Comments and Communications

The Occurrence of *Branchiostoma* (*Amphioxus*) in Mississippi and Louisiana

On July 5, 1951, one of us (HMH), and R. L. Caylor, first found *Branchiostoma* while collecting just below low tide on the tip of Chandeleur Island, La., approximately half a mile from the lighthouse. Subsequent collecting has revealed its presence at the following locations on the outlying islands in Mississippi: Petit Bois Island, Aug. 7, 1951, found by Shoemaker and Caylor; Horn Island, Aug. 16, 1951, found by Caylor, Hefley, and Shoemaker; Ship Island, Aug. 21, 1951, found by Hefley and students of invertebrate zoology at the Gulf Coast Research Laboratory, Ocean Springs.

The specimens occur just below low tide line, usually in 2–4 ft of water, on clean sand bottom. Many were found associated with submerged marine vegetation; others were obtained in open sand.

The animals seem to be structurally quite close to *Branchiostoma caribaeum* Sundeval, 1893, with a few minor differences. In certain respects they resemble *B. bermudae* Hubbs, 1922, but have more myotomes (60 as compared with not more than 57 for *bermudae*). Preliminary counts show the fin rays to fall between 297 and 312, which lies within the range for *B. caribaeum* (227–330). Work on the exact determination of the form is proceeding.

This is the first account of the genus *Branchiostoma* occurring in the islands of Louisiana and Mississippi, and constitutes two state records for the genus.

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ASTIA and GSIS

In a footnote on the first page of our paper "The Planning of Libraries for Military Research Establishments" (*1*), we described the government-sponsored Armed Services Technical Information Agency (ASTIA) as the successor to the unofficial Group for the Standardization of Information Services (GSIS). Shortly after the publication of the paper, we were informed that our definition of this relationship was not quite correct. We also learned that Robert Bray of the Navy Research Section of the Library of Congress had submitted a communication to Science (*2*) in which he pointed out our error.

Although we regret any erroneous impressions created by the footnote in question, we must acknowledge that Mr. Bray's explanation of the relationship of the unofficial GSIS to the official ASTIA is more accurate than our explanation only because his is more current. Our paper was submitted to Science in February 1951; we were notified of its acceptance in April 1951; and it was published in July 1951. During this six-month period drastic changes in the character of the relationship of the two bodies apparently occurred.

We deduced the relationship that we described in our paper from a statement in the published proceedings of the *Symposium on Standardization in Technical Information Services for Government Contractors* (*3*) held in New York on Jan. 29, 1951. The statement, made by Eugene Scott, chairman of the symposium, in answer to the question "What is the relation of GSIS to ASTIA?" was as follows: ". . . GSIS is a sort of stepchild of ASTIA, which we hope will be adopted when ASTIA grows of age."

From the foregoing statement, and from similar remarks made at public meetings by other members of the unofficial Group for the Standardization of Information Services, we assumed that this was a forerunner, which was eventually to be succeeded or absorbed by the Armed Services Technical Information Agency.

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References

1. Herner, S., and Heatwole, M. K. *Science*, **114**, 57 (1951).
2. Bray, R. S. *Science*, **114**, 622 (1951).
3. Institute of the Aeronautical Sciences. *Symposium on Standardization in Technical Information Services for Government Contractors*; held at the 19th annual mtg., New York, Jan. 29, 1951. New York: IAS, 21 (1951).

Fungicidal Properties of Antihistaminics

In the July 6, 1951, issue of Science (**114**, 15), I read with great interest an account by Reiss and Caroline of "The Metabolism of *Blastomyces dermatitidis*, Antagonists to the Growth-inhibiting Effect of Trimeton Maleate." They reported that *B. dermatitidis* was partially inhibited with 0.0003 *M* trimeton maleate, and the growth of the fungus was completely suppressed at 0.15 *M* concentration. Experimental *B. dermatitidis* infection in mice did not respond to subcutaneous injections of 40 mg/kg trimeton maleate.

Previously I read in the June 23, 1950, issue of Science (**111**, 689) the account by Carson and Campbell of "The Inhibitory Effect of Three Antihistaminics on the Growth of Fungi Pathogenic for Man." They tested pyribenzamine, antistine, and di-phenyl-pyraline against cultures of *Trichophyton, Microsporon, Epidermophyton, Histoplasma, Blastomyces, Cryptococcus,* and *Candida,* and found that di-phenyl-pyraline and pyribenzamine possess properties that are inhibitory to pathogenic fungi.

A year before the paper of Carson and Campbell was published, we carried on experiments on the fungicidal properties of eight antihistaminics; namely, trimeton maleate, chlor trimeton, thephorin, antistine,

benedryl, histadyl, neoantergan, and pyribenzamine. The following pathogenic fungi were routinely used in these tests: *B. dermatitidis, Coccidiodis immitis, Trichophyton rubrum, Candida albicans,* and *Cryptococcus neoformans.* All the antihistaminics tested exhibited fungicidal properties, but thephorin and benedryl were the most potent. We submitted the manuscript first to a bacteriological journal and then to a pharmacological journal, but the paper was not accepted by either journal because, it was contended, the findings did not sound scientific. Discouraged, we decided not to publish our findings.

Now that two papers have been published on the fungicidal properties of antihistaminics confirming our earlier findings, in the interests of science we wish to state that

1) All antihistaminic compounds possess inhibitory effect on fungi.

2) The editors of scientific journals and their consultants must not turn down or refuse to publish a paper simply because the findings do not comply with their trend of thinking. They should use a scientific approach, free of prejudice, in accepting or refusing a manuscript. Opinionated thinking stops or delays progress.

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Book Reviews

Nerve Impulse. Transactions of the first conference, March 2–3, 1950, New York. David Nachmansohn, Ed. New York: Josiah Macy, Jr. Foundation, 1951. 159 pp. $3.00.

Because of the enormous number of papers presented at national scientific meetings many sections must meet simultaneously. Lack of time and organizational difficulties do not favor critical discussions. This condition, aggravated by inadequate space in some of our leading journals and perhaps by some arbitrariness in editorial policies, accounts for the fact that our scientific archives become more and more storehouses of facts without proper correlation and evaluation of the data. Under these circumstances the venture of the Macy Foundation, of bringing leading scientists together for an informal discussion of important problems, is a laudable undertaking.

Nerve Impulse inaugurates a new series and covers several important topics. Grundfest introduces "Potentialities and Limitations of Electrophysiology," and Quastel deals with the biochemical approach to the problem of nerve conduction. This problem is further elucidated from the viewpoint of comparative physiology by Prosser and in its histological aspect, particularly with regard to synaptic transmission, by Bodian. A report on ion exchange and permeability concludes the book. The discussion is carried on at a very high level, and many more problems than are indicated by the review titles are competently dealt with. Some improvement in procedure and presentation seems desirable, however. Particularly in the first section, the discussion is rather turbulent and jumps too much from one topic to another as a result of lack of guidance by the chairman. This must have been felt by the participants, since in the last section the discussion is omitted but "incorporated" in Steinbach's report. The reprinting of a competently guided discussion that would steer between these two extremes would appear to this reviewer most helpful. It is further suggested that the references might be handled uniformly throughout the book. In spite of these criticisms this is a publication rich in information and in ideas.

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Physik und Chemie des Zellkernes. Protoplasma-Monographien, Band 20. Petr F. Milovidov. Berlin-Nikolassee: Naturwissenschaftlicher Verlag, 1949. 529 pp.

Although the publication date of Milovidov's monograph is 1949, the author informs us that he wrote it in 1938–39 and managed to make some changes up to 1943. Completed about the time when the present tide of interest in the physical and chemical behavior of the cell nucleus was just beginning to rise, it can hardly be expected to reflect those specific problems that are the occasion of such intense activity today. Prague could hardly have been the ideal place to look into the future of nuclear physiology during those years.

Biology being more of a cumulative science than some others, a thorough work such as this retains value and even timeliness without being up to date. Bandwagons change more rapidly than tunes. In the case of nuclear function, most of our current viewpoints are restatements in chemical, and therefore more precise, terms of ideas derived earlier from microscopic observation. Thus, the hypothesis supported by tracer experiments, that a major activity of the nucleus is the synthesis of ribonucleic acid for deployment in the cytoplasm, is the heir to the older "chromidia" hypothesis, based on numerous descriptions of the passage of basophilic particles from nucleus to cytoplasm. In Milovidov's work, such older hypotheses are discussed thoroughly and with reference to a great wealth of specific cases. Milovidov's monograph merits serious study by those who are