Any scientist desiring to work on biological aging will be warmly received by the NIH staff. But warm reception and shared philosophies do not necessarily imply flow of dollars; good research proposals are also a requirement.

Strehler’s hope that “in 1977 a book review such as this would be an undiluted discussion of solid achievements” must rest largely on the attraction of bright, imaginative scientists into the field of aging. This will be accomplished by the demonstration of research competence in the laboratories where aging research is now in progress.

Bernard M. Wagner
College of Physicians and Surgeons, Columbia University, and Rockefeller University, New York 10021

. . . How can the present feeble research effort be improved? I would suggest . . . creation of a Research Commission on Aging which would be directly responsible to the President and Congress and the transfer to this commission of authority for all basic biological research on aging—in effect, formation of an AEC of biological aging.

There are very good reasons for taking so strong a stand. For one, Verzar’s experimental work relating the amount of cross-linkage (chemical bonding between molecules not normally joined) in collagen to the age of the collagen is of the same order of importance to aging research as the discovery of nuclear fission was to nuclear research. (The developmental stages are quite comparable.) Another reason is that the impact upon the world will be even greater from the significant extension of the human lifespan than it has been from the application of nuclear power. . . .

Perhaps the greatest need is for the testing of the various hypotheses on the origin of aging. For example, at present there is a considerable question as to which is the more important in producing biological aging—the accumulation of nongenetic cross-linked material, or the accumulation of mutations. Further, there is the question of just how important cross-linkage is in producing mutations. Obviously these questions are not going to be answered by theoreticians alone; solid experimental data are essential. . . .

Why haven’t steps been taken before now? There are a number of barriers, including psychological ones. The lingering influence of ancient moral and religious ideas predisposes people toward accepting the inevitability of aging. The story of Adam and Eve explains that mankind lost eternal youth through original sin, and stories of arrangements with the devil (Dr. Faustus) point the moral that the search for eternal youth is evil. . . . A military barrier may develop because of the possibility that men would be less willing to risk their lives in battle if the lifespan were longer and old age more attractive. . . . A political barrier might develop if certain heavily populated countries considered any effort at lowering the death rate to be a biological weapon intended for use against them. . . .

Yet considerable prestige would accrue to the country that first bestows extended youth upon the rest of the world, and an increased lifespan would permit an increased rate of scientific and economic progress. . . .

Donald G. Carpenter
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Unfortunately, Wagner’s letter does not deal substantively with the historical issues discussed in my review. Of course, he has every right to interpret the past in the light of his own beliefs, but one might have hoped for something more than blanket denials of facts presented by the organization in question—facts that are generally substantiated by my own observations during the past 11 years as section chief in the Gerontology Branch, NICHD, NIH.

Carpenter’s observations are interesting and constructive; and although I, too, believe that a more intensive and systematic approach than that presently in effect is needed, if we are to understand this problem in the next quinquennium, I neither believe that anything as massive as a Manhattan Project is needed or desirable, nor that we should emphasize too heavily problematical political or military consequences of a breakthrough in this field. Rather, what is needed is primarily imaginative, dedicated, and constructive leadership within appropriate governmental or private organizations, and a corresponding resource commitment—a commitment smaller than that required for one Moon Shot.

Bernard L. Strehler
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Student Skeptics Study UFO's

The recent demonstrations in Washington and Wolfe's editorial, "Student unrest" (27 Oct., p. 443), as well as Tom Lehrer's records, show that young people "gotta have a cause," opposing U.S. government policy, or university faculty, or "the Establishment." It seems to me that this tendency to opposition can be effectively exploited in teaching science and other subjects. As an example, I am now teaching a course on "Flying Saucers," capitalizing on student interest in UFO reports that discredit the scientific establishment. The 50 students who signed up (for a class "limited to 20") will get the fundamentals of astronomy and physics that apply to UFO sightings. By the end of one semester, they should at least understand what is involved between the flying-saucer hypothesis and the laws of mechanics, radiation, and physics of the upper atmosphere (see Markowitz, "The physics and metaphysics of unidentified flying objects," 15 Sept., p. 1274). The only trouble is that I don't know enough social psychology to capitalize fully on this broad topic.

In the realm of physical science, it would help if E. U. Condon and his panel in Boulder, Colorado, would publish some statutes on the UFO reports, estimate the labor necessary to analyze one, and give interpretations of a few typical cases. Students will read such materials avidly (looking for loopholes) and will certainly learn some astronomy and physics in the process.

THORNTON PAGE
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Are Dental X-Rays Dangerous?

During Senate hearings on S. 2067, the bill which would set standards governing radiation hazards (15 Sept., p. 1292), Albert Richards, speaking for the American Dental Association, pointed out that dentists live an average of 1.4 years longer than the rest of the white male population and that dentists who die of diseases of the blood and blood-forming organs, including leukemia, live to an average age of 71.2 years while the parallel figure for the general population for death from these diseases is 68. His arguments are invalid for the following reasons:

1) The blood-forming organs, the bone marrow and the spleen, are deep within the body. A dentist is exposed only to scattered radiation which has a kilovolt-peak far lower than that of the primary beam. Therefore, only a very small percentage of this radiation would ever reach the blood-forming organs as most of it would be shielded out by the bones and overlying tissues.

2) There are less than 15,000 cases of leukemia in the United States per year. Therefore, the data may be statistically invalid, as there are less than 100,000 dentists practicing in a population of 200 million. If dentists were to get leukemia at the same rate as the general population, they would have roughly 7 to 7½ cases per year, and certainly in a group of 100,000, it is hard to see this as a statistically significant figure.

3) The universal use of x-ray in dentistry is an event of the last 20 years so that many of the older dentists have not had x-ray machines during their whole practice experience. Also, many have delegated this technique to assistants and hygienists.

Richards' figures comparing death rates from blood diseases of dentists with that of the general population are not realistic because virtually no dentists are under the age of 20, while many leukemia deaths occur among children. The incidence of skin cancer which could result from low kilovolt radiation would provide a more significant comparison. If the number of dentists having skin cancer were compared with another professional group not exposed to such radiation reliable data could be obtained. Most every dentist knows of a colleague who has contracted skin cancer from his x-ray machine.

FRED M. MEDWEDEFF
21st and Hayes Medical Building, Nashville, Tennessee 37203

Age-at-death figures referred to by Medwedeff are from the study entitled Mortality of Dentists, 1955-1960, by the American Dental Association's Bureau of Economic Research and Statistics. The study, based on death certificates of dentists received from a majority of the state health departments, covered a period of 6 years. In accord with the International Classification of Diseases, Injuries, and Causes of Death (World Health Organization, Geneva, ed. 7, 1957), leukemia and aleukemia were included under "neoplasms," not under "diseases of the

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blood and blood-forming organs." The mean age at death of dentists dying from neoplasms was 67.5, compared to 66.3 for nondentists. These figures are for white males, and are age-adjusted to take into account age differences in the populations at risk (the number of living dentists 25 and older, and white male population 25 and older). No death occurring at age less than 25 was included for either dentists or nondentists, so Medwedeff's assertion that "virtually no dentists are under 20" is irrelevant and misleading.

Death from cancer was studied also in an earlier investigation conducted by the Bureau of Economic Research and Statistics, *Mortality of Dentists, 1951–1954*. The mean age at death caused by neoplasms was 67.7 for white male dentists 25 and older, and 65.2 for white male nondentists 25 and older. In this study, too, the difference in age distribution of the populations at risk was taken into account.

Therefore, over a period of 10 years, dentists dying from neoplasms were older than the comparable general population group dying from neoplasms.

In a national survey of dentists conducted by the Bureau in 1950, 92.4 percent reported having x-ray equipment in their offices. This would indicate that x-ray machines have been in wide use in dentistry for considerably longer than 20 years as indicated by Medwedeff. Certainly earlier equipment and procedures caused greater dentist exposure than current usage.

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Symbols and Symbolic Codes

The recent issue of *Science* (13 Oct.) was distressing for one interested in the development of symbols and symbolic codes for diagrams.

The new symbol for "biohazards" (1) was chosen on the basis of two criteria, one of which was "uniqueness," interpreted as lack of prior meaningful associations. A good deal of research has indicated that symbols capitalizing on appropriate prior associations and meaningful stimulus structure are often superior to arbitrary "signs" (2). When new symbols utilize arbitrary stimuli, it is not uncommon for lapses of meaning to occur, even with trained persons. Although the symbol chosen for biohazards may become meaningful for persons engaged in constant work on such projects, it would not seem to provide any strong avoidance associations for nonlaboratory personnel who might come across such materials accidentally. I cannot help wondering why previously learned avoidance meanings were not considered (apparently). Offhand, one might think a skull and crossed test tubes would convey the desired meaning to both laboratory and lay persons better than the symbol chosen.

Second, Walsh's article was marred by a map (p. 243) which violated rather well-established principles of "S-R compatibility" (3) and standard coding techniques. Looking at the map, one would think that Massachusetts, Maryland, New York, Pennsylvania, and Texas received the lion's share. The confusion obligations. Only in reading the fine print does one discover that California received the lions' share. The confusion is compounded by the fact that up until the final division, increased shading is more or less correlated with increased funding.

Whether one is dealing with a life-and-death matter (biohazards) or simply with graphic communication, it is unfortunate that both research findings and common sense are overlooked in the development of so many symbolic displays.

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References


Reactions from Reed

Nelson's provocative and interesting article on Reed College (15 Sept., p. 1282) provided me with somewhat mixed emotions; one cannot help but cringe upon seeing one's beloved so exposed to public examination. In general, his facts seem to be both accurate and clearly presented, though somewhat more pessimistically interpreted than we feel is representative of our view. However, three points I believe should be clarified:

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I wish to disclaim the statement “a large part of the college sneers at big-time research.” If these words were used by me (which I doubt) they were certainly taken out of the context of their purported use. However, I do find shocking, particularly in an intellectual environment, any antipathy toward scholarly endeavors.

The unqualified “great preoccupation [of the biologists] with research” may have carried the implication that teaching is a secondary or ancillary concern of ours; this would be an unfortunate and mistaken construction, I made clear in our interview that the
biology department considers research a most important adjunct to teaching. In rapidly moving and shifting fields, such as biology, research activities provide insurance that the teacher will maintain an awareness of current developments in his field and bring this awareness to his classroom. Thus, our "preoccupation" with research has an extension beyond its intrinsic justification.

In his seeming preoccupation with the flamboyant, Nelson neglected the more substantive aspects of our interview. Omitted was an account of the integration of our students into the scholarly activities of the biology department, through direct participation in faculty research, independent projects, and thesis work. This learning by doing with its one-to-one student-faculty relationship is a basic aspect of our educational philosophy. and I suppose it takes some "preoccupation" with research to provide a real environment in which this philosophy can materialize. Also omitted was an account of the recent inception of a postdoctoral program, which has as a primary aim the development of teacher-scholars. This program provides new Ph.D.'s with an opportunity to examine and develop their proclivities toward teaching and to make progress as active scientists.

A "preoccupation" with research makes a postdoctoral effort of this sort feasible. Thus, we are strongly committed to research, we are deeply involved in teaching, and we believe the two efforts are crucially complementary.

GABRIEL LESTER
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As one of the "smug" and "arrogant" traditionalists referred to in Nelson's article on Reed College (15 Sept., p. 1282) I am surprised to find that a liberal arts college needs a Mission. I have been naive and innocent enough to believe that it is the function of a liberal arts college to bring together a faculty and student body with a mutual concern for learning. The faculty ought to be the most able which can be lured with nothing more than promises of hard work and low salary, and the students should regard learning as their major reason for being in a college. Nelson's article tells me that, in addition to library and laboratory facilities appropriate to this joint enterprise of faculty and students, we now need a Mission.

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selves committed to, and actively engaged in, learning (as scholars, writers or researchers) and if they are also committed to transmitting what they are continually learning to these students who actually desire to learn, I can see no reason for imposing on this relationship a Mission.

It is apparent that Nelson was infected by the myth makers. For example, those characteristics of a college community which lead the uninformed to conclude that an institution is "a bastion of Bohemian appearance and left-wing thought" have long since become such common characteristics of every major college or university in the country as to be hardly worth a comment. A number of points were missed. For example, the $400,000 to $500,000 yearly fund-raising effort is just a bit more than the direct financial aid given by the college to its students. In proportion to the size of the endowment and the yearly operating budget, I would guess that this is one of the highest student financial aid budgets in the country.

If the dropout problem is to be considered a serious one, the comparison with Swarthmore omitted vital information. Reed gambles on many more doubtful admissions than Swarthmore, as a comparison of applicants to admissions for the two schools indicates. Not all of these gambles disclose the jewel in the rough stone: however, enough do to make a good argument for the vice of gambling.

About my colleague's lugubrious predictions of an early demise for liberal arts colleges (Reed in particular) unless something is done, I can only say that they also represent an ancient and conservative point of view shared with such famous figures as Professor Burgess of Columbia (1884): President Harper of Chicago (1900), "Three out of four colleges must be reduced to academies or modified into junior colleges"; President Butler of Columbia, "If the American college is to be saved it must reduce its course of study to two or three years"; David Starr Jordan of Stanford (1903), "As time goes on the college will disappear, in fact, if not in name. The best will become universities, the others will return to their place as academies" [F. Rudolph, The American College and University—A History, (Vintage Books, New York, 1965), p. 443].

MARSHALL W. CRONYN
Department of Chemistry,
Reed College, Portland, Oregon 97202

Cancer magister Dana is the most important commercial species of crab found on our Pacific coast. Over 35 million pounds with a value of $5.5 million are processed annually. Until recently, research on larval stages has been limited. Marine biologist Richard L. Poole of the California Department of Fish and Game, Marine Resources Operations, Menlo Park, has found and described 5 distinct zoeal stages and one megalopa. All larvae were dissected under a StereoZoom Microscope. Due to their small size (total length of first zoea is 2.5 mm), the additional magnification range provided by the 2x attachment lens of the StereoZoom proved very helpful.

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Students of theoretical physics will find particularly useful the derivation of the generalized hydrodynamic equations, from conservation laws and the Galilean transformation, and the discussion of Bogolyubov's theory of a weakly interacting Bose gas, in which second quantization is used. These are carried through with several of the usually elusive intermediate steps displayed so that the treatments are clear, detailed, and interesting.

A strong point of the entire work is the clear presentation of the theory in close association with important experimental data. This leads to a large collection of interesting and useful figures. In fact, the figures and references in this book form by themselves a good introduction to modern liquid-helium physics. These figures are supplemented by an excellent appendix which is a small storehouse of experimental and theoretical data on the properties of liquid helium.

Gerald L. Pollack
Department of Physics, Michigan State University, East Lansing

Comparative Neurology


The two volumes under review represent the introductory chapters of an intended five-volume treatise on the comparative morphology of the vertebrate nervous system. These two volumes, however, belie their title and have little to do with vertebrate neurobiology. Accordingly it seems legitimate to consider each on its own merits as an independent work without prejudice to the following three volumes. Presumably these latter will deal more specifically with the subject of neuroanatomy and will represent a contribution to comparative neurology.

Volume I begins with a brief and unexceptional discussion of basic neurological concepts. A third of the volume is then devoted to a brief outline of the major phyla of the plant and animal kingdoms and a somewhat lengthy account of various aspects of organic evolution. The final half of the volume is undoubtedly of most interest, for in it Kuhlenbeck presents in detail a discussion of the history, philosophy, and methodology of comparative morphology. He is an advocate of the form-analytic approach, in which primary attention is directed to topologic location and orderliness of structure. Homology and analogy acquire purely morphological definitions in terms of the relation of a structure to a composite ideal form or Bauplan, and have neither functional nor phylogenetic connotations. Such a strict separation of form and function represents an approach which is perhaps foreign to many active neuroanatomists today, and many of the issues raised in the final sections of volume I seem of less immediate importance than they might have 30 years ago. Nevertheless, the approach advocated is logically valid, and the question of its usefulness to current neurological thought will be determined only upon the appearance of the final three volumes which embody it.

Despite the encyclopedic work of Bullock and Horridge on invertebrate nervous systems, there remains a definite place for a modest, competent summary of invertebrate neuroanatomy. Volume II purports to be such a review, but unfortunately it fails decisively. The treatment, which is on the level of an elementary zoology text, is diffuse and unorganized, with extensive interruptions for unconnected or irrelevant observations. Much of the material seems derived from secondary sources, and literature citations are too frequently neither the best nor the most recent. Although undoubtedly much of the work on invertebrate neuroanatomy was done before 1930, the omission of adequate discussion of many more recent findings, such as those of Panin and Horridge on coelenterate nerve nets or of Alexandrowicz on arthropod ganglia and receptors, seems remarkable. In many places implications of the findings of comparative neurophysiology for structural organization seem to be misunderstood or ignored, and obsolelente or unusual terminology may cause some confusion. Illustrations are profluse but of uneven value; many are poorly integrated with the text and may have been extensively modified or simplified from the original. The volume ends with a review of theories of the origin of the vertebrates.

I have difficulty in imagining the audience for either of these volumes. Those interested in the history or philosophy of morphological thought as exemplified by the German school may find the first volume of value. The second volume, however, is not sufficiently authoritative to serve as a reference for any professional scientist, and it is too poorly organized and written to be recommended as an introduction to invertebrate neuroanatomy for scientist, student, or layman.

Donald M. Maynard
Department of Zoology, University of Michigan, Ann Arbor

Books Received


The Changeless Order. The Physics of Space, Time and Motion. Edited with introductions by Arnold Koslow. Brazilian, New York, 1967. viii + 328 pp., illus. $7.50.


Indians of Brazil in the Twentieth Century. Edited and with parts translated from the Portuguese by Janice H. Hop-


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Calendar of Events—January

National Meetings


7-12. American Chemical Soc., New Orleans, La. (Meetings Manager, 1155 16th St., NW, Washington, D.C. 20036)


9-11. Chemical Marketing, Hopatcong, N.J. (Saul Gordon Associates Center for Professional Advancement, P.O. Box 66, Hopatcong 07843)


14-18. Society for Cryo-Ophthalmologists, Miami Beach, Fla. (J. G. Bellows, Executive Secretary, 30 N. Michigan Ave., Chicago, Ill. 60602)


15-17. Noise Measurement and Control, Hopatcong, N.J. (Saul Gordon Associates Center for Professional Advancement, P.O. Box 66, Hopatcong 07843)

15-17. Paint, Varnish, Lacquer, and Related Products, Cincinnati, Ohio.

Reviewed aspects of the early history of concepts involved in betatron acceleration, as well as items in the early development and operation of the betatron.

The symposium, planned by the Subcommittee on Radiation Dosimetry of the American Association of Physicists in Medicine, was sponsored by the Association, the New York Academy of Sciences, and the Office of Naval Research. Additional financial support was provided by the American Roentgen Ray Society and several accelerator manufacturers.

The proceedings will be published by the New York Academy of Sciences.

Lawrence H. Lanzl
University of Chicago, Chicago, and International Atomic Energy Agency, Vienna

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- 3-10. Tando Research, intern., conf., and seminar, Madras, India. (X. S. Thani Nayagam, c/o Dept. of Indian Studies, Univ. of Malaya, Kuala Lumpur, Malaysia)
- 5-6. Medical Days of Reen, intern. mtg., Collalbo, Italy. (M. DeMichelis, Via Modica 6, Milan, Italy)
- 12-16. Mollusca, symp., Cochín Ernakulam, India. (The Convenor, Marine Biological Assoc. of India, Marine Fisheries Post Office, Mandapam Camp, India)
- 16-20. Psychiatry, 2nd African colloquium, Dakar, Senegal. (Societe de Psychopathologie et d'Hygiene Mentale de Dakar, Service de Neuropsychiatrie, BP 5097, Centre Hospitalier de Fann-Daker, Dakar)
- 19-24. Diseases of Colon, Rectum and Anus, intern. seminar, Bombay, India. (R. K. Menda, Chairman, P.O. Box 677, Bombay)
- 24-31. Australian and New Zealand Assoc., for the Advancement of Science, 40th Congr., Christchurch, New Zealand. (H. R. Penfold, Univ. of Canterbury, Private Bag, Christchurch 1)
- 29-30. Canadian Soc. of Chemotherapy, 4th annual mtg., Montreal, Quebec. (I. Tétreault, Service de Recherche, Hôpital Saint-Jean-de-Dieu, Montréal-Gamelin, Quebec, Canada)