

The anti-zymotic or anti-fermentive treatment of Asiatic cholera / by J. Scoffern.

Contributors

Scoffern, J. 1814-1882.
Royal College of Surgeons of England

Publication/Creation

London : Stanley Fowler, 1866 (Benjamin Pardon, printer)

Persistent URL

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THE ANTI-ZYMOTIC

OR

ANTI-FERMENTIVE TREATMENT

OF

ASIATIC CHOLERA.

BY

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LONDON:

BENJAMIN PARDON, PRINTER, PATERNOSTER ROW.

MDCCCLXVI.

PUBLISHED BY STANLEY FOWLER, PHARMACEUTICAL
CHEMIST, 36, ELGIN CRESCENT, NOTTING HILL.

THE ANTI-SYMPHONY

ANTI-FERMENTATIVE TREATMENT

OF
ASIANIC CHOLERA.

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MDCCCXXXV.

THE ANTI-ZYMOTIC OR ANTI-FERMENTIVE TREATMENT OF ASIATIC CHOLERA.

GRADUALLY the mystery that environed this terrible disease is clearing away, and pathologists begin to see the dawning of a law.

Many investigators, working independently of each other, and, consequently influenced by no predisposing bias, have come to the conclusion that cholera is generated and propagated by a ferment. The late lamented Dr. Snow was of opinion that the cholera germ, as we may familiarly call it, was distributed by the water of a locality, *almost* wholly, if not wholly. Subsequent investigations, but more especially those of French Pathologists last year, have proved that air is a means of propagation in no less a degree than water.

Having had occasion to study this French evidence as set forth in the *Comptes Rendus* of the Academy of Science, and other journals, I find it impossible to resist acquiescence to the general belief as to the fermentive origin of cholera. This study left a deep impression on my mind, and suggested a line

of treatment whereby reliance was placed on the administration of some convenient anti-ferment, given at an early stage of the disease. I have reduced the theory to practice with complete effect, according to the evidence of my own experience, and that supplied by the practice of some friends. The outlines of this practice having been made public through the columns of a daily newspaper, letters have poured in, beyond my power to answer individually, hence the origin of the following remarks.

In past years, and in a manufacturing capacity, I have had much experience with sulphurous acid and the sulphites. The action of this class of bodies in checking fermentations had long been a well-known fact to chemists, and gradually a mass of evidence had accumulated, which led the writer to suppose that these agents had been found competent to check the choleraic influence, whatever that influence may be.

In France last year, the immensity of persons employed in copper smelting works was so marked a fact, that on the strength of evidence thus supposed to be adduced, copper preparations were administered remedially or prophylactically.

No success attended this practice; and, reasoning on the premises no success should have been anticipated. Seeing that within the limits of the case, copper is not a volatile metal, it is not easy to understand how its incidence to the systems of the workmen could have been expected. Yet, only on the affirmative assumption, could the administration of copper be advocated.

But sulphurous acid, in abundance, is evolved from copper smelting works. Copper could not have been the preservative agent: sulphurous acid might.

From time immemorial, the fumes of burning sulphur have been used as a powerful disinfectant: now these fumes are nothing else than sulphurous acid.

In the years 1847-8-9, I was operating with sulphurous acid in a sugar refinery at Cork. The cholera was making great ravages there. Not one of our men was affected with the disease: not one had diarrhœa even. The only known or recognisable condition of life in which they were placed otherwise than their neighbours, was the atmosphere of sulphurous acid in which they were bathed, and had to breathe. Was their immunity fairly referable to the presence of this anti-putrescent disinfectant? Their case does not, of course, furnish any crucial proof; there are no crucial proofs in pathology. The issue, nevertheless, cannot be accepted in any other sense than as favourable to the general assumption of the anti-choleraic power of sulphurous acid.

Furthermore, in the year 1849, whilst cholera was so rife in London, a model room was fitted up at Shears's, the Copper-smiths, of Bankside, Southwark, one of the most affected localities. The model room was for the conducting of my sulphurous acid process of sugar refinery on a small scale. A staff of workmen was appointed to manage this process. Not one of them suffered from cholera, or even diarrhœa with any severity. Nevertheless, others of the establishment were stricken and died; in fact, Bankside, during 1849, was a veritable cholera nest.

These facts strongly impressed me at the time, but, being then much engaged in chemical pursuits, and wholly absolved from medical practice, I did not give them publicity, or carry them out to any practical development.

At the time to which reference is made, viz., the year 1849, the zymotic or fermentive theory of cholera had barely taken hold of the minds of pathologists; otherwise, probably the suggestion of administering sulphite of lime might have found immediate application.

In the years 1849 and 1850, I had occasion to be much in

Belgium, Holland, and Spain, superintending the establishment of my process of sugar refining by the conjoint action of sulphurous acid and acetate of lead. Cholera was present in each of these localities, but it never invaded the precincts of my factories.

Joining this personal evidence to much collateral, it would be a matter of surprise if, on resuming medical practice, I had failed to give trial to an agent which had seemed in my own case, and that of others, to have been of such unpremeditated avail. Reasoning on the premises, the presumption was that, granting the fermentive theory of cholera, the administration of an anti-ferment should promise success.

The fact need not be insisted on that pure uncombined sulphurous acid, or even the solution of this acid in water, would be wholly ineligible as a remedy for ingestion to the stomach. The only means of administration would seem to consist in uniting it with some convenient base; one that, acted on by the gastric juice, should liberate sulphurous acid in the stomach. Various considerations led me to adopt the sulphite of lime.

It is a white powder, almost tasteless, and not decomposable by ordinary atmospheric agencies. It is cheap, readily prepared, its strength never varies, and the means of testing its purity or impurity are easy.

The bisulphite contains more acid—the active agent—as the name “*bisulphite*” indicates, but it has the disadvantages of being more difficult to prepare; more prone to decomposition; more costly.

The investigations of French Pathologists conducted last year, are extremely valuable, as conveying much certain information relative to cholera, and the laws of its propagation; points that have hitherto been obscure.

First, in regard to contagion or infection, it was proved that the distribution of cholera, followed the line of motion of persons and things. This fact would seem on first consideration, to bear out the infection or contagion theory. It really does nothing of the kind. The question, whether cholera be immediately communicable, or whether it sets up a specific fermentation, must be studied and answered before a definitive reply can be adduced to the other question, whether cholera be infectious or contagious.

The determinate conclusion that cholera follows the line of motion of persons and things, is, however, enough for practice. That alone suffices to prompt the physician's measures and shape his action.

This *brochure* is popular rather than professional; seeing that the choleraic influence even more concerns the public than it does the profession; and though a remedial agent is indicated, I am one of those who believe in the superior efficacy of good ædileship, in the establishment of general hygienic measures, rather than in the power of any remedial agent or agency whatever.

A general belief in the efficacy of so called disinfectants in relation to cholera prevails, and has prevailed, but an especial point with me will be to show that nations (and the matter is national) have never yet proceeded on any large and philosophical plan of action.

Before disinfectant measures can be adopted with any degree of faith, it is above all things necessary that evidence should be adduced of their competence. It is not a general vague belief that we want, but a *certainty*. To aid in begetting that certainty the following facts are subjoined.

During the cholera accession at Marseilles last year, all letters and parcels arriving from Egypt and Turkey were disinfected in the entrance department; being disinfected, they

were given out at the next department, each department having its separate staff of officials.

The results were marked. Amongst the officials of the entrance department, several died of cholera. Of the others none had the disease.

The clothes of a man deceased of cholera were conveyed to the neighbourhood of Marseilles to be washed. Hitherto no case of cholera had broken out in that neighbourhood. *The laundress was stricken and died.*

These cases are quoted from memory, as sufficing for the occasion. It would be easy to multiply them. They leave no doubt as to two facts. (1) The communicability of cholera; (2) The abatement of the choleraic influence, whatever it may be, through the operation of sufficient disinfectants.

Demonstrations so marked and crucial are wanting here; nevertheless, English experience furnishes a loose corroboration.

Thus we find that at the present time in London the choleraic tendency everywhere prevails, inasmuch as nearly all are prone to diarrhœa; yet actual cholera only breaks out under circumstances of concentration, affecting certain localities. Waiving the question in what way the choleraic influence acts by contact or propinquity, we know it *does* so act, in proof whereof the death of nurses in attendance on choleraic sick.

One valuable deduction of French experience in relation to this disease is the easy curability of it if taken in time. The French Pathologists altogether demur to the view commonly received, according to which a functional distinction is made between choleraic diarrhœa, and collapsed cholera. Both are cholera they say; wherefore, to neglect the diarrhœa because it does not bear a choleraic name, is to violate reason, to disparage therapeutics, and to swell a death record in opposition to all sound statistical principles.

Notwithstanding all the admonitions long conveyed, the British public are not alive to the importance of attacking the first symptoms of diarrhœa. Far too commonly, these symptoms are either passed unregarded, or they are treated in a way repugnant to pathological indications. It is only by regarding diarrhœa in choleraic times as an integral stage of actual cholera, that the physician will have full scope afforded for the exercise of his art. On the necessity of attending to the symptoms of diarrhœa, all who have given heed to this matter are agreed; it remains to determine on the variety of treatment.

Four general systems had found favour in the early stages of cholera, not to state the one that it is an object of this notice to set forth. They may be individualized as the repressive, the eliminative, the acid, and the antacid.

The object of the first is to check, through the administration of opiates, aromatics, and astringents, the flow of gastric secretion.

The eliminative is based on the assumption that peccant matter should be evolved through the administration of purgatives.

The acid treatment consists in the administration of sulphuric acid; the pathological theory of which is not well known.

By the antacid system it is sought to neutralise free acids through the administration of chalk or alkaline carbonates.

Respecting these, I would remark that according to French experience, the eliminative system has been successful in the least degree. In point of fact the French physicians warmest in advocacy of this system once, have come to testify against it.

The treatment which the writer has seen cause to regard with most confidence, he calls the "*anti-zymotic*" or "*anti-fermentive*." It consists in the administration of small doses

of sulphite of lime alone, or in combination. I almost hesitate to state that the practice has been *invariably* successful in checking all such cases of diarrhœa as have come under my care, yet such is the absolute fact; and such is the experience of my neighbour and friend, Dr. McCormick, who has carried out the administration in his own practice.

The theory of the action of any medicine must ever be involved in doubt, and, therefore, open to debate; but, subsequent to the ingestion of sulphite of lime, certain results demonstrably follow.

First, it must be decomposed by certain free acids contained in the gastric juice; and the direct results of its decomposition must be the evolution of sulphurous acid gas. Second, the evolved gas is an agent far too energetic to remain in the digestive canal void of *some* operation. Operate it does, and energetically; in proof of which, however large the dose swallowed (I have myself taken a drachm) no sulphurous acid is ever eructated or otherwise extruded from the system. What, then, becomes of it?

I believe that it exerts its force in deoxygenating fermentive matters, and thus checking fermentation; similarly to what would have happened if operating exterior to the vital organism.

Dr. McCormick and myself, in our respective practices, have usually administered the following to adults:—

R Calcis sulphitis, drachmam.

Pulveris ipecac. comp., grana viginti.

Pulveris hydrarg. cum creta grana viginti.

Misce et in chartulas iv. distribue: quarum una tertiis horis ingeratur.

My practice has been to administer each dose in a small quantity of beef-tea thickened with arrowroot. Beef-tea is best extemporised from the *extractum carnis* of Baron Liebig.

To children, I have administered small doses of the sulphite in cinnamon water, after every relaxed discharge. I have given, in this way, five-grain doses to a child only one year old, and with complete effect.

It need hardly be stated that I do not recommend the zymotic treatment as suitable to the advanced stages of cholera. On the contrary, in that case I should entertain no hope of its success. Deferring to the opinion of those who have had much experience with cholera treatment, in its collapsed or advanced stage, I should be disposed to have most confidence in the inhalation of some highly oxygenated gas, probably the protoxide of nitrogen. This is the opinion of my friend, Dr. McCormick; who has asked me to co-operate with him in preparing a medical breathing apparatus. I am amongst those who believe that the therapeutic value of breathing artificial atmospheres will be demonstrated after sufficient trial. It never yet *has* been adequately tried. The experiment in its most perfect development could best be tested in hospitals and other large medical establishments. The aërial treatment of disease has hardly yet been advanced beyond the stage of applying an air bag to a patient's lips. A far more efficient plan would consist in immersing the patient in a complete air or gas bath.

HYGIENIC PROPHYLACTICS.

IN this most important department, the executive of these islands is wofully deficient. We want a little enlightened despotism. The case requires an ædile dictator, absolved from the trammels of board routine and parochialism. Let us scrutinize, seriatim, a few of British hygienic operations, and examine what they are worth.

Lime-washing.—Nobody doubts but that milk of lime is a powerful disinfectant, in its way; but milk of lime is not lime wash, according to a plasterer's notions; and British hygienists (not hygiests) have not seen fit to take the matter out of the plasterer's hands. British lime-wash is made up of milk of lime (efficacious), whiting (inert), and animal size (putrescent and highly prejudicial). I believe ordinary British whitewash to do more harm than good.

Sewer flushing by water.—It is astonishing that a people, so mechanical in most things as the English, should act so inconsistently in much that relates to sewage economy. Who, knowing anything of the qualities and agencies of a gas, can ever believe in the repressive power of "*stink traps*?" The trapping of a sewer in one place may, indeed, diminish the outflow of a gas at that one point, but the operation constantly tends to determine a gaseous outflow at other points. To restrain gases in tubes of brick and mortar (sewers), is obviously absurd. The very porosity of the material solicits emergence, through the well-known function of gaseous exosmose.

Water flushing exalts the gaseous evil. If a pint measure, holding noxious gas, have water poured in, then, proportionately, as the water enters, the gas outflows. This is a necessity; and this is the result of water flushing.

Why has not the expedient dawned to our ædiles of having recourse to aerial flushing? Nothing can be more easy; few expedients less costly. Suppose a few brick shafts to be run up vertically from the sewage system, each shaft charged with glowing coke in suitable frame work. Under these circumstances, there would be an upward draught; the sewage air would pass through the glowing coke; all organic matters would be decomposed,—*burnt*; all hydrosulphuric acid—a most

noxious poison—would be changed, through combustion, into *sulphurous acid*: thereby, a poison would be actually metamorphosed into a disinfectant. By such a system of aërial flushing, it is demonstrable that all noxious miasms, and poisonous gases, would be extracted from the sewers and converted into harmless or even disinfectant results.

Ventilation.—A German writer (Kohl), wrote that the English have two dominant crazes: one, ventilation; one, evangelization of the Chinese. Pure air, of course, is good; but can even the most perfect means of ventilation, as ventilation is conducted, furnish air to a chamber of greater purity than the general stock of air outside? Evidently not. Then, why not purify it in transitu, during its entrance to a chamber? In many private houses, this could hardly be done, I am aware; but nothing would be more easy in the case of hospital wards specially adapted to the sick. Given, a close box (a chamber), the supply of air forced into that chamber through chemical materials, competent to purify it, then, would not that air be pure? Supposing all the air supplied to a sick chamber to be forced through two vessels, one holding oil of vitriol, the other, slaked lime (both cheap enough), would not the entrant air be pure? *Must* it not be, if chemistry be a certain science, and if chemical processes be not a delusion?

Does it not convey a reproach, moreover, to the British Board of Health, that the choleraic atmosphere has not been analysed competently and exhaustively? Hitherto, aërial analysis has merely involved operations upon a few hundred, or, at most, thousands, of cubic inches. Results have been perfect, so far as they have gone; but who, amongst chemical pathologists, would expect to evolve a miasm out of a few thousand cubic inches of air? The practical analysis would consist in trans-

mitting large volumes of air, measured through an ordinary gas meter, through chemical agents of known power; competent to dissolve and retain each, a certain class of impurities. This done, the air would go in impure, would come out pure. The impurities—miasms, or what not—would remain dissolved, and might, perhaps, be made amenable to analysis. The real analytic operation would turn, not upon the direct analysis of air, but of materials (each small in bulk) holding things the air had given up. No single experimenter could accomplish this, save by the expenditure of an amount of time, that very few individuals can command. It would easily fall within the competence of the Board of Health. Under such a treatment, we might even hope to isolate and determine the nature of the blue mist spoken of by Mr. Glaisher.

Chloride of lime.—Is efficacious if enough of it be used. This material, however, has grave disadvantages. The chlorine evolved, hurts the eyes, and is bad for the lungs. Chloride of lime, if thrown down water closets, rapidly destroys the metal fittings.

Sulphate of iron.—A handful in a pail of water, is, perhaps, the best material for the flushing of water closets.

Carbolic acid, I believe, would be useless but for the sulphurous acid which, either purposely or adventitiously, it holds.

Animal charcoal, has been proposed as a medium for water filtration. I have had more to do with animal charcoal than most people; and I know that no amount of animal charcoal can separate the odorous matter of putrid blood. I know, moreover, that the separative power of animal charcoal rapidly

decreases, and that, when the limit of its efficiency is reached, a putrescent fermentation is established in its bulk, exaggerating all it was intended to abate.

Ozone is the most powerful of all disinfectants, but it cannot be conveniently prepared and operated with.

Reverting to the medical use of sulphite of lime, I solicit the experience of those who may employ it. A plain record of cases is desired, failures as well as successes. The necessity of testing the agent by acetic acid, hydrochloric acid, and turmeric paper, or reddened litmus paper is imperative. Many unprincipled manufacturers are sending it out impure.

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