Editor's Page

The Central Office wishes to thank the members of the Society for their splendid response to the questionnaire sent out last fall. The information received will be very helpful and will facilitate our labors considerably. There are still a few members who have not returned the forms. It will be appreciated if those of you who have neglected to send in the information will do so soon. In order to carry on the activities of the Society an up-to-date membership record on all members - associate, regular, retired and honorary - is essential. Thank you for your cooperation.

We call your attention to the announcement of the physiology course for physicians and the enclosed program. The Council will appreciate your taking time to publicize the course widely. It is felt that the Society not only has an obligation to its members in providing means (meetings) for interchange of research ideas but as stated in the constitution - "to promote the increase of physiological knowledge and its utilization." Such courses as this one for physicians is certainly a way of bringing the latest physiological knowledge to one using group.

PHYSIOLOGY COURSE FOR PHYSICIANS

(Note enclosure with this issue)

Please note the inclosed program for a course on "The Physiological Basis for Diagnosis and Treatment" to be given at San Francisco, March 30 to April 3, 1960. The course is particularly designed for physicians and is being presented just prior to the Annual Sessions of the American College of Physicians. The course is under the directorship of Dr. Julius H. Comroe, Jr., and is sponsored by the American Physiological Society in cooperation with the University of California School of Medicine.

The Society requests that you as members of the Society bring this announcement to the attention of your clinical colleagues and practicing physicians in your area or at least post the announcement on a prominent bulletin board. Many members of the Society are on the faculty thus indicating their interest in bringing the latest physiological findings to their clinical colleagues. Will you as non-participating members show your interest by seeing that the course is well publicized? The University of California does not benefit from any net profit but the Society will.

RUSSELL HENRY CHITTENDEN

In order to familiarize the present membership of the Society with one of its founding fathers who was the third person to serve as president of the Society, the following is an abbreviated sketch of Russell Henry Chittenden taken from the History of the American Physiological Society published in 1938 and from various biographical materials--particularly that by H. B. Vickery, published by the National Academy of Sciences in 1945.

Professor Chittenden was born at New Haven, Connecticut, in 1856. His ancestry can be traced through eight generations of Connecticut forebears to a William Chittenden who migrated from England. Russell Henry Chittenden was raised and educated in New Haven. He graduated from the Sheffield Scientific School of Yale University in 1875 with the degree of Ph.B. He continued his studies at Yale and also studied with Kühne at Heidelberg. In 1880 he received his Ph.D. degree from Yale. This was the first such degree in physiological chemistry given by an American University. During his senior year as an undergraduate he was appointed as an assistant in physiological chemistry and had charge of the first laboratory course in the subject given in this country. It is noteworthy that this course originated not in a medical school but in a university department of chemistry. Owing to this circumstance it developed along lines quite different from those followed by medical chemistry as given in the professional schools, which at that time was concerned not so much with the scientific aspects of the subject as with the application of chemical methods to diagnostic and toxicological problems.

Chittenden early held the view, and strenuously maintained it throughout his life, that physiological chemistry is a true biological science just as are zoology, botany or morphology. In 1930, he wrote: "Morphology and physiology were the two main divisions of biology, the one dealing with form and structure, the other with function. Physiological chemistry was to be considered simply as a part of physiology, having to do with the study of the chemical functions of the living organism, animal or vegetable, as the case might be. This being so there was justification for the development of physiological chemistry in a broad biological course of study that aimed to present a more or less complete picture of the phenomena of life. Moreover, the environment so provided tended to emphasize the true position of physiological chemistry as a biological subject not restricted to the necessities of any branch of applied science. To limit the study of physiological chemistry to the needs of medicine, for example, would be to defeat the end in view, viz., the expansion of physiological knowledge in all its varied aspects. Medicine in the end would profit most from a broad development of physiological chemistry, realizing that every new fact brought to light is in time liable to contribute something to that fund of knowledge which is of direct use, hence of practical value, to the every day practitioner of medicine."

Chittenden was appointed Professor of Physiological Chemistry at the Sheffield Scientific School of Yale University in 1882. He immediately began building a strong graduate course in physiological chemistry. Two of his first Ph.D. graduates were Lafayette B. Mendel (1893) and Yandell Henderson (1898).

Together with Bowditch and Martin, Chittenden was instrumental in giving to American physiology an independent standing. Before this period young men interested in physiological science were obliged to go abroad to obtain the necessary training, but with the development of these three laboratories, facilities were provided at home, and an increasing number of young students and graduates in medicine took advantage of the opportunities offered.

Chittenden's important scientific work falls into two main categories. During his early teaching years he became interested in the action of enzymes in the processes of food assimilation. This finally led to the general field of nutrition and included fundamental investigations that prepared the way for the outstanding work of his pupil and successor, Mendel. Chittenden's own studies of protein requirements of man were regarded by him as his most significant work. The second main category of his scientific work is his investigations in the field of toxicology, particularly the effects of arsenic, alcohol and sodium benzoate.

His official position and his rising distinction in the field of physiological chemistry made Chittenden an influential member of the American Physiological Society from the beginning. Among the original group he and Vaughan were the sole representatives of the chemical side of physiology, but it was not long before other men with chemical training were taken in, and the subject began to appear on the programs of the meetings with increasing frequency, so much so, in fact, that in a relatively short time it became desirable to form a separate--although affiliated--society, the American Society of Biological Chemists. Chittenden was elected to the Council of the American Physiological Society at its third annual meeting, 1890, and thereafter served in this position for fifteen years. In 1896 he was made president of the Society and was reelected in successive years until 1904, the longest service of any president of the Society. It was during his administration that the Society began to grow rapidly in numbers and in the extent of its activities.

Chittenden retired from his position as Professor of Physiological Chemistry in 1922. His twenty-one years of retirement were happy ones. Few are so fortunate as to be able to see the principles that have been fought for through the years develop and flourish as it was his good fortune to do. He had brought back with him from Germany an ideal of teaching through doing that he made the basis of his courses, and physiological chemistry was started, literally from zero at the beginning of his teaching career, and was fostered until it grew and branched in innumerable unanticipated directions. His students carried his methods and principles to almost every other medical school in the country. Truly he was the dean of American biochemistry.

FOURTH BOWDITCH LECTURE

Physiology of Taste

LLOYD M. BEIDLER

Professor of Physiology
Florida State University, Tallahassee, Florida

Taste receptors respond to hundreds of different chemical stimuli over a large range of concentration. They monitor the food intake and serve a protective as well as an esthetic function. Lower animals rely heavily on the taste and olfactory receptors to determine whether food will be accepted or rejected. Man's food, however, is so diversified and in many cases so artificial that he no longer is capable of judging the acceptance value or need of particular foods on the basis of chemoreceptor responses alone.

The taste bud is the organ of taste. Taste buds are located on fungiform, vallate and foliate papillae of the tongue and are innervated by the chorda tympani and glossopharyngeal nerves. A chorda tympani nerve in the rat innervates taste buds on about 90 fungiform papillae. Each taste bud is about 30 μ in diameter and 60 μ in length and contains taste cells in different stages of development. Each cell has a number of villi-like projections commonly called taste hairs that project from the tip of the taste bud through the taste pore and into the aqueous layer covering the surface of the tongue. Each taste hair is about 4 μ in length and 0.2 μ in width. The taste cells may be injured by unusual temperatures and chemicals so that there is a continual turnover rate of taste cells within the bud. Colchicine arrests mitotic division during metaphase. When colchicine is injected into a rat, considerable degeneration of taste cells is observed within 6 hours and the entire taste bud disappears within 24 hours. The electrophysiological response of the taste receptors to a NaCl stimulus declines to 50% 5-7 hours after colchicine injection and is almost completely absent after 8 hours.

The taste buds are maintained by the taste nerves. Forty hours after section of the chorda tympani, the electrophysiological response is absent and the taste cells are degenerating. A short time later the taste buds completely disappear and only reappear after the regenerating taste nerve fibers enter the papillae.

The continual turnover of taste cells must have some (as yet unknown) functional significance for the physiology of taste. Since the taste buds on the front two-thirds of the tongue are very close to the surface, they are probably more easily insulted than are those which are located in deep grooves at the back of the tongue and are therefore better protected.

It is commonly accepted that there are four basic taste qualities, salty, sour, sweet and bitter. Are there separate taste cells for each quality? To answer this, small micropipette electrodes were inserted into the cells of the taste bud located on a fungiform papilla of the rat or hamster. Various chemical stimuli were placed on the tongue and the magnitude of the taste cell depolarization was measured. Each taste cell studied had a different sensitivity to each chemical stimulus. Some cells responded to NaCl, to sucrose and to HCL whereas others did not respond to sucrose but only to NaCl, HCl and quinine, for example. The response to various cations also varied from cell to cell.

Since a single taste nerve fiber innervates several taste cells and transmits the information from all these cells to the central nervous system, it is important to know whether fiber specificity for taste quality exists. The response of single taste nerve fibers from the rat, cat, hamster, frog and rabbit was studied. Each fiber was found to have a different sensitivity to a given chemical stimulus. Fibers responded to one or more of the stimuli corresponding to the four basic taste qualities. The taste fiber does not respond with the steady frequency of nerve firing such as one finds, for example, in visual fibers of Limulus, but the activity is very irregular instead. For this reason, single taste fiber activity is difficult to quantify. Neural activity is recorded in less than 50 msec. after the chemical stimulus is applied to the surface of the tongue. The response declines during the first two seconds and then maintains a constant level until the chemical stimulus is removed from the tongue.

In order to obtain quantitative information from the population of taste receptors, the total neural activity is electronically summated and displayed on a Sanborn recorder. The tongue is placed into a flow-chamber, so that solutions can be continually flowed over a constant

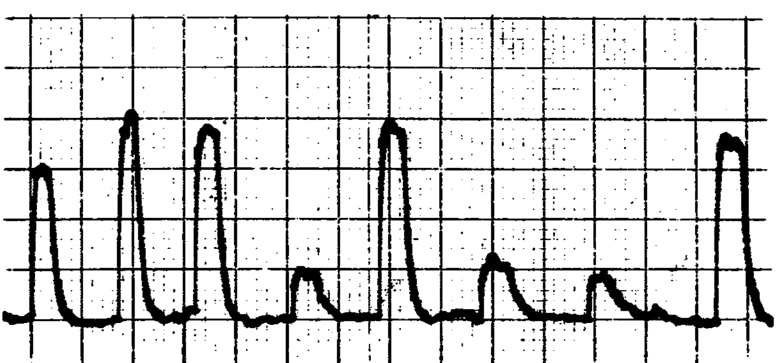

Fig. 1. Integrated electrical response of chorda tympani of rat to various 0.1 M chloride salt solutions flowed over tongue of rat: NH_4Cl, LiCl, NaCl, KCl, NaCl, RbCl, CsCl, NaCl. Test solutions are interspaced with water rinses. Time scale: 1 large division = 20 seconds.

area of the tongue. Figure 1 shows the integrated response to a series of 0.1 M chloride salts. The magnitude of response is easily measured and the response to a control stimulus is constant in magnitude within 10% over a period of 30 hours. Thus, hundreds of different chemicals can be used as stimuli at various concentrations on the same experimental animal.

The total nerve response to a given stimulus varies from one species of animal to another but is rather constant within a given species. For example, all rodents respond very well to NaCl and not too well to KCl whereas carnivores respond much better to KCl than to NaCl. There is a correlation between the Na and K red cell content and the magnitude of taste response (see table 1). It has been observed in 15 species that those animals with high K red cell content have taste receptors that respond well to NaCl and poorly to KCl. How rigid is this correlation? Some species such as sheep show individual variations in red cell content although no variations in magnitude of taste response to Na and K is observed from one animal to another. No causal relationship between ion content of red cell and taste response is known.

TABLE 1

	Red Cell Content $\frac{Na}{K}$	Relative Taste Response $\frac{Na}{K}$
Carnivores		
Racoon	19.3	0.75
Cat	17.0	0.67
Dog	12.0	0.43
Average	16.1	0.62
Rodents		
Rat	0.12	2.0
Hamster	0.11	2.8
Guinea pig	0.14	2.6
Average	0.12	2.5

Cats possess few taste cells that respond to sugars whereas rats show a fair response and hamster and guinea pig receptors respond well to sucrose. Similar variations between species of animals have been observed with many other substances.

What is the mechanism by which the chemical stimulus excites the receptor? The large number of chemicals that stimulate without injury suggests that the substances need not penetrate to stimulate. The ease with which the stimulus is removed also indicates that the mechanism of stimulation does not involve strong binding forces. Let us assume that the ion or molecule is bound to some molecular site on the surface of the receptor. Then, from the law of mass action:

$$A + P \rightleftharpoons AP$$
$$C \quad N-Z \qquad Z$$

where:

A = stimulating ion or molecule
P = unfilled binding site
AP = filled binding site
C = concentration of chemical stimulus
N = total number of sites available
Z = number of sites filled at concentration, C.

The equilibrium constant of the reaction is:

$$K = \frac{Z}{C(N-Z)} \qquad \text{Equation 1}$$

Assume that the receptor response, R, is proportional to the number of sites filled,Z, and that the maximum response obtainable, R_s, occurs when all the available sites,N, are filled. (There are a number of different sites so that a sucrose molecule would not be expected to fill a site suitable for Na^+, for example.)

Then:

$$R = aZ \qquad R_s = aN$$

Substituting in Equation 1 and rearranging:

$$\frac{C}{R} = \frac{C}{R_s} + \frac{1}{KR_s} \qquad \text{Equation 2}$$

This is an equation that describes the magnitude of response in terms of the concentration of the stimulus, the maximum response and the equilibrium constant. Since the latter is the only parameter not directly measured, it can be found by solving Equation 2. That the equation is sufficient to describe mathematically the data is shown by the fact that the data fit a straight line when $\frac{C}{R}$ is plotted against C (see fig. 2). This does not necessarily mean that the proposed mechanism is a correct one but merely proves that Equation 2 describes the data. This equation is similar to the well-known Langmuir's adsorption isotherm.

Since equilibrium values of the responses are used, the relative change in free energy, ΔF, may be obtained from the relation:

$$\Delta F = -RT \ln K$$

where R is the gas constant and T is the absolute temperature. The magnitude of ΔF, several kilocalories per mole, indicates a very weak binding force similar to that found in the binding of cations to proteins. The forces are much weaker than those usually associated with enzyme-substrate reactions.

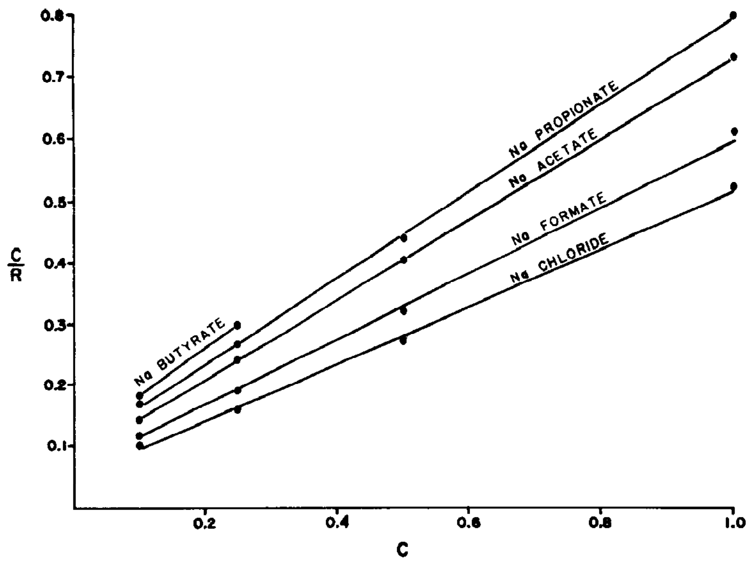

Fig. 2. The ratio of the molar concentration of the stimulus and the magnitude of the integrated response of the rat chemoreceptors is plotted against the molar concentration of the stimulus.

Temperature independence of the reaction is shown by the constant magnitude of response to NaCl, for example, at 20°C, 25°C and 30°C. Thus, $\Delta H = 0$ in the relation:

$$\Delta F = \Delta H - T\Delta S$$

Since ΔF was found to be negative, all the change in free energy is in the form of a positive change in entropy, ΔS. This fact suggests either that the cation loses some of the water of hydration in binding to the sites or that the receptor molecule changes shape slightly when its sites are filled. A similar positive change in entropy has been observed when Na^+ is bound to a protein, in vitro.

Although the receptor response does not appreciably change between temperatures of 20° to 30°C, Hahn (1936) has found that the taste sensation of NaCl does depend on temperature. This discrepancy may be accounted for by interactions between temperature cells and taste cells in the taste cortical projection area. Landgren (1957) observed single cortical cells that responded to both temperature and chemical stimulation of the tongue.

The binding sites for salts do not appreciably change when the pH of the solution changes from 3 to 11. Thus, most carboxyl groups could not be considered as suitable binding sites, but phosphate groups, for example, would account for much of the data.

Various acids do not elicit the same magnitude of taste receptor response at equi-molarity nor equi-hydrogen ion concentration (see table 2); 150 millimolar butyric acid is required to initiate the same magnitude of response as 5 millimolar HCl. However, the former represents only 1.41 millimolar hydrogen ion whereas the latter is 5.0 millimolar. Such differences are very similar to those found in the study of adsorption of acids to wool protein. The acid binding to protein increases greatly with ionic strength. Such findings are similar to those noted subjectively in taste. The addition of Na acetate to a solution of acetic acid may change the hydrogen ion concentration by a factor of 100 without appreciably changing the degree of sourness of the solution.

TABLE 2

Response equal to that to 0.0050 M HCl

Acid	Molarity (mM)	mM H
Sulfuric	2.2	4.2
Hydrochloric	5.0	5.0
Dichloroacetic	9.0	7.8
Formic	11.6	1.46
Lactic	15.6	1.4
Acetic	64.0	1.08
Propionic	130.0	1.35
Butyric	150.0	1.48

All of the above information leads to the conclusion that chemical taste stimuli bind to sites of a polyelectrolyte structure on the surface of the so-called taste hairs. The binding of most electrolytes and non-electrolytes is very weak. Each receptor surface is not identical in molecular arrangement so that the relative strength of binding of various cations differs from one receptor to the next. This difference may be exaggerated between species. Since taste stimuli such as sugars and salts bind to quite different sites, competitive inhibition is not necessarily expected. When sites are occupied, the receptor molecule undergoes a small spatial rearrangement that eventually initiates depolarization of the taste cell, and the generation of nerve action potentials follows.

What is the relation between the taste receptor response and the sensation of taste? Can the latter be predicted from the former? A measurement of taste sensation must be used to study this problem.

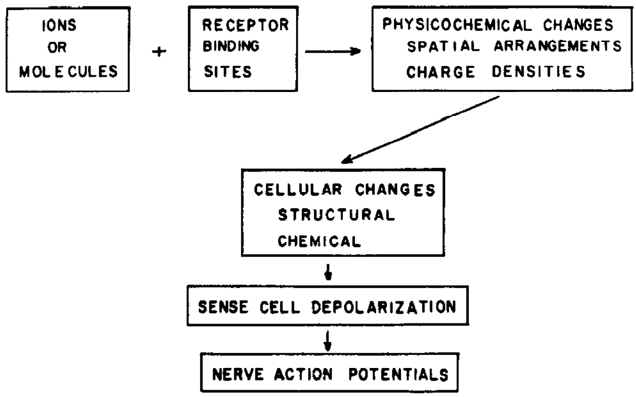

Fig. 3. Schematic of theory of taste receptor stimulation and nerve action potential initiation.

The ability to distinguish between two different concentrations of taste stimuli is one of the simplest taste measurements. Threshold concentration is first determined, then the next higher concentration that can be detected as different from the threshold is observed. This is called the just noticeable difference or j.n.d. Successive j.n.d's are found and the number of j.n.d.'s are plotted against concentration. The data of Lemberger (1906) for sucrose may be fitted by a function similar to that described by Equation 2. If the taste equation can adequately describe the j.n.d. data, then $\frac{C}{R}$ vs. C should be a straight line where R is now measured in terms of j.n.d.'s. The closeness of fit is shown in figure 4.

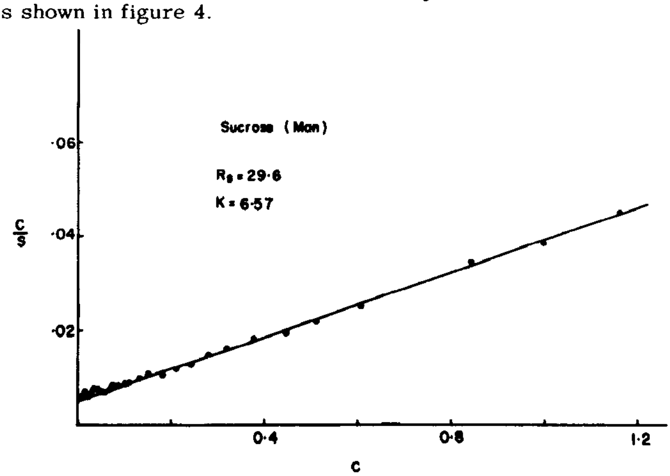

Fig. 4. The psychophysical data of Lemberger (1908) can be described by Equation 2. The stimulus concentration is C and the number of discriminatory steps (j.n.d.) between threshold and concentration C is S. The equilibrium constant of the taste reaction is K and the total number of j.n.d.'s possible is R_s.

The well-known Weber ratio, $\frac{\Delta I}{I}$, where I is the intensity of stimulus, is often used as a measure of discrimination in psychophysics. In taste, concentration is the unit of intensity so ΔC can be found by solving for R in Equation 2:

$$R = \frac{CKR_s}{1+CK}$$

If the difference in response to two different concentrations is needed:

$$\Delta R = \frac{C_A K R_s}{C_A K+1} - \frac{C_B K R}{C_B K+1}$$

Assume that each j.n.d. represents an equal magnitude of neural activity, ΔR, assign this value unity for convenience and solve for C.

$$\Delta C = \frac{1 + 2KC_B + K^2 C_B{}^2}{K(R_s + KC_B + 1)}$$

The predicted curve for $\frac{\Delta C}{C}$ vs. C fits the experimental data.

What is the relation between Fechner's law and the fundamental taste equation? Fechner stated that the magnitude of response is a logarithmic function of the intensity of stimulus. This law agrees reasonably well with the taste equation over the medium concentration range but diverges at both high and low concentrations where the taste equation better describes the data.

To summarize, there now exists basic knowledge concerning the response of taste receptors to a wide variety of chemical stimuli. A physicochemical theory has been formulated which allows one to predict many of the electrophysiological characteristics of the response of the receptors as well as certain simple psychophysical relationships. A knowledge of the mechanism of taste receptor stimulation enables one to better understand many factors important in taste sensation. The study of the physiology of taste will also contribute to our knowledge of the interactions between chemicals and living systems.

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SPRING MEETING

Chicago, April 11-15, 1960

The 44th Annual Meeting of the Federation will be held in Chicago, April 11-15, 1960. Scientific sessions will start Monday morning at 9:00 o'clock. This is a change from previous years when the sessions started Monday afternoon. The extra half day was necessitated by the large number of papers.

Registration: The Federation Office and Headquarters will be at the Conrad Hilton Hotel. Registration will be at the Conrad Hilton only, opening at noon on Sunday, April 10. Registration fee will be $10. If you have registered in advance please bring your receipt with you and present it at the Advance Registration Desk. Registration desks will be open till 10:00 P.M. on Sunday and from 8:00 a.m. to 5:00 p.m. Monday through Friday. Tickets for special functions will be on sale in the registration area.

Exhibits: Exhibits will be located in the Conrad Hilton and will be open from Monday morning until Friday noon. Exhibits will close at 5:00 p.m. Monday through Thursday.

Placement Service: The Federation Placement Service will be located in the Palmer House.

APS Headquarters: The Morrison Hotel will serve as Headquarters for the Physiological Society. Most of the Physiology sessions will be held in the Morrison. Physiology and Pharmacology will have some integrated programs at the Palmer House and the Morrison. The Business Meetings of the Physiological Society will be at the Morrison. The Executive Secretary will maintain an office in the Normandy Room of the Morrison to assist members.

Special Sessions of APS: As has been the practice the last few years the Society will arrange 8 sessions with special 30-minute introductory reviews by selected persons. The Teaching Session, under the leadership of R. E. Johnson, will deal with standards for graduate training in physiology. The speakers will include E. M. Landis, T. C. Ruch and Daniel Harris. The Society is sponsoring a symposium on "Learning and Motivation" led by R. B. Livingston. In cooperation with the Pharmacologists Harold Himwich is organizing a symposium on "Animal Behavior and Neurophysiological Correlates."

STUDENT LABORATORY EQUIPMENT TO BE DEMONSTRATED AT LOYOLA UNIVERSITY

W. C. RANDALL

In view of remarkably active interest in efforts to modernize the teaching facilities of the medical student laboratory, the Physiology and Pharmacology Departments of the Loyola University School of Medicine are planning to demonstrate their student laboratory equipment during the period of the Federation Meetings in Chicago, on April 13, 1960. This laboratory is equipped with twelve four-channel Polygraphs, Statham pressure transducers, Grass Force transducers, Grass S6 stimulators, and numerous other transducers fabricated in the Departmental shop for the performance of other student experiments. An interconnecting system has also been devised which makes it possible to employ any unit as a "master" and any or all others as "slave" units during a demonstration or during a regularly scheduled experiment.

The laboratory experiments are scheduled so that one half the class employ the Polygraphs in "recording" experiments while the other half perform "non-polygraph" experiments or are in conference. The student-instrument ratio is established at 4 to 1. All conventional physiology experiments are performed with any combination of one to four variables recording simultaneously. The equipment permits accurate recording of such functions as systolic and diastolic blood pressure, respiration, action potentials (including EKG, EEG, EMG, etc.), tissue or electrical resistance, temperatures, plethysmograms, force of muscle contraction, etc.

The Medical School is situated in the West Side Medical Center at 706 S. Wolcott Ave. (1900 West and 700 South) and may be reached quickly and inexpensively from the Dearborn Street Subway. Take either "A" or "B" trains west to the Medical Center. Get off the train at the Medical Center stop from "A", or Polk St. stop from "B" trains.

FEDERATION PLACEMENT SERVICE

A Personal Interview Service is provided at the Annual Meetings of the Federation of American Societies for Experimental Biology.

The Placement Offices in Chicago will be in the Exhibition Hall of the Palmer House. They will be open for candidate registration Sunday, April 10, from 1:00 p.m. to 5:00 p.m. and Monday - Thursday, April 11 - 14, 8:15 a.m. to 5:00 p.m. Employers' registration will begin Monday, April 11, at 8:15 a.m. Interviews will be scheduled Tuesday through Thursday 8:30 a.m. to 5:00 p.m.

These facilities are open only to registrants at the Chicago Annual Meetings and to those registered with the Placement Service. An annual fee of $3.00 for maintaining registration is required of candidates for positions. An employer must be an annual subscriber or a subscriber to the February 1960 list of candidates.

Advance registration with the Placement Service may be made through the Washington Office before March 28, 1960. Both employers and candidates may register after arriving at Chicago.

The Placement Service publishes:

Lists of Candidates: February, May, August and November
Lists of Positions: March and September
List of Fellowships: October

Subscription Price:	
Calendar Year	$20.00
Single issues:	
List of Candidates	$10.00
List of Positions	$ 5.00
List of Fellowships	$ 2.00

The last lists to reach subscribers before the Annual Meeting are:

February List of Candidates: Information to be included must reach the Washington office before February 1
March List of Positions: Information to be included must reach the Washington office before March 1.

For further information address:

PLACEMENT SERVICE
Federation of American Societies for Experimental Biology
9650 Wisconsin Avenue, Washington 14, D.C.

CARLSON MEMORIAL FUND

A Memorial Fund is being accumulated at the University of Chicago for the purpose of establishing an annual A. J. Carlson Lecture in Physiology. The Chairman of the Memorial Fund Committee is Dwight J. Ingle, Professor of Physiology, University of Chicago. It is hoped that friends and former students of Dr. Carlson will wish to contribute to the fund.

FALL MEETING, AUGUST 23-26, 1960

Combine your vacation with the meeting

The Fall Meeting of the Society will be held August 23-26, 1960 at Stanford University, Stanford, California, and the University of California Medical Center in San Francisco. Note the earlier than usual time. This was planned deliberately so that eastern members can combine family summer vacation and APS meeting and still be back east by Labor Day.

Registration will begin on Monday, August 22. Tentative plans provide for the Refresher Course on Renal Physiology to be held the morning, afternoon and evening of Tuesday, August 23. Sessions of regular 10-minute papers will be held on Wednesday and Friday at the Stanford School of Medicine on the Stanford campus. On Thursday, August 25, bus transportation will be provided for the morning symposia and luncheon at the University of California Medical Center in San Francisco. After luncheon on Thursday, the "Three Bridges" bus tour will continue to the redwoods in Muir Woods, back to the Berkeley campus of the University of California and the annual dinner and Presidential Address at the Claremont Hotel in Berkeley. Buses will return the group to the Stanford campus between 11:00 and 12:00 that night.

Housing during the meetings will be in Stanford University dormitories at $7.00 per person per day for lodging and meals. Those planning to combine their family vacations with the meetings should make reservations in nearby motels if they are bringing children below age 12. Families, including older children, can be accommodated in the dormitories. An attractive Ladies Program including a day's bus trip to Monterey and Carmel is being planned.

Inquiries for any preliminary information, before receiving your regular announcement, should be addressed to Dr. J. M. Crismon, Department of Physiology, Stanford University, Stanford, California, or to Dr. Leslie Bennett, Department of Physiology, University of California Medical Center, San Francisco, California.

FIRST INTERNATIONAL CONGRESS OF HISTOCHEMISTRY AND CYTOCHEMISTRY

The First International Congress of Histochemistry and Cytochemistry will be held in Paris, August 28 to September 3, 1960. The American Histochemical Society is one of the organizing groups. The Secretary General of the Congress is Dr. R. Wegman, 45 Rue des Saint-Peres, Paris 6 e, France. Travel arrangements are being made by the Association of Academic Travel Abroad, Inc., 550 Fifth Ave., New York 36, N.Y.

REPORT TO COUNCIL ON COUNCILOR'S TOUR

H. W. DAVENPORT

From October 23 to October 31, 1959, I visited the physiology departments of the University of Western Ontario, the Mayo Foundation, the University of Minnesota and the University of Manitoba. One of my hosts pointed out that a trip on which I met four past presidents of the Society was not to the grass roots but rather to the old oaks. My hosts arranged for me to meet most Society members and many nonmembers as well. I was able to discuss problems relating to the American Physiological Society with persons in several pre-clinical departments. The chief topics I discussed were Society finances, compulsory subscriptions to journals, maintenance of standards in physiology, the Federation, and the relation of APS with Canadians and the Canadian Physiology Society. According to custom governing the behavior of a visiting fireman (or as they say in bilingual Canada, un pompier visitant) I earned my way by giving a talk on "The Metabolic Basis of Gastric Acid Secretion, or Twenty Years of Wasted Effort."

In general, members feel that they do not know what the Society finances are. They would like to have a much fuller report of the budgets, both past and proposed. Members who have looked at the previous reports think that they are meaningless, and they would welcome an understandable budget published in THE PHYSIOLOGIST. The budget would be more readily intelligible if the operations of the trust funds and grants were listed separately. Members are particularly in the dark about the Board of Publication Trustees and its relation to the rest of the Society. After I explained as well as I could the financial situation and the use of dues money, I received the impression that there would be no strenuous objection if dues were raised. Members are willing to pay for the functions of the Society if they know the facts. If increase in dues were coupled with dropping compulsory subscription to journals there would be no objection at all.

There is a minority opinion that compulsory subscription to a journal is an appropriate part of membership in the Society. I surmise that subscription to Physiological Reviews weakens this feeling, and I suspect that opinion would be more vigorous if it were subscription to the American Journal of Physiology which is required. One member would like to have compulsory subscription to AJP coupled with a vigorous effort to make it a better journal. The majority opinion appears to be indifferent to compulsory subscription. These members have no objection if subscription really does something for the journal or for the Society. On the other hand, if compulsory subscription does no good, either for the publications or for the Society, then it should be dropped. Some members are astonished that subscriptions do not help the Society. This is another aspect of the confusion and ignorance concerning the functions of the Board of Publication Trustees, and members in general would welcome a clear exposition of this.

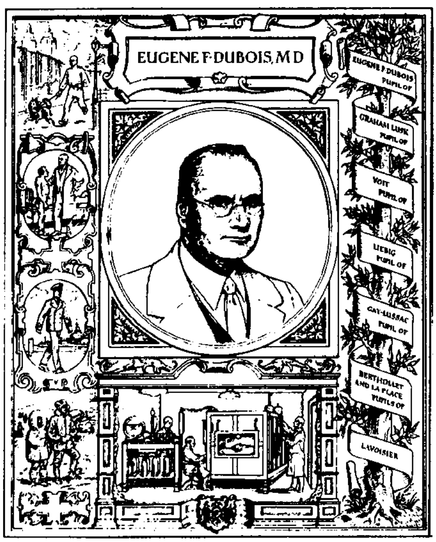
Members are interested in the way in which the American Chemical Society sets standards for chemistry departments, and a description of this in THE PHYSIOLOGIST would be welcomed. Members of weaker physiology departments believe a similar function on the part of APS would be helpful. APS could supply information or set standards concerning the support of physiology departments. Even general information as to how many staff members are required for teaching medical students, other kinds of undergraduate students, graduate students and for research would be useful. Since Government agencies are very wary of this kind of activity, some members suggest that APS seek funds from other foundations to carry out such work. Although all members are opposed to setting up an American Board of Physiology, they recognize that the medical boards have fulfilled useful functions.

Canadian schools frequently use external examiners, particularly for Ph.D. theses. There is strong feeling that American schools would do well to set up external examination systems at the level of the preliminary examinations for the Ph.D. American members believe that APS might facilitate this by providing lists of examiners or by setting up a means by which schools could obtain the services of external examiners. On the contrary, there is a vigorous minority opinion that external examiners are undesirable because they tend to enforce uniformity. External examinations would level off the strong points of departments and make departments anxious to see that every topic was covered in a standard way.

It is the universal opinion that the Federation serves a useful purpose and that APS relations with it should be preserved. Members want to attend the Federation, and in general there is no desire for regional meetings of the APS itself. Members are alarmed at the size of the Federation, the enormous difficulty of attending the meetings and the cost. They are horrified at the idea of lengthening the meetings. Members all recognize that the ten-minute paper is the source of many difficulties, and they are willing to see some limitation on it. They believe that the Fall Meetings of APS are useful in allowing unrestricted presentation of ten-minute papers. They also inform me that in most instances travel funds are not tied to the giving of papers. As for means of restricting presentation of papers, the members are not uniform in their opinion. They understand that papers cannot be edited, and they have no enthusiasm for limiting the frequency with which members can present papers. If it were possible papers might be restricted at the source, each department presenting a certain number of its best papers. I gathered that restriction of presentation of papers to members alone, that is elimination of the right of introducing a paper, would be accepted without much difficulty. Although members would like more symposia at the Federation meetings, they would not care for restriction of papers to certain topics. Several members suggested that APS charge $5 as a program service fee for each paper presented, the money going to the Society General Fund. There is general concern about the quality of the papers presented and especially about the abstracts.

APS is in fact a North American Society, and Canadian members value membership in it. In many respects Canadian and American physiology is a unit, for both have the same employment market, sources of money, problems of training and of maintaining standards. Some Canadian members would welcome increased recognition of the international character of APS.

Recruiting graduate students for physiology seems to be no more successful in the places I visited than in the places I am more familiar with. One member strongly expressed the opinion that we are debasing the profession by our recruiting methods. We should make it hard to get into rather than open to any one we can pull off the streets.



THE EUGENE F. DUBOIS LIBRARY

The Eugene F. DuBois Library was dedicated on November 24, 1959 by the staff of the Second (Cornell) Medical Division, Bellevue Hospital, New York. The library occupies the exact site of the former metabolism ward of the Russell Sage Institute of Pathology where Dr. DuBois did his pioneer research in human metabolism.

Dr. DuBois was Professor of Medicine at Cornell from 1930 to 1941 and Professor of Physiology from 1941 to 1950. The accompanying plaque shows his scientific heritage and illustrations of his activities.

The library is maintained for the use of the clinical and research staffs of the Second Medical Division. The library is well adapted for seminars and clinical conferences.

PHYSIOLOGY IN ORBIT[1]

WALLACE O. FENN

One of the main reasons why I chose the title "Physiology in Orbit" was that this represents a logical sequel to the title of a former address which I gave some years ago at a meeting of the American Physiological Society which was called "Physiology on Horseback"(1). I can hardly say that that former occasion was in the rugged pioneering days when horseback was the only means of travel nor was it in the horse and buggy era. It did represent however a time, and almost the first time, when physiologists in large numbers were being called upon by the Government for help in problems concerned with military operations, including particularly submarine and aviation medicine and later a vast amount of radiation biology, and we had begun to feel that physiology after all had found recognition for itself in the market place where men of action were in the saddle. We found this a stimulating experience although it did divert many of us from the uninterrupted pursuit of fundamental physiological problems. To our surprise, however, we found new fundamental problems facing us which were sometimes even more exciting and interesting than those we had left behind. Every problem, I have found, is basic if the investigator puts some basic thinking into it. This increased demand for physiologists has increased the supply little by little; it has increased our knowledge of physiology and so has contributed to the advance of medicine; and on the whole it has been a boon to the profession.

Now we have encountered another and even more novel challenge. The dazzling prospect of a sight-seeing trip in space around the earth or the moon or Venus or Mars is a piece of Science Fiction which seems not unlikely to come true at least within the lifetimes of some of us. And wherever a man is involved in such adventures the physiologist is also heavily engaged. At first glance there is perhaps little to worry about in the way of new fundamental problems in physiology. Mostly it is a matter of devising good engineering gadgets to produce and maintain the cabin environment within tolerable limits and these limits are already rather well known. We must be prepared for surprises, however, and it is certainly not inappropriate to spend some time thinking seriously about what these surprises might be and how they might best be handled.

To this end many new organizations and committees have appeared and these have included microbiologists and physiologists as well as representatives of all the other sciences. I have been involved in some of these myself in a small way, particularly COSPAR and CETEX. Now COSPAR sounds to me like some sort of new type of floor varnish but it does stand for Committee on Space Research and it was set up

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[1]Presented at the Dedication of the new Biology Building, Burrill Hall, University of Illinois, Urbana, September 8, 1959.

by the International Council of Scientific Unions last fall at its Washington meeting and was designed to include scientists of all interested nations and disciplines. CETEX is not a committee for the exploitation of Texas, as some have supposed, but rather a Committee on Contamination by Extraterrestrial Exploration. The chief business of CETEX was to consider the possible dangers of contaminating the moon or the planets by rocket impacts. More extensive study of the same problem has been continued in this country by two committees set up by the Space Science Board of the National Academy of Sciences. One of these composed of experts on the East Coast is called EASTEX and one on the West Coast is called WESTEX--a rare bit of scientific humor indeed! These two groups of scientists are giving detailed consideration to some of the basic problems of extraterrestrial exploration. After all, we know little about the chemical conditions of the surface of the moon exposed as it has been to intense radiation for millions of years, and with all its crevices and valleys gradually filling up, I suppose, with all the star dust encountered in the lunar orbit since time began. This great shining moon of ours is a magnificent object indeed for scientific investigation. How can we guess what the effect would be of an impact from a nuclear explosion or even an unarmed rocket containing a great variety of organic molecules new to the lunar landscape? It is possible that a small organic "seeding" of this sort could have far-reaching or at least detectable effects on the chemistry of the lunar surface. Life as we know it would seem quite impossible on the moon with an atmospheric pressure of 10^{-8} atmospheres in a gas composed of argon, CO_2, SO_2, Xenon and krypton (2) but some other form of non-terrestrial chemistry might be encountered and it is of great scientific importance to know just what the present lunar environment is like before we disturb it by renegade rockets. The gravity on the moon is so small that all but the heaviest molecules will in time attain escape velocity and depart either because of thermal energy or collision with particles from the sun in the so-called solar wind (2). The traces of atmosphere which remain could presumably be significantly contaminated chemically for unknown periods by a nuclear explosion.

On Mars or Venus of course the possibilities for life are far better and the danger of biologically contaminating these planets with spores or viruses from the earth is more realistic. If we are in a position to choose, therefore, it would be better to make as many measurements as possible on any target planet by telemetric techniques before permitting any actual physical contact with a terrestrial object of any sort. When the time is ripe a soft landing on the moon or Mars of an automatic television camera with broadcasting equipment would certainly give TV audiences a big thrill and might well provide dramatic scientific data.

It was the problem of CETEX to map out some preferred order of experimentation in space so that there will be the least possible interference with future observations. The Committee recommended that an attempt be made to sterilize by fumigation any space vehicle which may be launched even if it is only going to the moon, mostly to get the habit started before someone scores an accidental or intentional bull's eye on Venus or Mars. This recommendation was strongly supported last summer by the Bioastronautics Committee of the National Research Council. It has been suggested by Platt (3) that the whole lunar surface may be a mass of free radicals formed by the prolonged and intensive radiation and that in that case "the first man who plants a rubber boot" or other organic structure on the moon might start an explosive or chain reaction or, as he put it, "may be in for an unpleasant surprise." While this seems a most improbable eventuality it nevertheless prompted the reasonable suggestion that as a precautionary measure, before putting a man on the moon in rubber boots, it would be well to fire a salvo of rubber boots at the moon from an orbiting vehicle just to see whether anything serious did occur. In this respect that popular song must have been prophetic which "fired salutes with the captain's boots" in the course of a lunar sail. Joking aside, there is some point to intelligent planning of space research so that things are done in the best possible order.

Little of all this, however, is either physiological or microbiological and there are many questions which might be of interest to those who will look into the future from their desks in this new laboratory. How will man react and survive for extended periods when like Coleridge's Ancient Mariner he is "alone, alone, all all alone, alone in a wide wide" gravity-free empty space? How will he provide himself with food and water and oxygen and dispose of his carbon dioxide and other excreta? Will it be possible for him to balance his microcosm as the macrocosm of the earth is balanced by including a sufficient number of photosynthetic plants? And here indeed is a problem which involves much more than simple engineering principles. Ideally we wish to put into our satellite the proper combination of plants and animals so that we can obtain a perfect balance of the cycles for carbon, oxygen, water, hydrogen, nitrogen and minerals, not to mention calories. The problem is in part how some of the incident energy can be picked up as free energy and put to human use before it is degraded into heat. All water lost from the body must be recovered and used again and the metabolic water must be broken up to provide oxygen for inhalation and hydrogen for synthesis of foodstuffs. Nitrogen excreted in the form of urea or otherwise must be fed to bacteria or algae and rebuilt into protein to be used for human food and the proteins formed must include all those essential for human nutrition. To achieve perfection in the solution of such a complex problem is certainly not easy. It will involve perhaps the development of new species of algae, symbiotic combinations of algae with protozoa or bacteria and perhaps new chemical methods.

If, however, we could learn how to imitate the photosynthetic methods of plants on a commercial scale we might solve this problem of the regenerating satellite chamber in a more sophisticated way and at the same time we might solve the food problems of the world by thus short-circuiting the green plants in a manner as revolutionary of our way of life as would be the commercial development of energy from nuclear fusion. If as much money and effort were put into this problem of photosynthesis as is now going, for example, into the search for a cure for cancer, it might well pay off handsomely in the course of ten years. It does not appear to be an impossible task and, I venture to guess that the solution of this important problem will turn out to be another of the great scientific accomplishments of the twentieth century.

The far-reaching implications for the future of the human race of some of these possible by-products of space research seem to me ample justification for the effort, however great, however crazy indeed it may seem to be. For many very wise people are very scornful of our efforts to conquer space and feel with some justification that these huge sums of money could be better spent in other ways. The realistic, compelling answer to this is of course that a mastery of the technique of rocketry appears to be essential for defense of the free world and we absolutely cannot afford to neglect it. The expenditure may however pay off in other unexpected ways. It has already given us the famous Van Allen radiation belt, or the earth's corona, as the Russians call it. Anyhow it will bring us a more thorough knowledge of the universe in which life has originated and the attainment of such knowledge is certainly one of the tasks of man, the first animal with a brain which appears to be capable of understanding the laws of physics and chemistry which have resulted in the origin of life.

To understand the origin and nature of life and hence to understand ourselves and how we function appears to me to be the greatest and most worthwhile goal which man could find upon this earth. Certainly a lifetime devoted to the promotion of the happiness and well-being of others is a noble enough motive for any individual but human happiness as an objective of the whole human race seems to me selfish and ignoble and unduly limited compared to the search for an intelligent understanding of life. We need not neglect the well-being and happiness of others, however, in our striving for intellectual achievements for this is a necessary means to that end. Meanwhile let us aim high and strive mightily for our supreme objective out in the great blue yonder.

Perhaps the quest for a complete understanding of life, of cells, of man, and of man's brain is by its very nature impossible. Some of you may recall an amusing little book called "Flatland" by Abbott (4) which showed that the brain of a person brought up in 2-dimensional space was quite unable to understand 3-dimensional space. Indeed he described a fanciful case of a 2-dimensional man who somehow escaped into 3-dimensional space and thus was enabled for the first time to visualize a third dimension. When he returned to 2-dimensional space he tried to explain his discoveries to his colleagues; but a scientist must be able to measure what he talks about and so they defied him to measure the height of a man when all he apparently had was width and length. When he failed they put him into prison for life as a subversive character who talked complete nonsense. In the same way it may be impossible for a 3-dimensional brain to understand a process which might function not only in a 4-dimensional space-time continuum but perhaps in 5-dimensional space.

Physiology I like to define as 4-dimensional anatomy, for anatomy deals only with length, breadth and thickness while physiology must consider in addition how these change with time. Physiology deals with rates of change, velocities, accelerations and decelerations, flows and rhythmic cycles, all of which involve time. Perhaps psychology should be defined as a 5-dimensional science. Certainly brain function and psychic phenomena, to say nothing of cellular function and differentiation, have many dimensions which we have not yet begun to really understand. Biology may, therefore, have a fifth dimension which we have not yet penetrated. Considering the Newtons and Einsteins and Pasteurs in our scientific history, however, we need not despair yet and we must proceed on the assumption that the human brain is a kind of machine which may eventually achieve an understanding of the fundamental physico-chemical mechanisms by which it, itself, is able to think. Thinking (according to Readers Digest) a small boy defined as, "when the mouth stops and the head talks to itself". A machine which talks to itself and understands itself, and knows that it understands itself is really something to struggle for--unless it is somehow a contradiction in terms, as in the story of Flatland--unless it is an unattainable fifth dimension.

For these reasons, among others, I am inclined to defend the study of space science if only because it is part of the problem of understanding the nature of life and the nature of our awe-inspiring universe. What kind of life will the laws of physics and chemistry produce in the environment of Mars or Venus? What kind of primitive pre-life molecules may be found on the moon? In the book of space the events leading up to the origin of life may well be found faithfully inscribed. This book of space is now opening to us and its pages are waiting to be read by the intrepid astronauts of the future. If nothing more it will have a good and revealing chapter on the chemistry and perhaps the biology of other satellites in space.

Anyhow, whether we like it or not, physiology or biology is being thrust into orbit because it is being called upon to give advice on biological problems encountered in space travel. Many physiologists are already devoting full time to techniques for putting mice, monkeys or men into space and bringing them back alive. I do not propose to consider this aspect of my subject any further at this time however. Rather I want to speak about physiology in general. To what extent it is really in orbit as a scientific discipline.

If we say that physiology is in orbit of course this might merely mean that it is going in circles with no expenditure of energy and no useful accomplishments. This interpretation, however, I will dismiss summarily at the start. I think we could fairly say that physiology has come of age as a science. Perhaps it is not really an adult science as yet for an adult has been described by Mr. P. Brundage, former Director of the Budget, as one who has stopped growing at both ends but continues to grow in the middle, by deposition of inert material or fatty infiltration. I think physiology is still growing not only in the middle but at both ends also.

Both physiology and bacteriology are sciences in their own right quite independent of medicine although deriving much support from medicine. Both of them share some of the handicaps experienced by biology. And the greatest of these is just public ignorance of biology.

Your taxi driver never heard of physiology and has very erroneous notions of what biologists do beside catching butterflies and bird-watching. The prestige of biology is very low compared to the prestige of physicists who dazzle the public by splitting atoms and shooting off rockets at the moon or creating artificual northern lights for the delectation of the public. The public needs a lot of education before biological research will be appreciated, and before biology will receive the credit it deserves for many advances now commonly attributed to doctors of medicine. To do this we must start in the secondary schools and give the children some reasonable concept of the phenomenon which we call life. They must know something about chromosomes, genes and nucleic acids, about mutations and adaptations, about protein synthesis and photosynthesis until some of them are captivated by the glitter of the opportunities in biology and decide to become serious students of the subject.

Shortage of graduate students is a widespread complaint in science. I do not believe this is because our young people are too lazy to study. Rather it is due to excessive competition from industry and other attractive careers which provide more money with less labor. In Russia, where I believe there is less competition of this sort, there appears to be a greater demand for places as graduate students. From what little I saw in Moscow this is certainly true there, in physiology at least.

There is also competition with biology from the M.D. programs in medical schools and here the contrast is indeed impressive. Throughout the country the preclinical staffs in medical schools are beating the bushes for graduate students while at the same time they serve as members of Admissions Committees screening perhaps over 1000 applications for a class of 70 medical students. Meanwhile in the class rooms there are medical students paying their own living expenses plus $1000 tuition and working at the same table with graduate students receiving $2000, $3000 or even $4000 stipends plus free tuition. Thus there is an annual differential of $5000-$6000 between the two groups. The glamour of medical practice has something to do with this but it is also true that the M.D. has a higher market value. How different the situation is in Russia where a license to practice medicine is cheap and easy compared to a doctorate and the salary is only half as great, or thereabouts. Somehow the Russians have managed to fix their incentives in such a way that advanced study is one of the most sought-after jobs in the country and only the best can qualify for them.

In this country improvement in this direction depends more upon gradual education of the public than upon any expectation that Congress will rearrange the laws and the salaries. To this end it seems to me that our education in biology is particularly defective, perhaps especially in the secondary schools. There are good reasons, however, for expecting some improvements. Many organizations, including the American Institute of Biological Sciences and the American Physiological Society, are deeply interested in the advance of biology and in general it seems to me that biologists are on the move and are no longer content to hide their light under a bushel. We have had our revolution in physics with the splitting of the atom. We are due for one in biology. What the focal point of this turning movement may be I do not know unless it is the splitting of the gene or the synthesis of living matter. Anyhow biologists are no longer chasing butterflies but they are burrowing deeply into the chemistry and physical chemistry of the cell, into the innermost secrets of life itself. The whole scale of biological operations has changed and in another ten years we shall be able to look back upon the last twenty-five years as a veritable revolution.

I conclude therefore that physiology or biology is in orbit in more senses than one. I suppose that no biology laboratory ever started at a time when the future looked quite so fantastically exciting and promising for scientific developments in biology as it does today with the microelectrodes of the neurophysiologists ticking off the tunes in the confused pathways of the central nervous system, with the biochemist all but synthesizing viruses, with the microbiologist creating new species with new adaptive enzymes for new environments, and finally with the astronaut invading the forbidden realms of outer space. I am convinced that the future will see both physicists and chemists turning increasingly toward biology as the field in which revolutionary new discoveries can most easily be made with the many new and potent techniques at their command.

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FIFTH INTERNATIONAL CONGRESS ON NUTRITION

The International Congress on Nutrition will be held in Washington, D.C., September 1-7, 1960. The Congress is being held under the auspices of the International Union of Nutritional Sciences, The American Institute of Nutrition and the U.S. National Committee, International Union of Nutritional Sciences of the National Academy of Sciences--National Research Council. The President of the Congress is C. Glen King and Milton O. Lee is the General Secretary. Information can be obtained by writing to the Office of the General Secretary, 9650 Wisconsin Ave., Washington 14, D.C.

THE 1959 SUMMER WORKSHOP

ROBERT E. SMITH, Director

When in the fall of 1957 Ladd Prosser approached me on the matter of directing the 1959 Workshop for the teaching of undergraduate physiology, I accepted perhaps more from a sense of duty than anticipation of a new and exciting adventure. I had been aware only that the Workshop had become a recurrent summer activity of the Committee on Education of the American Physiological Society under the financial sponsorship of the National Science Foundation. Also I was told that the class would consist of teachers of biology interested in developing more intensively the physiological content within their various biological curricula. Now in retrospect, I shall always regard this Workshop as a truly rewarding personal experience, for which I am most grateful to the many who helped make it possible.

Eventually future became present in February of 1959 when word came that the 1959 Workshop would be held in August at North Texas State College in Denton, Texas. This would be as usual a two-week session and this year would convene applicants and discussants from the colleges in the southwestern United States. All of this appeared quite overwhelming to one unaccustomed to such affairs, especially to one already beset on all sides with an almost fantastic coincidence of inescapable commitments in diverse directions; but so one learns to say, with the good people of Buenos Aires, "yo aprendio algo."

Actually, all that had to be done for the Workshop was to write an introductory letter to our host, Dr. J.K.G. Silvey, the Director of Biological Sciences at N.T.S.C. in Denton and to the President, Dr. J.C. Matthews, to firm up the exact dates (24 August to 4 September) and to send out a few invitations to potential discussion leaders within the geographic area. In the interim, the Director was left free to conjure up a schema of what the program should accomplish. Once having written and discarded this idealized image, he could then return to the practical realities of implementing the teaching of more and better physiology by the overworked and underpaid teachers of biology in the four-year colleges.

Our mission was clear: we were to impart the spirit and the word of Physiology to our disciples. They would be chosen to include from among the applicants especially those teachers most actively engaged in the teaching of physiology within the diverse curricula offered by their schools. Their students would derive from the premedical, predental and the nursing curricula, and still others from the teacher training and physical education programs. A small but important contingent would go on to graduate studies at various universities. Thus with the ample target presented in this broad spectrum of needs and interests, it would be hard to make a total miss and yet quite impossible to achieve the totality of perfection; we could reasonably hope, therefore, only to give something of special value to each with perhaps more of the general to all.

By the time of opening on 24 August, our original group of some 26 participants had been reduced through various personal circumstances to 18 finally attending. These, however, proved to be a splendid group of serious teachers, with diverse training and teaching interests. In common, they had an abiding interest in developing better programs in their physiology courses.

The discussion leaders attended according to plan in overlapping relays, with an average daily number of four present throughout the two-week period.

The daily program was generally divided between lectures in the mornings and appropriate experiments and demonstrations in the afternoons. The evening sessions were convened at 7:00 p.m. and usually concluded by 8:30. These dealt mainly with background or curricular subjects, arising either through popular demand or a demonstrated need.

Realizing a priori that these teachers were in the main seriously limited both in their budgets and logistic facilities, we sought to implement our theory and principles with practical but inexpensive laboratory exercises. For example, we discussed and demonstrated teaching situations pertaining to elementary human physiology (a subject of major importance to many of this group); here we emphasized class studies that could be carried out on human subjects with minimal equipment, e.g. peripheral vascular reflexes and their control, respiratory and cardiovascular responses to work loads, the measurement of acid-base and salt-water balance in man, muscular fatigue and neuromuscular transmission, nerve conduction velocity and the reflex arc. With cold-blooded vertebrates and the smoked kymograph, the classical frog nerve-muscle preparations and turtle heart experiments were demonstrated, as was the pressure-flow relationship in the isolated hindlimbs of the perfused frog preparation (à la Burton). It was surprising to learn how few had previously seen a turlte heart preparation.

Under John Sealander a two-day period at the outset was devoted to ecological factors and their physiological implications in animal behavior. This opened the way for class exercises on the cooling curves of the frog in air of varying temperature and humidity and also that of the turtle submerged at various water temperatures. Concurrently demonstrated were exercises on the turtle heart rate and ciliary movement of frog epithelium as functions of temperature.

Turning to the more general aspects of metabolic activity, Smith developed the basic tenets of Rubner on surface/mass relationships and heat exchange in homeotherms. Also elaborated were the principles of design for class experiments to test the empirical weight-power relationships between body size and metabolism. Lester Matthews created de novo a simple metabolator and successfully tested it on the mouse and guinea pig. It was emphasized that equations of similitude of the above type could be easily applied to various class projects on differential growth (heterogony). These could entail merely the measurement and appropriate plotting of weights or volumes of the part or organ vs. that of the whole body in a graded-size series of a given species.

This section was followed by a two-day sequence under Bernard Abbott and Carl Melton on bioelectric phenomena and the electronic methodology and equipment, including construction by the class of a simple amplifier and a stimulator suitable for such work. A timely introduction to the field was presented on Tuesday night in a lecture on the history of bioelectricity given by R. D. Tschirgi.

On Friday and Monday, Austen Riggs introduced the group to theory and class methods of study on hemoglobin and other respiratory pigments and, in addition, gave a brief introduction to enzymes and intermediary metabolism and of the manner in which these were linked to the synthesis of ATP and other labile potential energy pools through the electron transport system. This material was very stimulating to the group; it likewise complemented nicely the subcellular functional specializations described earlier by Smith in an evening lecture on cellular fine structure. Teaching aids in the latter had included the recent Upjohn monograph on cells and a quantity of electronmicrographs contributed by Lester Matthews from Baylor Dental School and the Wadley Foundation Clinic.

The last two days of the Workshop were largely devoted to concepts and exercises in human respiratory physiology ably presented by Walter Freeman and Douglas Stuart. In an earlier lecture period, Stuart also presented the principles and a class-teaching technique for the cardiovascular responses of man under imposed work loads; in turn, this complemented an excellent evening lecture given the previous week by Abbott on the basic nature of muscular work in man and animals.

Among these evening sessions were two given by Smith, respectively on modern concepts of renal function in the mammal and on techniques and interpretation of salt-water balance, data readily obtainable from class exercises on human subjects. The latter session was supplemented by a demonstration of the use of conductimetric measurements for determining total electrolyte content of urine. Likewise, a Beckman flame photometer was shown to the class and its potential value for class studies in electrolyte balance emphasized.

Earlier during the Workshop Tschirgi offered a discussion on the preparation and teaching applications of slides and related visual aid material in physiological course work. A composite list of teaching films (courtesy of Dr. Sam Tipton) was also distributed to the class; however, no films were actually shown during this Workshop.

The long-awaited series of class exercises in general and comparative physiology arrived early the second week. fresh from Wellesley College, where Louise Wilson and the committee under Sam Tipton had worked long and hard to compile, test and edit this material for general distribution. In many respects, the arrival of these exercises was a highpoint of interest to both leaders and class participants for it greatly amplified the other lecture and laboratory notes also being issued from the Workshop throughout this period.

Among the material available on display was included the excellent collection of physiological monographs sent out, along with copies of the title listings, from Society headquarters by Dr. Daggs. This was supplemented in a small way by a number of recent laboratory syllabi obtained by the Director from various sources. Additionally available were the set of electromicrographs loaned by Dr. Matthews and copies of the Scope monograph, The Cell, as noted earlier.

The one field trip of the Workshop was taken on Saturday, August 29, when the class went into Dallas to visit the Department of Physiology at Southwestern Medical School and the preclinical departments at Baylor Dental School with its adjacent graduate research facilities in the Wadley Foundation Clinic (courtesy respectively of Drs. Carl Melton and Lester Matthews). This provided for most of the class the unaccustomed sight of first-class lecture and laboratory teaching facilities, flanked by such research equipment as tritium counters, an electronmicroscope, and a well-equipped electrophysiological research laboratory.

The present report could not be complete without an expression of appreciation and gratitude from the Workshop participants for the excellent spirit of cooperation and logistic support so generously accorded them by the people of North Texas State College. Particular acknowledgment must be given to President J. C. Matthews, Dr. J. K. G. Silvey and Dr. and Mrs. James R. Lott, and finally to those culinary artists of the house staff who daily offered the most irresistible of epicurean delights.

In summary, the 1959 Workshop of the American Physiological Society at North Texas State College was evidently a most gratifying educational and personal experience to both class members and discussion leaders. The keynote of the program was to furnish material of direct application to the teaching of undergraduate physiology. This we feel was accomplished both in letter and spirit and should bring forth fruit.

In closing, we of this Workshop are pleased to offer our encouragement and bon voyage to Dr. Guyselman for his 1960 Workshop at Carleton College, Northfield, Minnesota.

Participants

Sister Joseph Marie Armer, Incarnate Word College, San Antonio, Texas
Raymond O. Berry, Texas A & M College, College Station, Texas
Monroe David Bryant, Austin College, Sherman, Texas
Sister Mary Lucy Corcoran, Incarnate Word College, San Antonio, Texas
G. Ethel Derrick, Central State College, Edmond, Oklahoma
Eleanor Duke, Texas Western College, El Paso, Texas
Sidney W. Edwards, Southwest Texas State College, San Marcos, Texas
William T. Fitzgerald, Lamar State College of Technology, Beaumont, Texas
Eb C. Girvin, Southwestern University, Georgetown, Texas
August W. Jaussi, Oklahoma State University, Stillwater, Oklahoma

Donald D. Jewel, Southeast Missouri State College, Cape Girardeau, Missouri
John F. Locke, Sr., Mississippi State College, State College, Mississippi
Richard Dana Moore, Hardin-Simmons University, Abilene, Texas
Edith M. Selberg, Colorado State College, Greeley, Colorado
George Allen Vaughan, Central College, Fayette, Missouri
Sister M. Walburga, College of St. Teresa, Kansas City, Missouri
William Richard Widner, Howard Payne College, Brownwood, Texas
Christopher L. York, Mary Hardin-Baylor College, Belton, Texas

Discussion Leaders

Bernard Abbott, Zoology, UCLA, Los Angeles, California
Walter J. Freeman, Physiology, University of California, Berkeley, California
James R. Lott, Biology, North Texas State College, Denton, Texas
Lester Matthews, Physiology, Baylor School of Dentistry, Dallas, Texas
C. E. Melton, Physiology, Southwestern Medical School, Dallas, Texas
Austen Riggs, Zoology, University of Texas, Austin, Texas
Paul Saltman, Biochemistry, University of Southern California, Los Angeles, California
John A. Sealander, Zoology, University of Arkansas, Fayetteville, Arkansas
Robert E. Smith (Director), Physiology, UCLA School of Medicine, Los Angeles, California
Douglas Stuart, Physiology, UCLA School of Medicine, Los Angeles, California
R. D. Tschirgi, Physiology, UCLA School of Medicine, Los Angeles, California

FIFTY-YEAR MEMBERS

Amberg, S., M.D.	03 R	Loeb, L., M.D.	07 R
Brooks, C., Ph.D.	10 R	Marine, D., M.D.	10 R
Brown, E.D., M.D.	07 R	McClendon, J.F., Ph.D.	10 R
Dawson, P.M., M.D.	00 R	McGuigan, H.A., Ph.D.	07 R
Erlanger, J., M.D., Sc.D.	01 R	Meek, W.J., Ph.D.	08 R
Eyster, J.A.E., M.D.	06 R	Miller, F.R., M.D.	08 R
Fischer, M.H., M.D.	01	Murlin, J.R., Ph.D.	06 R
Forbes, A., M.D.	10	Opie, E.L., M.D., Sc.D.	06 R
Guthrie, C.C., M.D.	05 R	Osterhout, W.J.V., Ph.D.	10
Hale, W., M.D.	08 R	Richards, A.N., Ph.D., Sc.D.	00 R
Harrison, R.G., M.D., Ph.D.	06 R	Snyder, C.D., Ph.D.	07 R
Hawk, P.B., Ph.D.	03 R	Sollmann, T., M.D.	02 R
Herrick, C.J., Ph.D.	07 R	Voegtlin, C., Ph.D.	08 R
Jackson, D.E., Ph.D.	10 R	Wiggers, C.J., M.D., Sc.D.	07 R

RESEARCH TRAINING AND THE NATIONAL INSTITUTES OF HEALTH[1]

FREDERICK L. STONE

National Institutes of Health

It is a pleasure and an honor to participate in this symposium on recruitment. I know of few subjects of more fundamental importance to the growth of science than the continuing addition of new minds and fresh ideas. I am purposely emphasizing ideas because science's real need today, as always, is primarily for minds and secondarily for hands.

The Nation's health, as well as its economic and social well-being, depends to a striking degree for its continued progress upon a small group of scientists and other career workers in the various scientific and professional fields composing the health sciences. The self-evident national need and the tense world situation give our resources of scientific and professional manpower a position of crucial importance. In maintaining the Nation's health and security, and in strengthening our scientific potential, many different kinds of scientists will play key roles.

There is no magic formula for the relief of scientific manpower shortages; each shortage requires separate analysis in order to determine how it can best be alleviated. Imbalances between supply and demand result from various causes and call for different kinds of corrective action; in addition, shortages of scientific manpower always have qualitative as well as quantitative aspects. It must be borne in mind that two second-rate scientists are not a substitute for one first-rate scientist. The present shortages of scientific manpower are the inevitable reflections of scientific discoveries and technological change. For example, recent advances in fundamental genetics have resulted in an acute shortage of these scientists and have increased the awareness of medical schools that medical genetics must be added to the curriculum.

As a further complicating factor, young men and women cannot be specifically trained now for specialized fields of research which may open in the future, say in ten years from now. The scope and quality of their training and research experience must be such as to prepare them to convert their skills to work on problems in new fields. Only through the provision of such convertibility can we provide a potential reserve to be drawn upon in times of need.

In this day, when a branch of science or technology can become antiquated overnight, it is hazardous to try to predict a favorable future

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[1] Presented in a symposium on "Recruitment of Graduate Students" at the Federation of American Societies for Experimental Biology meeting, Atlantic City, April 15, 1959.

for any narrow scientific category. There will be scientific advantage, therefore, only to those individuals who acquire the kind of fundamental training that will enable them to move back and forth over several scientific disciplines. Individuals so trained will be able to make significant contributions under all circumstances; individuals more narrowly trained will be more or less at the mercy of circumstances.

As an indication of an imbalance between past training and present activities, the data revealed in Mirror to Physiology, A Self-Survey of Physiological Science[2] (pp. 188 and 189) are most revealing. Nearly 50% of the 362 professional animal physiologists who were polled reported that further training in physical sciences and mathematics would be helpful. Because of the current applicability of chemistry, physics and mathematics to many biological studies, it seems safe to make the projection that they will continue to apply in the future, no matter what areas arise in importance, and that current training plans ought to be suitably arranged. Although legitimate questions might be raised as to the possibility of producing a competent physical scientist and a competent physiologist within the same individual, far less objection and doubt can be leveled at providing opportunities for each to be able to converse with and understand each other. The manifold advantages of team research are called to mind.

There is no easy way to ensure that we will have adequate resources of scientific manpower to meet our various needs. Nothing short of a determined, long-term cooperative effort involving Government and our educational institutions will fulfill this goal. In the basic health sciences the achievement of this goal will involve four broad and related objectives: 1) to strengthen our existing institutions which educate and train our scientists; 2) to aid in the support of a continuous large flow of competent, highly motivated graduate students and fellows through the medical and graduate schools of our universities; 3) to expand the opportunities for intensive training of predoctoral and postdoctoral candidates; 4) to encourage and support the utilization of research-trained manpower--specifically, to improve the teaching and training functions of our universities.

No satisfactory and precise study of the needs for research-trained manpower in the basic biological and medical fields related to the health sciences has ever been made. It is impossible, therefore, to provide precise forecasts; however, a series of informed estimates are available. A study has been made by the Secretary's Consultants on Medical Research and Education and published in the report of this group entitled "The Advancement of Medical Research and Education."[3] This document, now popularly called the "Bayne-Jones Report," dealing with this problem and the estimate of need for 1970 is quoted below:

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[2]Gerard, R. W. Mirror to Physiology, A Self-Survey of Physiological Science. Washington: American Physiological Society, 1958.

[3]The Advancement of Medical Research and Education, Final Report of the Secretary's Consultants on Medical Research and Education. Office of the Secretary, Department of Health, Education and Welfare, Washington, D. C., June 27, 1958.

The estimated net addition of 3,200 M.D.'s in research and 16,000 doctorates in the sciences related to medicine would fall about 6,000 short of the 25,000 new medical researchers required by 1970 for the projected medical research effort. There will also be shortages of technicians, nurses, and other ancillary personnel. A portion of the deficit could be avoided, perhaps, by more effective utilization of existing personnel and facilities. Nevertheless, it would appear that an additional 6,000 M.D.'s and/or Ph.D.'s beyond the supply now projected will be needed if the Nation's medical research program approximates the 1970 estimates.

This passage indicates the national needs for scientific manpower as viewed by a distinguished group of medical educators and scientists. A portion of their further discussion and specific recommendations is quoted.

TRAINING GRANTS

The objectives of the training grants administered by the National Institutes of Health are to help produce the kinds and numbers of people required for an expanded medical research program, and more well-trained people in a number of professional fields to meet service needs in important disease categories. These objectives have been well served by the training programs, which, like the research grants programs, have evolved to meet changing needs.

1. General Research Training Grants

Until recently, all training grants provided by the National Institutes of Health were for training related to a disease. However, a general research training program has been inaugurated under which individuals may be trained for sciences basic to medicine without reference to the application of the training to problems of disease. This is a sound development, because provision of large sums to educational institutions solely for disease-oriented training is not the procedure best calculated to produce superior investigators.

In Grants for Experimental Training Programs, the Consultants recommended that:

Recruitment of people for medical research will be largely futile if satisfying careers in research are not available for those who are trained and drawn into research.

In the Senior Research Fellowship Program, the Consultants recommended that:

The Consultants warn emphatically that senior faculty positions with tenure will not be available for most of those who complete training under the senior research fellowship and similar programs, unless the schools secure substantially increased general income, or other means are found to support permanent faculty positions.

In the face of these serious recommendations which apply with equal strength to nearly every basic medical science, I would now like to turn to the specific subject of this discussion, that is, the recruitment and manpower problem facing the field of physiology as I see it.

The national manpower need in the area of physiology can be described in two words: 1) quantity and 2) quality. It is common knowledge that medical schools, graduate schools and research institutes are finding it difficult or impossible to secure well-trained physiologists in several of the physiological subspecialties, e.g. neurophysiology, cardiovascular physiology etc. However, quality is the major issue. The problem here is not that the candidates for training are of low native ability. It is that the intellectual demands placed upon physiologists are becoming greater each year. While physiology is a basic medical science, it is not a basic science in the pure sense but derives from a large number of basic university disciplines. Much of the difficulty in training physiologists is that there is no clear, single road leading to graduate work in mammalian and human physiology. Few students know as freshmen in college that they want to become physiologists, and lay the proper foundation for it. Rather they drift into physiology late in college or after four years of medicine and find themselves in a situation for which they are poorly-prepared.[4] Students and departments therefore tend to settle for something less than the ideal training for advancing physiology in future years and maintaining its stature as a subject vis-a-vis, for example, biochemistry, which does not have these problems.

As a consequence of drawing students from such diverse sciences as psychology on the one hand and physics on the other, a certain specialization within the scope of a single degree is necessary if the student is to make maximal use of his previous specialized training. At the same time, a student must be grounded in all phases of physiology if ultimately he is to be at home in a medical school department of physiology and eligible for advancement up the academic ladder, including becoming head of a physiology department. Accordingly, if this premise is allowed, the art of graduate education lies in balancing two conflicting desiderata--specialization and breadth. The material sides of this art are the fellowship mechanisms which enable the student to devote full time to his training, the necessarily prolonged predoctoral program, and considering predoctoral and postdoctoral training as a unit in planning. Another purpose to be served by such planning is to produce Ph.D.'s who are capable of continuous self-education and self-improvement--a necessity to meet the changing complexion of the subject.

In addition, although there are obvious remedies, economic factors may operate to hinder the period of postgraduate training which is of increasing importance today in providing a young graduate physiologist with the research and teaching experience on which a future academic career must be based. Concern has been expressed over the possible effects of an early academic professional appointment which may, be-

[4]Editorial note: The Teaching Session at the Chicago Federation Meetings will deal with the subject of standards for graduate training in physiology.

cause of its demands, seriously decelerate the rate of professional development initiated by graduate studies.

An informal survey of graduate training in physiology shows that although there are departments which have graduated large numbers of students, it is the departments that graduate one or two students a year which have occupied a more dominant position in the sense of placing their candidates in commanding academic positions. One responsible factor is that the latter departments have trained students to become independent researchers and thinkers, whereas the larger departments have made graduate training a matter of amassing the requisite number of credits, usually in ordinary medical school courses, and the performance of "production line" research. In the smaller departments the philosophy that a relatively few students of high caliber can be stimulated to self-development by close association with the faculty and a minimum of didactic work has been emphasized with success for years. Graduate training beyond medical courses in physiology should be accomplished by a technique devised to reconcile the conflicting demands of intensive and extensive familiarity with physiological literature and modern techniques and to train the candidate for scientific independence. Such methods may make graduate training a matter of "education" rather than "instruction."

This goal is obviously difficult to achieve under the present system of training. Non-didactic techniques and self-education are notoriously slow ones, and the dividends are remote and the demands on the faculty time are particularly great. More time and more financial support is therefore needed. Can a fellow--pre- or postdoctoral--develop independent research ability if his research is tied to a faculty member's research grant? Can he always choose the field of greatest interest to him or is his choice dictated by the kinds of grant support available to him? These become questions of fundamental importance.

A final point in this philosophy is the belief that a student should make a substantial contribution based upon an advanced research technique. A good student should do better than his supervisor and should germinate new and independent ideas and techniques even though they cannot always be embodied in the thesis. From a practical point of view, a sure-fire, pot-boiler thesis, though it brings the Ph.D. quickly, may be harmful to a man's career. Before he can attain a national reputation he may take a position, develop a research laboratory while meeting the traumatic demands of his first teaching position, execute a significant piece of research, and thereafter wait as long as a year for his paper to appear. It may require four to five years to do what could be done by devoting adequate time for thesis research.

May I recapitulate here the several points of my discussion which have been mentioned in passing and which I know are difficult to put into practice under the present circumstances. These are:

1. The need to draw into physiology students with a firm undergraduate or graduate training in the university subjects

cognate to physiology, especially physics, mathematics, physical chemistry and psychology.

2. The need to maintain avenues into physiology at various times in the academic career and to be prepared to remedy defects in the basic training of the individual while in graduate school.

3. The need for intensive work in a restricted phase of physiology together with a broad training in all aspects of physiology. The latter could gainfully include familiarity with pathological physiology and biophysics.

4. The need to place considerable responsibility on the student to develop himself--to learn the techniques of learning--rather than succumb to the temptation of didactic teaching in an effort to meet the multiple demands of both specialized and comprehensive training.

5. The need to make thesis research capitalize on the inventiveness and enthusiasm for new ideas, which is a property of the young intellect, and make the thesis both the basis for national recognition and ground work for future development of new approaches.

6. The need to develop graduate and postgraduate training in which research is problem-centered, rather than technique-oriented.

Twenty years ago, it has been said, the Ph.D. physiologist tended to acquire a technique and an apparatus and then to look for problems for which the experimenter, the technique and the apparatus were suited. Sometimes they clung to these methods through inability to change long after the methods were out-dated and sterile. Is not the proper way to start with questions and then develop the apparatus and techniques to study the problem? Obviously, if a man is problem-centered he is not a gadgeteer. Physiologists, to be respected, should continue to contribute not only ideas but apparatus and methods. Operating under this philosophy many progressive faculty members have developed new apparatus and new techniques, or adapted and continuously improved classical ones. Some examples are: single cortical unit recording, digital computation of latencies and intervals, ultramicroelectrode intracellular recording, pyramidal tract recording, cinefluorography, cardiac diameter gauges and other methods of recording from intact unanesthetized animals, multiple-channel electrocardiography, rapid infrared CO_2 analysis, gradient calorimetry, servosystem analysis of stretch reflex and respiration, etc.

In summary, I have attempted to make suggestions relating to the field of physiology which might provide training programs established in depth as well as breadth, and to bring to bear upon physiology a variety of university disciplines. This, I feel certain, will open to many departments new sources of graduate students and postdoctoral fellows having exceptional intellectual ability and backgrounds useful to physiology.

It has been a pleasure to discuss this important topic and I trust the fact that I have dealt with the problem at an abstract level, and have not belabored the technique of "grantsmanship", will not be considered an egregious oversight. I feel the importance of the question of recruitment in this field far transcends a technical discussion of the various supporting programs available. If necessary, these can be discussed at another time and place.

PAPERS FROM BLOOD FLOWMETERS SYMPOSIUM NOW AVAILABLE

The Institute of Radio Engineers on Medical Electronics has devoted its December 1959 issue exclusively to methods for the measurement of the flow of blood. This issue encompasses the papers which were presented at the Blood Flowmeters Symposium at the University of Nebraksa College of Medicine in Omaha on June 17-19, 1959. The Symposium was sponsored by the Omaha-Lincoln Chapter of the Professional Group on Medical Electronics of the Institute of Radio Engineers, the Medical Extension of the University of Nebraska College of Medicine and the University Research Council.

The issue includes additional papers from investigators who are employing methods for measuring blood flow which were not represented at the Symposium. The issue contains a total of 26 papers and discussions.

Copies of this publication may be procured from The Institute of Radio Engineers, Inc., 1 East 79th Street, New York 21, New York, at $7.20 per copy.

INTERNATIONAL CONGRESS OF GASTROENTEROLOGY

The International Congress of Gastroenterology will be held in Leyden and Noordwÿk aan Zee, the Netherlands, April 20 to 24, 1960. The Secretary General is Dr. C. Schreuder, 16 Lange Voorhout, The Hague, the Netherlands.

BENNET DOWLER

The Forgotten Physiologist

H. S. MAYERSON

In 1938 Dr. Walter J. Meek (1) published a delightful paper entitled, "Bennett Dowler, a forgotten physiologist,"[1] in which he wrote, "It would seem that a physiologist who had written thirty volumes of manuscript on his science and who had actually seen eleven hundred pages published, should have some claim to permanent remembrance among his kind. And yet this voluminous writer is now almost unknown. His pamphlets are among the rarities of the Surgeon General's Library, far too valuable to be sent out by express mail, but only the most meager details of his personal life can be unearthed. Even New Orleans, for many years his home, no longer seems to remember him, and the biographical cyclopedias do not agree on the date of his death."[2] After reading Dr. Meek's paper, I proceeded to make inquiries and realized that Dr. Dowler was indeed unknown in New Orleans. As a physiologist working in New Orleans it seemed incumbent on me to learn more about this ingenious, fearless and controversy-loving investigator.

Bennet Dowler was born in Elizabeth, Ohio County, Virginia, on April 16, 1797. He was the son of Edward Dowler and Elizabeth Riggs. Nothing is known of his early years except that he was educated in Virginia and Pennsylvania, and was graduated from the University of Maryland in 1827. Immediately, or soon after, he set up a practice in Clarksburg, West Virginia, and also served as postmaster. While in Clarksburg he published his first article on the use of calomel in the treatment of cholera (2). During these early years, Dowler formed the habit of meticulously recording details of all of his clinical cases, scientific experiments and ideas in folios. These later served as a valuable storehouse of material to be drawn on as the occasion demanded.

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[1]Dr. Meek's spelling of Dr. Dowler's first name was incorrect. All references to him that I have been able to find indicate that his first name was spelled "Bennet" rather than "Bennett."

[2]Dr. Meek was uncertain as to the date of Dr. Dowler's death. It is incorrectly given as 1866 by the Biographisches Lexicon and the index of the Surgeon General's Library. The date 1879 is also incorrect as given by Kelly and Burrage in their American Medical Bibliographies. The correct date is indicated by the death notice published in the Daily Picayune on November 12, 1878:

> Died
>
> Dowler - On Monday, November 11, 1878, Dr. Bennet Dowler, aged 83 years, a native of Virginia and a resident of New Orleans for the last 40 years. Friends and acquaintances of the family and of Henry Voss, also the medical faculty, are respectfully invited to attend his funeral from his late residence, 96 Constance Street, this evening at 4 o'clock.

We do not know why Dowler left Clarksburg for New Orleans in 1836. He may have been interested in studying yellow fever and cholera, a few cases of which he had treated, or perhaps the founding of the medical school in New Orleans attracted him. In all events, he soon became absorbed by the variety of disease which he saw in New Orleans and began a career of tireless investigation and intensive practice until his death in the yellow fever epidemic of 1878.

His first publications after coming to New Orleans appeared in 1841 (3,4) and were entitled "Coup de Soleil, or Stroke of the Sun" and "Observations on Solar Asphyxia, Coup de Soleil or Sunstroke." He distinguished "solar asphyxia" from "solar exhaustion," He characterized the former as "probably a universal lesion of the nervous system" in which "the skin is extremely hot and generally dry; there is a choking sensation and a total loss of sense." In solar exhaustion or syncope "the skin is moist, pale and cool; the breathing easy, though hurried; the pulse is small and soft; the vital forces fall into a temporary collapse, the senses remaining entire." Dowler was one of the first investigators to understand the value of acclimatization to heat and repeatedly stressed its importance. To him "an acclimated horse or mule is worth much more than one not so protected." He also perceived that actual heating of the body resulted when heat loss was inadequate. In these early publications, he records that he has not had a chance to measure temperature after death but feels it is very high for hours after death.

This early interest in temperature was maintained for the rest of his scientific career.[3] He soon collected sufficient data to validate his impression of a post-mortem rise in body temperature.[4] With the help of a young German working in a local pharmacy, he made his own mercury thermometers (5) with a range of from 70^{o} to 120^{o} F which could be read with an accuracy of $1/4^{o}$. Measurements on hundreds of patients dying from yellow fever, cholera, sunstroke etc., soon confirmed his belief that body temperature after death was higher than any maximum level during the disease. As a physiologist, however, he realized that the disease itself disturbed temperature regulation and that, to be valid, his conclusions must be based on healthy individuals. With characteristic ingenuity, he obtained permission to attend public hangings. We can conjure up a vivid (and unusual) scene: Dowler inserting his thermometers in various locations of the unfortunate victim just before the noose was sprung and continuing his observations for some time after. Dowler's obsession with the importance of the post-mortem rise in body temperature led him to propose the ther-

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[3] In volume 12 of the New Orleans Medical and Surgical Journal for 1855, Dowler has 7 articles on "Experimental Researches into Animal Heat in the Living and in the Dead Body" for a total of 102 pages.

[4] Although Dowler may not have been the first to describe the phenomenon of post-mortem rise in temperature, he accumulated more data than any other investigator. Pembrey writing the chapter on "Animal Heat" in Shäfer's Textbook of Physiology published in 1898 gives a table of observations by various authors. He does not include Dowler's data although with the exception of 2 observations made by Davy, all of the data is for work done later than 1855.

mometer as a means of testing death (6), "possessing, as it does, superior certainty over that of the stethoscope...while the ausculatory test takes for granted that there can be no temporal inaction of the heart, and that all its motions can be heard, the thermometrical test takes nothing for granted without the most indubitable proof. Its great axiom is that man, in his living state, maintains a uniform temperature, independent of the surrounding media; while a dead man, like other inert matter, has no independence of this kind, but steadily responds to, and is governed by, calorific conditions altogether physical - heating and being heated, receiving and radiating caloric. This is not the result of speculation, but of prolonged and varied experimental research."

Dowler understood that the temperature of different parts of the body was not the same (7) and reported that cold water applied to the forehead resulted in a decrease in temperature of the hand and cooling of the foot decreased the hand temperature. Simultaneous measurements of hand and urine temperatures showed that the temperature of the hand was 1.13^{o}F higher. He had read and studied Beaumont's experiments on digestion (8) and reports measurements of temperatures in the stomach under various circumstances.[5]

Dowler's interest in temperature was catholic. He measured temperatures of fishes (9) and of water in Lake Pontchartrain, and is probably the first and only person to privide a detailed temperature log of the Mississippi River made when he proceeded from New Orleans to Cincinnati by river boat to attend the organization meeting of the American Medical Association in 1850. Dowler was chairman of the committee on medical sciences of the American Medical Association and his report of 1851 drew the following comment in the American Journal of Medical Sciences (10): "This report, although written in the peculiar and, as we conceive, vicious style of its author--abounding in new-coined words and novel phrases, alien to a pure English diction, is nevertheless a fair exposition of the more important facts in anatomy, physiology, medical chemistry, etc.... The report of Dr. Dowler will be read with interest. The author has succeeded in throwing a form of originality over materials that had already been in possession of the reading portion, at least, of the profession."

Soon after coming to New Orleans, Dowler became interested in the alligator and its use as an experimental animal. In 1946, he published the first of many papers entitled, "Contributions to the Natural History of the Alligator" (11) in which he says, "In this paper, it is not to give the anatomy, physiology and habits of the alligator in systematic detail, but to point out some important facts in its history,

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[5]Dowler was very much impressed with Beaumont's experiments on St. Martin for the New Orleans Medical and Surgical Journal 17:63, 1860, he writes in a footnote to the descriptions of Beaumont's experiments.

"Born 1796; spent $3,500 in his experiments; applied to Congress for a reimbursement of his expenses, in thus promoting scientific discovery; got nothing; and died at St. Louis in 1953, deserving a national monument to his memory."

freed from the trammels of artificial classification, and to correct certain errors, which, for several thousand years, have been accumulating, until the herpetological account of this saurian has, at length, become as fabulous as that of the Griffon itself."

"Men who have but one idea - be that calomel, quinine, or venesection, and who, under the pretense of being practical, reject every other inquiry as 'Stale, flat and unprofitable' will no doubt, think that crocodelian investigations are unworthy of their attention. It were easy to show that comparative anatomy, physiology, and pathology, afford an inexhaustible mine of useful knowledge, especially to the practical physician." To further justify his use of the alligator as an experimental animal he argues: "For the anatomical rather than for physiological reasons, my vivisections have been confined chiefly to the alligator, an animal whose anatomy, physiology and psychology place it above frogs, turtles and salamanders which have been generally relied on by experimenters. How unlike soever the alligator is the man, these later are more so. If frogs are good, alligators are better."

As Dr. Meek pointed out, Dowler thought, for a time, that he was the first person to use alligators but later it appeared "that LeConte in Georgia had also used alligators and actually had priority of twenty days. Dowler's work was, however, much better known." Certainly his experiments, or "demonstrations," attracted considerable local interest. On the morning of a planned experiment, Dowler and his medical friends would assemble at the French Market, pick out a suitable animal, and proceed to the courtyard being used as an amphitheater, the group being augmented by curious "young bloods" who found Dowler's demonstrations excellent entertainment. This they must have been, with Dr. Dowler whacking away with cane or hatchet, or applying lighted matches at various points, for he had no use for galvanic or other types of "fancy" stimulation.

Dr. Meek detailed Dowler's extensive work on the nervous system of the alligator, explaining that Dowler was unquestionably the first American worker to study compound, coordinated reflexes in a spinal-cord-controlled animal and that no one else in the whole world had yet noted or written of irradiation in the spinal cord, pleurisegmental innervation or inhibition of the reciprocal muscles. Dr. Meek also points out the surprising fact that although Dowler saw all of these things, he vehemently argued against the principle of reflex action. "But nature denied his conclusions before his very eyes. Irritation to the front legs did influence the hind ones. Instead of denials a truly great mind would have sought for harmonizing explanations. That there was more concerned than a simple reflex he abundantly proved. If he had only imagined a connection from arcs at one level to those at another level, all his observations would have fallen into order and he would have made for himself at one stroke an enduring name."

Dr. Meek discussed Dowler's great interest in the post-mortem contraction of muscle and the fact that "in the bayous of Louisiana was promulgated the doctrine of the independent irritability of muscle." Dowler also found that blood flowed from opened veins of persons recently dead from yellow fever and proved to his own satisfaction that there was a post-mortem capillary circulation.

From 1854 to 1861, Dowler was editor of the New Orleans Medical and Surgical Journal. He used the Journal not only for publication of his own scientific articles but for discussion and criticism of prevailing concepts and as a forum for his own philosophy. He was irate at Montgomery Medical Society for excluding women for "there is no sex in science.... The question is one of competence not gender." He expounded his views on the anesthetic effects of cold and of CO_2 and covered an unbelievable range of topics. The early numbers of the Journal carried repeated exhortations to readers - first for payment of subscriptions and then for articles for publication. The latter plea was obviously not too successful and Dowler was frequently forced to draw on data from his voluminous folios to meet the printer's deadline. In the 1854 volume of the Journal, there are 12 articles written by Dowler, beginning with a discussion of cataracts in the aged and including "Researches upon the anatomy, physiology, natural history and care of the tape-worm or Tenia Solium."

Dowler's interest in comparative physiology also led him to write an exhaustive account of the natural history of the mosquito (13). He discusses, among other things, its nervous system, muscular force, longevity, reproductive power and partiality to foreign blood. In regard to the last point, Dowler was more patriotic than any native. Not only did mosquitoes shun the native Creole, but the latter was more resistant to yellow fever than the "foreigner." To prove this point, Dowler made a detailed tabulation of the longevity in New Orleans with respect to birth place using for his source inscriptions on tombstones in the various cemeteries.

Dowler wrote incessantly on yellow fever, railing at the common belief that it was due to poor sanitary conditions or that quarantine was an effective means of controlling the spread of the disease. In 1859, in "Remarks on the Proceedings of the late Quarantine Convention held in New York" (14) he wrote, "It is reasonable to assume that the fomites of yellow fever, when discovered will not prove to be either ordinary filth or simply impure air, but some specific, peculiar local agent, which produces a specific or peculiar disease - not any disease indifferently, as cholera, intermittent rheumatism, small pox, typhus, etc." Here again Dowler narrowly missed his niche in the Hall of Fame by failing to make the connection between the "peculiar local agent" which he implicated and the mosquito about which he wrote so extensively.

The great crusade of Dowler's scientific life was his battle against the concept of central nervous system domination which was beginning to evolve as a result of the work of Bell, Magendie and Marshall Hall. Meek commented in this regard, "Dowler really understood the situation with considerable clearness. For ten years or more he opposed the growing doctrines on the nervous system in a great battle of his own, using both philosophy and experiment as his weapons." Dowler also used his position as editor to good advantage. In the first number of the New Orleans Medical and Surgical Journal under his editorship, Dowler prints a short account of the visit of Marshall Hall to New Orleans in 1854. Hall gave two lectures and "also performed some experiments on the alligator, illustrative of his peculiar views on the physiology of the nervous system, which appeared to be conclusive and satisfactory to most of those who witnessed them, but not to everyone. The animals were rather small, besides being somewhat torpid at this season of the year. We have the full reports of Dr. Hall's two lectures which we...are compelled to reserve...for want of space..." The lectures were never published.

Dr. Meek discussed Dowler's continued complaint of not being properly recognized and can find no reason for it. Certainly Dowler did not "hide his light under a bushel." He sent his reprints regularly to the Boston Medical and Surgical Journal, whose editor admired Dowler and wrote at least one editorial each year between 1845 and 1856 on Dowler's work. In fact the editor repeatedly urged Dowler to expand his articles into monographs. Dowler obviously also felt that he had been neglected in Europe for in the New Orleans Medical and Surgical Journal in 1858 (15) we find the following letter from Brown-Sequard dated, Paris, December 12, 1857:

> My Dear Sir; I take the liberty to say, that you are greatly mistaken if you believe that your name and your discoveries are known in Europe; and that you must accuse yourself of the ignorance of the learned (so called) men here, concerning what you have done. This is not because you live at one of the parts of the circumference of the enlightened circle of this world that you are unknown. It is because you have not employed the proper means to be known. You have sent your printed papers to the "Academie des Sciences," or the "Academie de Medecine" or to illustrious men. If you needed a tomb for your papers you could not do anything better than to send them to such "unreading" men, or "locked up" libraries. The only chance you may have had to have your papers read by some active, young and intelligent man is that the "illustrious" men may have sold them at two cents a pound, in "physical weight" to a grocer, and that, in buying coffee or sugar, an intelligent young man may have found your papers and secured them at four or five cents a pound. But this is a very narrow chance, and there is no wonder that nobody knows you here.
>
> Why did you not send your papers to the men who have time to read - to the fifty young physiologists of France and Germany? Don't you know their names? Don't you know that they write more and lecture more than the illustrious men?
>
> I have been fifteen years engaged in physiological researches. If you are "au courrant" of what is done in the centre of the circle I spoke of a moment ago, you must know that I was engaged more than eleven years ago in experiments on subjects on which you yourself have been at work, and you have never sent me, or at least I have never received from you, one single bit of your printed papers: and here, among the most active young men, I do not know one who is not like me. I may name Rouget, Colin, Verneuil, Vulpian, Broca, Bouley, Gobaux, Faivre, Grubler, Davaine, etc. I might fill the page with names.

There are active young societies in Paris, and above all, the "Societe de Biologie," where your name has never been heard, except pronounced by me. Have you sent a single paper to this society? Your name is not on the catalogue of their library. Accuse yourself, therefore, if you shine only in the United States, and even there in "our common" great "country" I have vainly tried to procure most of your publications. I have succeeded in procuring only three of your papers, and they are not the most important.

Now, my dear sir, I want you to do what would be the most useful to your reputation, and, therefore, I hope you will not blame me for this letter.

Believe me, dear sir,
Respectfully yours,

E. Brown-Sequard

It is pertinent to add that Dowler reprinted and enlarged on an abstract from the October, 1858 number of the American Journal of Medical Sciences which contained a notice that Brown-Sequard had acknowledged Dowler's discovery of post-mortem rigidity.

Dr. Meek mentioned Dowler's profound distrust of the professional physiologist who was then appearing on the scene. Dowler frequently took occasion to deride experiments made "at the expense of the State and by salaried professors" which, he says, in the estimation of certain persons derived "so great a dignity from the state ceremonial, as to compensate for their otherwise worthless character--experiments as remote as possible from man and his healthy and morbid conditions." Dowler refused to accept appointment to the medical school faculty in New Orleans and although there is record of his appointment as Chairman of Physiology and Pathological Anatomy in Memphis Medical College in 1851, he evidently refused this appointment also, for there is no record of his having left New Orleans.

Dowler became a corresponding member of the Academy of Natural Sciences of Philadelphia in 1848 and for the succeeding five years agitated for a similar institution in New Orleans. His efforts resulted in the organization of the New Orleans Academy of Sciences in 1853.

Dr. Meek found no articles by Dr. Dowler dated later than 1856. Actually, however, Dr. Dowler was active for at least ten years after this and I have found references to 103 articles (including book reviews and case reports) during this period on fantastically varied topics. The last articles appeared in the short-lived New Orleans Medical Record in 1866.[6] The first is a long polemic on Bell's theory

[6]The New Orleans Medical Record was started in 1866 with Dowler as editor and Dr. S. R. Chambers as co-editor and proprietor. The latter was a visiting physician at Charity Hospital and F. M. Mumford, a medical student at Charity Hospital, was listed as co-owner. Only four numbers of the journal ever appeared.

and the more recent exposition by Dalton on different kinds of nerves, and it also includes a discussion of consciousness. That Dowler had not lost any of his fire is shown by the "punch line" of this article: "If there are nerves for tickling or itching, the part tickled or scratched, not the brain, will get the credit or the blame." The second article is a discussion of a report on confinements by LeFort.

Bennet Dowler should be remembered not only as a pioneer physiologist and investigator but as a leader of a distinguished group of clinicians, an expert on yellow fever, and an outstanding, influential member of his community. He led an exciting, turbulent life and at the time of his death was one of the best-known citizens of New Orleans. A curious, imaginative investigator, he resembled Magendie in breadth of interest but, unfortunately lacked Magendie's humility and was too absorbed in his own importance to be influenced by current work or thought. As Meek concluded "...the story is more than one of distorted reasoning. It is a story of great effort and of real accomplishment on the one side and an almost complete negation on the other. Physiology as a whole has moved through the same mire of doubt as that which entombed Bennet Dowler.... Besides, it may be a good lesson in humility."

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ADDITIONAL RUSSIAN TRANSLATIONS[1]

List prepared by
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Copies are available from the Translation Center of the John Crerar Library in Chicago, Illinois.

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THE CARE AND USE OF ANIMALS

The suggestion has been made to the APS Committee on the Care and Use of Animals that some physiologists may be performing experiments on animals without the use of proper anesthetization. Such inexcusable procedures are, of course, contrary to the "Guiding Principles in the Care and Use of Animals" which has been approved by the Council of the American Physiological Society. The Committee feels that this may be an appropriate time to publicize and re-emphasize the "Guiding Principles" in hope that such practices as those referred to above may be discouraged. The Committee suggested that the "Guiding Principles" be published in THE PHYSIOLOGIST along with the reminder that:

1. Adherence to the "Guiding Principles" is an obligation of membership in the Society.

2. Articles based on experiments violating the "Guiding Principles" shall not be acceptable for publication in the journals of the Society.

(Copies of the "Guiding Principles" as printed below are available on request.)

GUIDING PRINCIPLES IN THE CARE AND USE OF ANIMALS

(Approved by the Council of the American Physiological Society)

Only animals that are lawfully acquired shall be used in this laboratory, and their retention and use shall be in every case in strict compliance with state and local laws and regulations.

Animals in the laboratory must receive every consideration for their bodily comfort; they must be kindly treated, properly fed, and their surroundings kept in a sanitary condition.

All major operative procedures must be done under a general anesthetic; minor operative procedures may be done under local infiltration anesthesia. If the nature of the study is such as to require that the animal survive, acceptable technic must be followed throughout the operation. If the study does not require survival, the animal must be killed in a humane manner at the conclusion of the observations.

The postoperative care of experimental animals shall be such as to minimize discomfort during convalescence. All conditions must be maintained for the animal's comfort in accordance with the best practices in small animal hospitals or in accordance with the practices followed in human medicine and surgery.

When animals are used by students for their education or the advancement of science such work shall be under the direct supervision of an experienced teacher or investigator. The rules for the care of such animals must be the same as for animals used for research.

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Director of Laboratory