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### THE PRACTITIONER'S NEED OF AN INTELLIGENT SKEPTICISM\*

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WHEN my name was added by you to the roll of those, who have given your annual discourse in the years that have gone by since the custom began, I realized that I had been taken into a truly goodly company and was proud of the honor. At once my thoughts turned to an annual discourse delivered almost one hundred years ago (ninety-five years ago to be exact), which by many is regarded as one of the classics of American medical literature, and I thought, had I the wisdom and choice of words of Jacob Bigelow I might, as he did for the profession of his day, bring to you, the profession of today, a message that is as truly needed now as then. Jacob Bigelow entitled his message "self-limited diseases", while I shall call mine "the practitioner's need of an intelligent skepticism".

Both of us have in common the idea expressed in the quotation graven in translated form on one of the buildings of the Harvard Medical School, "life is short and the art is long; the occasion instant, experiment perilous and decision difficult" and are concerned with the apparent too great readiness of the medical profession to assume without any intelligent skepticism that what follows necessarily is caused by what immediately precedes.

In Jacob Bigelow's time this attitude of mind had led physicians into plans of treatment with drugs in large doses coupled with depleting measures such as vigorous emesis, active catharsis and repeated copious bleedings. They believed that, because their patients got well, they cured them. In our own time similar trends of reasoning or perhaps mere assumptions, devoid of any logical process of mind, perpetuate ineffective therapy in individual patients as well as in various diseases, or induce the use of expensive medicaments, where ones of little cost are equally efficacious.

It is a remarkable thing that in 1835 Jacob Bigelow's keen, analytical mind selected a group of diseases as being self-limited. His keenness of perception is attested by the fact that with

the lapse of ninety-five years and with all the progress that has been made in medical science during this period but one disease, called by Jacob Bigelow self-limited, can be transferred definitely to the group over which the medical profession can exert direct control and cure.

To understand what Bigelow meant by self-limited diseases, let me quote from his annual discourse before the Massachusetts Medical Society in 1835.

"Certain morbid processes in the human body have a definite and necessary career, from which they are not to be diverted by any known agents, with which it is in our power to oppose them. To these morbid affections, the duration of which, and frequently the event also, are beyond the control of our present remedial means, I have, on the present occasion, applied the name of Self-Limited Diseases.

"By a self-limited disease, I would be understood to express one which receives limits from its own nature, and not from foreign influences; one which, after it has obtained foothold in the system, cannot, in the present state of our knowledge, be eradicated, or abridged, by art,—but to which there is due a certain succession of processes, to be completed in a certain time; which time and processes may vary with the constitution and condition of the patient, and may tend to death, or to recovery, but are not known to be shortened, or greatly changed, by medical treatment."

Self-limited diseases Jacob Bigelow divided into three classes: (1) the "simple", in which the disease observes a continuous time and mostly a definite seat; (2) the "paroxysmal", in which the disease having apparently disappeared returns at its own periods; (3) the "metastatic", in which the disease undergoes metastasis or spontaneous change of place. Jacob Bigelow includes under (1) whooping cough, measles, chicken pox, scarlet fever, vaccination, smallpox, erysipelas, typhus (typhoid?), salivation by mercury; under (2) epilepsy, angina pectoris, mania, melancholy, asthma dependent on emphysema of the lungs, tubercles, gravel in the kidney and symptoms produced by

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ascarides in the rectum; under (3) gonorrhoea, mumps and acute rheumatism. Were we today enumerating self-limited diseases, as they were defined by Jacob Bigelow, scarlet fever seems almost the only one we could eliminate from Bigelow's list as now being directly curable. What Bigelow had in mind in relation to symptoms produced by ascarides in the rectum possibly now should no longer be grouped as a self-limited disease. The acute attacks of epilepsy and angina pectoris now can be mitigated, though the underlying disease cannot be cured. On the other hand Bigelow's list easily could be lengthened greatly by the addition of examples, for unfortunately we have learned to recognize new diseases decidedly more rapidly than we have found out how to cure them.

Bigelow was far from being a nihilist in matters therapeutic. He realized that much could be done by the physician to mitigate the discomforts of the patient during the course of these self-limited diseases, and that often the patient could be rendered fitter to resist these diseases. Though grouped as self-limited, it by no means followed that the patient would survive his self-limited disease; death often overtook the patient before the self-limited disease ran its course and reached its limit; often, too, proper treatment prevented just such deaths. Bigelow sought to impress that it was far better for the physician to do nothing rather than by what he did to harm his patient's chance of survival or to render his patient less rather than more comfortable. Bigelow was seeking to inculcate skepticism as to believing that a given therapeutic effect resulted necessarily from what had been done by way of treatment. Just such skepticism, still, is greatly needed by the medical profession.

Failure in present times to recall Jacob Bigelow's point of view is, as in his time, leading practitioners to perpetuate ineffective measures of treatment. They still argue that, because they gave to patient A a certain form of treatment and he got well, patient B with the same disease conditions will be cured by the same form of treatment which was given to patient A.

Physicians always, and rightly, have been desirous of improving methods of treatment and so naturally lend a ready ear to clever descriptions of what new drugs will accomplish. These new drugs often are costly, a fact which renders them less desirable from the patient's point of view but more desirable from the viewpoint of he who makes and sells the drug. The physician reasonably should be skeptical of advice emanating from him who will profit by the sale of the medicament being extolled and should seek evidence from other sources. It may be that the new drug is a great improvement on older remedies, but it would be wiser to await extensive testing on large numbers of patients under conditions of controlled observation. Frequently it turns out after such critical trial that

the new drug is even less efficacious than the old. It should be recognized that the determination of the value of therapeutic measures is exceedingly difficult. The fact that something new is being tried usually exerts a very direct effect on the mind of the patient, and improvement may result from this mental effect and not from the thing being used in treatment. The physician's mind, too, is not uninfluenced by extraneous conditions such as novelty, enthusiasm of experiment, etc., toward a bias in favor of the therapeutic measure being tested. Nothing could attest this better than the periodic rise in the value attached to some form of treatment with subsequent depreciation often to total discarding of the method. One familiar with the history of medicine well might construct a curve with upward and downward swings and prophesy that in a certain year an enthusiasm for a certain treatment again would be due, as does the astronomer plot the curve of the path of a comet and foretell for us when again the comet will return to our sky.

Knowledge of these things should make us skeptical, and this skepticism ought to prevent many unfortunate enthusiasms, which periodically have swept over the medical profession, but alas, they do not. Practitioners today, as those in Jacob Bigelow's time, are fully as guilty of false reasoning and unwarranted enthusiasms for therapeutic measures. This applies not alone to the rank and file of practitioners; their leaders, too, not infrequently are misguided and misled.

Let me take a few concrete examples to show why, in my opinion, more skepticism needs to be exercised by practitioners than is the case at present.

The theory of focal infection well will serve as one such. This is a theory which has a considerable degree of clinical and experimental support, and yet many among the best trained medical men are unwilling to accept much of the evidence offered in its support and feel unwilling to apply it to the extent advocated by its chief supporters, though granting the validity of the theory and its applicability in certain patients. My own feeling is one of considerable skepticism as to many of the claims of the adherents of this theory, though recognizing the occasional striking improvement in certain patients that follows clearing up a genuine chronic focus of infection.

In the hands of many practitioners extraction of teeth, removal of tonsils and drainage of sinuses have become veritable panaceas of cure for all varieties of pain and many other symptoms. Had they the sort of healthy skepticism that it seems that they should possess, instead of carrying out at once some one of the procedures just referred to, as so often is done, they would go through some such process of thought as the following. The theory of focal infection appears to be a tenable hypothesis sup-

ported by numerous observations, even if not proved. If so, the first thing to do is to demonstrate the presence of a focus of infection in the given patient. Next, careful examination of the patient should be made to determine the absence of evidence of other possible cause of the symptoms. Then, consideration should be given to determining whether the focus of infection can be cleared up, and if so, whether the removal of the focus of infection at that stage of the disease will have any effect on the disease process. Finally, will the necessary surgical procedures do injury to the already ill patient? To follow such a train of thought surely is desirable, but judging by my own experience with patients seen in consultation or referred to the Peter Bent Brigham Hospital, surely this cannot be done generally by physicians.

Far too often it seems that, with no real evidence of disease in them, sound teeth are extracted, normal tonsils are removed or uninfected sinuses are drained, though it might have been possible, before acting, to obtain much better evidence than was obtained as to whether a focus of infection existed in these places. It appears to be forgotten that normal teeth usually function better than artificial, that most uncomfortable and serious sore throats do occur in those who have undergone tonsillectomy, that tonsillar residues, left behind even by skillful operators, often maintain infection as did the original tonsils, that drainage of a chronically infected sinus frequently fails to result in a normal sinus, and that some of these procedures, especially in older patients, entail a prolonged and tedious period of convalescence, while an occasional patient dies as the result of such operations. With these thoughts one should question always the advisability of attacking a possible focus of infection to see whether its removal will help the patient. Moreover, often even when disease is present in teeth, tonsils or sinuses, more careful estimate of the patient's condition would show the futility of expecting help from attempts to clear up these foci of infection.

Failure to reason as indicated above is all too often illustrated by patients I see. Very recently two patients came to my wards within a short time of each other; both suffered pain in the back; both had had teeth extracted as a measure of treatment. Each had metastatic carcinoma of the vertebrae to cause his pain. Another patient had pain in his side from a neoplasm in his lung, and teeth were extracted. Tooth extraction had been futile in these patients and would not have been carried out, had thought been given to the patient's history, and a thorough examination of the patient been made. The teeth may have been bad, but their extraction could have no helpful influence on the patient's condition, and well may have set the patient back in his general condition.

Severe and advanced nephritis is seen increasingly in patients recently having undergone

operation, particularly extraction of teeth, to cure a real, or supposed, focus of infection. Even a casual study of the patient would have shown that long since the stage has been passed in which any such procedure could be of any possible benefit.

Bacterial endocarditis is seen not infrequently in patients in whom teeth recently have been extracted after there is ample evidence that bacteria were already growing on the heart valves; how any one with any knowledge of bacterial endocarditis could think that the removal of even a badly infected but non-painful tooth could in any way make this type of patient more comfortable or stay the progress of a fatal malady, I cannot see.

Patients with trifacial neuralgia coming to the hospital for surgical treatment notoriously are edentulous. Had tooth extraction been of help, the patient would not have progressed to the point of seeking surgical amelioration of the pain.

Over and over again patients with arthritis are seen sans teeth, sans tonsils, sans appendix, sans gall bladder and, dependent on sex, sans prostate or Fallopian tubes, in varying combinations, with little or no discoverable evidence that any of these structures actually were diseased, when their removal was advised, and this all too frequently in that form of arthritis known to be rarely related to focal infection.

The extent to which this is going on in our medical community is indicated by a recent answer of a medical student, when asked to make a diagnosis of an x-ray of a skull. Immediately came the answer, "brain tumor". "Why?" said the instructor. "The patient has no teeth" was his answer. This student had observed that, almost as a rule, patients with brain tumor had had teeth extracted by way of treatment before the diagnosis of brain tumor was made.

I am sorely tempted to present somewhere a paper entitled "the damnable practice of pulling teeth" to emphasize the skeptical attitude of mind which the practitioner should apply before advising in regard to measures of treatment involving focal infection in relation to disease. Focal infection probably is a causative factor of numerous symptoms, and I am not opposing their removal in such cases, but I insist that (1) there must be obtained definite evidence of the existence of a focus of infection; (2) the patient must be examined carefully to eliminate other cause for the disturbance; (3) before treating the focus of infection by its surgical removal, there must be evidence that the patient's condition is not too advanced to be remediable; and (4) the physician must feel sure that the surgical procedures incident to treatment of the focus of infection will not harm the patient. Finally, I have no sympathy with the practice of removing teeth or tonsils or draining a sinus or operating on any other structures on the basis that the patient's condition may result from focal infection and that focus

may reside in the structure whose removal is advised. This latter seems to be the line of reasoning of many physicians.

The way in which digitalis is used, is another example of a need for skepticism. The best medical authority of the day maintains that all forms of digitalis have the same effect, that all are effective when given in adequate dosage, and that rarely is it necessary to give digitalis by other than the mouth route. The U. S. Pharmacopoeia provides three varieties of digitalis preparation for mouth dosage with all necessary standards to ensure efficiency of these preparations. There is satisfactory evidence that the average drug store is supplying these preparations according to pharmacopoeial standards. There is evidence that to date no effective digitalis preparation of any sort is non-nauseous, when given to a point just beyond full digitalization; when non-nauseating digitalis is produced, the profession will have acquired a most dangerous drug, unless some methods better than those now available to practitioners are found to indicate when enough but not too much digitalis is given. There is no evidence that any special form of digitalis is superior to pills of powdered leaves or tincture, except that very rarely a special preparation is needed for intramuscular or intravenous use. This is information readily accessible and repeatedly presented to the medical profession. Were it kept in mind, the physician would be rightly skeptical of much that is told him by the manufacturing pharmacist. As all forms of digitalis are good, here it is largely a question of cost, but the physician should consider cost.

The pill of powdered digitalis leaves is about the simplest possible form in which digitalis can be given by mouth. Long experience with it has shown me that in this form it is satisfactorily effective. Powdered digitalis leaves often are dispensed in capsules. The only difference between the two is the increased cost in capsule form. According to a circular just received from a manufacturing druggist, the amount of digitalis used at the Peter Bent Brigham Hospital per annum, if given in capsule form, would cost \$288.00 more than if dispensed in pills. As both are equally effective, why not save \$288.00 a year or \$2880.00 in ten years? In similar way \$210.00 a year can be saved by using pills rather than a certain proprietary form of digitalis; more if other preparations were used. Pills cost less than tincture of digitalis and are equally effective. We save at the Brigham \$120.00 a year by not using tincture. Now if certain special forms of digitalis are used, the increased cost is vastly greater. Were we to use digitalis in ampoules as a routine, the increased cost of digitalis at the Brigham for a year would be \$12,840.00 or \$15,140.00, depending on whose preparation we used, over the amount actually expended for digitalis in pill form, though there are extremely few instances in which there is

any need for intramuscular or intravenous digitalis, and except for these, in my opinion pills are as good as any other form of digitalis.

I have used digitalis as an example, for every physician frequently receives advertising material or listens to a drug house representative extolling the virtue of some special form of digitalis and so is very familiar with this situation. With digitalis it can be easily shown that the chief difference between all of these is cost, and the advantage is solely to the producer and not at all to physician or patient. Exactly similar conditions hold for many other groups of drugs, though it is not always so easy to determine the actual value in treatment and cost relationship as is the case with digitalis.

Just at present hypnotic drugs are much advertised, and new forms continuously are being introduced. Progress is being made slowly, but there seems no need for so many hypnotics, some of which actually are less effective as hypnotics than older forms, and often ones equally effective as older ones cost more. Recently I became dissatisfied with the effects obtained from hypnotics as used at the Peter Bent Brigham Hospital, having the impression that results were not so satisfactory as were being obtained four or five years ago. I noted that several of the more recently introduced hypnotics were being used. I suggested to one of my colleagues that he make a few tests of hypnotics in regard to efficiency. He selected 10 of the more recent ones, which had been accepted in New and Non-official Remedies of the American Medical Association for 1930 (luminal, amytal, ipral, neonal, phanodorn, bromural, carbromal, sabromin, chlorbutanol and butyl chloral hydrate) and compared them with codeine, barbital and chloral. Rather small doses were used, and the hypnotic effect was regarded as satisfactory, if in less than one hour the patient fell asleep and slept through the night. When more than one dose was required to get this effect, this was noted, and an index of efficiency was calculated from the number of doses needed to cause a good night's sleep in a given group of patients with an index of 100% as a perfect score, which would be the index had twenty doses of a given hypnotic caused twenty nights' satisfactory sleep in a given group of patients. About twenty tests of this type were run with each hypnotic. Any unpleasant after-effects were noted; these were infrequent and not relatively different with the different hypnotics. As far as these studies go, there was no evidence of the superiority of any of the ten tested over barbital, chloral and codeine; some were distinctly less effective. Costs varied much; expressed in a numerical comparative way barbital was 3.7, chloral 0.75 and codeine 9.5. All of the others cost more than barbital, going as high as an index of 25. No more definite statements than these should be made, based on such preliminary observations,

but at least they justify us in being skeptics, when listening to descriptions of the great value of some new hypnotic. It sounds as if some of these new hypnotics were introduced in order that the particular firm might have a special hypnotic for sales value. It is well to remember that barbital, the cheapest of this group of new hypnotics, costs five times more than chloral for the same number of equally effective doses and that several cost almost seven times as much as barbital, or thirty-five times as much as chloral in doses of equal efficiency.

Some diseases require long periods of time before any very serious consequences result, and in these it is particularly difficult to judge the results of any newly introduced form of treatment. Rheumatism is such a disease, for it may take 10 to 15 years before cardiac insufficiency results from an acute attack of the disease which in itself is almost of negligible consequence. Syphilis is another, for in syphilis except for its contagiousness the primary and secondary stages are quite mild as a rule, and 15 to 20 years usually must elapse before the serious consequences appear in such forms as syphilitic aortitis with aortic insufficiency or aneurism or as general paresis.

It is only in the present period, for example, that we are in a position to judge of the efficacy of salvarsan in curing syphilis. Early we learned that Ehrlich's hope that it would be a drug capable of speedily sterilizing the body of spirochaetes was doomed to disappointment. Recently disquieting reports begin to appear, which indicate an increase in syphilitic aortitis and general paresis to suggest that salvarsan by itself has not been a really effective method of treating syphilis. Much more evidence is needed, before we can feel sure of this, but such suggestive evidence, as we have, should justify skepticism of any remedy for syphilis, including salvarsan, advocated as other than an adjuvant of mercury and iodides. We should keep in mind that about twenty years' observation of patients is needed to determine whether any new anti-syphilitic remedy is a real cure; little is gained from speedy clearing of primary and secondary manifestations of syphilis, if years later the patient is to develop aortic disease or general paresis or some other equally serious late manifestation of syphilis. So the practitioner should be skeptical of any reports advising methods for treating syphilis other than those reporting patients treated and followed for at least 20 years. Much the same applies to rheumatism though here evidences of impending cardiac disaster appear far in advance of that disaster.

These examples will serve to indicate why physicians should take a skeptical attitude toward claims for efficiency for newer forms of medicaments. Even when effective enough to justify inclusion in New and Non-official Remedies, it does not follow that they are more effective than older forms, or if equally effective,

it may be that they cost more and so have in that sense a distinct disadvantage. Many new remedies constantly are being advertised. Often one can form no idea from the circulars what they consist of, and there is offered no real evidence of their efficiency. In my opinion these should not be used at all by physicians until such time as they have been studied in well organized clinics or observed and reported on by well trained clinicians. It is to be remembered that in a relatively short time newer drugs now are investigated by the American Medical Association and soon included in New and Non-official Remedies. Such as do not receive this stamp of approval scarcely should be used at all by practitioners. Even those accepted may prove to be of no great service.

At the present time much of ill omen is reputed to result from sluggish bowels. The high colonic irrigation is the fetish of many and particularly of those who treat arthritis. By them we are told of horrid messes removed from bowels, messes which they say must have tarried many days in the large bowel. A justified skepticism about all of this should be aroused by such observations as the following: post-mortem examination rarely reveals evidence of such colonic retention as they describe; patients with Hirschsprung's disease (megacolon) may move their bowels only at weekly or fortnightly intervals, and they rarely, if ever, show arthritis; x-ray observation tells us that any enema is a high enema, for the colon fills quickly from end to end with greatest ease; there is a considerable body of evidence to show that colonic contents move abnormally quickly through the gut in many arthritis patients, while slowing this rate of flow brings improvement in the arthritis. So far, I know of no positive evidence in favor of this theory as to the cause of arthritis and its cure, and the many uncured arthritics, who have undergone the treatment, at least seem to justify doubt as to any constant efficacy from colonic irrigations. Skepticism seems justified, even though one may not agree with one skeptic who says that the high colonic irrigations are but a form of masturbation.

Extravagant claims for treatment with endocrine glands are made with great frequency, unsupported by satisfactory clinical evidence. In this field surely there is much need for skepticism. I might remind you that thyroid gland substance, which is so definitely effective in myxedema, besides being efficacious in myxedema, is advised for the treatment of obesity, nephrosis, sterility, arthritis, low blood pressure, lack of appetite, heart block, etc., as a preventive of postoperative thrombophlebitis and as a general tonic in peopless people. In drawing room conversation it seems that almost every individual says her physician is giving her thyroid, and the rare person, who discovers that her physician is not prescribing thyroid for her, feels that surely there is something lacking in her doctor. It

hardly seems possible that one substance could accomplish the cure or amelioration of so many disturbances. Anyway the proof adduced certainly is not flawless, and skepticism is advisable.

Preventive medicine is another field in which, unfortunately, extravagant claims all too often are made. To prevent a disease or a group of diseases is a great accomplishment. Modern methods have curtailed and in certain instances almost eliminated a number of diseases, by discovering and preventing means of transmission, and by developing protective measures applicable to the community and to the individual. Parts of the globe, formerly scarcely habitable by our race, have been rendered safe for the white man. No one would or could belittle these accomplishments. However, little advance has been made in the prevention of the occurrence of many diseases, especially the chronic diseases that play so large a part in the disability of individuals in our part of the world.

The problem of preventive medicine in relation to chronic disease in the adult appears to me to be very largely unsolved, or to put it another way, we have very little knowledge of what methods should be followed to decrease the incidence of those chronic diseases such as nephritis, cardiac disease, hypertension, arteriosclerosis, etc., which constitute the cause of death in a very large per cent. of adults, especially in those beyond the forty year period of life. Even a chronic disease of known infectious origin, such as tuberculosis, according to many observers, may have been influenced very little by our methods of preventive medicine; some even go so far as to say that the entire antituberculosis campaign has been unproductive of any direct result, that the decrease in tuberculosis began before the antituberculosis campaign had its inception, antedated the discovery of the tubercle bacillus and has merely steadily progressed in spite of all of this. Perhaps this is not true, but at any rate there is enough ground for doubt to justify an enquiring skepticism of the results of preventive medicine even in such a disease as tuberculosis, and as far as chronic disease of great frequency in adults, but of unknown etiology, are concerned, there seems very little ground for the rather generally implied claim that preventive medicine is in a position to formulate adequate measures of prevention.

This is not to say that no progress has been made, and that of prevention of chronic disease in the adult we have no knowledge. It is, however, to say that our knowledge along preventive lines is scant, and much of the advice that we can give is in large part empiric or yet in an experimental stage. This should serve to temper the enthusiasm of claims for preventive medicine and stimulate zeal for further investigation, extensive in its range and prolonged over long periods of time.

Let me illustrate what I mean by using chronic nephritis as an example. After some

twenty-five years of particular study of this disease, what have I to advise when the patient with slight evidences of presumably early chronic nephritis presents himself for examination and an outline of what to do to stay the progress of the disease? It must be confessed that there is but little in a concrete way to do beyond correcting such conditions as are observed on physical examination, if they can be corrected, and saying to the patient to lead a rational, normal life of decreased strains and to avoid infections. Advice as to diet and fluid intake is entirely empiric. Avoidance of infection for the nephritic may be somewhat helped by advice but probably awaits a solution of the problem of prevention of upper respiratory tract infection before much progress can be made. So far we have made almost no advance in managing upper respiratory infections, and yet these seem to be very important factors in the cause of chronic nephritis as well as of other chronic diseases of adults. An intelligent physician can help much in planning a more balanced, saner life for the individual, and this will help to prolong the time before the patient succumbs to this disease.

What I have said of nephritis also applies to the various vascular lesions and their results, to cardiac disease, to chronic bronchitis and emphysema, to chronic degenerative disease of the central nervous system except those of syphilitic origin, to gall stones, peptic ulcer, gout, cancer, etc. Of some we know more as to cause, of others less, but of none, much, and until we know more of causes we cannot expect to do much to prevent. It seems to me wise to acknowledge freely our limitations in preventive measures and desirable to investigate more and talk less of prevention of disease.

The periodic physical examination is highly desirable, for certain defects and diseases, easily discovered thereby, may be treated in such ways as to help in the prolongation of healthful life. On the other hand such extravagant claims for the periodic health examination as the following which I quote from a recent "health bulletin" can but be harmful:

"It is probably safe to say that if every person, young and old, infants, pre-school children, school children and adults, had an annual complete physical examination and followed the physician's advice, at least ten years could be added to the average length of life to say nothing of the misery and unhappiness which could thus be avoided.

"Such examinations would avoid many cases and most deaths from tuberculosis, many cancer deaths, a large number of heart cases, much of the rheumatism which is found in middle adult life, many cases of nephritis, and most of the physical breakdowns which occur so frequently in persons between forty and sixty years of age."

To publish such statements will not lead the intelligent physician to enthusiasm for the

periodic health examination, while a more moderate statement would make it evident that the periodic health examination is of enough benefit to make it a wise investment for every individual. Practitioners, it seems to me, should be even more skeptical than they are of much of the so-called health propaganda of the day.

Enough examples have been given to make clear that many practitioners would profit by a healthy skepticism and especially in regard to therapeutic measures. Such an attitude is entirely compatible with progress. Skepticism does not imply that new methods of treatment should not be tried. They should be tried with the careful check of controls, comparing the new with older methods. Organized hospitals are best equipped for such comparisons, but every well-trained practitioner may conduct his own studies, if he but carefully observes.

To doubt is desirable; not to try would be reprehensible. Every physician should be an investigator. Much of the practice of medicine becomes, by reason of its nature, an experiment. The same disease in different individuals causes different reactions, and so treatment needs constant variation to fit this individuality of our patients. Careful notes of the results of treatment of any sort can be made by any physician; this is investigation. When the accumulated data is studied critically, deductions of very considerable value may be made and important light thrown on therapeutic measures. There remains much that can be studied with

very simple apparatus on patients at home as well as in hospitals. In recent years so many complex pieces of apparatus have been devised to record this or that about patients, that we are tempted to forget that the physician's five senses and a critical intelligence with very few mechanical aids still are capable of making important advances in medicine. It is well to remember that most of what we have learned in the past in medicine came from such simple methods rather than from the application of complicated apparatus. In the field of methods of treatment there still is need of much investigation. The group of self-limited diseases, as defined by Jacob Bigelow, is a very large one. The fact that scarlet fever rather recently has been removed from this group lends encouragement to the idea that others, too, may be removed by discovering methods for their cure or prevention. Many conditions that now can be ameliorated, may, by reason of investigation, be brought under better control. Many remedies, that now we use, need a better understanding of why and how they work. Many undiscovered remedies probably await the results of trial. Valuable still should be the contributions of students of medicine, who with intelligent skepticism combine thought and knowledge in the study of the many problems that confront practitioners of medicine. An intelligent skepticism assuredly is needed by practitioners now as in years past.

## INDUSTRIAL MEDICINE AND PUBLIC HEALTH\*

BY W. IRVING CLARK, JR., M.D.†

AS we look back, the past twenty years has in many respects been revolutionary. Manners and customs have changed. New economic theories have been propounded and acted upon. An amendment to the constitution affecting the intimate lives of the people has been in operation. Mechanical inventions have speeded forward. Life moves in a new tempo.

It is natural that the practice of medicine should have been affected by such basic changes and that new methods should have arisen to meet new problems. It has seemed to me that the discussion of one of these new branches of practice might well be considered especially as it appears to present interesting possibilities of future development and to be allying itself with one of the older branches of medicine: namely, that of public health. The branch of practice to which I refer is Industrial Medicine.

At the beginning of the present century the United States was an agricultural country. The

chief products were derived from the soil, prosperity was determined by the size and quality of the crops. Manufacturing, while it was making great strides, was still of less value than agriculture. Factories were becoming an important factor in the employment of labor, but though numerous, most of them were small in size, each one employing a limited number of operatives. It was the era of the beginning of big business. Slowly but with growing momentum, factories began to group themselves into great corporations employing a thousand or more employees. At the end of a decade progress was accelerated. Then came the war calling for every manufacturing resource of which the country was capable. With the war came industrial medicine.

Whether this new development appeared as a result of the antituberculosis crusade, whether it was inspired by the Workmen's Compensation Acts, which became effective at this time, or whether it was a normal development of medicine to meet a great need, is a matter of conjecture. It is safe to say that it did not develop as a branch of Public Health. Its contact with this service in its infancy was insignificant.

\*The Shattuck Lecture delivered before the Massachusetts Medical Society June 17, 1930.

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