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Stevens (Ed. B.)

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THERAPEUTICS
OF
ZYMOTIC DISEASES.

—...—
BY EDWARD B. STEVENS, M. D., OF CINCINNATI.

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THE MATHS

BY J. J. D. J. J. J.

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THE THERAPEUTICS
OF
ZYMOTIC DISEASES.

—•••—
BY EDWARD B. STEVENS, M. D., OF CINCINNATI.
—•••—

There are two noteworthy points in the present aspect of pathology, viz: 1st—The gradual tendency or disposition to determine, seriatum, many diseases contagious—or in some way individually communicable, heretofore held in dispute; 2nd—a very steady tendency to return to the old doctrines of a blood pathology. To some extent these propositions are corollary; the determination of one group of pathological opinions suggesting the probability of the other.

Whether many of our present views are the result of a more exact knowledge than ever heretofore—or that we are simply traveling in medical cycles—will of course be themes for skeptical disputation.

Without attempting to reach real exhaustive views, I had the pleasure of reading to this Society some general remarks on the recent views of blood pathology at our last meeting. In that paper I briefly announced the importance of healthy blood—the uniformity with which certain changes occur in that fluid in particular individual diseases—and the general *idea* of zymosis.

At present there is a very well understood scope of pathology embraced in what is agreed upon as constituting the zymotic diseases. Thus, for instance, we have a large group of diseases which are supposed now to be developed by some *miasmatic*

or peculiar *infectious emanation*, and of which the *specific fevers* are the type, including cholera, erysipilas, pyemia, hospital gangrene, etc. Then we have a second group which are said to be *implanted* diseases—some material substance or specific poisonous principle is inoculated—*syphilis* being the type. We have properly enough a third group, made up of those diseases which result from deficient diet, or bad diet—*scurvy* being the type.

Now in most of these instances to understand what is taking place in the blood, we have only to dismiss the idea of vague mysterious agencies of disease—and regard them, whether known or unknown, as performing these operations after the same manner as do the well known and distinct poisons. Take for example the zymosis of mercury—of iodine—of arsenic; either of these poisons being capable of producing its own peculiar catalytic condition. For instance, what has happened when you have produced a mercurialization of a person? A certain well understood series of changes has been produced in the character of the blood; in general terms these are—*less red globules—less fibrin—less albumen*; and instead, you find *more water—certain foreign fetid matters*. Here is a zymotic or catalytic condition produced by the introduction into the blood of a peculiar—well known foreign or toxic element. The pathological train of events which are liable to manifest themselves with this changed condition of the blood constitute mercurial disease—just so we have certain, peculiar and uniform results following the introduction of arsenic or iodine or potash into the system. That we are able to take advantage of these properties of certain drugs for curative purposes does not in the least change the value of the illustration. Indeed it is quite possible, that the time may come, when we may control the infectious principle of ague, or erysipilas, or other specific diseases, to the most important curative results. At any rate, we can scarcely be at a loss to understand that each of these specific fevers, has its distinct poisonous origin. And just as belladonna exhibits its characteristic action by dilating the pupil, and throwing out a peculiar rash, and never varying in its specific change in the blood status, so are we to understand

that the poison of scarlatina, or variola, each tend to the production of its own uniform cutaneous eruption, as fixed as the blush of belladonna; and in like manner the infectious principle which produces ague, produces such an uniform and *peculiar* zymosis that periodicity, cold, fever, etc., will inevitably result. The processes which take place in erysipelas, gangrene, syphilis, etc., are still obscure, poorly understood for the most part, but the results are so uniform, that we can scarcely refuse to recognize uniformity of origin, and the presence of well defined elements of disease as distinct as mercury, iodine and arsenic.

One point somewhat peculiar to these infectious principles of disease, or at least contrasting with the poisonous drugs to which we have compared them, is the wonderful power of self-multiplication which seems to pertain to them. Indeed herein do we arrive at the idea of a proper zymotic process. As we have already said Liebig fancifully likening the changes which occur with the blood to the process of fermentation. And we fancy that in some respects the adoption of this term has been unfortunate. It tends to mislead—many persons seeming to regard all zymotic conditions as true or actual conditions of *ferment*, while properly the term is only introduced as expressing a *manner*—a family likeness. Doubtless the whole mystery is involved in the wonderful nature of the cell, and the rapidity with which infection multiplies itself when once introduced within the blood, being only one of the laws of its own cell growth. The smallest atom of varioloid poison multiplies itself so rapidly, and to such an extent as to involve the whole system, and endanger, by its modifying influences, all the functions of the individual. This is, doubtless, a cell development which the conditions of the system have favored—not a ferment; and there is probably a fallacy in many of the therapeutical suggestions of the day growing out of this tendency to regard zymotic conditions as only modifications of *actual ferments*.

Empiricism in remedies holds a strong place in our affections. We are prone to accept isolated specific views of treatment (or isolated specific remedies,) for isolated symptoms; being

misled by resemblances, without sufficient regard to the requirements of *rational* medicine. It certainly becomes us to dwell more than we are tempted to do, upon the relations of cause and effect; and in the application of remedies, when abstract results are claimed, contrary to well known laws, whether physical, chemical, or physiological, we may safely indulge in a fair degree of skepticism. Still we are not to forget that all true rational medicine is the growth of an accumulation of empiric results. A therapeutics adapting itself to these ideas of zymosis, however, is a legitimate result of rational inquiry; and no views in medicine are more generally received as true than some of the older theories of therapeutic processes.

Mr. Aitken in his new and elaborate work on "The Science and Practice of Medicine," makes *dietic diseases*, of which are purpura, scurvy, rickets, bronchocele and some other kindred diseases, his third order of zymotic affections. In all the diseases however of this order, there is no reason to say that we have a poisonous element introduced into the system, which at the expense of the blood proceeded to multiply itself and disintegrate that fluid. But the material for the supply of healthy blood and its regular manufacture for the uses of the system is withheld. The blood becomes thereby truly vicious—perhaps even we may say zymotic; but it is well to remember that no fair zymotic process has taken place; foreign elements are therefore not to be removed by processes of vital elimination, or eliminative therapeutics, so much as a restorative policy is demanded. Remedies—hygiene, food—which will restore deficient elements are called for; that is to say, for *dietic diseases*, we must resort to a *dietic plan* of medication.

A sufficient illustration of this point is afforded in the treatment of scurvy, which, indeed, Aitken regards as typical of this group. Citric acid, or potatoes, supplied in abundance to sufferers with scorbutic disease, affords to the system something which is essential to its healthy condition. Thus far this antiscorbutic principle remains undemonstrated, but these articles in an eminent degree, together with most vegetables, all unripe fruits, etc., serve as restoratives of the wanting

element. In other broken down states—the bitter tonics, astringents, the ferruginous preparations fulfil an important purpose; but when they do so, they all act upon the same philosophy, viz: they become agents which serve in some way to supply or restore elements which have become wanting, and which are necessary for the healthy functions of the blood. Now, in the treatment of this entire range of diseases, the introduction of remedies must have reference to these already established principles. Our therapeutics will only be improved by a more exact knowledge of the principles or elements of which the blood has become deprived, and a more precise mode, or a more prompt and efficient mode of supplying these essential properties.

In the case of *ergotism*, as indeed of any disease which results from poisonous vegetation mingled with the food, we come to have a condition of the blood more closely allied to what we understand as true zymosis, and the gangrenous form particularly, of ergotism, as described in certain epidemics, would suggest the idea of a multiplication of the fungoid cell growth of the spurred rye, though we by no means claim that any such cell multiplication can be shown; and it is possible that the poisonous principle of the fungus acts purely as a direct poison just as other poisonous matters—the result taking place just in proportion to the amount of the active principle introduced into the system. The therapeutics indicated are rational and evident. The blood is to be improved by correct and nourishing diet; tonics and stimulants to increase the vitality; and antispasmodics and narcotics for special or occasional symptoms. I say the therapeutics is rational, because the purpose to be accomplished is the elimination of mischievous materials, and the substitution of blood materials of a healthy character.

Syphilis represents a group of diseases in which a minute particle of poison has been implanted in some abraded tissue, and from thence as a focus “undergoes a multiple process of elaboration or development.”—*Aitken*. A certain pretty well established period of this process is a period of incubation—a certain extent of multiplication progresses—a certain degree

of disintegration occurs; tissues degenerate, and with the cachexin thus produced, certain uniform phenomena occur in a regular and peculiar order; and although we do not understand the steps which have taken place in the natural history of such diseases, yet we certainly know enough of its original virus, and the uniformity of the characters resulting from this process of elaboration, to arrive at just conclusions of the indication for treatment.

In the application of therapeutical principles, syphilis is the type of an order, including hydrophobia, glanders, malignant pustule, &c.,—each elaborating from a small implanted virus—after the same general plan; and we shall very certainly arrive at some equally definite agencies in each of these affections in the progress of our art. In all the changes of medicine, mercury almost universally holds its place in the confidence of the profession as the important remedy in the elimination of syphilitic poison. Formerly, I thought it a sufficient explanation of its mode of action to suppose these two to be *antagonistic poisons*. I think we are able to arrive at a better philosophy—one that will apply to the *modus operandi* of other therapeutical processes—to suppose that in the destructive process set up by mercury, the syphilitic principle is involved, and that the curative result takes place by the eliminating process, encouraged or augmented, and which tends to sweep out all of the toxic elements from the blood together, and that mercury is only a valuable therapeutic agent in the more complete elimination which may follow the mercurial poisoning in preference to the syphilitic poisoning.

It will be evident, we hope, that the purpose of this paper is not to present a didactic review of well known facts in medicine, nor to discuss, to any extent, their elaborate application, but simply to suggest the philosophy which underlies the therapeutical problems connected with this whole field of inquiry. We pass on, therefore, to a like brief notice of the zymotic processes in another group of diseases—that extended group which many writers are fond of styling the essential fevers, or miasmatic diseases—using the word miasm in its full, and, perhaps, exact sense, of some peculiar infectious principle.

The atmosphere undoubtedly carries the impalpable virus of many forms of disease. Ague is at once suggested to us as the most familiar of these miasmatic zymoses. It is surely a blood disease, though we are, perhaps, not entirely able to isolate its origin as we can that of variola or glanders. A quarter of a century ago, our distinguished fellow-citizen, Dr. Daniel Drake, made many researches amongst the bogs and swamps of the Miami Valley, to determine this question, without satisfactory results. Professor Salisbury has recently brought a like patience of research to bear upon this investigation, and reports his results with confidence, as demonstrating the cause of ague to reside in certain cryptogamous sporules, which he has named "ague plants," or "ague palmellae." Whether we shall be able, on further investigation, to accord free assent to the views of Professor Salisbury, remains for confirmation.* The probable pathological changes in intermittent disease is, however, not affected, and we have more evident to our inspection a beautiful illustration of that blood process which is so kindred to fermentation. Professor Salisbury states that these "ague plants" are capable of most prolific multiplication, and when absorbed into the circulation, they probably enter at once upon this process of duplication—a period of increase more or less definitely protracted, which probably corresponds to the period of incubation. The cold stage, the fever, the sweat, are evidently instinctive operations set up for eliminating the virus. It does so in part; but a part of these cryptogams, or zymotic principles, remain to go through another process of multiplication, and in a couple of days they have acquired such volume that we have a periodic return of the cold stage and its sequences. Now, according to the investigations of another reliable writer (*Headland*), these zymotic processes take place at the expense chiefly, or prominently, of the *taurine* of the blood. This authority has also come very ingeniously to the conclusion that the benefi-

*Dr. Salisbury's theory of ague, and supposed discovery of its cause, having been published in one of our most responsible American medical journals, has attracted a good deal of attention, both at home and abroad. In making allusion in this paper to these claims of Dr. Salisbury, we neither affirm nor deny their reliability. We simply use them as illustrating our views.

cent action of quinia, and other bitter vegetable tonics, is by virtue of its supplying a principle bearing a *strong elementary resemblance or affinity to taurine*.

There is no necessary contradiction in these views; and if both be true, we begin to arrive at some rational views of the therapeutics of all intermittent diseases of this group. Quinia acts *first* as a restorative, supplying necessary material for healthy blood action; and *secondly*, enables the system thereby more successfully to resist the influence of the miasmatic spore; but it does not destroy or eliminate this virus: the system tends to its own eliminative processes; the mucous secretions—the sweat—the urine—all engage in this work. But hence, also, when ague is held in check by quinia, it is probable we have sufficient spores still uneliminated to enter again upon a process of multiplication; so that after fourteen or twenty-one days the phenomena of ague are ready for repetition. And still further, we hereby understand why *arsenic*, whose action in the blood certainly favors very largely the eliminative process, may, and often does, eradicate ague germs, when persevering quininism has only held the disease in check. Quinia being what *Mr. Headland* styles a restorative blood remedy, supplying materials wanting, while arsenic is a calalytic, counteracting morbid materials, and conveying them out of the blood.

Now, I am satisfied that the rationale in the case of ague equally applies to a large number of Zymotic diseases. Take, for example, diphtheria. *Kuchenmeister*, of Dresden, first suggested that the diphtheretic deposit of croup or diphtheria proper, was soluble in lime water. Acting upon this hint, *M. Biermer*, Professor of Clinical Medicine in the University of Berne, made therapeutical experiments in the treatment of pseudo membranous deposits, with lime water applications, especially administering lime water pulverized or atomized, as by the apparatus of *Siegle*, so that the patient *inhaled* the atomized spray of lime water, thereby addressed to the local deposit, and at the same time taken with the atmosphere into the circulation, by way of the pulmonary parenchyma. In imitation of *M. Biermer*, a similar use of lime has been tried

in the treatment of severe cases of diphtheria, the vapor being inhaled during the process of slacking the lime. M. Biermer's cases gave evidence of improvement immediately with the inhalation of the remedy. Such repeated reports have been given as to render the lime treatment of diphtheria too important to be overlooked. When this agent is taken into the circulation it is reasonable to suppose that it supplies some alkaline principle which has either been consumed or neutralized by the process of zymotic multiplication of the diphtheritic poison, just as quinia supplies taurine in intermittent fever. While it is equally probable that other remedies may be generally indicated to meet the state of disintegration of the blood in this class of diseases, and sustain the system, while the process of elimination may be in course of completion.

The alkaline treatment of rheumatism, as ably taught by Mr. Fuller, and confirmed by the abundant experience of other practitioners, has an analagous explanation.

The views of Professor Polli, of Milan, have been largely reproduced in the medical journals of this country, and are, therefore, presumed to be familiar to physicians; still, it would be a singular oversight to make no allusion to his suggestions in such a paper as the present, especially when we must confess our profession is proverbially slow to accept and adopt—if not to learn new facts.

Professor Polli started on the assumption that zymotic diseases were the ferments, and hence to be neutralized by such agents as arrest this process out of the body. Hence his introduction of the alkaline sulphites as the remedies which should meet the desired want. The experiments of Professor Polli, so far as reported, appear chiefly confined to counteracting zymotic poisons intentionally introduced in the veins of the lower animals; as, for example, the virus of glanders in a dog. His results are very interesting, and apparently demonstrate that in some way these sulphites certainly do exert a very wonderful antagonism upon zymotic processes and conditions.

A large number of therapeutical experiments have been reported, based upon these suggestions of Dr. Polli, and many of them are highly important in their practical bearing. Dr.

De Ricci has contributed several papers for the *Dublin Quarterly Journal* which have attracted much attention. Regarding the reports of Polli, as demonstrating an actual discovery, he inferred that the sulphites must become practically applicable to the treatment of the whole range of these zymotic diseases; that if their action neutralizes so virulent a poison as glanders, it must serve to arrest the course of small pox, hydrophobia, erysipelas, infection from dissecting wounds, scarlatina, diphtheria, etc., etc., etc. He consequently set up extended series of therapeutical experiments in the treatment of this range of diseases.

I have already expressed my regret that the word *zymosis* has been adopted—*catalysis* would be freer from objection; and I very certainly object to the idea that these remedies act as mere neutralizers of a ferment. Nevertheless, De Ricci gives many cases where his anticipations are fully confirmed.

In a case of choleraic diarrhoea, induced by the inhaling of the infectious atmosphere of a death bed, he gave the bisulphite of soda, in 20 gr. doses, combined with infusion of quassia as a vehicle. This dose repeated every half hour, the symptoms speedily abated, and the patient convalesced satisfactorily.

Two cases of measles in a severe form; one case of poisoned wound, with constitutional symptoms, were severally treated in like manner with bisulphite of soda, in 20 gr. doses, and with rapid convalescence.

These accounts have rapidly been followed up with rose-colored reports—yet too numerous, and from gentlemen too careful and reliable to admit of serious doubt.

Dr. Cummins, of Ireland, has made a number of trials with the bisulphite of soda, more particularly in scarlatina; but he very rationally suggests that this remedy is, properly, only to be expected to serve a purpose of arresting catalytic processes in this disease during the stage of incubation. When the rash has made its appearance the noxious action has already reached its completion, and the system enters upon its efforts at elimination, and, therefore, no marked benefits are to be expected from this plan of treatment at this stage of these erup-

tive diseases. Dr. Cummins claims that in his hands the alkaline sulphites have served as an effectual prophylactic to typhus.

Dr. F. L. Monroe reports to the *Boston Medical Journal* his treatment of spotted fever and measles with the sulphites. At the Port Hospital, Galloupe's Island, Boston Harbor, 141 cases of measles were treated by Dr. Monroe, from March 1st, 1865 to May 12th. Most of these cases were treated only with 4 gr. doses bisulp sodæ, in syrup, repeated every two hours; only three cases resulted fatally after adopting this treatment, and he says "I discharged men about as rapidly as I admitted them."

Dr. W. V. V. Rosa, of Watertown, N. Y., reports for the *Lancet and Observer* a single case, of a scrofulous, anæmic condition supervening upon confinement, which he treated with marked and satisfactory results with sulphite of lime.

Perhaps, however, the most satisfactory report—because the fullest, and conducted upon the most systematic plan—is a paper read to the American Medical Association, in 1865, by Dr. Fisher, of Chicago. That paper now comes fresh to hand in the recently issued volume of *Transactions* for last year. Dr. Fisher reports an extensive series of experiments and observations personally, and embraces the individual reports of several of his neighbors of that city. His experiments are made in the whole range of zymotic diseases, and reports the use of sulphites of lime and sodæ in a large number of cases. Dr. Fisher remarks "that he has prescribed these sulphites with good success in almost every well marked blood poison case that he had been called upon to treat, without regard to the location or form of the disease—whether hospital gangrene, phlebitis, malignant erysipelas, scarlatina maligna, diphtheria, variola, uterine phlebitis, or malignant typhoid fever; not, however, to the exclusion of other remedies indicated in the particular case, but for the purpose of relieving the system of the constitutional effects of the poison, by preventing its morbid ferment or action on the vital fluid, until sufficient time had elapsed for the poison to be eliminated from the system."

In the cases treated by Dr. Fisher, the sulphites were ad-

ministered in 3i doses; repeated early in the case every two hours, afterwards at longer intervals. Professor N. S. Davis contributes to Dr. Fisher's report his experience with sulphite of sodæ in several cases of variola, erysipelas, cerebro spinal meningitis and puerperal phlebitis. He also prescribed 3i doses, at frequent intervals, and with valuable results.

It would not be just to forget that these views have been seriously combatted. Thus, my neighbor, Professor Bartholow, of Cincinnati, contributed two articles to the *Lancet and Observer*: one on measles and its treatment with sulphites, the other more especially discussing the whole doctrine of zymotic diseases, and his experience in the use of these remedies. Professor Bartholow conducts his experiments with much care, and we accord to him a large degree of patient, logical character in his investigations. He is skeptical as to any material therapeutical effect upon these cases with the sulphites, and denies the doctrine of fungoid origin and zymotic ferments. He furthermore expresses the confident opinion that the sulphites are uniformly converted into the sulphates in the stomach, and hence, chemically, there can not possibly be any advantage in their use. At present, we simply summon the large array of respectable and credible witnesses, to prove results; and certainly, just now the weight of testimony inclines to the important value of this class of medication in this extended group of diseases. Whether the explanation of these results will bear a strict analytical examination remains to be seen.

In our own opinion, the great mistake in the doctrine of sulphites in zymosis, is *first*, in supposing they are uniformly applicable to all cases of blood poison—although present reports indicates a very wide range; *and second*, that they arrest a process of fermentation as they do in ferments exterior to the body. When they are available in disease we doubt not it will prove to be just as quinine and arsenic are in ague, by supplying necessary principles in the blood, and aiding in processes of elimination, with possibly a third element of importance growing out of what seems to be the antagonism of poisons, as we see in opium and belladonna, or in alcohol and the virus of the snake

Some discussion of cholera as a blood disease, together with the incidental consideration of many kindred topics, come naturally to suggest themselves as growing out of the views I have attempted to bring out in this paper. But our purpose is rather suggestive, as already stated, than elaborative, as we dismiss the subject.

Some discussion of choice as a kind of chance, together with
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