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4 December 1970

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AS

COVER

Aerial view of the ruins of the ancient city of Teotihuacán in the Valley of Mexico near Mexico City, showing the ancient city's principal avenue, the "Street of the Dead," and its four principal structures: the Pyramid of the Moon (foreground); the pyramid of the Sun (at center, left); the Ciudadela enclosure (beyond the Pyramid of the Sun, left); and opposite the Ciudadela, the Great Compound (largely unexcavated). The city extended to the foothills in the distance. See page 1077. [Roger Millon, University of Rochester]

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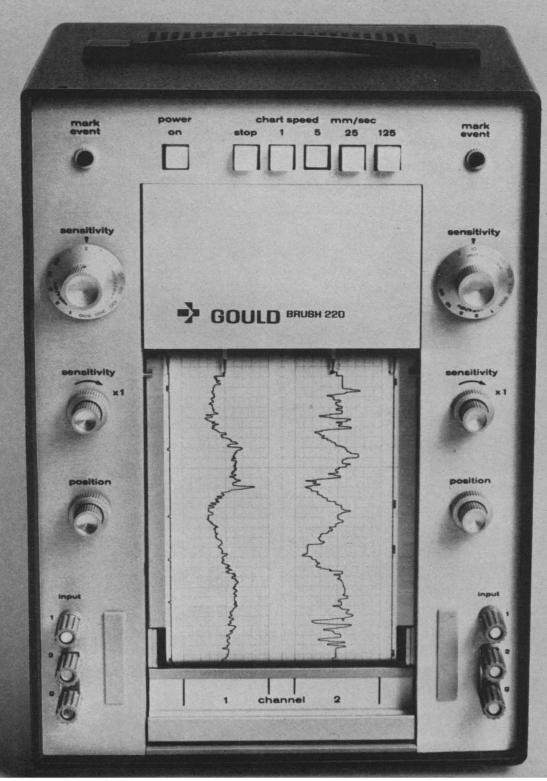
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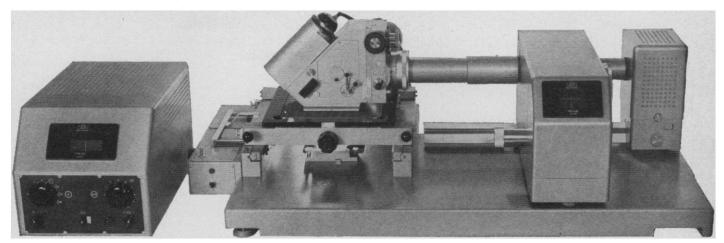
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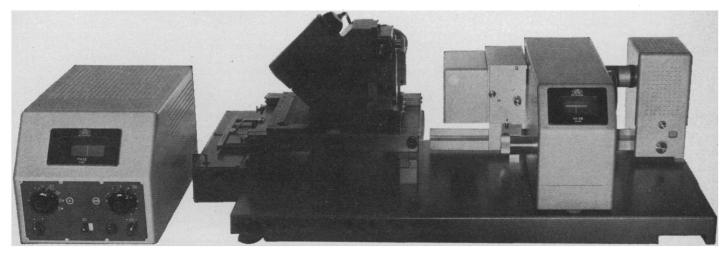
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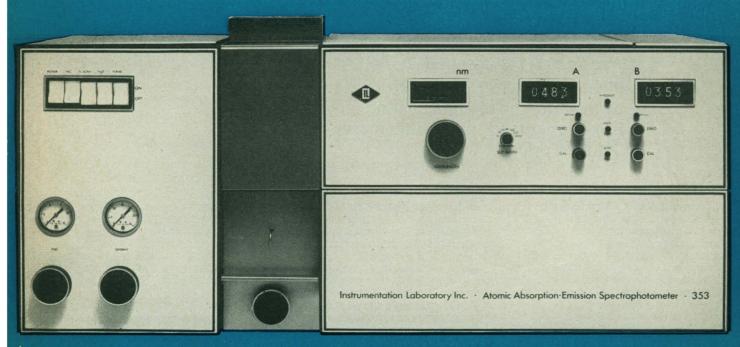
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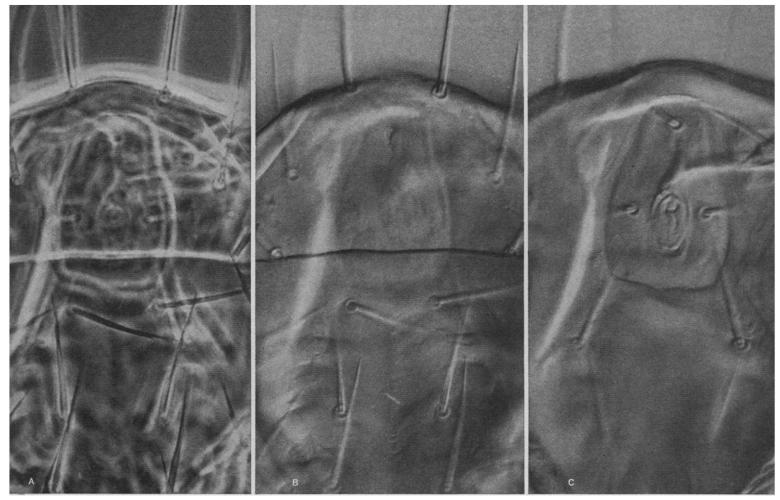
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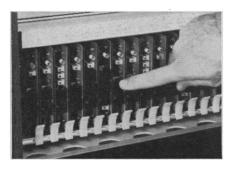
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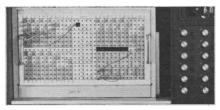
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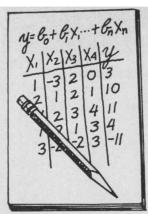
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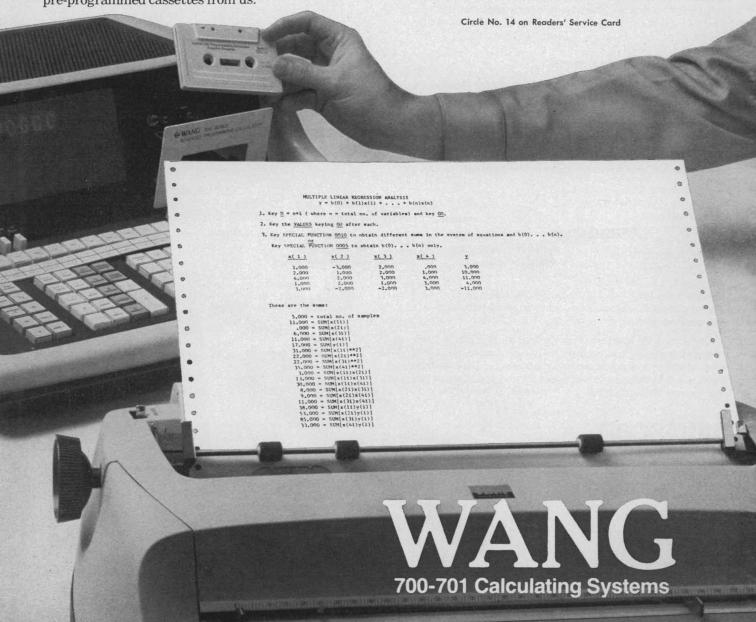
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NASA's space program has had more publicized federal financing, but its "giant leap for mankind" has produced data no one could derive from the earth's thin crust, which is almost certainly related to the moon's composition and age.

I suppose that a geologist can be excused for his ignorance of parts of the universe other than Planet Earth, such as the asteroids and comets that may have formed craters, mascons, and lunar maria, or the solar wind and geocorona that can be observed best from the moon, or the globules in interstellar material that we astronomers think may be forming other planetary systems, or the intergalactic material that may be detected by the Apollo lunar surface camera experiment. If he wants to understand Preston Cloud's editorial (18 Sept., p. 1159), he can get some help from the book Origin of the Solar System (Macmillan, New York, 1966), an elementary treatment edited by the undersigned.

THORNTON PAGE

Department of Astronomy, Wesleyan University, Middletown, Connecticut 06457

Vietnam: AAAS Herbicide Study

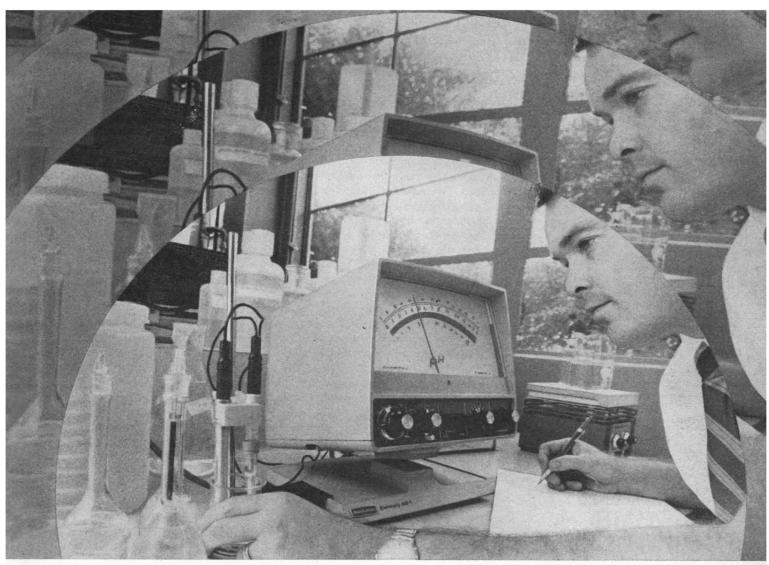
Philip Boffey's account (2 Oct., p. 42) of the difficulties of the AAAS team investigating the effects of the military use of herbicides in Vietnam is fine as far as it goes. . . . I was at the Plant Science Laboratories, Fort Detrick, Maryland, when Arthur Westing, a member of the AAAS investigating team, appeared on 8 April to receive a briefing on the activities of the laboratories. His opening comment, made before 15 to 20 members of the laboratory, was "I am the enemy." Matters got worse as he went on to explain that his mission for the AAAS was to assess damage done by herbicides. When asked whether he thought that all changes caused by herbicides should be classified as damaging, he replied that he hadn't thought about possible beneficial effects. He stated that his committee's function was not to balance benefits, such as the prevention of ambush or the detection of enemy supplies in transit, against the deficits, such as the incursion of bamboo; the committee would not, in short, consider the fact that there was a war being fought nor that herbicides were probably the least lethal weapons in use. Before he had

heard a word from the staff of the Plant Science Laboratories Westing made it clear that the committee was going all out to prevent the use of herbicides in Vietnam. Later on, during the briefing, he was very disappointed to learn that the military was not employing paraquat in Vietnam since that would have made his job (of proving that the United States was poisoning Vietnam) a lot easier.

Sometime after that encounter, I

read a report prepared by A. H. Westing, E. W. Pfeiffer, J. Lavorel, and L. Matasso entitled "Report on Herbicidal Damage by the United States in Southeastern Cambodia" (1). The report, dated 31 December 1969, was based upon 4 days of "intensive field investigation" from 25 to 29 December. Their trip was within the areas bordering Vietnam, in particular the Parrot's Beak and Fishhook regions which were the prime targets of attack by United States and South Vietnamese forces in April-June 1970. They spent their second and last days at Chup and Mimot, respectively, where large caches of military supplies and a vast underground military headquarters were discovered. One must remember that in December 1969 Prince Sihanouk had not yet admitted that North Vietnamese forces occupied the Cambodian border areas despite 5 years of American and South Vietnamese protests against the Cambodian sanctuaries. Then early in 1970 Sihanouk demanded that Hanoi remove the 45,000 to 60,000 troops (the number varied) and supplies they had stationed in his country, and not very long after that Sihanouk was in exile and the Royal Government of Cambodia (now a republic) went to war against the North Vietnamese. It appears that Cambodia, for 5 years at least, was indeed a very vital sanctuary for the Communists, providing them with a supply and headquarters complex from which they could mount offensive actions in the provinces in the southern part of South Vietnam. It is no wonder then that some of the most intensive defoliation missions were on the heavily forested border in an attempt to create a zone where aerial, if not ground, reconnaissance was facilitated.

Against this background the concluding remarks from the report by Westing et al. make interesting reading: "We feel particularly grieved about the innumerable direct and indirect losses suffered by the innocent local populace. The extent of these losses can never be



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ROLE OF CYCLIC AMP IN CELL FUNCTION

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On the Development and Properties of the Brain Adenyl Cyclase System—G. A. Robison, M. J. Schmidt and E. W. Sutherland / Synthesis and Degradation of Cyclic 37,5'-Adenosine Monophosphate in Frog Erythrocytes—O. M. Rosen et al. / Cyclic Nucleotide Phosphodiesterase—W. Y. Cheung / A Role of Cyclic Nucleotides in Brain Metabolism—N. D. Goldberg et al. / The Measurement of Cyclic Nucleotides by Radioimmunoassay—A. L. Steiner, C. W. Parker and D. M. Kipnis / Factors Influencing the Accumulation of Cyclic AMP in Brain Tissue—T. W. Rall and A. Sattin / Effect of Membrane Depolarization and Biogenic Amines on the Formation of Cyclic AMP in Incubated Brain Sliese—H. Shimizu, C. R. Creveling and J. W. Daly / Dynamic Aspects of Neurohormonal Control of Cyclic 3',5'-AMP Synthesis in Brain—G. Krishna et al. / Prostaglandins and Cyclic AMP—R. W. Butcher / The Actions of Hormones on the Adenyl Cyclase System—L. Birnbaumer, S. L. Pohl, H. Michiel, J. Krau and M. Rodbell / Cyclic 3',5'-Adenosine Monophosphate and the Effects of Hormones on Hormone-Sensitive Lipase from Adipose Tissue—M. M. Appleman and C. L. Sevilla / Neural and Hormonal Regulation of Pineal Adenyl Cyclase Activity—B. Weiss and J. W. Crayton / Pineal Gland: Stimulation of Melatonin Production by Norepinephrine Involves Cyclic AMP-Mediated Stimulation of M-Acetyltransferase—D. C. Klein and G. R. Berg / The Receptor Protein for Cyclic AMP in the Control of Glycogenolysis—D. A. Walsh, E. G. Krebs, et al. / On the Mechanism of Action of Cyclic AMP—P. Greengard and J. F. Kuo / Phosphorylation of Histones in Vivo under the Control of Cyclic AMP and Hormones—T. A. Langan / Cyclic AMP—Redicated Adrenergic Synapses to Rat Cerebellar Purkinie Cells: Combined Structural, Physiological, and Pharmacological Analyses—B. J. Hoffer, G. R. Siggins and F. E. Bloom / Behavioral and Vegetative Effects Produced by Dibutyryl Cyclic AMP Injected into Different Areas of the Brain—G. L. Gessa, G. Krishna, J. Forn, A. Tagliamonte and B. B. Brodie / In

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Editors: E. Costa and S. Garattini

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determined satisfactorily and will never be compensated adequately. We have seen at first hand how particularly drastic this type of military action is for people whose very existence is so closely tied to the land. Cambodia is a small nation attempting to remain neutral between East and West and to remain at peace with its neighbors despite enormous external pressures from all quarters. We cannot understand and we cannot condone the violations of Cambodian territory by the United States, for which the present report furnishes but one example. Despite a week of free and unhampered travel by automobile, on foot, and by low-flying aircraft along hundreds of kilometers of the border, we could find no evidence of Viet Cong activity in Cambodia; nor did our repeated conversations with Cambodians and Europeans living along the border suggest any such activity."

Westing and Pfeiffer may have written this report in all sincerity but they could have, and should have, gone to other authorities to find out just what was going on militarily at Chup and Mimot. They wrote a beautiful propaganda piece-regardless of their intentions and the ultimate value of their report in assessing 2,4-D and 2,4,5-T damage to rubber plantations—for the Communists. At least one other report on herbicide damage in Cambodia had already been prepared without such propagandistic overtones. . .

Boffey stated that the AAAS study team "has been denied access to information that is important to the success of the study." We should not expect the military or any other governmental agencies to cooperate with all individuals simply because the AAAS has appointed them to a committee. The team should collectively and individually meet standards required of other government employees who have access to confidential information. An individual's past performance could determine that he was a risk. The "military roadblock" to which Boffey refers may have been Westing's creation, begun months earlier. The AAAS board appointed Matthew Meselson to head the study team and Westing was his first appointee. Nothing in Westing's behavior at Fort Detrick or in his prior writings on the use of herbicides in Vietnam suggests that he meets the minimum standards for objectivity and dispassionate concern for the truth which is the sine qua non for scientific inquiry. The AAAS, to maintain its credibility as an organization for the

advancement of science, should reconsider appointments to this study team.

One last word about the AAAS study on herbicides in Vietnam: the full report of the dozens of experts who attended a special conference at Woods Hole last June should have been published in toto in Science so that all of us could have evaluated the advice they gave.

ROY M. SACHS

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Reference

1. T. Whiteside, Defoliation (Ballantine, New York, 1970), appendix, pp. 117-132.

Sachs is correct in gathering from my informal remarks at Fort Detrick in April that the mandate from the AAAS to its ad hoc herbicide committee limited this group to an assessment of the biological effects of the herbicides used in Vietnam. He is wrong in suggesting that only potentially adverse effects would be looked into, although it was largely because of a concern over the possibility of such effects that the committee was established in the first place. He is absolutely wrong in claiming that I portrayed the committee as being intent on "going all out to prevent the use of herbicides in Vietnam." Rather the committee is and always has been committed to determining how best to study objectively the ecological and public health consequences of the military use of herbicides [Science 167, 37 (1970); AAAS Bull. 15, No. 3, 7 (1970)]. My obviously inept attempts at humor singled out by Sachs were intended to break the ice with a group that was in small part hostile to my visit. The subsequent cordial cooperation, assistance, and advice that I have continued to receive from the senior scientific personnel at Fort Detrick suggest to me that they did not share Sach's interpretation of my remarks. A report of the herbicide committee will be presented at the annual meeting of the AAAS in Chicago on 29 December and is to be published shortly thereafter.

I shall not comment on Sachs's lengthy critique of the report of my Cambodian trip (which predates my association with the AAAS committee). It is in the open literature and can be judged by anyone. A fuller account will appear in Chemical Warfare in Indochina, to be published by the Macmillan Free Press, in early 1971. For further insights into my personal opinions on

the military use of herbicides, Sachs and others are referred to another brief article of mine in the *Friends Journal* 16, 193 (1970). It should be obvious, however, that my personal political and moral views are separate from and irrelevant to the AAAS study.

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More Mislabeling-More Frustration

Letters by Goldman (16 Jan.) and Prescott (12 June) concerned the increasing unreliability of labels on radioactive biochemical products, even those obtained from the more reputable suppliers. Our recent experience indicates that the same complaint extends to the calibrated and certificated radioactive standard sources furnished by such "reputable suppliers." Not only do the standards differ widely, but there seems to be an increasing lack of understanding of the most fundamental definition of terms which at one time, at least, had been unequivocally defined.

We recently undertook some experiments requiring the use of 14C. In order to properly interpret some preliminary results involving planchet counting, we borrowed from our colleagues two 14C reference disc sources obtained from different suppliers, each labeled as representing a specified number of microcuries. Since the resulting calibrations differed by more than 50 percent, we inquired of the suppliers as to the method of preparation. The first supplier provided full details on construction, indicating appropriate correction factors for absorption by the thin cover, and so forth. Repeated phone calls to the second supplier failed to uncover any details concerning our specific source, and provided conflicting stories concerning their 14C sources in general.

Our attempt to purchase another reliable calibration standard proved disastrous. Unlike the "weightless" samples used by the first supplier, this third supplier prepares his ¹⁴C sources by collecting ¹⁴C-labeled BaCO₃ on filter paper. He then calibrates the source and expresses the microcurie content "based upon emission rate and not contained activity." The first two standards shipped by this third supplier differed by a factor of 100, neither was near the catalog microcurie value, and both were more than a factor of 4 below their respectively certified microcurie

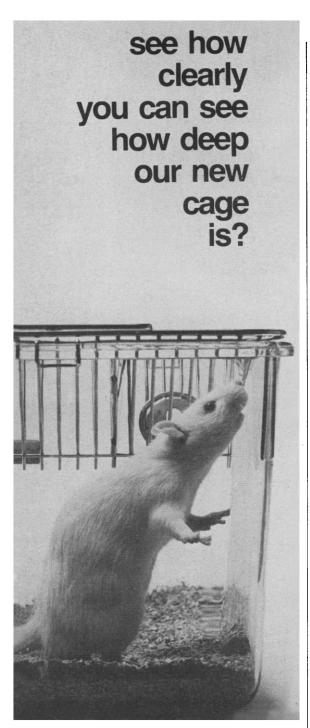
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value by their own rather arbitrary definition. The third attempt to fill this order produced a source which seemed to be high by just a factor of 2. A telephone call to the supplier produced the information that the source had been "calibrated for 2π geometry" and that the listed microcurie value on the attached certificate should be corrected to so state. I must confess that had I seen such a statement on the certificate I would not have known whether to multiply or divide by 2 for 4π geometry.

I cannot help wondering what new theories have been reported in the literature based upon research results about which the authors were sufficiently naive to accept such "standards" at face value. I shudder to think of the possible consequences if a similar erroneous standard is sent to the radiology department of a hospital and is used to determine dosage of some radioisotope delivered to a patient.

A. Broido

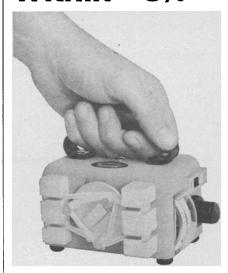
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Some can certainly be classified as highly dangerous, others as of medium effectiveness, while the rest can be grouped according to their lower activity. Maybe a simple color code on the container can provide sufficient warning. The label should provide information on volatility, solubility, and half-life under the conditions of treatments commonly used. Instructions for methods of safe disposal should also be included. None of these compounds should be shipped in single-walled glass containers, and efforts should be made to employ vessels which do not overturn easily when opened.

It takes months to reveal the consequences of human contamination with carcinogens, and several generations are needed for genetic damages to surface after exposure to mutagens in organisms with a breeding system similar to that of man. Since the majority of the research laboratories are associated with schools, the inexperienced, young, and most susceptible persons have the highest chance for dangerous exposure. Some of the hazards could be effectively minimized by the adoption of the simple and inexpensive measures suggested here.

No new legislative or governmental actions are needed; only the regulations of the federal Food, Drug and Cosmetic Act should be extended to a number of old and new chemicals and perhaps expanded. The user institutions should adapt and adopt rules for handling carcinogens and mutagens similar to those in existence for radioactive compounds.

G. P. REDEI

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Errors in Telegraphed Texts

With respect to my summary of the work of Katz and his colleagues, in the issue of 23 October which contained the article on the Nobel prizewinners (p. 423), readers will recognize easily that José del Castillo's name came out misspelled and that Merter Lectures are really the Herter Lectures, but many will not know that a quantum of acetylcholine probably contains something like 10⁴ molecules rather than 104.

A. R. MARTIN

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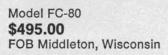
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Educational Obsolescence

The obsolescence of education in rapidly developing fields of knowledge has become about equal in rate to the obsolescence of an automobile. In 5 to 7 years it is due for a complete replacement. Consequently, our times, to a degree generally quite unrecognized, demand a major reconstitution of the educational process, which must become one of lifelong renewal. Perhaps a month out of every year, or 3 months every third year, might be an acceptable new pattern. Indeed, instead of cramming the educational years of life into adolescence and early maturity, a more efficient plan might be to interrupt education with work periods after elementary school, high school, and college. In any case, programs of continuing education for all professional people must become mandatory, and the educational effort and expenditures must be expanded by at least a third to permit adequate retraining and reeducation.

The obsolescence of education differs only in degree in the several sciences, and even in the humanities. . . . I recently heard a distinguished historian, who had been in university administration for only 5 years, say that his field had advanced so much in the interim that it would take him a year of concentrated study just to catch up with its development. In spite of a very general recognition of these hard facts, little is done to alter our pattern of education to cope with them. Physicians continue to practice medicine, although 50 years have elapsed since their youthful preparation. Dentists do the same. Lawyers and engineers live comfortably on their antiquated stock in trade. Teachers slide steadily downhill through failure to grasp new developments in their own subjects. Perhaps worse is the virtually universal ignorance, on the part of educated men and women, of any advancement of knowledge outside their own professional specialties. Surely we need a complete and thoroughgoing change in attitude toward "adult education," a careful planning of programs and courses appropriately designed for the intelligent adult who has become out of touch with his new world, and a mandatory, cyclic renewal of training for the professional specialist.

The cry of the student throughout the land is for "relevance" in the curriculum. What does he mean? Change has become the major feature of human civilization, driven by an increasingly rapid development of enormous powers to modify and control raw nature; and the advance of science is the principal factor in this technological revolution. Education must prepare each person to cope with changes that are unpredictable. As Heraclitus wisely said, "No man steps into the same river twice." The river flows, and the man ages. All is change. Yet the life of an ancient Greek was not so different from the life of Thomas Jefferson as Jefferson's life is from ours today. The content of the curriculum should therefore embrace both the timely and the timeless, for topical and social relevance and timeless natural law alike deepen our perspective and assist us to adapt ourselves to altered circumstances. Unfortunately, the timely and the timeless are both often displaced by the trivial, or the significance of the two former is left obscure, so that to the young mind they seem to be trivial. This must not be!—BENTLEY GLASS, Vice President for Academic Affairs, State University of New York, Stony Brook

Adapted from The Timely and the Timeless (Basic Books, New York, 1970).



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SCIENCE, VOL. 170



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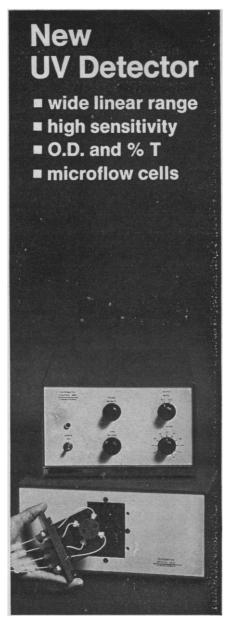
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Two papers by Bouix followed in this session. The first one dealt with the formal theory of integral equations in determining current distributions on finite length antennas in homogeneous media. The second paper was concerned with the influence of a reflecting surface on the field of a dipole in an otherwise homogeneous media. The paper seemed rather classical, but it may provide analytical insight.

The final paper in this session, by G. Franceshetti, dealt with "The Influence of Excitation Gap on Antenna Performance." The main point of the paper was to establish the validity of quasi-state formulations in low-frequency antenna radiation in conducting media. On the basis of the oral presentation, it appeared that the actual analysis was only carried out for spherically shaped antennas with azimuthal gaps. As expected on intuitive grounds, the static results are valid provided the antenna dimensions are reasonably small compared with the skin depth in the conductor. In principle, the results can be generalized to other geometries, but much additional work needs to be done. The experimental work described was carried out in a scaled model tank. In the discussions, Wait emphasized the importance of accounting for induced polarization effects of metal-electrolyte interfaces. This can be particularly troublesome at audio frequencies.

The session on transmission was supposed to have been initiated by a paper entitled "Transmission of Radio-correlated Low Audio and Sub-audio Frequency Signals through Sea Water." The paper, with five authors, was withdrawn for unknown reasons by the sponsor (U.S. Army, Fort Monmouth). The abstract indicated that transmission experiments between shore and boat were carried out at a distance up to 1.6 nautical miles. Apparently, correlation of radio and conduction current signals was to be undertaken with a view to improved range coverage.

"Surface Wave Propagation from VLF Arrays in Polar Seas" was the title of a paper given by A. W. Biggs. This paper dealt mainly with mixed-path effects for ground-wave propagation over high-latitude terrain which is highly inhomogeneous. The semi-empirical formulation of Millington was used to estimate the gross nature of the expected field variation of coastlines between sea and arctic land. Surface wave effects in polar ice, and all the complicating factors such as brine content, pore volume, and surface rough-



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ness, were also considered. The design of antenna arrays using up to five elements was described. The paper indicated that many unusual and perplexing effects could be expected.

The final paper in the session, presented by R. Lindquist, was entitled "Some Results in Connection with a Search for Maximum Underwater LF/ VLF Coverage." This paper described a simple method to determine fields in seawater (in the seas around Sweden) from a fixed VLF station on land. The distribution of salinity and the coastline geometry was considered in the analysis. The final results displayed the frequency and range dependence for a wide variety of cases. No new analytical approaches were needed, but the author demonstrated a deep understanding of the problem and a capability to adapt known methods to the problem at hand.

The session on VLF/ELF electronic noise contained a group of four papers on naturally occurring signals in the ELF range. One, by H. L. Konig, dealt with the mechanical and electrical design of magnetic and electric dipole receiving antennas. The results should be of considerable value to experimenters and many practical hints in the construction and operation were generously supplied. The second paper, by G. Mattern, dealt with a rather qualitative discussion of VLF radio noise during a sea voyage between Germany and Ecuador. The noise was recorded with a 500-hz to 5-khz bandpass receiver using, apparently, a vertical whip antenna. It was concluded that, for below 40°N latitude, the principal contribution to the noise was lightning discharges in African and South American storms. The third paper, by E. Selzer, dealt in a very general fashion with the problems of doing EM noise studies at great depths in the ocean and in freshwater lakes. Many intriguing aspects, such as hydromagnetic propagation and ocean bottom noise, were briefly mentioned.

The fourth paper on noise, by E. F. Soderberg and M. Finkle, was a successful attempt to correlate measured ELF noise fields at the surface and beneath the sea. The experimental results were consistent with a straightforward theoretical calculation using a plane-wave and homogeneous half-space model.

The session on HF/VHF backscatter was probably the highlight of the whole conference. The contributors represented the leading workers in the field, and the audience was in a position to appreciate the impact of the results. Again, however, the lack of participa-



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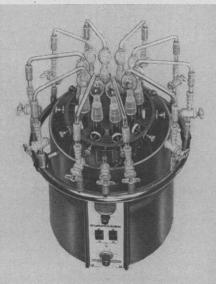
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tion from physical oceanographers was noticed.

It was very fitting that the first paper was given by D. D. Crombie who, in 1955, had first identified Bragg-type resonant backscatter as the principal mechanism in determining the reflected ground-wave HF signal from a rough sea. Crombie's paper (coauthored by J. M. Watts and W. H. Beery), dealt with the spectral characteristics of the Doppler-shifted backscattered signal. He attributed differences from the "simple theory" to four factors: (i) nonlinear interaction between the waves causing the coherent backscatter and the rest of the sea spectrum; (ii) changes in phase velocities depending on wave amplitude (that is, Stokes effect); (iii) drifts duced by the wind; and (iv) tidal drifts.

An important contribution to this session was a theoretical analysis by D. E. Barrick who used a boundary perturbation treatment for interaction of an electromagnetic ground wave and the rough sea surface. The results provided a quantitative estimate of the effective ground-wave attenuation for an HF ground wave on a rough sea. Also included in his analysis was the special case of backscatter, and here his results were consistent with earlier calculations of others.

A third paper in this backscatter session was given by W. H. Shonfeld, who considered a semi-empirical analysis of microwave backscatter (that is, clutter) from the sea. Parameters such as range, wind, wave height, and antenna height were considered.

A rather controversial paper was given by R. K. Moore on the use of radar to determine oceanic winds (up to 50 knots). He suggested that the scattering coefficient at centimetric wavelengths is a well-determined function of wind speed and, with the use of satellites, this should open up a new field in oceanography. During the discussion, however, it was pointed out mainly by J. W. Wright (Naval Research Laboratories) that, for high wind speeds, the situation is very complicated. Thus wind speeds may not be deduced easily from such microwave data.

In his paper "Doppler Spectra in Microwave Scattering from Wind Waves," J. W. Wright used a laboratory wind-wave tank and obtained Doppler spectra on vertical polarization at X-band (3.2 cm) and K-band (1.25 cm) for depression angles between 15° and 80° and wind speeds between 2.25 and 8 m/sec. By Fourier-analyzing optical

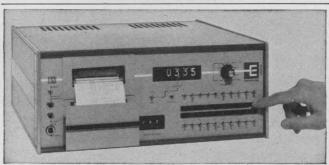
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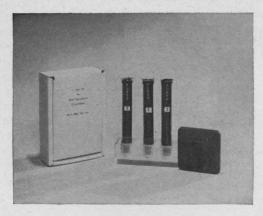


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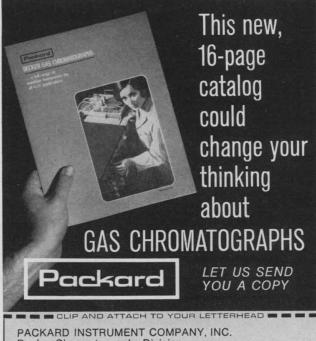
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data, he was able to obtain two-dimensional slope spectra of the wind waves. He concludes that microwave scattering from low-speed wind waves (in a short fetch tank) can be satisfactorily accounted for by low-order Bragg scattering. Wright conjectured that the Doppler bandwith is a kind of inverse lifetime for the Bragg resonant wind wave. This suggestion, attributed to Crombie, means that the currently accepted views by oceanographers on wind waves are incomplete at best. There was no rebuttal from any physical oceanographer in the audience!

An unscheduled but useful presentation was given by L. Wetzel on the signal characteristics received beneath a rough ocean. He compared several different models proposed on earlier occasions.

A round-table discussion on the Radio papers was held. An edited account of this is being prepared by A. W. Biggs. It served as a useful mechanism to extend the discussions which followed each of the papers above. Unfortunately, the format of the round-table session was not conducive to informal discussion. It may be better in future meetings of this kind to dispense with "round-table" discussions and instead allow more time for discussion after each paper.

This meeting, organized by the Electromagnetic-Wave Propagation Panel of the Advisory Group for Aerospace Research and Development (AGARD), was held at the Centre National de la Recherche Scientifique in Paris. The program chairman was P. Halley from Saclay, and the cochairman was T. Hodara (Tetra Tech, Inc., Pasadena, California). The EM Wave Propagation Panel is under the chairmanship of K. Davies (Environmental Science Services Administration, Boulder, Colorado), and the executive officer is C. R. Smith (U.S. Navy, AGARD staff, Paris).

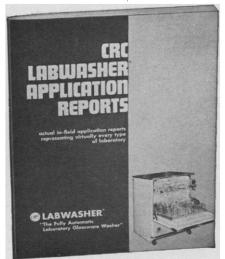
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motor function in the oral-facial regions was the subject of such a seminar held on 19 to 22 January 1970, in Honolulu, attended by 22 investigators, including nine representatives from Japan.

The major topics under consideration included thermoreceptor and mechanoreceptor activity, and central mechanisms of oral-facial sensation and movement. Basic neurophysiological mechanisms and correlated ultrastructure underlying sensory and motor function were emphasized.

There was considerable interest in thermoreceptor activity in the face area. A common finding was that specific thermoreceptive myelinated afferents were usually "cold" fibers; they increased their activity with cooling and decreased their activity with warming. "Warm" fibers were uncommon. However, a high density of warm spots confined to an area of skin across the bridge of the cat's nose was reported; electrical activity was recorded from myelinated fibers whose activity was approximately the reverse of that seen in "cold" fibers.

Units responding to both thermal and mechanical stimulation were found in the trigeminal ganglion, medulla, and thalamus of monkeys. These units responded to rapid cooling with increased dynamic discharge rates and had response profiles that increased linearly over the entire range of cooling temperatures. It was suggested that both the thermal and mechanical units and specific thermal units function in the central coding of temperature change.

In another study, the subjective responses to temperature changes in man were compared with the peripheral neural activity evoked in the rhesus monkey by a comparable series of thermal stimuli, on the assumption that the sensory capacities in the two systems were similar. It was found that the responses of A delta thermoreceptive fibers alone can transmit sufficient information centrally to account for man's subjective estimation of temperature change in the cooling direction over the range of 10°C. In light of the paucity of "warm" units found by various investigators, it was suggested that warming might be coded by the magnitude of the decrease or cessation of activity of "cold" units. However, in man, such changes in the activity of "cold" units did not transmit sufficient information centrally to account for the subjective estimation of warmth.

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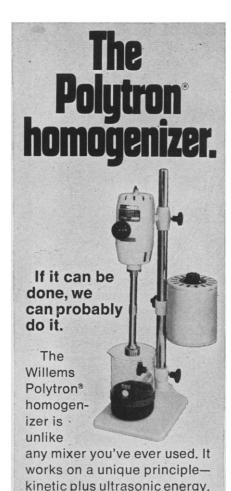
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tense discussion dealt with jaw-opening and jaw-closing reflexes. A number of participants reported that stimulation of cutaneous nerve fibers from the mouth (inferior alveolar, lingual, maxillary) resulted in inhibition of the jawclosing reflex. It also was found that stimulation of these sensory nerves excited the digastric, a jaw-opening muscle. However, stimulation of sensory nerves evoking jaw closure had no effect on jaw openers. Thus, the reciprocal interaction of antagonistic sets of muscles controlling limb movements is incompletely present between muscle antagonists involved in jaw movement.

Intracellular recordings from masseter motoneurons revealed that there were two phases of inhibition evoked by sensory nerve stimulation, an early phase lasting about 15 msec, followed by a late phase enduring for approximately 100 msec. The early phase clearly had the characteristics of an inhibitory postsynaptic potential (IPSP). The late phase of inhibition had some of the characteristics of an IPSP, but presynaptic inhibitory mechanisms were not ruled out. It was shown that the early IPSP was evoked via a disynaptic pathway involving an inhibitory interneuron located in the supratrigeminal nucleus.

In another report, single unit activity was recorded from the face motor cortex in awake monkeys during voluntary jaw movement. Half of the units were correlated with jaw opening, and the other half with jaw closing. The level of maintained unit activity was not correlated with the force of muscle contraction, in contrast with previous reports on the activity of motor cortex neurons active during limb movements.

Much controversy centered about studies on presynaptic modification of afferent input in trigeminal brainstem and thalamic nuclei, and the morphological basis of such findings. In one study, stimulation of high-threshold proprioceptive afferents from the masseter muscle produced presynaptic depolarization of lingual afferent fiber terminals from tongue mechanoreceptors, and also suppressed the lingulodigastric reflex. Other investigators studied the excitability changes in single primary afferent and corticofugal fibers projecting to trigeminal brainstem nuclei, and in thalamic afferents projecting to ventrobasal thalamus. Presynaptic depolarization was the predominant effect of conditioning stimulation on the excitability of primary afferent and corticofugal terminals.

However, in thalamic afferent endings, conditioning stimuli usually produced an early presynaptic depolarization followed by presynaptic hyperpolarization at longer intervals between conditioning and testing. It was reported that, under barbiturate anesthesia, excitability increases and decreases rhythmically; the periodicity was similar to that of intrinsic thalamic spindle rhythms, suggesting that presynaptic hyperpolarization may be responsible, in part, for the facilitatory phases of thalamic rhythms.

Some earlier data suggests that axoaxonic synapses represent the morphological substrate for the presynaptic depolarization found in many parts of the somatic afferent system. Electron microscopic evidence presented at the conference revealed that such synapses occurred between axons in the spinal cord, trigeminal brainstem nuclei, and ventrobasal thalamus. However, corticofugal axons which projected to trigeminal brainstem nuclei and exhibited presynaptic depolarization of their terminals (see above) were not involved in axo-axonic synapses. In addition, medial lemniscal terminals in ventrobasal thalamus were always presynaptic to other axons, in disagreement with the postulated functional role of axo-axonic synapses in presynaptic depolarization. Thus, the relationship between axoaxonic synapses and presynaptic depolarization remains inconclusive.

The most fruitful discussions between Japanese and U.S. participants were on the central organization of the trigeminal brainstem nuclei and their role in sensory transmission and motor outflow. U.S. scientists were very interested in the techniques used by Japanese scientists to unravel the excitatory and inhibitory mechanisms controlling jaw movements. For their part, the Japanese scientists were particularly interested in the ultrastructural organization of trigeminal sensory and motor nuclei. The exchange of ideas should lead to new and exciting advances in our understanding of the trigeminal system.

R. DUBNER

Neural Mechanisms Section, National Institute of Dental Research, Bethesda, Maryland

Note

1. In addition to sponsorship by the U.S.-Japan Cooperative Science Program, the seminar was supported by the National Institute of Dental Research, National Institutes of Health. The proceedings will be edited by R. Dubner and Y. Kawamura, cochairmen of the conference, and will be published by Appleton-Century-Crofts, New York. A list of the participants and a summary of each presentation can be obtained by writing to Dr. Dubner.

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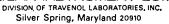
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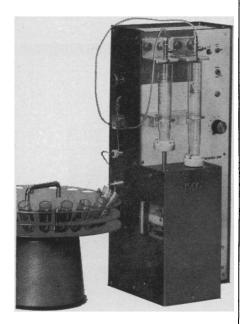
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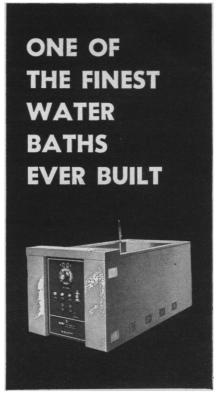
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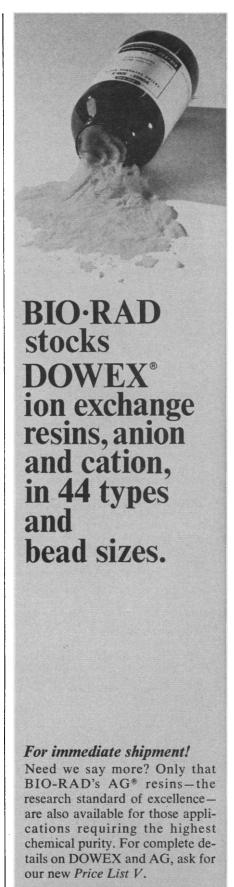
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