SPECIAL ARTICLE

ANNUAL DISCOURSE — UNPROVED HYPOTHESES

THOMAS R. DAWBER, M.D., M.P.H.

ONE of the few advantages of becoming a senior citizen in the Medical Society is the privilege of being allowed to philosophize about various aspects of our field of endeavor. Although previous orators addressing the Society have not all been elderly in years, all have had wide experience that made their comments noteworthy on the varied subjects on which they chose to discourse. One thread runs through the cloth of these dissertations — a desire to improve medical knowledge and the level of medical care and practice proceeding from it.

Medicine is not a pure science, and its successful practice must include a great deal of art. Some of our colleagues may believe that the major factor of success in practice involves art rather than science. Most of us, however, prefer to believe that the science of medicine is our primary concern but that varying degrees of art may be needed in the practical dispensing of our product. The difficulties we now face, with increased hostility from the public and from the government, may be due essentially to lack of adequate attention to the art of medicine, largely related to the personality and attitude of the individual physician. That this facet of the candidate for medical-school admission has not always received adequate attention is undoubtedly true. Certainly, we should encourage admissions committees to examine more carefully the personality factors of medical-school applicants, which may be the major contributor to improvement in the practices of medicine as an art.

Although it may be possible to gloss over some inadequacies in the scientific aspect of our efforts by improving the art of medicine, we are now increasingly questioned about the scientific basis of our professional work. Not only are questions raised about the manner in which we have performed but doubts are expressed about what we have advised or done. We are frequently asked: Was the operation necessary or even desirable? The answer is not as easily determined as many imply. One example is the current reaction to hysterectomy. Even some of our colleagues imply that if no abnormality is found in the uterus the operation was ipso facto unnecessary. However, is not a hysterectomy desirable before any evidence of a malignant process is present and the only way we now know to prevent cancer of the body of the uterus? Certainly, if we could predict high susceptibility to this disease, removal of a normal uterus in women no longer having any need for this organ would represent the highest standard of scientific medical practice, rather than be classed as a useless, dangerous procedure performed merely to fill the pockets of money-hungry surgeons. To insist on demonstrated abnormality as an indication for surgery is to play Russian roulette with the patient's life.

Since the scientific knowledge on which our practice should be based is in a constant state of flux it becomes difficult to lay down a fixed set of rules by which we can work ad infinitum. On the other hand, we can no longer proceed merely on the basis of "in my experience" or "it is my belief." The scientific basis for what we advocate will increasingly be demanded. The purpose of the present dissertation is to consider what is our responsibility for ensuring that the medicine we practice has a scientific basis, and to suggest a rational course when, as is so often the case, the hypotheses on which we base our action are still unproved.

THE SCIENTIFIC APPROACH

Scientific investigation calls for the development of hypotheses that can then be subjected to adequate testing to determine their validity. A hypothesis is defined as "a proposition or set of propositions set forth as an explanation for the occurrence of some specified group of phenomena either asserted merely as a provisional conjecture to guide investigation (working hypothesis) or accepted as highly probable in the light of established facts."[1] Another definition is "a mere assumption or guess." A hypothesis in simplest terms thus states that X explains the occurrence of Y. This statement may be pure conjecture, or there may already be some evidence to support what is otherwise merely a belief.

The word "prove" has several meanings but as used in "proving a hypothesis" simply means "to subject to a test, experiment, comparison, analysis or the like."[1] The development of hypotheses for study is the first step in a scientific investigation. Preferably, a hypothesis should be stated in such a way that the steps necessary to prove it should be readily apparent. The basis for a hypothesis should, so far as possible, be determined by facts. It is extremely unlikely that hypotheses concocted at random would be worth the effort of testing, although if a sufficiently large number of such hypotheses were tested, some would un-

From the Department of Medicine, Boston University School of Medicine, 80 E. Concord St., Boston, MA 02118, where reprint requests should be addressed to Dr. Dawber.

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doubtedly be supported. Those developed from careful observation of facts gathered from experience are much more likely to be accepted. However, we must always remember that conclusions based on our own experiences may be biased or may be the result of chance. No amount of uncontrolled observation can be a substitute for scientific study. In addition, if the hypothesis is reasonable there is a much higher probability of a positive outcome of the tests to which we may subject it. However, what appears reasonable in the light of today's knowledge may be totally unreasonable when more facts are available.

The medical practitioner may not see himself as an investigator; yet he should realize that each patient he sees presents another test of the hypotheses that govern his practice. Thus, if the patient has a streptococcal infection the physician will put to test the hypothesis that an appropriate antibiotic in adequate dosage will bring about resolution of the infection without complications. He knows this effect is not achieved 100 per cent of the time but that this hypothesis has been supported by a vast amount of evidence, with the result that it must be accepted. In this situation, his practice has a firm scientific basis.

In effect, every action in clinical medicine results from the formulation of a hypothesis that through education, training and experience we have learned to accept or reject. Our difficulties arise when we accept hypotheses without adequate supporting evidence or reject them in the face of substantial proof of their validity. The unwise acceptance or rejection of hypotheses may result from failure to make any attempt to prove them, performance of unsatisfactory tests, contradictory findings from what appear to be adequate tests, or downright refusal to accept facts that are not in agreement with our preconceived notions. A common error results from the substitution of one hypothesis, which may be more practicable to test, for the real hypothesis, the testing of which the investigator finds beyond his capabilities.

A glaring example from the past that strikingly demonstrates an error in medical practice caused by failure to test the hypothesis on which therapy was based is apparent in our approach to frostbite. In a recent medical textbook the treatment for frostbite is given as follows: "...it is best to rewarm the frozen tissues as rapidly as possible until thawing has occurred."[2] However, when we examine some of the older textbooks we find exactly the opposite recommendation. One writer advocated that "Sudden changes of temperature should be avoided. The old well-known friction of frozen limbs with snow or with very cold water is not to be neglected."[3] As recently as 1929 another stated, "Rubbing with snow or with ice water until the color returns is the usual first aid measure."[4] The hazard of "bringing the patient suddenly into a warm room" was stressed.

The reasons for the 180° turn in the treatment of frostbite are interesting and show clearly why it is necessary to subject even the most reasonable hypothesis to adequate testing. Until recent times the practice of gradual warming of the frostbitten extremity was unquestioned. Apparently, no inquiring mind had asked for the evidence on which to base the acceptance of this hypothesis. Interest in the effect of freezing on cells and tissues was greatly kindled when it became practicable to use freezing as a method of preserving food for distribution and sale. Studies of the best methods of freezing and thawing food items soon revealed that when vegetables, meat or fish were frozen as rapidly as possible and thawed in the same manner such foods most closely resembled the fresh product — the least disruption of tissue cells resulted.

A simple test of methods of warming a frozen extremity to normal body temperature could have determined the error of the slow-temperature-rise approach. Previous experience had demonstrated that when a cold extremity was exposed to heat considerable pain resulted, apparently because of the hyperemia resulting from the raised temperature. Avoidance of pain appeared desirable and supported the hypothesis that a rapid rise in temperature might be deleterious. This hypothesis was accepted and until a few years ago was never subjected to test. The error in frostbite treatment resulted from failure to test the hypothesis and its acceptance with no supporting scientific evidence.

I now wish to discuss a few unproved hypotheses of current interest. These are merely examples, but they may serve to help us arrive at a reasonable approach to this problem.

Sodium Intake and Hypertension

For many years it has been apparent that the sodium ion is in some way related to blood-pressure control. The introduction of the rice-fruit diet was a major advance in the modern therapy of severe hypertension.[5] Although the blood-pressure-lowering effect of this diet was originally attributed to its peculiar protein content, later studies showed that the addition of a sodium salt negated the hypotensive effect and that it was the very low sodium content of the diet that lowered the blood pressure.

Observations in populations (e.g., the Yanomamo Indians of Brazil) have shown that the usual rise in blood pressure with age observed in most populations does not occur if the dietary sodium intake is essentially nil.[6] There is general agreement that a sodium-free diet lowers the blood pressure of almost all hypertensive patients and thus, if adopted sufficiently early in life, would prevent the development of much of the excess rise in blood pressure in these cases.

There is controversy regarding the beneficial effect of a decreased sodium intake that is above the very low level necessary to produce a therapeutic effect in

hypertensive patients. Is the blood-pressure-lowering effect of a "sodium-free" diet an all-or-none phenomenon? Animal experiments have indicated that strains can be produced that are particularly sensitive to an increase in sodium salt, and suggest that only certain human beings sensitive to sodium might benefit from a lowered intake.[7] The studies of Dahl and others have supported the hypothesis that blood-pressure level is to some degree proportionate to the sodium intake.[8] Our own studies in Framingham did not support a relation between the level of sodium intake and the level of blood pressure.[9] However, we were careful to point out that the level of sodium intake of those with lowest values was well above that previously associated with a therapeutic effect.

The demonstration that diuretics that increased the excretion of sodium salts were effective in lowering blood pressure greatly diminished interest in low sodium diets as a way of achieving this objective. As is so often true in medicine, when a practicable therapy for overt disease becomes available interest in primary prevention wanes. Physicians with offices full of patients with overt diseases can hardly be blamed for focusing attention on therapeutic methods immediately applicable to them. Contrary to the implications of many that physicians are only concerned with treating disease, my own observations lead me to believe that whenever practicable measures of disease prevention have been developed, they too are enthusiastically received.

The effectiveness of natriuretic drugs in lowering blood pressure and the usefulness of such efforts are now generally accepted although even the most enthusiastic practitioner would readily admit that the value of lowering blood pressure late in its course may be minimal and the side effects of the agents prescribed may be unpleasant and not without hazard. Certainly, a safe method of primary prevention of hypertension would be welcomed with open arms.

Hypertension is one of the most common disorders of the present time. Estimates have been made that the number of Americans suffering from this disorder is about 25,000,000.[10] Numerous epidemiologic studies, including the Framingham Study with which I have been personally associated, have attested to the overwhelming importance of elevated blood pressure as a risk factor in the development of stroke and coronary heart disease.[11] Clearly, an attempt to prevent the development of elevated blood pressure should be one of our top priorities in medical research.

Blood-pressure level is related to body weight.[12] The prevention of obesity is one valuable contribution to the prevention of hypertension. Suggestions have been made that excessive alcohol intake also contributes to elevation of blood pressure. That such elevations reflect other than an acute effect of alcohol or of incipient delirium tremens is not clear.

The best working hypothesis relating an environmental factor to hypertension is that the increased intake of sodium salts is responsible for a rise in blood pressure, and that decreasing the daily intake of sodium will have a favorable effect, both in the treatment of known hypertension and in the prevention of blood-pressure rise in those who are still normotensive. The fact that many people can consume large amounts of salt without development of hypertension and that, at the usual high level of sodium intake in our population, a relation to blood pressure level has not been clearly established is not sufficient evidence to reject this hypothesis.

Although the hypothesis cannot be rejected it also has not been accepted. Many physicians pay little attention to the salt intake of their patients even though large numbers of them are on diuretic agents aimed at decreasing body sodium. Almost all canned and frozen foods have large amounts of added sodium salts. Chefs and housewives routinely add salt to food in the cooking process. The addition of salt to food at the time of eating is widespread. The Food and Drug Administration has not considered sodium chloride a food additive. Salt has even been added to infant food, presumably to satisfy the taste of the mother. Only recently has an effort been made to curtail this practice. The taste for salt is acquired. There is no hazard in drastic limitation of intake.

The failure to accept the hypothesis linking sodium intake causally to hypertension is presumably due to lack of evidence. Has this evidence been sought to the degree warranted? Physicians with a particular interest in hypertension have often commented that a diet sufficiently low in sodium would accomplish the same blood-pressure-lowering effect as that achieved by diuretics, but have dismissed this approach on the grounds of impracticability in view of the difficulty of obtaining sodium-free foods. What they have failed to emphasize is that this impracticability has been created by the widespread commercial addition of sodium salts to food, a practice that could be controlled much more readily than almost any other considered to be of public-health value.

The hypothesis linking blood pressure to sodium intake, although supported by much positive evidence, must still be considered unproved. While we urge the research community to consider the wisdom of conducting a definitive test of the sodium-hypertension hypothesis what should our attitude be? The report of the Senate Select Committee regarding the nutritional goals for the country has suggested a drastic cut in the salt intake.[13] Others adopt an attitude that since a definite relation between sodium intake and disease has not been established, no change in our habits is warranted. The attitude that because a given hypothesis has not been supported sufficiently for acceptance it should be rejected is not reasonable, unless adequate testing has been conducted.

What is needed is a careful, long-term trial of the benefit of a restricted sodium diet — admittedly an extremely difficult task in view of the current almost

universal practice of sodium-salt addition. Considering the importance and widespread prevalence of hypertension, is not this hypothesis worth testing, no matter how difficult?

Tonsillectomy Prevents Respiratory Disease

Tonsillectomy is one of the most common surgical operations in the United States. Although the rate of tonsillectomy is apparently going down, until recent years from 800,000 to one million tonsillectomies were performed annually in this country,[14] at an annual cost of about $150 million and — more importantly — with a loss of life of up to 300 children per year. Morbidity from the procedure is not easy to measure but can be considerable.

Opinions regarding the value of tonsillectomy vary widely. Among nonphysicians there has always been relatively strong support for the procedure. Among physicians there is a wide range of opinion.[15] A personal (nonscientific) poll finds considerable difference of opinion among my own colleagues with backgrounds similar to mine. It is therefore interesting to review the scientific basis for the performance of this widespread and not unremunerative procedure. Many reports of the effectiveness of tonsillectomy in preventing "tonsillitis," sore throat, recurrent respiratory infection, bronchitis and middle-ear infections have been published, as have several articles reviewing these studies. Assessments of the relative merits of the various studies reported are rather discouraging. Only one prospective study of the efficacy of tonsillectomy has been conducted in this country, and that was begun over 50 years ago.[16] The results of that study did suggest a benefit in prevention of "tonsillitis" and respiratory infection, but the control group was relatively small. One might expect that if the tonsils were actually removed, ipso facto there could be no recurrence of "tonsillitis." All too frequently, however, only a portion of the tonsil is removed. Interestingly enough, in the latter study more bronchitis and pneumonia were observed in the treated children. Other studies have been conducted, but all suffer from many defects, which have been summarized in two recent reviews of this subject.[17,18] Each of the studies had flaws — e.g., selection of patients for operation, possible differences in control and treated groups, removal of controls and placement in treatment group for "ethical" reasons and postoperative evaluation based on opinions of parents. According to the reviewer none of the studies were designed to give a clear-cut answer.

When the methods of the studies conducted were evaluated and scored on the basis of scientific design and conduct all were found wanting.[19] Of particular interest is the fact that all studies conducted by otolaryngologists resulted favorably for tonsillectomy. The method of payment affected the rate of tonsillectomy, with fewer operations being performed when the method of payment was other than fee for service. The conclusion of this evaluation was that "...current data do not tell us whether the procedure is beneficial or not."

It seems strange that after all these years no serious attempt has been made to answer the question of the usefulness of this widespread operation and that we still read in the newspaper of the admission of all the children in a family to a hospital for consecutive tonsillectomies. Possibly, it is not so strange when we read the statement of one physician favoring this operation: "It is impossible to be statistically accurate in evaluating the results of an elective procedure such as T and A which has no accurately measurable parameter of success or failure. We must rely on the subjective evaluation of parents and of physicians using various criteria."[20]

If we are to accept this author's statement we must conclude that no answer to the usefulness of tonsillectomy can be obtained and that is it useless to try to find one. If such a conclusion is justified, the logical answer should be "cease and desist." We would have to agree with the conclusion of the evaluator previously referred to that "In this era of escalating health care costs, society can only afford therapies which have been demonstrated to be of benefit."[19] That tonsillectomy in the absence of very obvious indications provides any health benefit must be considered an unproved hypothesis. If the procedure is to be continued we have a clear-cut responsibility to put this hypothesis to adequate scientific test. If we cannot find sufficient evidence to support it the procedure should be abandoned. Realistically, I suspect that no definitive test of this hypothesis will be attempted but that the operation will gradually lose favor and disappear from the medical scene without any of us knowing whether it was of value or merely another medical fad.

Alcoholism Is a Disease

The hypothesis implied in this statement is that an underlying pathologic process is responsible for the chronic excessive intake of alcohol by certain people. The disease is not the excessive imbibing of alcohol and the resulting effects per se. Rather, these are considered manifestations of a pre-existing disease. Obviously, this hypothesis has appeal to those who are considered alcoholics, not to mention those, including federal, state and local governments, who profit from the manufacture and sale of alcoholic beverages. The hypothesis has received wide acceptance, possibly because of its constant repetition in print and on radio and television. This attitude persists in spite of numerous studies that find no differences in the handling of alcohol by so-called alcoholics as compared to those not so classed.[21] Clearly, if we are to make any progress in the prevention and cure of this widespread "disease" we need to know whether the

major factor — alcohol intake — is the cause or the result of the disease. Without such a fundamental piece of knowledge any approach to the problem appears irrational.

Any evaluation of known confirmed alcoholics suffers from the fact that alcohol is a toxic agent and any abnormalities, organic or behavioral, observed in those who have already been exposed could reasonably result from alcohol ingestion rather than be considered responsible for the excessive use of this agent. What clearly is required is a prospective study of a population before excessive alcohol use to determine the characteristics of those who are destined to become alcoholics. Some set of common characteristics that could fit a disease syndrome and are associated with the later excessive use of alcohol would need to be found. To my knowledge no such study has been done. It would be time consuming and costly, but in view of all the time, effort and expense now incurred in our attempts to salvage the alcoholic population, certainly some attempt to determine whether the major hypothesis on which the current approach to alcoholism is based has any validity seems justified. That alcoholism is a disease not resulting from alcohol intake is an unproved hypothesis whose continued repetition cannot be a substitute for adequate scientific investigation.

The Fat-Atherosclerosis Hypothesis

The major disease from which adult populations suffer and the No. 1 cause of death is directly or indirectly due to atherosclerosis. The deposition of atheromatous material, largely cholesterol, in the intima of the medium-sized and larger human arteries is responsible for the major disorders of the heart, brain, extremities and, to a lesser degree, many other organs. For many years the major hypothesis regarding the development and progression of atherosclerosis has been that fat and cholesterol in the diet are responsible for the high level of blood cholesterol in many populations and that such high blood levels are in turn responsible for the increase in the rate of development and degree of severity of this disorder.

A tremendous amount of research has been done, and volumes of literature printed regarding the relation of dietary fat and cholesterol to atheroma formation. A review of this subject is not possible in the space permitted. In brief, there is good evidence that populations differ in the average value of serum cholesterol and that these differences may be due at least partially to dietary intake. In populations such as our own with relatively high levels of serum cholesterol between-person differences are not readily explicable by individual dietary variations.[22] Elevated serum cholesterol levels can usually be lowered by diets low in saturated fat and cholesterol.[23] Numerous studies have attested to the fact that the incidence of coronary heart disease and other atherosclerotic manifestations is higher the more elevated the serum cholesterol found.[24]

At present it must still be admitted that the diet-heart-disease relation is an unproved hypothesis that needs much more investigation. Current clinical trials may help to answer this question, but they have been organized to answer the hypothesis that measures taken to lower serum cholesterol levels in adult men will decrease the incidence of coronary heart disease in those so treated. Failure to support it would still leave in doubt the possible benefit of a lifetime diet on atheroma formation. This is an example of hypothesis substitution to which I have referred earlier. It is not reasonable to expect that changes in dietary intake after 40 to 50 years on a diet high in fat and cholesterol would produce dramatic results. To establish the benefit of these changes might require far larger population samples than were selected for the studies in question.

It is quite possible that definitive answers may not be reached by any studies currently under way. Meanwhile, conflicting opinions regarding the recommendations that the physician should make are given. Some advocate a complete change in the dietary pattern of the United States population. Others suggest that since the diet-heart-disease hypothesis is still unproved, no change in present diet should be advocated. Physicians questioned about their own recommendations should always remember the comment of Mr. A. H. Sulzberger, former publisher of the *New York Times*, when asked for an opinion regarding the allegations of the late Senator Joseph McCarthy about communists in the United States State Department: "I believe in keeping an open mind but not so open that my brains fall out." It is impossible to disregard the observations on the incidence of coronary heart disease in populations normally subsisting on diets low in fat and cholesterol, just as it is unreasonable to dismiss the observation of low blood-pressure levels in populations on low sodium diets. Although a great deal more needs to be learned we should act on the basis of the best evidence we have and, in concert with the doctrine of "primum non nocere," advocate dietary restraint along the lines of the so-called "prudent" diet or the diet recommended by the American Heart Association. Meanwhile, we should support any effort to resolve the question.

The above examples are merely a few of the thousands of hypotheses that govern our approach to the prevention and cure of the many diseases that afflict mankind. Very few of the hypotheses involving the management of the bulk of disorders now affecting the adult population — hypertension, atherosclerotic diseases, cancer, arthritis, degenerative joint disease and senility — have been adequately tested or, if so tested, supported by unequivocal evidence. Almost every therapeutic measure that we take can be challenged. Cast adrift in this Sargasso Sea with no medical com-

pass, the physician can easily let himself be convinced that whatever course he advocates can be justified and ask, "Who has anything better to offer?" He should not forget that the proponents of Laetrile may well ask the same question as they look over the results of medically accepted cancer therapy.

In the practice of medicine we are in a somewhat different situation from those in the physical sciences. The patients who come into our offices cannot wait for endless research to be completed. In almost every case the decision regarding what treatment is better than another or than doing nothing is based on data that are often unavailable or equivocal. Yet even if only a 1 per cent improvement in outcome is expected in an otherwise hopeless situation it is worth an attempt. Even to the non-statistician it will be clearly apparent that to establish the benefit of a given form of therapy that improves the outlook to this limited degree would require vast numbers of subjects with and without the treatment under study. When various combinations of therapy are considered whose end component may contribute only a small increment of benefit the numbers of study subjects necessary becomes astronomical to the point that any definitive test appears totally impracticable. Faced with such a situation, we may well ask how scientific medicine can be practiced.

The physician must repeatedly ask himself what evidence supports the hypothesis underlying his action. This evidence should come from his knowledge and training in medicine substantiated by his own experience. His knowledge must be constantly renewed and increased from all continuing-education sources. He needs to learn to discriminate between the words of the so-called "expert" who is largely a technician and those of the well informed teacher who has both depth and breadth of knowledge that go far beyond mere technicalities. Reports of new findings and "advances" must be reviewed critically. Are the conclusions of the author justified by the facts he presents, or are they merely opinions coming from a source biased by the limited view of the super-specialist on the one hand and the generalist on the other?

Present efforts to maintain the continuing education of the physician are highly commendable. However, in our pursuit of knowledge we should remember that no amount of education can answer our questions if the knowledge we seek has not been revealed. If the definitive studies necessary to determine the value of a given therapeutic regimen have never been done no amount of reading will permit the physician to conclude other than that the hypothesis is still unproved.

As pointed out above, the mere fact that a hypothesis is reasonable is not sufficient to permit its acceptance. However, this is a good point at which to start. If what we do is reasonable on the basis of our best informed judgments concerning risk and possibility of benefit, and even if all the evidence is not in, we can proceed with some feeling of assurance. Our greatest responsibility lies in efforts to acquire the added knowledge necessary to make a truly scientific judgment. It is in this regard that I believe we have been negligent. No one is unaware that research is the backbone of modern medicine. Less clear is what research is necessary or desirable, what the priorities should be if there are limited funds and manpower, and who should determine those priorities. Until recently, medical research was commonly conducted by physicians who were at the same time in medical practice. Their close association with the everyday problems of patients gave them a considerable grasp of what new knowledge was needed. Unfortunately, lack of time and financial support limited their work to investigation on a relatively small scale. There were some efforts toward collaboration, but they were also very limited. Seldom could the individual findings of a number of workers be combined owing to lack of agreement about definitions of disease, selection of populations for study or methods of treatment. Even in recent times these are still problems.

The tremendous growth of medical research with large amounts of financial support from industry, foundations, individuals, health agencies and, particularly, the National Institutes of Health and other governmental (research) agencies has created an echelon of professional researchers concentrated in academic medical institutions. Although this has been a financial boon to medical schools it has had little effect on solutions of many of the problems seen in medical practice. Teaching hospitals have a highly selected population of patients with relatively advanced disease. The thrust of research is aimed at this small segment of illness. The researchers' interests are often narrow. Grant approval depends on peer acceptance by those with similar narrow academic backgrounds. There is almost no opportunity for physicians in practice to participate in the decision-making process. Yet they, more than anyone, are most aware of what further additional knowledge is needed to help the vast numbers of patients whom they are called upon to treat.

A few years ago the late Hon. John Fogarty, Congressman from Rhode Island, when serving on a congressional committee, asked a representative of the National Institutes of Health what research was being conducted on "shingles." He was still suffering from post-herpetic pain and his own affliction had stimulated his interest in the disorder, which he learned was extremely common. Yet he found that in spite of the prolonged discomfort and disability produced by this disease, very little interest had been generated in the medical research community toward prevention or better treatment of this common ailment.

Patients, together with their relatives and friends, frequently promote the study of certain often rare diseases and, through fund raising, underwrite academic research far beyond what may be warranted as

measured by the incidence of the condition. Neither patients nor academic researchers should exert undue influence in this most important aspect of medicine. Practicing physicians should have a much louder voice in suggesting the direction in which research should go. You will all remember the late Regional Medical Program, which was supposed to help solve this problem. Its failure, in my opinion, stemmed largely from the lack of a practicable plan to bring into the organization the very physicians it was supposed to represent and help.

New hypotheses are constantly being proposed. Physicians are under repeated pressures from patients and their families to use untested therapies based on hypotheses that are often purely conjectural. The willingness of their peer physicians to accept such hypotheses has a strong effect on their own actions. Only a few days ago one local group of physicians was cautioned against certain expensive procedures of no demonstrated value, used in the treatment of chronic respiratory disease.[25] The temptation to apply new untested forms of therapy is particularly great when we have little else to suggest. Once we have embarked on such programs it becomes difficult to abandon them even long after the evidence of their uselessness has been presented. If this evidence is never sought we may go on indefinitely advocating procedures that have no scientific validity.

Unfortunately, much of the research necessary to test the many unproved hypotheses in medicine is of a nature that requires considerable time, effort and expense. It is not glamorous and thus not popular in academic circles. It usually requires multiple cooperative investigations to acquire the data needed for analysis to reach a conclusion in a reasonable length of time. Frequently, a prospective clinical trial involving large numbers of subjects following different regimens must be conducted for many years before conclusions are reached.

The individual physician may believe there is little that he can do to channel research into the problems that he faces in everyday practice. Some have taken the time and made the effort to appear before congressional and other committees to urge support of certain medical research. Practicing physicians cannot usually become so involved since the cost in terms of loss of time from practice makes this a very expensive exercise. Although it would be desirable for individual practicing physicians to let their congressmen know the gaps in our medical knowledge as seen through their eyes, only limited input can be expected from this source.

Medical societies should assume more responsibility in advising their members regarding the scientific basis, or more frequently the lack of it, for the inclusion of new therapies in medical practice. If the hypothesis being followed has not been adequately tested both physicians and their patients should be so advised. If necessary, this warning should be frequently repeated. Medical societies could also serve as liaisons between physicians and government to indicate the major field of investigation needed. Medical research is too important a task to be left entirely to the researchers.

The responsibility to make sure that our science is not one that will continue indefinitely to be based on unproved hypotheses is largely ours. We should not be afraid to admit the gaps in our knowledge and to lend our support to efforts aimed at filling them.

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