

PROCEEDINGS OF THE THIRTY-SECOND ANNUAL MEETING OF
THE AMERICAN SOCIETY FOR CLINICAL INVESTIGATION
HELD IN ATLANTIC CITY, N. J., MAY 6, 1940

READ BEFORE THE SCIENTIFIC SESSION

PRESIDENTIAL ADDRESS

Functions and Dysfunctions of Learned Societies

BY ISAAC STARR

In times of intellectual activity men with common interests have always organized themselves into societies. I propose that we examine the *raison d'être* of such societies, ask what the aims are, and then, changing from the general to the particular, ask ourselves how well we are accomplishing the aims we have in mind.

Certainly we are not a club whose aim is the entertainment of the members. We do not come together for mutual admiration and we are not concerned with perpetuating the past. We are concerned with the present and the future, and the proper aim of learned societies, as I see it, is a double one: first, to set a standard; and second, to provide a service. What do we aim to accomplish in these directions and how successful are we in attaining our objectives?

The presidential address of our first president, Dr. Meltzer, was published in the *Journal of the American Medical Association* (J. A. M. A., 1909, 53, 508). I hope that you have read or will read it. In a fighting speech he defends the claims of clinical research against attacks emanating from two directions; first, the inertia of those in authority; second, the lure of what he calls the "golden calf" of practice. Though it is nowhere explicitly stated, Meltzer's ideal for this society stands out plainly; it is to be composed of young men who *will* have the truth at any personal sacrifice. May this remain our ideal still!

But times have greatly changed since 1909. The interest in research has increased until it is now a matter for newspaper headlines and the only criticism that could be made of those in authority, who are so often among the founders of our society, is that in certain places there seems to be some lack of discrimination between the search for truth and the writing of papers. But today all pay lip service to research, if they pay no more. So we find ourselves in a situation quite different from that existing when our society was founded and it is time to reconsider our aims.

The service we aim to supply may be considered under three headings. Let us take first that rendered by the *Journal of Clinical Investigation*. To those of us who are considered to be clinicians by physiologists, biochemists, and immunologists; and considered to be physiologists, biochemists, or immunologists by most clinicians; the existence of this journal has been a godsend. Without any particular effort to direct the subject matter sub-

mitted, it has become the locus of the papers concerned with clinical investigations made by quantitative methods, and so it is in the forefront of that change from descriptive and qualitative methods to the more exact quantitative measurements, a change which has made possible the great advances of physics and chemistry and which may well produce as great a revolution in medical practice. Let us pause to pay tribute to the Rockefeller Foundation whose financial support made the starting of our *Journal* possible, to the medical clinics, and to the Chemical Foundation which assisted it at a time of need, and especially to the editors who, entirely without financial recompense, have given so much time and effort to making the *Journal* what it is.

Our second method of service is in these meetings. I find that any attempt to read the medical literature systematically is overwhelming. I am counting more and more on meetings such as this to keep me on the firing line. But I sometimes wonder whether greater service might not be possible. Our programs have contained very little of what may be called synthetic thinking. It is true that 100 years ago, and more, medical speculation was almost wholly fanciful. From this we were rescued by the great pathological school with its insistence on demonstrated facts, and as a result the promulgations of theories became not quite *comme il faut*. But, on the other hand, facts are not of equal importance. In every investigation there is a period when one must decide which facts are worth discovering. To do this logically, one must picture to himself what the situation is. In this connection I want to point out a difference between clinicians and other scientists which is sometimes a cause of misunderstanding. Chemists, physiologists, and the like, working in pure science, are under no pressure to make up their minds. If their grasp of any situation does not please them, they can wait any length of time before committing themselves. But a clinician, confronted with a patient, must come to a decision, and to act intelligently he must create a picture of the situation, even though the evidence on which to base it is scanty indeed. Hence, clinical theories are, by the very nature of things, less firmly based than those in the branches of pure science. But they are not less important and, in my opinion, our program would be improved by studying the methods of the great English school of physi-

ology whose members have never been afraid to attempt to synthesize the facts into simplifying theories.

It is true that some persons, like imaginative children, seem quite unable to distinguish the facts from the figments of their imagination, but this is no excuse for failing to try to synthesize the facts we have. Scientists who have never made an observation or conducted an experiment may contribute signally to our understanding of nature. If one were only clever enough what might not be gleaned from the multitude of observations and experiments in the medical literature and from the long stacks of hospital records now gathering dust? We are spending much time, effort, and money in our clinics accumulating records, apparently in the pious hope that some day somebody is going to synthesize something useful from the multitude of data they contain.

Our third method of giving service is less tangible, but I believe that it is very important. It consists of the advantage that comes with a wide acquaintance among scientific investigators. The ideal doctor, like the philosopher, should have at his command the sum total of all knowledge and any idea that one man can cover the field of modern knowledge applicable to medicine is preposterous. We must cooperate. I hope that membership in this society makes it easier for young investigators to ask questions and obtain criticism from those whose experience and effort are in a different direction from theirs. I am convinced that a few questions asked of just the right people will usually secure a nearer approximation to the truth than can be discovered in many months of patient search of the literature. I hope that personal contact at our annual meetings may make such things easier.

If investigators would think of their confreres not as rivals but as friends cooperating against common difficulties, much would be gained for science in America. It is my hope that this society will bring about friendships of this sort and so avoid the silly acrimonious controversies which have so plagued some branches of science.

I think few people have any doubts about the value of the services rendered by this and other learned societies, but the difficulties begin when they start to exercise their other function, that of setting a standard.

We set standards when we select members for our society, and here our standards have changed. When this society was four years old the secretary sent out a circular letter urging the members to nominate friends who might be interested. Now the waiting list far exceeds the present constitutional limit of membership! In the earlier days interest in research was deemed sufficient. Now we demand tangible evidence of accomplishment. The descriptive essays which have played so large a part in medical literature are not good enough for us now.

The tremendous growth of research interest in America has greatly increased the number of available candidates and from those your council must try to select the best on the basis of the candidate's publications, his academic rank, and the letters from members about him. As is

inevitable, institutional pride and personal friendship color the recommendations so that the council has a hard time deciding. One problem is particularly bothersome. Candidate A has worked in a laboratory known to be highly productive, and its resources have been at his command. His production is superior to that of B, but B has been working without either scientific or financial support. Which is the better man? Time will tell but until it does I do not think the question can be answered with confidence, and when I admit this I admit that mistakes in the application of our standard are inevitable.

A second method of setting our standard is in the selection of papers for the program. This duty of the president must be performed by inspection of abstracts in a very short period of time. Again mistakes are inevitable. The selection is not always on the basis of intrinsic merit; general interest, timeliness, and the ability of the author to present his work clearly, must be considered. But due to the energy and cooperativeness of the members who annually submit about four times the number of titles which can be accommodated, I do not believe that there is another medical program of comparable size which, year in and year out, contains so much of real interest.

Our third method of maintaining a standard is by the selection of papers for our Journal. With this I personally have had no experience, but several facts should be brought to the attention of the members. Like the other scientific journals owned by learned societies, the Journal of Clinical Investigation was not born because of a widespread demand on the part of the membership. In its early days it was proper and inevitable that control should reside with the group who initiated it and who were in a position to finance it. But now, although our Journal is self supporting, the membership as a whole has neither taken the interest it should nor exerted the authority it possesses in the Journal's affairs. I suppose that this indicates complete satisfaction with things as they are. If it does not, it is the fault of the membership itself. Helpful criticism by members has been conspicuous by its absence and it might do much to strengthen the Journal.

During the foregoing discussion of the functions of learned societies, perhaps my next point has already occurred to you. It is that the two chief functions of learned societies are, in a measure, in conflict. Thus there are some who think that the membership should be enlarged and the program expanded. Their aim is to improve the service given by the society. This is opposed by others who point out that such a change would lower our standard of excellence.

Too much emphasis on standards is a cause of decay, often it is a psychological defense mechanism set up by persons no longer productive. The organizations which become more and more exclusive tend to die of dry rot. Mistakes made in the enforcement of the standard make them ludicrous.

I often reflect on our own shortcomings. The young

Pasteur, if nominated to this society, would probably have been turned down because he was a chemist; the young James Mackenzie because he was not connected with a medical school.

Nevertheless, while undue insistence on standards causes difficulties, abandoning them too far in the interest of service brings troubles of another sort in its train. For then the door is opened for the man whose real aim is personal advertisement rather than the search for truth. Uncensored programs are likely to be too long. Societies with low entrance requirements become so large that friendship between the members becomes impossible and the personal service on which I lay so much value is non-existent.

Somewhere, therefore, between the aim of setting a standard and the aim of giving a service, a compromise must be made. It is not for me as your presiding officer to dictate any line of conduct but I am going to suggest that the members, in attacking this and the other problems which confront learned societies, keep two principles in mind.

To illustrate the first I have, under the urge of a sense of duty as your representative, constructed a model of the society's activities.* Here I have a representation of our society radiating its beneficent influence in almost every direction. Now this timepiece is an integral part of the model for it indicates that the influence of our society extends, and will extend, throughout time. But the timepiece has another purpose which is important, for by means of it I am entitled to claim that our model is a four dimensional one and that it is therefore one (dimension) up on the model produced by my friend, the past president of the Association of American Physicians, at the last meeting of that distinguished body (DuBois, E. F., *Tr. A. Am. Physicians*, 1939, 54, 1).

But, in spite of all this, I stated that my model was designed to illustrate a point in my argument. The point is this: in dealing with learned societies, let us preserve our sense of humor. In some of the letters received by the secretary a sense of humor has seemed strangely lacking. The errors of today, if they be errors, can and must be corrected tomorrow.

As a final point I suggest a larger use of the ordinary processes of democracy in the solution of our problems. There has been a tendency in American learned societies to let the officers make all the decisions behind closed doors. The attendance at business meetings has become smaller and smaller and the proceedings more and more routine. In my opinion this is an unhealthy tendency, for I conceive that it is the duty of the officers to keep the members informed of the problems which concern them and that the members have an obligation to assist in making the necessary decisions. If our society is to

be made maximally effective, it must truly represent the best thought of the younger minds in medical investigation. The decision concerning its policies rests with you. And these decisions are important because there is no reasonable doubt that nowhere in this war tried world is there a group of young men whose opportunities for the advancement of medical research can be compared with those which have been granted to you.

The Chemical Properties of Scarlet Fever Toxin. By E. S. GUZMAN BARRON and (by invitation) GEORGE F. DICK and CARL M. LYMAN, Chicago, Ill.

The chemical properties of the scarlet fever toxin purified by Dick and Boor were studied by using skin reactions in human subjects to measure the activity of the toxin.

The toxin, as shown by other investigators, is very resistant to heat, for it can be heated for 1 hour up to 90° C. and for 45 minutes up to 100° C. with no loss of activity. Nor was there any loss of activity when it was subjected to the action of trypsin and pepsin for 24 hours. The toxin is also resistant to pH changes, for it remained active when kept for 24 hours at 25° C. from pH values of 1.08 to 11.18. It is resistant to the action of oxidizing agents such as H₂O₂, copper, and oxidized glutathione, and to that of reducing agents such as cysteine, glutathione, Na₂S₂O₄, and H₂ activated with Pt. black. Iodine and porphyrindin destroyed it by destroying free amino groups. Neither sulfanilamide nor its oxidation product obtained by irradiation with ultraviolet light had any action on the toxin. The isoelectric point determined with Theorell's cataphoresis cell at 3° C. was at pH 5.48. At pH 4.5 the toxin migrates to the cathode and the protein impurity to the anode branch of the cell, permitting further purification. The toxin is destroyed by 30 minutes' treatment with ketene and nitrous acid, showing that the presence of amino groups is essential for activity. On ultrafiltration through graded collodion membranes, the toxin filtered through membranes of a porosity that did not let cytochrome C pass through (molecular weight of cytochrome C, 13,000); on the other hand, membranes that let clupeine through (molecular weight about 2500) did not let the toxin through.

It may be concluded that scarlet fever toxin is a polypeptide with a molecular weight between 13,000 and 4000, its activity being determined by the presence of free amino groups.

The Significance of Antibodies Against Epidemic Influenza Virus. By E. R. RICKARD and EDWIN H. LENNETTE (by invitation) and FRANK L. HORSFALL, JR., New York, N. Y.

The respiratory disease experience of a representative suburban population of 800 individuals has been studied for 2 years. Blood specimens were obtained from all individuals in this group in 1938 and again in 1939. The titer of antibodies against the PR8 strain of epidemic influenza virus was determined on all these sera both by means of the neutralization and complement fixation tests

* At this point the president drew forth a curious looking object which at a distance resembled an orange thrust through with a multitude of long steel knitting needles that radiated from it in all directions. This he placed beside a clock.