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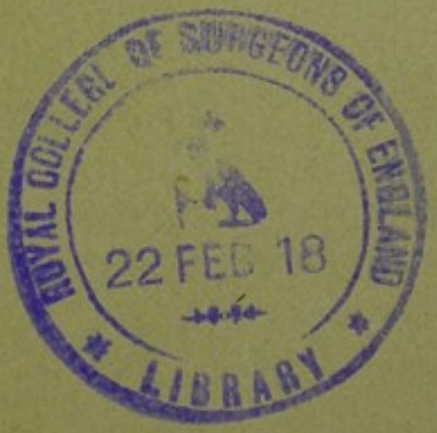
BY

WILLIAM PEPPER, M.D. LL.D.,

PENNSYLVANIA.



EXTRACTED FROM THE
TRANSACTIONS OF THE AMERICAN MEDICAL ASSOCIATION, 1881.

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ADDRESS IN MEDICINE.

I HAVE found it difficult to comply with the rule providing that the chairmen of the several Sections shall read addresses on the advances and discoveries of the past year in the branches of science included in their respective Sections. The past year, as regards the progress of practical medicine, has been marked, so far as I know, not so much by the announcement of new and important truths as by the accumulation of a vast number of carefully observed facts and of original investigations. These have already been presented to the medical world in the most accessible form in the various abstracts of medical science, and to summarize and reproduce them here would be an undertaking scarcely worthy of the audience I have the honor to address.

It has seemed to me, therefore, that it might not be inappropriate to offer a few practical remarks on some one of the medical topics that have occupied most attention of late. It may be safely stated that for some years the largest share of study has been devoted to those great general morbid processes which are grouped under the class of specific fevers or zymotic diseases. This is not astonishing. The wide prevalence and fatal character of some of these affections demand incessant efforts to detect and remove their causes, to limit their spread, and to modify their course. The enthusiastic zeal with which the study of all subjects pertaining to public health has been taken up of late years, and the hopes that are entertained widely, and probably with increasing justification, that this study will lead to a material decrease in the prevalence and fatality of these diseases, fully account for the extent to which the minds of medical men are occupied with the subjects of infection, contagion, zymosis, and the like. The theories upon these subjects have a fascinating interest, the methods of treatment based upon them are often ingenious and plausible, and it is most agreeable to have the

mind drawn away from the close study of the details of individual cases of disease to these broader fields of thought. It is true that there is a wide difference between the amount of fluent writing and talking about septic processes and zymosis, and the amount of positive demonstrable knowledge we have as yet acquired of these recondite matters. The terms and hypotheses that have been devised by investigators to assist them in their efforts to fathom the nature of epidemics have become the common property not only of the rest of us general practitioners, but of the public at large, of the sanitary engineer, and of the sanitary plumber; and the subject of blood-poisoning has rapidly become one of the fashionable topics of the day.

I speak with no lack of due respect and admiration for the earnest workers on these enormously difficult and important questions; and with no want of appreciation of what has already been done, or of hopeful confidence as to the triumphs of future investigators. But as a general practitioner I confess that I have been seriously impressed with the danger that follows from the too ready adoption of the popular theory of blood-poisoning to explain almost every case of obscure or anomalous character. Not only do we see this in regard to ill-defined febrile attacks without palpable local lesions, but the influence of a powerful school of medical writers is strongly in the direction of including one after another of what have heretofore been regarded as local affections, in the constantly enlarging class of specific, infectious, or zymotic diseases. It is no longer sufficient to recognize that there is an epidemic form of dysentery in which the system is unquestionably affected by some general morbid influence, and in which the disease presents unmistakable evidences of a septic character; nor to recognize that a case of idiopathic dysentery may give rise to a secondary infection of the system, with virulent septicæmia, through the absorption of putrid or septic matters from the seat of local disease. These conditions are familiar to us all; but we are now called on to admit that dysentery itself is essentially a zymotic disease, dependent upon a specific virus. What is true of dysentery may be said of several other affections; and I must admit that these claims seem to me too hasty and based upon insufficient evidence.

If it were a matter of mere speculative interest limited to the region of theory, it would matter little which view was held temporarily until the progress of accurate knowledge shall decide

finally on these points. But, unfortunately, it is impossible not to see that most practical interests are involved. When once it is supposed that a disease is a zymotic one, or that in any given case an infectious element exists, there is danger that it will be assumed that, as there is a specific poisonous cause, the symptoms are the necessary result of its action, and that the course of the case will exclusively follow and depend upon the evolution of the poison. The result of such an assumption on the treatment pursued must be serious. It will lead to mere scientific expectancy, and to the use of routine methods, to the disregard of that watchful and minute care in adapting remedies to the morbid conditions and the functional derangements present that constitutes the sole basis of rational and successful practice.

Let us consider this important matter somewhat more in detail. In the first place, it is clear that there are some diseases that may be taken as examples of pure specific zymosis, to employ the current term. Typhus fever occurs to me as perhaps the best instance of the kind. In uncomplicated cases of typhus we have an intense disturbance of general nutrition without any local lesion of the solids that modifies materially the course of the case. The symptoms are the result of the action of a specific cause; no antidote to that poison has yet been found; experience has shown that it will be eliminated from the system in a certain definite time, and therefore it is clear that our treatment resolves itself into the use of carefully adjusted means for supporting the patient's strength until this elimination is completed. If any one symptom attain dangerous prominence, we attempt empirically to moderate it. But typhus fever stands pretty much alone in its freedom from local lesions or complications, so that the above theory of treatment is applicable to other zymotic diseases only with considerable modifications.

Again, it is clear that if we possessed a perfect antidote for the poison of a zymotic disease, our medicinal treatment would resolve itself into the administration of that antidote in suitable doses and forms, with due regard to the state of the digestive organs. It seems as yet presumptuous to hope that we shall ever possess such antidotes for the acute infectious diseases as we have for syphilis or malaria. But I cannot omit a passing reference to the remarkable results that have recently been observed to follow the use of large doses of bichloride of mercury in diphtheria. The original paper that brought this subject to

my notice was by Dr. G. A. Linn, of Washington Co., and was published in the *Transactions of the Medical Society of Pennsylvania.* The extraordinary statements there made have been confirmed by the experiences, as yet unpublished, of several competent observers. A recent experience of my own was to me so striking that I may be pardoned for alluding briefly to it. I saw last month with Dr. T. J. Yarrow, of Philadelphia, a child of five years old, on the fifth day of a grave diphtheritic croup. Beginning with marked pseudo-membranous deposit on the tonsils, croup had supervened on the third day, and suffocation soon became imminent. Alum emetics secured the discharge of several unusually large and thick sheets of membrane, evidently from the whole length of the larynx. But slight relief followed, and on the fifth day it was clear that the membranous exudation occupied the bronchi. She was repeatedly made to breathe the vapor of slaking lime; emetics were given when suffocative symptoms became very urgent; quinia, chlorate of potash, and senega were given internally; milk and brandy were given as freely as possible. By the seventh day death seemed unavoidable. Aphonia had existed for five days; nourishment was refused almost entirely; the pulse was over 160, very small and feeble; the respirations were 70, and mere shallow gasps. There were marked dulness around the root of each lung, and impaired resonance over the lower lobes; the respiratory murmur was scarcely audible, and was mixed with feeble râles. Glandular enlargement had been slight, but the systemic infection was indicated by decided albuminuria, with quite numerous granular epithelial casts. The extremities were cool; the lips livid; the expression indicated advanced asphyxia. Dr. Yarrow and I agreed we had never seen a patient recover from such a condition; but it was decided to administer bichloride of mercury gr. $\frac{1}{12}$ every second hour, given in solution in elixir of bismuth and pepsin (as recommended by Dr. G. A. Linn), each dose also containing two drops of tr. nux vomica. No other treatment was used. In the next forty-eight hours a half grain of corrosive sublimate was taken. No movement of the bowels occurred, and not the least irritation of the stomach; on the contrary, a willingness to take nourishment began to reappear; and by the end of that time it was evident that the exudation was softening, and that more air was entering the lungs. The same prescription was continued with gradually decreasing frequency for a week, at the

end of which time convalescence was fully established; the urine gradually became normal; and the child is now in perfect health.

The extraordinary tolerance of such large doses of bichloride of mercury, and the rapid and progressive improvement from so desperate a state, make me regard this as one of the most remarkable cases I have ever witnessed, and will certainly induce me to give further trial to this remedy given in the same manner in cases of grave diphtheritic infection.

Unfortunately, we, as yet, know of no remedies that can be applied with such results, in most zymotic affections, although we cannot fail to see that such instances should caution us against the policy of mere expectancy and inaction that is so much in vogue in the treatment of all specific diseases, under the influence of the Natural History School.

The most serious practical injury that seems to me to follow the excessive prevalence of the doctrine of specific self-limited diseases, is the tendency to under-estimate the importance of local lesions, and of the peculiarities of individual constitutions, as explaining the symptoms and determining the course of diseases. Even in typically specific diseases, as for instance in typhoid fever, observation convinces me daily more and more strongly that it is impossible to get the best results from treatment, without paying the most careful attention to these questions. There are some local lesions, such as croupous pneumonia or meningitis, that may appear as complications of specific fevers, and which are much less frequent, and also much less apt to be overlooked, than catarrhal lesions of the mucous membranes. Nor is there any danger of being suspected of an attempt to revive the exaggerated, but most suggestive doctrines of Broussais, by dwelling strongly on the enormous importance of these catarrhal lesions, both as complications of specific diseases, and as the essential cause of many cases that are mistaken for specific febrile affections. Rindfleisch certainly does not exaggerate when he says that "the larger half of all the diseases to which humanity is liable, consists of catarrhal affections of mucous membranes, or of disorders complicated by them." In no other country in the world, and among no other people, can this statement be more clearly true than it is here among ourselves. Our physical characteristics, and the conditions of the American climate or climates render this incontestable. But to realize

this truth, it is necessary to conceive of catarrhal affections in their broadest sense. The conditions that attend acute catarrhs are congestion and swelling of the mucous membrane, enlargement of its lymphatic follicles, increase of, and changes in its secretions. It is, indeed, difficult to appreciate clinically more than the last of these conditions, and the term "catarrh" applies strictly only to it. But while in many cases the secretions are excessive, as in cholera morbus or dysentery or acute bronchitis, and constitute the most prominent symptom, there are many cases of catarrhal inflammation, where the secretions are perverted, but not excessive, so that the presence of the lesion cannot be recognized by any so-called catarrhal discharges. These are cases where, owing to individual peculiarity, or to the localization of the irritation upon some special anatomical elements of the mucous membrane, the amount of muco-serous discharge bears no proportion to the amount of epithelial proliferation or of glandular swelling. One of the best illustrations of this is the alveolar catarrh of the lung—a circumscribed catarrhal pneumonia—often involving small areas, near the roots, at the apex, or, indeed, in any part of the organ. It is an excessively frequent affection, and I have no hesitation in expressing my belief that, owing to the frequency with which it is overlooked and neglected, it constitutes the most common starting point of that most common form of consumption, catarrhal phthisis.

There may be scarcely any expectoration, and a cough that is not very annoying, and that may readily be regarded as merely nervous or irritative. Yet the capacity of localized catarrhs to induce marked symptoms of constitutional disturbance, is admirably shown in these cases. There will very often be observed a distinct febrile movement of an irregular, and often of a distinctly intermittent character; and, in consequence of the want of symptoms of marked pulmonary irritation, such cases are continually being mistaken for malarial fever, and being treated with so little vigor that, although the fever subsides, the local lesion is left smouldering, till renewed congestion awakens fresh activity, or till slow, cheesy transformation occurs and paves the way for septic absorption and the development of tuberculosis. Nothing but an unvarying practice of thorough, minute physical exploration of the lungs, in every case of acute disease, will obviate these fatal oversights; and I have no doubt that the

extreme readiness with which it is customary to regard irregular febrile attacks as malarial, has contributed powerfully to prevent this critical search for slight local lesions in such cases.

The fact that circumscribed catarrhal irritation of a mucous membrane, unattended with any considerable amount of catarrhal discharge, is able to excite marked febrile action, seems to me a matter of the greatest practical importance.

The theory that increased tissue metamorphosis and chemical interchange are the essential cause of, and commensurate with the elevation of temperature in febrile diseases, has been the prevalent one for some years past. According to this, it is easy to explain the fever that attends any inflammatory lesion of considerable magnitude. But practically, it has seemed to me to be the custom, if some inflammatory centre of adequate extent cannot be detected, to suspect that the fever is a specific one, due to the admission of an infectious principle into the system, and to blood-poisoning, and that the increased chemical changes in the blood are the cause of the febrile movement. This is merely another illustration of the present exaggerated tendency to regard diseases as specific, to which allusion has already been made; and in the hands of certain investigators, chiefly of the German school, whose writings have been so indefatigably and indiscriminately thrust on the mass of American medical students, it has led to methods of practice that appear to me superficial or irrational or mischievous. The degree of fever-heat has become a definite entity, to be determined, of course, by repeated thermometric observations, and to be accepted as dictating absolutely the line of practice. The system is to be supported by abundant food and free stimulation, and the fever reduced by measures proportionate in vigor to its intensity; and if these two indications can be successfully complied with till the self-limited stadium of the disease has expired, the treatment has been successful. It matters little what may be the particular antipyretic method that is associated with each degree of fever-heat, whether the patient is to have a wet pack or a cold bath so soon and so often as his temperature reaches 103°, or whether these may be postponed till it reaches 104°; that against which clinical experience protests, is the one-sided and arbitrary nature of all such rules.

We owe very much to recent observers, among whom Walton and Witherle, of our own country, rank prominently, for reas-

serting the complex and varied nature of fever, and thus furnishing a scientific basis for its treatment more in accordance with enlightened clinical observation. But it is chiefly from the elaborate and masterly experiments and reasoning of H. C. Wood, whose studies on the Pathology of Fever have recently been published by the Smithsonian Institution, and constitute, in my judgment, by far the most valuable contribution of the past year to scientific medicine, that the most complete conception of fever may be obtained.

It is impossible to assert that the positions assumed in this memoir have all been demonstrated as yet; but I think it will be generally admitted that "it has been shown that the degree of bodily temperature in fever depends, in greater or less measure, upon a disturbance in the natural play between the functions of heat-production and heat-dissipation, and is not an accurate measure of the intensity of increased chemical movements of the tissues."[1] It further appears altogether probable that an inhibitory thermic centre exists, and that this may be so depressed by morbid influences that tissue change and heat-production are increased; while, at the same time, owing to disturbance of the vaso-motor centres, the dissipation of heat is reduced. Some such complex mechanism, involving essentially the co-operation of the nervous system, certainly seems necessary to account for the peculiarities that fever presents in different individual cases of disease, and which call for more rational treatment than is recommended in most of the current writings on febrile diseases. Not only may the depression of the inhibitory centre be caused by the circulation in the blood of special morbid matters, but it seems clear that reflex irritation, from some centre of local disease, may be either so intense or so prolonged as to produce the same result. Moreover, it seems clear that such great differences exist between different individuals in the resisting power of the nerve centres, in this respect as in all others, that very different degrees of fever may result from an equal amount of local disease, or from an equal amount of septicæmia; and that the signification and therapeutic indication of given degrees of fever must vary correspondingly. In a case of typhoid fever, for instance, the temperature rises by the eighth day to 103° or 104°, and the pulse to 110 or 120. If this be the

[1] Fever: A Study in Morbid and Normal Physiology. Smithsonian Contributions to Knowledge, 1881.

result of a severe zymosis, with comparatively slight catarrhal irritation of the gastro-intestinal mucous membrane, and in a subject with good nervous tone, large doses of quinia or salicylic acid, and a full amount of suitable diet and stimulus, will doubtless be appropriate treatment; or if these fail, and the temperature continues to rise, the use of cold water externally is desirable. But, on the other hand, I have found a temperature of 104° + to 105° + reached daily for ten or twelve days, in cases of typhoid fever where the nervous, circulatory, and digestive symptoms all indicated that the septicæmia and the gastro-intestinal irritation were both mild, so that the high temperature was apparently due to a predisposition on the part of the nervous system to a failure of its heat-controlling function. In such cases, no serious harm has resulted from the sustained high temperature; and a simple plan of treatment, with scrupulous care to allay the intestinal irritation as much as possible, without any active measures to reduce the temperature, has been followed by satisfactory recovery.

In still other cases, where high temperature existed, associated with marked irritability of the gastro-intestinal mucous membrane, I have observed the use of powerful antipyretics, such as large doses of quinia or of salicylic acid (or of alcohol, given with the erroneous idea that it is an antipyretic in the ordinary sense of the word), to be followed by rising temperature, increasing pulse rate, and failing nervous power.

It was not uncommon, fifteen years ago, to see the steadily increasing aggravation of these symptoms, attended with failure of digestive power, diarrhœa and tympany, as steadily pursued by progressive increase along the whole line of treatment in the amount and frequency of the doses of quinia, turpentine, carbonate of ammonia, alcohol, milk, and beef-tea, until, finally, scarcely thirty minutes were allowed to intervene between the administration of some powerful drug or concentrated food.

It is not to be wondered at that the exhausted nervous system should become paralyzed by the intense reflex irritation from the inflamed mucous membrane, aided by the poisoning of the blood, which must certainly have been increased by filling the intestines with organic matter that could not be digested, but must have putrefied and furnished the most fertile soil for the continued development of fresh specific typhoid poison.

Broussais, who wrote his classical work on chronic inflamma-

tions before the distinction between typhoid and typhus fevers was recognized, and whose practical experience seems to have been almost exclusively with typhoid, naturally fell into the error of regarding lesions of the intestinal mucous membrane as an essential part of all fevers; and his judicious principles of treatment were carried to an extreme length by his zealous followers, so that the restricted diet and inert medication that was recommended in febrile diseases justly secured for Broussaisism, as it was termed, its rejection by the succeeding generation. But his real teachings were not thus extreme, and they embodied a truth, which has of late years been too much lost sight of under the influence of the modern theories of zymosis.

So fully am I impressed with the important part played by the gastro-intestinal mucous membrane in causing or aggravating the febrile symptoms of typhoid fever and other diseases, that I am convinced actual harm is often done by the use of remedies, calculated to reduce temperature when due solely to the blood-poisoning, but which are too irritating to be tolerated by a morbidly sensitive mucous surface. Undoubtedly the more general introduction of the external application of water as a means of reducing excessive pyrexia has done much good, both directly and indirectly, by checking the use of less efficient and more dangerous antipyretics. But I am satisfied that if the diet and treatment in the early stage of typhoid fever are carefully adapted to the degree of gastro-intestinal catarrh present, the need of cool baths or other extreme means of relieving hyperpyrexia will be comparatively infrequent.

This is not the time to enter into a more full discussion of the management of this interesting disease. So many different remedies and special methods of treatment have been vaunted, and their results attested by formidable statistics, that we may well marvel that there is any longer room for differences of opinion on the subject. But it seems to me that so long as this or any other general disease is regarded as a specific entity, and treated by any one special method, we shall never learn what is the lowest rate of mortality attainable. This result will be secured in regard to typhoid fever only when we employ a rational method, adapted to the complex condition present, and taking cognizance of the peculiarities of the individual constitution, and of the important local lesions as well as of the blood-poisoning.

If it is true that even in undoubtedly zymotic diseases the state of the mucous membrane, especially of the gastro-intestinal tract, has much to do with the production of the febrile symptoms, it is not surprising that we meet with many cases of continued fever that are dependent solely upon catarrhal irritation of this mucous surface. This catarrhal fever has been the subject of several well-considered articles during the past year, but I cannot believe that it is as well known as the frequency of its occurrence deserves. It arises from the ordinary causes of catarrhal inflammation, of which atmospheric conditions and changes are by far the most common, and runs a course resembling in many respects that of a mild typhoid fever. The duration varies from nine to twenty-one days; the febrile movement presents marked daily fluctuations; the tongue is coated, the abdomen rather full; the bowels occasionally loose, but more frequently quiet, though laxatives will provoke thin, light-colored, unhealthy stools; appetite is lost, and prostration is marked. Less commonly there may be associated nasal catarrh with or without epistaxis, or bronchial catarrh. The nervous symptoms are usually very mild, although in sensitive subjects the irritation of the nerves of the gastro-intestinal mucous membrane may excite quite severe reflex nervous disturbances.

The course of this form of fever may be greatly modified by treatment and diet, and it is probable that the statistics of typhoid fever are often vitiated by including a greater or smaller proportion of these simple catarrhal cases. The lesions must be of a moderate grade only, rarely going on to ulceration, since the prognosis is almost uniformly favorable. It is possible, however, by the continuance of bad hygienic influences, or by the use of too much and too strong food, and of free stimulation and of irritant remedies, to aggravate the catarrhal irritation, so that ulceration will ensue, the secretions will become more and more vitiated, and finally a secondary infection of the system may be established, with grave symptoms of toxæmia.

Lastly, it is interesting to note the important rôle that catarrh of the gastro-intestinal mucous membrane plays in connection with many inflammations of other organs. For instance, in pneumonia, the morbid influence that causes the pulmonary inflammation frequently excites also a gastro-intestinal catarrh that is really a distinct pathological condition, although it is customary to include its results among the symptoms of the

major disease. Practically, however, it demands that our treatment shall be adapted to the complex condition present, and any plan of treatment that addresses itself solely to the process in the lung must often prove ineffectual if not mischievous. Any one who has witnessed the excellent results of prompt treatment in acute pneumonia, including suitable depletion or counter-irritation according to the grade of the disease and the constitution of the individual; carefully restricted diet; absolute rest; sedatives and resolvents given with a critical regard for the state of the gastro-intestinal mucous membrane, and quinia, given, if need be, by the rectum so as to avoid increasing gastric irritation; will hesitate long before accepting the theory that this is a specific, self-limited fever, the symptoms of which are the necessary results of the evolution of a special poison in the blood. When we reflect upon the policy of inaction that is sure to follow the adoption of such a theory in regard to pneumonia, to rheumatism, or to dysentery; upon the disregard of important local conditions as being wholly subordinate to the hypothetical blood-poison; upon the tendency to direct treatment solely to the reduction of the elevated temperature without due regard to the complex mechanism and varying significance of such pyrexia, and by means which may at times react unfavorably on the organic conditions present; it seems to me full time that a halt shall be cried to the rapid spread of such speculative doctrines, which are open to severe criticism in theory, and which certainly seem opposed to the results of extended practical experience.

Important as are the varying expressions and relations of catarrhal inflammation in its acute forms, fully as great interest attaches to the study of chronic catarrhs. Close study of the course of very many chronic diseases will convince us that they do not depend on one continuous morbid process, pursuing a slow and necessary course; but that the affected part, having had its power of resistance reduced by the original inflammatory attack or by a series of congestions, has finally become the seat of chronic catarrhal changes that are continually being increased by the recurrence of fresh attacks upon more and more trifling provocation. This history finds constant illustration from every mucous surface of the body, and the fuller clinical study of these chronic catarrhal processes, in the light of our modern pathology, would be replete with interest. It must suffice here to allude

to the remarkable results produced, in persons of sensitive nervous temperament, by long-continued catarrhs, even of small extent. The prolonged local irritation ends by causing a morbid reflex excitability of the nerve centres with exhaustion of nerve power, such as we are so painfully familiar with in gleet, in chronic uterine catarrh, and in nervous dyspepsia. It is often one of the most difficult problems to decide, and upon the decision must depend the method of treatment to be pursued, whether an overtaxed or depressed state of nervous system has been the initial trouble, and has led to the impaired function of the stomach or uterus; or whether the long-continued local disease has gradually induced the nervous disturbances. All are familiar with both of these relations; and the powerful effect produced on the nerve centres by prolonged local catarrhs serves to illustrate the important part that is undoubtedly played by acute catarrhal irritation of the gastro-intestinal mucous membrane in the production of the nervous symptoms of many acute inflammatory or zymotic diseases.

The injurious effects of chronic catarrh are, of course, most marked when, as in the case of the catarrhs of the alimentary tract, they lead to mal-assimilation of food, and consequent depravation of the blood. It is not necessary that these catarrhs should be attended with such excessive secretions as to constitute a direct drain upon the system. There may be a single, partially soft stool, daily; or there may be a single undigested stool once in two or three days, and, during the intervals, entire quiet of the bowels; or finally there may even be habitual constipation, and yet all the while a slow catarrhal process may be present in the epithelial layers and lymphatic follices of the intestinal mucous membrane. In many cases of so-called nervous prostration, in many cases of apparently causeless anæmia, the essential cause of the morbid condition will be found on close search to be a localized catarrh of some part of the alimentary tract, the cure of which by suitable diet, hygiene, and drugs will be speedily followed by restoration to health.

A second and sufficiently common result of chronic catarrh is the maintenance of a slow irritative fever. Allusion has already been made to the irregular or intermittent fever that often attends acute alveolar catarrh of the lung, or acute catarrh of the gastro-intestinal mucous membrane, and which may readily be mistaken for malarial fever. So also I have not infrequently

met with cases where an obstinate, irregular febrile movement, which had been regarded as the result of chronic malaria, but had entirely failed to yield to antiperiodics, was in reality dependent on a chronic gastro-hepatic or gastro-intestinal catarrh. This observation must be a familiar one to all who live in malarial districts; and yet I suspect that the importance of such catarrhs, either as a complication of malarial attacks, or as causing a fever simulating malaria, is hardly sufficiently recognized. It is not necessary to assume that in such cases, where catarrhal inflammation maintains an irregular fever, there is produced any septic matter to be absorbed and to excite the pyrexia, through its action on the blood or on the nervous centres. It seems probable that the recent investigations as to the mechanism of fever will lead to a more simple explanation through the direct agency of the nervous system.

Allusion must finally be made to another, and the most serious manner in which chronic catarrhs influence the system, so as to produce constitutional diseases. When the catarrhal swelling of the mucous membrane of a duct, as of the common bile-duct, imprisons secretions rich in organic matters, decomposition is apt to occur, and poisonous matters are formed and absorbed, which give rise to serious septic fever, such as that with which we are familiar, under the name of "hepatic" fever, in the above-mentioned pathological condition.

More serious still are the consequences that arise when, owing to the anatomical arrangements of the part affected, as in case of the alveoli of the lung, the catarrhal products themselves are retained, and undergo slow cheesy degeneration. The enormous frequency of alveolar catarrh, the conclusive evidence of its tendency to terminate in this way, and the demonstration of the connection between softening cheesy deposits and infection and tuberculosis of the surrounding tissues, and of the system at large, are facts that give to this process a supreme practical interest and importance.

If, in other diseases, the tendency of the day to overlook the local lesion, and to assume the existence of a specific, self-limited, constitutional infection or affection, is to be deprecated, far more dangerous and prejudicial is such an unwarranted assumption in regard to that large proportion of cases of phthisis, which originate in catarrhal processes. There are, undoubtedly, rare cases of primary tuberculosis; there are also many cases

where the constitutional tendency to the acquisition of tuberculosis is so strong that it is impossible to prevent its development, from the most trifling provocation; there are still other cases where the first attack of catarrhal pneumonia, for instance, causes irreparable injury, and leads inevitably to destructive phthisis; but my own experience points clearly to the facts I have already stated: that, in the great majority of cases, the disease begins as an acute circumscribed alveolar catarrh, which, from inadequate treatment, from inherent weakness of tissue, or from neglect of hygienic rules, so that similar attacks are allowed to recur and recur, paves the way for a chronic catarrhal process, of which cheesy degeneration and ulcerative softening, with or without tuberculous infection, are the common and fatal consequences. So, too, I can draw no other conclusion, from careful and extended clinical observation, but that in its early stages this, the commonest form of phthisis, can be largely controlled by treatment, embracing hygienic, dietetic, and medicinal means, assiduously and persistently employed. It is, of course, evident that of any given number of such patients who are placed under fair hygienic conditions, and trusted to the influences of nature alone, some proportion will recover, because in them, after the removal of the depressing influences that have reduced the tone of the system, and rendered it susceptible to catarrhal attacks, the tissues retain sufficient vitality to react, and to prevent the extension of the local disease, and because no special tendency exists to tuberculous infection. But it seems equally evident to me that this proportion is capable of being greatly enlarged, and the duration of the disease greatly reduced, by the persistent employment of skilful and appropriate treatment. And further, it seems clear to me that, to attempt to establish a theory that phthisis is a self-limited disease, in any special sense of the term, is to ignore the teachings of modern pathology, and the results of clinical observation.

I have been led much further in these remarks than I had proposed to go, but I have been drawn on by a sense of the great practical importance of a clearer recognition of the large part played by local catarrhal inflammation, in the causation of symptoms now too readily attributed to some hypothetical specific or zymotic process. I do not feel at all satisfied with the theories of treatment, largely based upon these hypotheses, that are found in many recent works on medical practice. I am sure

they do not accord with the sound practical experience of American physicians, and do not meet the conditions of disease with which we are familiar. I am hopeful that ere long a truly national American system of medical thought and teaching will be developed, which shall be animated by a more practical spirit, and shall embody the results of the broadest and most thorough clinical work.