OUR FUTURE?[1]

WM. A. SODEMAN[2]

Before proceeding with the substance of my discussion I want to express my sincere thanks for the honor and privilege of being entrusted with the guidance of the affairs of the Society for this past year. I am profoundly grateful.

In considering a subject for the presidential address, I, as I suppose have all presidents, considered what in my power I could do to make it a memorable address and a memorable occasion. Three important theoretical possibilities came to my mind. One, I could announce a very enlightening scientific study. Remember, I said theoretical possibilities. Two, there was the opportunity to develop a profound philosophical report which would seriously affect the future medical thought of all of you. Still theoretical! Thirdly, I could point out a satisfactory solution to the tangled problems of our own and related Societies. More theory! Finally there seemed to be a fourth and practical solution to make the occasion memorable. I could be brief! This approach was not only practical but also within my grasp. This approach I have chosen.

Preliminary to writing these words, again as I suppose many of the past presidents of our parent societies have done, I looked back upon previous presentations. A goodly number discussed our future. True, the terminology, varied. One time it was Horizons in Tropical Medicine or, after World War II, Tropical Medicine in Peace. Dr. Meleney's address this evening, entitled Unfinished Business, implies the same approach. Interestingly enough the discussions and thoughts varied widely. I find this true of my own presentation in comparing it with the others. Although the title is time-honored, the content I believe you will find no more repetitious than the remarks of preceding presidents.

I would like to point out that our society, as an organization, stems from an interested group of Philadelphia physicians, who, in 1903, organized the parent society of one of our parent societies (Faust, 1944). This group of men included internists, pathologists, neurologists, neuropsychiatrists, dermatologists, a surgeon, opthamologists, otolaryngologists, a therapeutist, and general practitioners. Although many of the group held outstanding professorial positions on the faculties of the medical schools in Philadelphia, none were experienced in the field of tropical medicine. They were all identified with other disciplines in medicine but professed an interest in learning about tropical disease. The diversified fields of basic medical interest represented in the founding group maintains itself in our society today. The great upsurge of interest in the preventive aspects of disease, together with the concomitant advancements, has added many others in categories of laboratory science and the preventive approach to the membership. Indeed the term "tropical medicine," in the minds

[1] Presidential Address given before the American Society of Tropical Medicine and Hygiene at Louisville, Kentucky, November 13, 1953.

[2] Professor of Medicine and Chairman of the Department of Medicine, University of Missouri School of Medicine, Columbia, Missouri.

of most of us, now has the same connotation as the term "tropical health." The word medicine is used in the broad sense, as it is used in the name of a medical school. We all recognize as paramount in the tropics the major problems of disease control and health development and maintenance.

The point I want to make from the above discussion is this. We represent a cross section of the health profession, from clinicians and pathologists to epidemiologists, public health administrators, sanitarians and sanitary engineers, held together for varying reasons by interests that relate to health in tropical climes. What, in our future, is most important in maintaining and furthering these bonds? How can we effect an expanding interest and make greater strides in tropical health?

Tropical medicine itself, or indeed medicine generally, is not a science. The collection of facts as represented by the reports of our scientific program does not in itself make up a science. As those facts enter into the formulation and understanding of general laws of epidemiology or parasitology, for example, they lead to the organization and generalization of knowledge in those fields and produce the general laws which constitute a science. Medicine, and tropical medicine, will become sciences in their own right only after the collection of facts carries us to the establishment of basic laws of health and disease (Stevenson, 1949). We are not far on this road, a road which should represent our real future in tropical medicine. Our research is highly organized; our scientific achievements have been notable; our understanding of disease processes has increased immeasurably in recent years. But we lack proper advancement in the organization of this knowledge into important underlying principles. Some phases of our activity have advanced more remarkably than others along this road. Those interested in malaria can fittingly claim this honor to a greater degree than the rest of us. By calling themselves malariologists, even though they are epidemiologists, public health administrators, clinicians, and representatives of other basic scientific areas, those interested in this field give outward evidence of the coordinated effort they have developed. This cooperation in approaching the many facets of malaria had led to the synthesis of many of the facts and data relating to the various aspects of malaria into general laws which govern the development and spread not only of that disease but of other insect-borne diseases as well. Again much of this advancement is based upon the concept that a great reservoir of disease rests in animals other than man, and that the natural history of those diseases is governed in large part by the activities in that reservoir. This concept entails a coordination of effort on the total problem of the disease, upon the organism, the host *and* the environment. Indeed, the great advances in science have come not from the collection of new data, but from such correlation and the development of new concepts. Such basic laws we must seek. The success of our future in tropical medicine rests primarily upon our attempts to establish this goal.

What conditions are necessary to reach this goal? I think it is clear that one of the requirements is a greater interplay between those of us interested in our various basic fields. The highest development of specialism is not in medical

practice, but in research. One organism or group of organisms, one disease, or one organ, represents the interest of many of us. Under these conditions we may lose our insight into the total problem. Let me give you a minor but glaring example of inadequacies from such specialization. A short while ago, I saw a patient with hookworm disease and severe anemia. We treated the patient once and counted 2,000 worms in the stools. That sounded pretty good, generally. Doctor Paul Beaver had examined the original stool specimens and estimated from the eggs present that there were approximately 6,000 worms in the patient's bowel, indicating that our seemingly good results were really poor. A second round of treatment and still a third gave us enough worms to account for the 6,000 estimate, all together. Our medical students, impressed by the statement in their text that a hookworm can remove about 0.67 cc. or more of blood per worm per diem, indicated to us that the anemia resulted from the activity of worms carrying out this degree blood removal. If one knows the total blood volume of the average person, which is about 4–5,000 cc., he realizes that this situation in this patient would cause depletion of almost all the blood in the body in one day. Having discovered this through our prodding, the students then resorted to the additional statement in the text that it is likely that older worms produce considerably smaller blood loss, possibly not more than 0.1 cc. per worm daily, and calculated that this patient would lose at this figure 600 cc. per day, or the amount of a usual blood donation. One does not have to know anything about hookworms or the action of hookworms to know that these statements were not true in our patient. Laboratory investigation has indicated that a hookworm may remove blood to this degree, but not that all hookworms in a human infection are that efficient. Such statements represent a lack of correlation of information between the hemotologic physiologist and the parasitologist.

You might consider the example I have given as isolated and unimportant. I could multiply it many times if I wished, for it is a mere example of numerous instances of this type. It simply emphasizes the point of view that a collection of facts without organization and integration does not lead to a total understanding of the problem. The late Sir James Mackenzie, one of our greatest modern physicians and a clinician whom I have admired tremendously, wrote the same thing in different words when he said "for the intelligent practice of medicine and the understanding of disease, the simplication of medicine is necessary. . . . I hold that the phenomena which are at present so difficult of comprehension, on account of their number and diversity, are all produced in a few simple ways, and that with their recognition what is so complex and difficult will become simplified and easy to understand. This means a recognition of principles and a knowledge of their application" (Wolff, 1953).

The understanding of such principles must entail a cutting across the lines of various specialities and various disciplines. When this happens, whole new vistas open up which we did not appreciate before, and those of us with facts in our own little field begin to see ways to fit those facts into the general laws. An outstanding example of this is the stress and adaptation syndrome, in which a wealth of seemingly unrelated observations is integrated. Since man's adaptive

and protective capabilities are limited, his response to many sorts of noxious agents and threats may be similar, the form of the reaction to any one agent depending more on the individual's nature and past experience than upon the particular noxious agent evoking it, and this adaptive-protective reaction may be far more damaging to the individual than the effects of the noxious agent *per se*, be it direct effects of microorganisms, climate, physical forces, disruption of customs or habits, or others (Wolff, 1953). In the light of this concept, large groups of individuals, those interested in infections, the endocrinologists, cardiovascular specialists, pathologists, psychiatrists, all begin to find ways to fit the collection of facts in their own field into an organized system of understanding which governs events in all these fields. One can realize why chronic psychic trauma and chronic amebic infection might give the same symptoms.

If we are interested in *man* in the tropics, it is obvious that we must take the astounding advances in laboratory research on the agents of disease and the meagre gains in clinical observation in man and integrate them with each other and with the multitude of environmental factors playing on both. The one discipline which crosses all lines of activity, be it laboratory or field endeavor, is epidemiology. Knowledge obtained from the bedside study of the sick individual, and that of the controlled conditions in laboratory experimentation, both solid bases for modern medicine, requires, for thorough understanding of control and causation of disease, observations under natural conditions in the whole population. If epidemiology is called the natural history of disease, all medicine is no more than the natural history of disease with attempts to block it as early as possible. The clinical method and the experimental method are both then bound up with it. Encompassed in this scope, which we may broadly call medical ecology, is medical geography, a part and parcel of epidemiology. Those who profess to be epidemiologists, who are by no means all of those practicing or using epidemiology as you can see, have long recognized that infections are more than the interplay between the exciting agent and the host. The environment plays a profound role. Again the malariologists were early to recognize this, chiefly through study of the agent residing there. It is itself complex and many sided, with physical, biological, and social components (Gordon, 1952). As such, a disease is the result of no simple contest between the host and the agent of the disease. A great variety of environmental factors have a part in its origin, and a balance of affairs determines the issue. There can be no complete understanding of the process until the interplay of all three of these factors is evaluated. The laboratory investigator dealing with the agent studies but a fraction of the problem and collects facts which are inexplicable without knowledge of the host and environment. The clinician studying the host alone similarly reaches important limitations in his study.

All disease is of multiple causation. In some diseases in which a specific agent is not known in the causation, we study the multiple causes which are known very intensively and attempt to establish their relationship very extensively. This is true in peptic ulceration of the stomach or in high blood pressure. In disease entities in which we know the specific agent, there is a great tendency

to study the specific agent to the exclusion of the other etiologic factors. If we can isolate the specific agent, take it into the laboratory and manipulate it, we may of course learn very much about it and other causes, but we may be so interested in the agent that we study it to the exclusion of the other causes as well. We turn away from the relationship of host, agent, and environment. We are apt to study effect of environment on agent and not on host except for mechanism of transmission. But it is in the total relationships that we find factors of extreme importance. In pneumococcic pneumonia the agent may be in the throat of the individual but not produce the disease. It may take a common cold or an alcoholic bout to lay the ground work in the bronchioles to produce the disease entity. The common cold or the alcoholic bout are as important in etiologic relationships as is the pneumococcus itself in the production of disease in that patient. Similar factors are important in many of the diseases that we consider tropical. In amebic disease for example we find in some areas of the tropics when the cool season comes on, that amebic dysentery tends to disappear. The agent is there; the host is there; environment as a medium permitting spread of the agent has not changed remarkedly. Why has dysentery disappeared from the wards? The explanation most likely rests in host factors and can be explained by adequate study of the host. Increase in the host resistance of some sort or other is responsible for the reduction in the activity of the disease and the development of the low grade chronic disease with cystation. That is why I have always thought personally that the study of excystation and encystation is one of the most important in the field of amebiasis. It involves the relationship of man to the organism.

How little we know of man in his reactions and adaptation to tropical climate itself! The host is often little studied except as the agent relates to definite changes in him. And we cannot have a full understanding of his disease reactions until we gain such knowledge. Acute effects we know in some degree. But information on the more subtle changes of long term residence in the tropics is most fragmentary. We do know enough of the changes in thyroid, adrenal, and pituitary function to realize that widespread physiologic effects are at work, effects in and on organs and systems concerned with protective mechanisms, growth and metabolism, and reaction patterns. These modify responses to invasive agents and to therapeutic procedures. They may help explain, in part why clinical pictures differ in tropical and temperate practice, why drugs seem more effective in one part of the world than another, and why man fares better in one area than another. Indeed, they may help explain the modification in occurrence, as well as clinical activity, of some cosmopolitan diseases in the tropics and could lead to understanding of the processes involved, to discovery of facts helpful in the understanding, treatment, modification, and eradication of these "nontropical" diseases elsewhere in the world.

I have tried to use this discussion chiefly to emphasize the fact that specialism in study has great limitations. Specialized concentration on technics and fact finding in narrow fields have carried us far in our study of man in the tropics. But our ultimate goal is systemization and understanding of these facts, of

fitting them into a rational pattern. A broad concept of the disease, or of disease as a whole, must be stressed now and in the future. Our future rests in the development of more facts, that is true, in furtherance of the tremendously important laboratory investigations of the past, advancement in the field and clinical studies long under way. But it rests more importantly upon the correlation of the data obtained and to be obtained, and the synthesis of that information into an understanding of the underlying principles and general laws governing these processes. If our future is pointed toward that goal, the interests in tropical medicine of everyone in this room will be multiplied many times. The ties that bind us to tropical medicine, as workers and scientists in those fields which are the handmaidens of tropical medicine, will become ever stronger, and tropical medicine will be guided toward the achievement of stature as a science.

REFERENCES

FAUST, E. C., 1944. The American Society of Tropical Medicine, *Am. J. Trop. Med.*, **24**: 63–69.

GORDON, J. E., 1952. The newer epidemiology, in *Public Health Lectures*, Unitarian Service Committee Inc., Boston.

STEVENSON, I., 1949. Why Medicine is not a Science. *Harpers Magazine*, **198**: 36–39.

WOLFF, H. G., 1953. *Stress and Disease*. Charles C Thomas, Springfield, Illinois.