food; I based the calculation, of course, on the transpiration ratios and the assumed diet of 2 pounds of bread and 1 pound of meat per day per person. I understand that the latest census studies indicate a slight leveling in the population growth curve, and this curve, of course, is the key to any calculation of timetables. Furthermore, I am certain that long before we begin to approach Malthusian limits we will not be insisting on a daily ration of steak. Any wholesale dietary shift from bread and meat to, say, marine plankton would make the transpiration ratios meaningless although few Americans today would construe such a shift as a gain in our standard of living.

CHARLES C. BRADLEY
Division of Letters and Science,
Montana State College, Bozeman

“Critical Periods” in the Development of Behavior

Scott’s interesting article (1) on “critical periods” in behavioral development merits a thoroughgoing critical review bearing on the validity of his general conceptions of behavioral ontogeny basic to the idea which he has extrapolated from embryology to the study of behavior. Here, however, we comment specifically on certain inferences that might be drawn from his allusion to our recent article on behavioral development in cats (2).

In discussing his concept of critical periods, Scott reports us as having “suggested that there are critical stages of learning—that what has been learned at a particular time in development may be critical for whatever follows.” Although we are not disposed to dispute this broad statement, it is not ours. In our view, any such sentence should have a more comprehensive context, to the effect that what the young animal may attain in behavior at any phase of ontogeny depends upon the outcome of earlier development in its every aspect. The point we wish to emphasize here, however, is that our position might be seriously misunderstood in at least two important respects from Scott’s allusion to our article. (i) Although, as our study of social behavior in newborn kittens (2, 3) indicated strongly, learning is involved at all phases in behavior development, our findings have broader and very different implications for social ontogeny than might be gathered from Scott’s mention of the work. (ii) The
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context indicates that Scott has misunderstood the criticisms we have offered (2, 3) of his "critical periods" concept.

The gist of our objection is not that we favor describing three critical periods of social development, corresponding to the three main stages in the ontogeny of social-feeding behavior in kittens for which we found evidence, or whatever number of critical periods might be advanced as an alternative. We were interpreting our results from a viewpoint definitely at odds with Scott's notion of "critical periods" when we wrote (2): "These considerations favor a very different view of the concept of 'critical periods' from the one now held by many writers. In the social development of the cat, we are led to the idea that striking changes in the essential progression are grounded not only in the growth-dependent processes of maturation but also, at the same time, in opportunities for experience and learning arising in the standard female-litter situation. This conception of social ontogeny encourages stressing not just one or a few chronologically marked changes in the behavior pattern, but rather indicates that normally each age period is crucial for the development of particular aspects in a complex progressive pattern of adjustment." We consider the implications of this theoretical viewpoint for developmental research very different from those of Scott's concept.

Evidence supporting our view demonstrated that in kittens, at all age periods, social approaches preliminary to feeding behavior undergo a course of development in the litter situation significantly different from the behavior of kittens reared under conditions of isolation and fed from an "artificial mother" (2, 3). No evidence was found for any time interval in which the different conditions of rearing failed to produce a pattern of feeding approaches and suckling in kittens reared in isolation that was significantly different from that in normally reared kittens of corresponding ages. This result had been predicted from the theory of social ontogeny (4) which guided our work.

Those who examine this theory (4, 5) and related considerations (2, 6, 7) will find an emphasis up the fusion of maturation (growth-contributed) and experience (stimulation-contributed) processes at different stages in
behavioral ontogeny, together with the contention that the contributions both of maturation and of experience (the latter including, but not confined to, conditioning and learning), as well as the interrelations of these contributions, may differ greatly according to stage in any animal. This theory thus differs sharply from Scott's, with its emphasis (see 8) upon factors of maturation presumably specific for "critical periods" and its apparent assumption that "learning" is a distinct and probably a delayed contributor.

From our theory of behavioral development (4-6), we conclude that factors of maturation may differ significantly in their influence upon ontogeny, both in the nature and in the timing of their effects, according to what relations to the effects of experience are possible under the existing conditions. We found, in support of this view, that gains in suckling made by kittens reared in isolation differed greatly from gains made by litter mates reared by the mother, and that the kittens reared in isolation were, at best, only partially adapted to the demands of social feeding and suckling of the mother at the time of their return to the litter. The differences between the experimental animals and the control litter mates were striking. In no single phase of development during the first 2 months of life did these two very different conditions of rearing—with mother and litter or isolated, with an "artificial mother"—fail to have very different effects on the development of suckling responses, despite the presumable equivalence of potential factors of maturation for kittens in the two groups. The results indicate that the actual effects of maturation differed considerably in the two cases. We do not find such evidence compatible with the meaning of maturation that would seem to follow from the critical-period hypothesis.

What is social behavior? Scott states (1) that in puppies the period of socialization begins at approximately 3 weeks of age. We submit, however, that much of the evidence he cites bears only tangentially on the question of when socialization really begins. Although his article is mainly concerned with social behavior, it does not deal primarily with intraspecies behavioral relationships but deals, instead, with the responses of puppies to human handlers, of young birds to artifacts, and the like. Under the heading, "Process of

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<th>Cage No.</th>
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<td>#88</td>
<td>2&quot; to 3½&quot; long; 1½&quot; wide</td>
<td>Mice from 10 to 40 grams</td>
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<tr>
<td>#90</td>
<td>4½&quot; to 5½&quot; long; 2½&quot; wide</td>
<td>Rats/hamsters from 150 to 285 grams</td>
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<tr>
<td>#91</td>
<td>5½&quot; to 7½&quot; long; 3¼&quot; wide</td>
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<th>ULTIMATE VACUUM, MM Hg</th>
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<td>(Lithers/Minute)</td>
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primary socialization” we find, for example, citations of tests in which machine-fed and hand-fed puppies “became equally attached to people . . .” hand-fed puppies “yelped more when they saw the experimenter . . .” “hungry puppies became more rapidly attached to the handlers . . .” and “separating young puppies overnight from their mother and litter mates . . . speeded up the process of socialization to human handlers.”

The degree of equivalence between such results and the ontogeny of inter-actions among species mates may prove to be appreciable, but it has not been determined. We suggest that Scott, in basing his principal treatment of social development upon experimental inter-ventions featuring responses to conditions other than association with species mates has been led astray. In our studies of the social-feeding behavior of kittens we were able to dis-tinguish early forms of intraspecies social responses not evident in reactions to human handlers or to artifacts such as the “artificial mother.” A depend-ence upon the latter two sources of evidence might have led us to neglect aspects of intraspecies behavior which we found crucial for the understanding of social ontogeny.

With due emphasis upon intraspecies behavioral relations, we maintain that processes of socialization and forma-tion of the social bond begin at birth, if not earlier (2-6). Kittens make con-sistent progress from an hour or two after birth in becoming oriented to their environment and to species mates, in becoming adjusted to the litter situation and the “home area,” and in making individual, distinctive responses to particular nipples. Such aspects of behavior concern reciprocal relations of dependency with the mother and with litter mates, hence are social.

Because Scott was concerned with giving an account of the critical-period concept and illustrating it, evidence centered on the ontogeny of intra-species responses may not have seemed relevant to him. Yet, because his reference to our article might imply that it supports his own concepts of behavioral development, we assert that there is an important difference be-tween his view and our own.

T. C. SCHEINERLA
JAY S. ROSENBLATT
Department of Animal Behavior,
American Museum of Natural History,
New York

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Statements of contrasting theoretical positions are useful if they stimulate the collection of additional data, leading in turn to the modification and evolution of theory. I am sure that the comments of Schneirla and Rosenblatt were offered in this spirit, as is my reply.

The points raised in their letter go far beyond the scope of my original article. I believe that most of them are complementary rather than contradictory.

1) Failure to find a critical period for primary socialization in the cat. In order to thoroughly understand the development of behavior in a given species, one should have timed data on the development of basic behavioral capacities, including sensory, motor, and learning capacities, as well as on the development of patterns of social behavior. In addition, one should have a description of adult social organization, to which behavioral development seems to be strongly related. In the absence of such general background information for the cat, I have been unable to evaluate Schneirla and Rosenblatt’s data in terms of evidence for the existence or nonexistence of a critical period for primary socialization. Although cats are not highly social animals, I would be surprised if there were no such period in this animal, in view of the wide occurrence of the phenomenon in social birds and mammals, and even in insects.

2) Role of learning in behavioral development. With respect to the importance of learning in behavioral development, our points of view are supplementary rather than antithetic.

Where Schneirla and Rosenblatt emphasize the fusion of maturation and learning, I would add that the learning process also changes with time and can itself be thought of as a developmental process. Much evidence indi-
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states that the rate of development of the capacity for learning varies widely from species to species, but additional information is badly needed along these lines. To my knowledge, in no species, not even the human, has a study of any simple learning capacity been carried completely through the stages of early development.

3) Nature of the process of socialization. Schneirla and Rosenblatt have concerned themselves with the point at which the process begins; my article dealt with the period during which the process proceeds most rapidly. Both kinds of information are necessary for understanding the process. As I said in the article, processes are not necessarily limited to the periods in which they are most prominent.

4) Experimental methodology. Two principal methods of studying the development of social behavior have been used: cross-fostering between species (or upon models) and social isolation. The two methods produce quite different results. In general, cross-fostering transfers social relationships but often has little effect on the development of social behavior patterns. Social isolation prevents the development of social relationships and may distort or inhibit the development of certain social behavior patterns. Both methods contribute to our understanding of the process of socialization. Schneirla and Rosenblatt observe that the cross-fostering method is more likely to reveal the existence of critical periods; this method has not yet been applied in the cat.

5) The nature of behavioral development. Two testable hypotheses can be stated (in stating them I do not ascribe either extreme of the two viewpoints to Schneirla and Rosenblatt). (i) Behavioral development is a unitary process, and what takes place in one period of development is dependent on everything that went before. (ii) Behavioral development may be composed of many different processes having a considerable degree of independence, both within the individual and between species. The actual facts probably lie somewhere between these two viewpoints.

Behavioral development is an extremely complex phenomenon. Interpreting any given set of data it is important that alternative hypotheses be considered and eventually tested.

J. P. Scott
Roscoe B. Jackson Memorial Laboratory, Bar Harbor, Maine
Bodega Bay Nuclear Plant

The editorial "Civilian nuclear power" in the 14 December issue (1) very appropriately brings attention to the recent Atomic Energy Commission report which documents U.S. achievement in reactor technology. Although the quotation from this report cites the remarkable reduction in cost of fission-generated electricity to an "estimated 5.5 to 6 mills [per kilowatt-hour] for a large plant to be built in the near future at Bodega Bay, California," the editorial fails to point out that such economical operation of the proposed plant would be possible primarily because of the site. Were the plant to be situated in a more remote place where cooling water, transportation facilities, and other features of construction and operation are less readily available, the electricity could not be sold at so low a price—that is, nearly in the range of cost of electricity generated by fossil fuels.

The Bodega Bay reactor, would be the largest nuclear plant for generating electricity in the world. Water wastes would be discharged into the ocean, with resulting thermal changes and deposition of radionuclides similar to those in the discharge from the Hanford reactors into the Columbia River. Low-level gaseous wastes would be discharged at a point windward of a highly productive agricultural area which supports a 70-million-dollar-a-year dairy industry in the San Francisco milkshed. The plant would conform to AEC regulations regarding safety measures; nevertheless, the proximity of the reactor site to populated areas and areas of agricultural production would create special problems which should be taken into consideration. The site is just under ¼ mile from the San Andreas fault (previous regulations regarding the location of reactors stated that "no facilities should be located closer than ¼ to ½ mile to a known active earthquake fault," but the regulations were altered in April 1962 to read only "¼ mile"). Thus the hazard of possible natural catastrophe is added to the hazard of possible man-made catastrophe. Normal operating conditions would add detectable radiation to the local biosphere.

The site has long been considered a unique natural preserve; for this reason Bodega Head had been selected by the University of California as the location for a marine biological laboratory. A
The faculty committee of the university has so far failed to find an alternative laboratory site which would be as suitable, quite apart from considerations of accessibility. The propriety of the political maneuver by which the Pacific Gas and Electric Company acquired land rights has been questioned; moreover, alterations in federal and local regulations have facilitated the development of the project, although none of this help can be classed as subsidy.

The implications of the AEC report might well have been examined in the editorial from the viewpoint of the AAAS Committee on Science in the Promotion of Human Welfare: What is the impact of science on society as exemplified by nuclear technology at Bodega Bay? The AEC report states that over half the electricity in the United States may be generated by nuclear reactors by the year 2000. While such reactor development can be beneficial, is it not fitting to ask whether those who are affected by such growth are participating in the decisions which make such economic developments feasible? Are consumers of electricity willing to buy the product for 5.5 mills per kilowatt-hour and take an admitted but as yet unmeasured health risk (2) and sacrifice the ecological and scenic assets of Bodega Bay? Where will the public have an opportunity to register its opinion? The AEC has not yet held hearings for granting the license for the Bodega Bay reactor. (Recent legislation has reduced the number of mandatory public hearings for reactor licensing. The AEC report suggests that further simplification and streamlining of licensing procedures will encourage utilities to develop more nuclear power stations. Would it not be in the public interest to hold hearings regarding site selection before the utilities present a fait accompli?) Will the development of nuclear power demand that Bodega Bay be selected, as the only suitable location? Will all who are affected participate in that decision?

Malcolm L. Peterson
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References

The proposed Bodega Bay Atomic Park of the Pacific Gas and Electric Company is cited in a recent editorial as a breakthrough in nuclear power economics. The editorial quotes excerpts from AEC chairman Seaborg’s recent report to the President and asserts that the power rate figure “does not involve a subsidy.”

It happens that this particular power plant will be built on a scenic headland, about 50 miles north of San Francisco, which was included in the 1955–56 California State Park master plan. For some reason the state master plan did not receive open and public consideration by the Sonoma Planning Commission, and this apparent pocket veto made it possible for Pacific Gas and Electric to purchase the initial site on Bodega Head and to acquire an additional 67 acres for an exclusion zone through exercise of its rights of eminent domain. Further, Sonoma County has granted an easement for three lines of high-tension wires along the entire length of its only coastal park, Doran Beach County Park.

Bodega Head had been selected by the University of California as the site for its future Marine Biological Laboratory. Through construction of an access road and the emission of 250,000—perhaps eventually 1 million—gallons of hot water per minute, the power station will transform the area from a unique class A site to a class B site for a marine facility. The history of this somewhat controversial matter is documented in the booklet “A Visit to the Atomic Park,” published and circulated under the auspices of the Sierra Club in San Francisco.

The power economics of the Bodega Bay reactor are discussed in an article by Dresner and Weinberg in Reviews of Modern Physics [34, 747 (1962)]. These authors point out that the price of fossil fuel at Bodega Bay is much higher than the average for the rest of the United States. They go on to say that the exact method of calculating the fixed plant cost, the initial outlay for fuel, the plutonium buy-back arrangement, the lease charge for U₃O₈ hold-up, and the assessment for spent-fuel reprocessing will all be instrumental in dictating the ultimate cost of mills per kilowatt-hour at the Pacific Gas and Electric facility. The authors state: “These prices are set largely by government fiat; the current prices represent a substantial government subsidy for the Bodega Bay plant.”

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problem can eventually be solved, Dresner and Weinberg conclude that the development of "breeder" reactors which can utilize abundant low-grade uranium and thorium ores is "one of mankind's most important ultimate tasks." Reactors of the Bodega design will decimate the national reservoir of high-grade uranium ore without making any substantial contribution to technology. Nonetheless, convertors of this type are fostered by the AEC, probably because, as Chairman Seaborg has said, the intensely competitive nuclear equipment industry is "over-capitalized and under-used at the present time" and it is thought necessary to keep the uranium industry "viable" during the period of transition to breeder reactors.

Finally, the Price-Anderson Act of 1957 provides a federal liability indemnity of $500 million for each catastrophic accident in a nuclear installation. Through this subsidy, ethical standards of safety may have been bypassed and ultimate liability transferred to the taxpayer [A. Ackermann, Proc. Am. Soc. Civil Engrs. 87, No. PP2 (1962)]. This point is especially relevant to the Pacific Gas and Electric reactor in view of the proximity to the San Andreas fault and the meteorological instability of the Bodega area.

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In my editorial I cited the proposed Bodega Bay plant with its estimated cost of 5.5 to 6 mills per kilowatt-hour merely as an example of a continuing trend toward lower costs of nuclear power. I was unaware of the controversy over the site of the proposed installation. On that matter I have little knowledge. With respect to estimating future trends in costs of nuclear power I do have a special basis for the opinion that figures considerably lower than those cited for Bodega Bay are in sight. Indeed, Alvin Weinberg has recently spoken of costs as low as 1.5 mills per kilowatt-hour for a very large installation.

As for the matter of subsidy, the monetary value of the government insurance is small. The utilities are re-
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required to obtain a substantial amount of insurance with private companies. This covers most of the accidents which might occur. The government insurance in effect amounts to a $60 million deductible policy. The utilities pay AEC a fee of $30 per thousand kilowatts of thermal energy as a premium to obtain the additional coverage up to $500 million.

It is difficult to establish what the true competitive position of oil and nuclear energy would be in the absence of federal action. The government has set an artificially high price for natural uranium. In a free market its price might be half what it is at present. The 27½-percent depletion allowance for oil is worth about a billion dollars a year to the petroleum industry. If this allowance were removed, the competitive situation with respect to energy in California would be considerably changed.—P.H.A.

Activists and Nonactivists

The article “Divergent reactions to the threat of war” [Science 139, 88 (11 Jan. 1963)] raised a number of important issues, mainly in the realm of group perception. For instance, the re-study seemed to suggest that both groups had been attentive to those arguments and points of view presented publicly that were most closely in accord with their previous thinking. After intensive public propaganda by opponents of Medicare, as well as by its supporters, the oasis group was more against it, and the People for Peace group were more in favor of it.

In their conclusion the authors express surprise at the “similarity between the two groups” and suggest that this may have been due to the “particular community studied.” May I suggest another alternative, which evidently was not considered by the authors? It is significant that out of a community of 8000 people (3500 adults?) only 54 sought concrete community action of any kind during the Berlin crisis (unless there was other activity which the authors do not tell us about). Perhaps the two groups are relatively similar in a number of ways because they represent the activists in the community. They are probably the best-informed members of the community and those who tend to respond to emergency conditions with some kind of concrete ac-

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tion. Despite fundamental differences between the two groups, the many overlapping areas of attitude and belief would seem to support this hypothesis.

In any case, it would be interesting to compare the beliefs, attitudes, and personalities of the members of these two groups with those of a sample of nonactivists in the same community, to see whether the differences between the joiners and the nonjoiners is any greater than the differences between the two groups of joiners.

EDWARD J. JAY
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University of California, Berkeley

Ultrafiltration
Membrane Performance

Clark states (1) that the driving force for ultrafiltration is the chemical potential. This is a misleading statement. Any difference in chemical potential that arises across the membrane is a (hitherto unexplained) result of the pressure ultrafiltration process and not its cause. The driving force for ultrafiltration is the applied-pressure difference across the membrane. It is immaterial whether this pressure difference be expressed as such, or as, for instance, a chemical potential gradient, but in order not to confuse cause and effect, and in order to explain clearly the various phenomena inherent in pressure ultrafiltration, it is best to consistently view the external energy source, the applied-pressure difference, as the driving force for ultrafiltration.

A paradoxical phenomenon is this: The higher the pressure is, the more salt the cellophane membrane retains in an ultrafilter containing a solution of salt in water. Clark (1) has not explained this phenomenon. For its explanation one needs more facts than just the data from Ambard and Trautmann's Table 2 (2), given by Clark. A few more useful data can be found elsewhere in their book: Tables 4, 5, and 6 demonstrate that salts with bivalent anions and monovalent cations are more strongly retained than salts with monovalent anions and monovalent cations, while salts with bivalent cations and monovalent anions are hardly retained at all, as compared to salts with monovalent cations and anions.

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fact that cellophane is negatively charged, it becomes clear that the salt retention must be due to electrostatic repulsion of the anions by the negatively charged membrane, a repulsion which is strongest when the anions carry the highest charge and weakest when the cations carry the highest charge (3).

The paradoxical phenomenon that the salt retention becomes more pronounced at higher pressures can only be due to the contribution of the streaming potential to the charge of the membrane (4). For the streaming potential \( E \), caused by pressing the solution through a charged membrane, is proportional to the applied pressure \( P \), to the potential of the membrane \( \xi \), and to the dielectric constant \( \varepsilon \), and inversely proportional to the viscosity \( \eta \) of the solution and to its conductivity \( \lambda \). This relationship is usually expressed, according to Overbeek (5), as

\[
E = \frac{P \xi}{4 \pi \eta \lambda}
\]  

(1)

A final proof of this hypothesis would be furnished if it could be shown that salt retention is higher at higher values of \( \xi \) potentials of otherwise identical membranes and under otherwise identical conditions. I recently made an experiment along these lines, ultrafiltering salt solutions at various \( \rho \)H's, and I found that the salt retention is indeed highest at the highest \( \rho \)H (when the acid groups of cellophane are highly dissociated), and lowest at the lowest \( \rho \)H (when these acid groups are hardly dissociated) (6).

Equation 1 for the streaming potential also shows why at the lowest salt concentrations the salt retention is highest: When the conductivity \( \lambda \) of the solution is lowered, the streaming potential \( E \) becomes higher (see 2, Tables 4 and 5). The necessity for turbulence above the membrane for obtaining maximum salt retention (see 2, Table 1; 4) is now easily understood, for without it salt enrichment just above the membrane would occur, locally raising the conductivity \( \lambda \) of the solution, which would cause a lowering of the streaming potential \( E \) (7). The after-sight surprisingly small influence of temperature changes becomes clear when it is realized that while the conductivity \( \lambda \) rises with the temperature, the viscosity \( \eta \) diminishes at a slightly higher rate. So a rise in temperature will result in a slight decrease in \( \eta \lambda \), resulting in a small increase in the streaming potential \( E \)—an increase even
further attenuated by a slight decrease in the dielectric constant of water, $\varepsilon$. Thus, the theoretical increase in streaming potential $E$, when the temperature is raised from 8° to 18°C, can be calculated to be of the order of 1.6 percent—a value in reasonably good agreement with Ambard and Trautmann’s observation (2, Table 3), that the salt retention rises 1.8 percent under these conditions.

It becomes evident that the requirements for an ideal membrane for desalting with the pressure method are as follows. (i) It has to have the highest possible charge (either negative or positive); (ii) it has to have a very small pore size, of the order of 30 Å or less [otherwise the electrostatic repulsion of small ions seems to be ineffective (4)]; (iii) it has to be as thin as possible (to allow for the highest rate of filtration at a given pressure); and (iv) it has to be strong (to withstand the highest possible pressure) (8).

C. J. VAN OSS
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National Veterinary College,
Alfort, France, and National Institute of Agronomical Research, Paris

References and Notes
3. Ambard and Trautmann’s thesis is that all these phenomena can be explained by invoking differences in hydrated volumes of ions at different concentrations. But (hypothetical) differences in hydration can explain neither the difference in influence of multivalency of anions and cations nor the enhanced salt retention at higher pressures.
6. C. J. van Oss, in preparation. (When a 10-milliequivalent sodium chloride solution is ultrafiltered under a pressure of 110 lb/in$^2$, the salt retention rises 5 percent at pH 3 to 20 percent at pH 11 for the first ultrafiltrates.)
7. The obvious necessity for agitating the solution has nothing to do with the overcoming of Clark’s “film resistances,” for the rate of filtration remains unchanged, whether or not the salt solution is agitated.
8. I am indebted to Professor A. M. Monnier (Laboratoire de Physiologie Générale, Sorbonne) for many fruitful discussions we have had on this subject.

Numerous ultrafiltration data show that salt rejection increases as the pressure difference across the membrane increases (1-3). Two hypotheses were advanced to explain the increase in salt rejection with increase in pressure: (i) that pressure that pressure affected the hydrated ions, presumably increasing hydration (1). The first purpose of my report (4) was to show that neither of

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15 MARCH 1963
Table 1. Summary of calculations for Fig. 1 from data of Ambard and Trautmann (1).

<table>
<thead>
<tr>
<th>Data: Filtering solution (NaCl, mg/1it.)</th>
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<td>460</td>
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<td>Calculations: $n_1 (10^{-1} \text{ mole NaCl/mole } H_2O)$</td>
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<td>$160$</td>
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<td>$1,300$</td>
<td>$197$</td>
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<tr>
<td>$1,200$</td>
<td>$28$</td>
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The data from Ambard and Trautmann which appeared in my report were selected because they illustrated the points which I wished to make quite clearly. Unfortunately, those data also masked other effects, because the initial salt concentration was held constant. Van Oss is quite correct when he maintains that the kind of salt and the concentration have marked effects on the rate of flow of salt across the membrane. Table 1 and Fig. 1 show the results of some calculations based on other data from Ambard and Trautmann (1) in which the initial salt concentration, $n_1$, is allowed to vary. The approximation that the difference in chemical potential, $\mu'_1 - \mu'_2$, is equal to $RT \ln (n_1/n_2)$ is used in the calculations. Since the slope of the line in Fig. 1 is equal to unity, the data show that the rate of flow of the salt, $J'$, remains directly proportional to the difference in chemical potential of the salt even though the initial concentration of the salt changes tenfold. The data also show that the resistance to flow varies inversely with the square of the initial concentration of the salt. Thus, in the method which I suggest for predicting the flows of salt and water during ultrafiltration, the changes in flow rate for different salts and different concentrations which cannot be accounted for by changes in chemical potential are ascribed to differences in resistance to flow. Why these resistances vary as they do is an interesting problem, on which van Oss's views may shed some light. I am sure that, as we learn more about ultrafiltration, the viewpoints of both van Oss and myself will be modified until we meet upon some common ground.

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15 MARCH 1963
Emotional Response to Nonreinforcement

In an interesting report describing cycles in the force of a lever response during the execution of fixed-ratio reinforcement sequences [Science 138, 516 (1962)], D. E. Mintz interprets his results in the light of a supposition that the animal discriminates the strength of its own response on the occasions when reward is obtained. This explanation places the phenomena in the category of response-shaping by operant discriminants.

There is, however, a clear possibility that the changes in response force during fixed ratio reinforcement sequences reflect, rather, an emotional reaction of the organism to nonreinforcement. One would expect such responses, if they occur, to contaminate the strength of instrumental level press, because tension of both smooth and skeletal muscle is part of the pattern of emotional discharge.

That conditioning procedures produce emotionality is, of course, a classic finding of Pavlov and Liddell. A particular verification as regards the case of the fixed-ratio paradigm demonstrates that gradually shifting the ratio toward higher values produces, in the cat, muscular tension which may conveniently be measured by the number of vocalizations emitted but which includes, in a pattern, tail-flicking, flexion of limbs, pacing, and so on [M. F. Halasz and H. F. Hunt, report to the 3rd World Congress of Psychiatry, Montreal (1961)]. Thus, the schedule employed by Mintz seems to be one where failure to obtain reward may have emotional repercussions. If the consequent tonus be thought of as cumulating from one nonreinforced response to the next, and as declining over successive reinforced presses, the curves presented might well be accounted for by simple superposition of general tension on the lever response.

References
The respondent side effects of operant schedules are often such as to interact with the positively rewarded instrumental behavior. Since these elicited reactions are in many cases primitive, phylogenetically and neurally, parsimony requires that they be considered before more complex discriminations are invoked by way of explanation.

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It is quite possible that the force variations during fixed-ratio responding are mediated by emotional responses, as Halasz suggests. However, in the study reported, the systematic changes in force were generally enhanced through repeated exposure to the reinforcement schedules, contrary to what one might expect if emotional habitation played a significant role. Moreover, the cycles that showed high and low forces typically had a period of less than 30 seconds. Twenty-five daily variations in force with cycles of this relatively high frequency do not appear to reflect the more integrative changes in tonus that would be produced as a concomitant of emotional response.

A discriminative basis for the increase in the force of response after nonreinforcement has been proposed by Notterman and Block [J. Exptl. Anal. Behavior 4, 289 (1960)]. They suggest that the organism learns to respond “harder” when reinforcement does not occur. This direction of change is learned in an environment in which an increase in vigor frequently produces reinforcement after other behavior has been inefficacious.

Nature provides relatively few situations in which “softer” is an effective direction of behavioral change. It is possible that within the evolutionary process there has been selection for an increase in vigor of response and the phenomenon has become a basic property of the operant. This pattern may frequently be correlated with a more general pattern of emotional response of the sort Halasz has noted. However, the force changes may well be functionally independent of the emotional pattern.

Donald E. Mintz
Department of Psychology,
Princeton University,
Princeton, New Jersey

15 March 1963

FIVE GASES IN FOUR MINUTES WITH FISHER'S NEW GAS PARTITIONER—carbon dioxide, oxygen, nitrogen, methane, carbon monoxide. Speed and sensitivity of new Gas Partitioner mean many extra analyses daily. One lab is making 80 a day. Columns can be changed for analyses of various gaseous mixtures. Two advances improve reproducibility: A constant-current supply replaces batteries, gives currents of 5 ± 0.25 or 7 ± 0.25 milliamps directly from any 105-130 volt 50/60 cycle a-c line; built-in gas-sampling valve lets you take reproducible samples directly from gas streams in pilot plant or mill, reproduce peak heights within ±1%. Full facts in free Bulletin FS-275. Call your Fisher branch or write Fisher Scientific Company, 139 Fisher Building, Pittsburgh 19, Pa.
New Products

A neutron monitor, the 9120 spherical Dosimeter, which provides a close approximation of total human body dosage, is designed for monitoring personnel areas in reactor or accelerator facilities, or around any type of neutron source. It is sensitive to all neutrons within the energy range of thermal to 15 Mev. Dose rate or cumulative dose is obtained in a single, immediate reading. The measurement is omnidirectional. The instrument consists of a LiI(Eu) scintillation detector surrounded by a 12-inch sphere of polyethylene, a transistorized preamplifier, mobile stand, and decade scaler. The detector assembly is also equipped with hardware for wall or ceiling installation. The advantages of the instrument result from its detector-modulator geometry which provides a response approximating that of the human body. Deviation is less than the uncertainty of the neutron flux equivalent of amrem. Readings are independent of the energy distribution of the incident neutrons. Since all neutrons from thermal to 15 Mev are detected, no knowledge of the energy spectrum is required. Multiple readings, graphical interpretations, or instrument geometry considerations are unnecessary. Unlike other neutron monitors, such as films, proportional detectors, tissue equivalent detectors, or fission threshold detectors, the 9120 spherical Dosimeter has no gaps in its energy response. The intermediate energy range from epicumadium to 100 kev is also efficiency detected. The sensitivity of the unit is approximately 3000 counts/mrem. Readings may be made from 0.1 to 1000 mrem/hr. Models with a range up to 10,000 mrem/hr are also available on request. Background count is approximately 2 count/min. The instrument is insensitive to gammas.—R.L.B. (Texas Nuclear Corp., 359 E. Howard Ave., Des Plaines, Ill.)

Circle 1 on Readers' Service card

X-ray-tube driver is a variable pulse width and slope hard tube modulator equipped with a −75-kv output transformer to drive an x-ray tube. The modulator is capable of producing pulse shapes with variable rise times and pulse widths as follows: peak voltage, 0 to 15 kv negative; peak current, 25 amp; pulse width, 2 to 5 μsec; rise time 0.5 to 5 μsec; pulse repetition rate, 180 ±10 percent per second; pulse rises quickly to a predetermined pedestal from 50 to 100 percent of peak, and then slopes up to the full voltage. The modulator can be externally triggered. Output of the pulse transformer is 75 kv peak voltage and 5 amp peak current. The transformer is oil impregnated and its bifilar winding is rated for 4 amp r.m.s.—J.S. (Sperry Gyroscope Co., Great Neck, N.Y.)

Circle 2 on Readers' Service card

Peristaltic pumps are designed to transfer liquids and semi-liquids by means of peristaltic action on plastic or rubber tubing. Tubing is inserted into two tube-holding channels, one on either side of the pumping mechanism. Thus the pumps can be used for multiple as well as single-tube operation, making it possible to multiply the capacity of the pumps or to feed multiple circuits simultaneously. Since the action on one side of the mechanism is 180° out of phase with the other, it is possible to accomplish virtually pulse-free pumping by using two tubes in parallel. All motion is at right angles to the tubing, eliminating the tendency for the tubing to creep. A pair of screw-driven compression plates is used to occlude the tubing and to adjust for various diameters. Maximum tube capacity is two ½ by ⅜ inch plastic tubes, one on each side. The pumps will accommodate as many smaller tubes as circumstances permit. The design of the pumps is such that flow rate is directly proportional to speed over a wide range of pressure. Model 600-1200, designed for low-volume, high-accuracy work, features a synchronous reversible motor and accurate gearbox with 12 gearshift positions. In operation, any speed over a 5000-to-1 range can be selected simply by turning the gearbox knob. Average reproducibility is 0.1 percent. Flow rates are obtained from 0.0042 to 228 ml/min. Output pressure is in excess of 200 mm-Hg. Tests indicate negligible hemolysis. Model 500-1200, designed for high-flow rates, is driven by a heavy-duty, high-speed, variable-speed motor which is controlled by a solid-state speed control producing rates over a 50-to-1 range with a given size tube. Feedback circuit assures excellent reproducibility. Tests indicate negligible hemolysis. Flow rates are obtained from 1.4 to 760 ml/min. Output pressure is in excess of 700 mm-Hg.—R.L.B. (Harvard Apparatus Co., Inc., Dover, Mass.)

Circle 3 on Readers' Service card

Portable salinometer (model 621) uses magnetic induction to sense the ratio of conductivity of a sample of seawater under test to the conductivity of standard seawater. Any temperature difference between the sample and the standard is compensated by a double-bridge circuit using platinum resistance thermometers immersed in the samples. The compensator is said to eliminate the need for controlling the temperature of the sample. Range of conductivity ratio is 0 to 1.5, corresponding to salinities of 0 to 53 percent. The volume of sample required by the measurement is 50 ml. Accuracy of measurement is said to be ±0.003 percent. Temperature compensation range is 0 to 40°C.—J.S. (Hytech Div. of Bissett-Berman Corp., G Street Pier, San Diego, Calif.)

Circle 4 on Readers' Service card

Magnetic tape delay system (model 102-DR) provides a time lag of 0.3 to 10 sec. The system comprises a tape delay transport mechanism and two data converters. Time delay is accomplished