LECTURE

ON THE

DIFFUSION, PATHOLOGY, AND TREATMENT OF ASIATIC CHOLERA.

BY

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[AT the present juncture, when we are again threatened with an invasion of Asiatic cholera, it has appeared to us that the publication of that portion of Sir Thomas Watson's revised Lectures on Medicine, which relates to the mode of diffusion, pathology, and treatment of this disease, would be of considerable professional interest and advantage. We have accordingly requested and received authority to publish the following portions of this lecture from advance sheets of the new edition now passing through the press.]

A fourth great visitation of cholera—that of 1865-66—has come and gone since I last addressed you on this subject, and it has been far more fertile of instruction on many interesting points relative to the disease than any of the three preceding epidemics.

Very few, I imagine, of the original doubters remain unconvinced to the doctrine which I have held from the beginning, that epidemic cholera is *catching*: that it results from a material poison which is portable, capable of being conveyed from place to place, and communicated from person to person—or from inanimate substances to which it clings, such as articles of furniture, or clothing. That the morbid matter floats also in the air, and may be wafted about by its currents, is a general and well-founded belief. I think, with Dr. Baly, that when it travels over great distances, as from one country or region to another, it uses the vehicle of human intercourse; but that it may be and often is diffused over smaller spaces, as from one part of a town to another, or from a tainted port to a ship anchored to leeward, by the movements of the atmosphere. The long migrations of the disease are not made rapidly. Its rate of progress never exceeds, and is often slower than, that of modern travelling. Its primary appearance in an island or a kingdom is always at its outer boundary. In our own country, for example, it first planted its foot in a seaport town on the *east* coast, over against the mainland where cholera was raging, and whence ships had very recently arrived. The same is true of its subsequent visitations. On the other hand, the crews of vessels sailing from healthy places remain free from the disease until they have entered an infected port, or held intercourse with an infected shore.

In his statistical report of the Royal Navy, published in 1858, Dr. Bryson says:—"The medical records of the (naval) service have been searched in vain to discover an instance in which either cholera morbus or yellow fever made its appearance amongst a ship's company, unless one or more of the men or officers had previously—within at most twenty-one days—been exposed in some house, ship, or locality where the infectious virus which emanates from persons ill of the one or the other of these diseases existed. The spontaneous origin of either malady, far away from an infected locality, is unknown in the naval service."

That the atmosphere forms one vehicle of infection seems clearly proved by some incidents ascertained respecting the last epidemic before it struck this country. I copy them from the *Times* newspaper for October 15th or 16th, 1865:—"Five miles from Gibraltar stands the little town of San Roque; and San Roque and Gibraltar were abruptly smitten by the plague, not only on the same day, but almost at the same moment. At Gibraltar it was a sudden access of the malady; at San Roque a first outbreak. At a small town near Toulon the plague fell upon the place in the night; and thirty cases occurred simultaneously between evening and morning." (This, let me observe in passing, might *possibly*, though not probably, have happened from the use of drinking water as a vehicle of the poison.) "At Constantinople it was observed that, while the cholera was raging, all the sea-gulls which used to flit over the waters of the Bosphorus deserted the place, nor did they reappear till the disease had departed and the atmosphere became pure once more."

Compare this with an extract from the *Dublin Morning Register* respecting the first epidemic—that of 1832:—

"A Westport correspondent, upon whose veracity we place reliance, has communicated to us the following extraordinary fact:—In the demesne of the Marquis of Sligo, near Westport House, there is one of the largest rookeries in the west of Ireland. On the first or second

day of the appearance of cholera in this place, I was astonished to observe that all the rooks had disappeared; and for three weeks, during which the disease raged violently, these noisy tenants of the trees completely deserted their lofty habitations. In the meantime the revenue police found immense numbers of them lying dead upon the shore near Erris, about ten miles distant. Upon the decline of the malady within the last few days, several of the old birds have again appeared in the neighbourhood of the rookery; but some of them seemed unable, from exhaustion, to reach their nests. The number of birds now in the rookery is not a sixth of what it was three months ago."

A striking proof that the air may be a vehicle of infection—that the poison may enter the lungs with the breath—is furnished by the fact that two pilots took the disease in consequence of having their open boat towed by a ten-fathom rope at a considerable distance astern of the steamship *England*, on board of which cholera was raging. They were never on board the vessel. Both of them had cholera, and one of them died of it. Both took the disease home, and transmitted it to their families, near Halifax, where the disease had been unknown for many years.

But although the infection thus proceeding from the bodies or the excretions of the sick, and entering by the lungs the bodies of the healthy, may strike and destroy individuals here and there, it seems very doubtful whether the disorder can become epidemic, except in certain conditions of the atmosphere.

It appears from the extremely interesting report of Mr. Glashier on this subject, that "the first three epidemics were attended with a particular state of atmosphere, characterised by a prevalent mist" (he is speaking of London and its immediate neighbourhood)—"thin in high places, dense in low. During the height of the epidemic in all cases, the reading of the barometer was remarkably high, and the atmosphere thick. In 1849 and 1854, the temperature was above its average, and a total absence of rain, and a stillness of air amounting almost to calm, accompanied the progress of the disease on each occasion. In places near the river the night temperatures were high, with small diurnal range." He goes on to enumerate, as characteristic of the atmosphere at these periods, "a dense torpid mist; and air charged with the many impurities arising from the exhalations of the river and adjoining marshes; a deficiency of electricity; and (as shown in 1854) a total absence of ozone, most probably destroyed by the decomposition of the organic matter with which the air in these situations is strongly charged."

The ozone here mentioned is endowed, as I told you formerly, with peculiar purifying properties. It has a high oxidising power, in virtue of which it unites with, decomposes, and so destroys miasmata, while it is at the same time itself proportionately destroyed. There is no ground for ascribing cholera, as some have done, to the absence of ozone—except in the sense of there not having been a sufficient quantity of it in the atmosphere to counteract all the poisonous miasm which actually produces that disease. The total absence of ozone affords presumptive evidence of the presence of atmospheric impurities.

A remarkable law of *altitude*, that is of elevation above the level of the Thames, has been announced by Dr. Farr as governing the mortality from cholera in this metropolis; and if here, so doubtless, under similar circumstances, elsewhere. "The elevation," he says, "of the soil in London has a more constant relation with the mortality from cholera, than any other known element." The mortality is inversely as the altitude.

This law of altitude—so important and so practically valuable—is but an expression of the result of many concurrent circumstances. The material poison of cholera will be likely to gravitate, as the marsh poison gravitates, with which it has many points of analogy, to the lowest part of the atmosphere; where the high barometrical pressure is the greatest, and vaporous diffusion therefore the least; where unwholesome exhalations from the soil and from the water are the most abundant; where the dispersing and diluting influence of winds is least felt. Indeed the air may be completely stagnant while on the neighbouring heights a brisk breeze is blowing. The lower regions of the atmosphere are the hotter also as well as the moister; and under the agency of a high temperature the organic impurity with which the air is charged runs more readily into decomposition. The inverse law of altitude is therefore an intelligible law. We see also how it may sometimes be disturbed or broken, under exceptional circumstances.

At the time when Mr. Glaisher's observations were made, the river Thames had become, without metaphor, the common sewer of this enormous and ever-growing town. Foul with the daily and hourly influx of abominable filth, it was offensive to the senses, and a cause of added foulness to the incumbent atmosphere. When we learn from Mr. Glaisher that during the summer months the night temperature of the river is considerably above the minimum temperature of the air, and that its vast area was simmering all night long, and throwing off clouds

of noisome and noxious vapour, we need be at no loss to account for the special unhealthiness of those quarters of the town which lie nearest to its banks.

But however unwholesome and pernicious the atmosphere may thus become, it cannot generate cholera, unless the specific exciting poison of that disorder be present also. In the autumn of 1859 the Thames stank horribly; yet we had no cholera. On the other hand, there is good reason, I say, to believe that this poison can never create a spreading pestilence, unless it meets with a congenial atmosphere. The foul air lends force and diffusion to the poison, and aids, or causes, its increase.

Notwithstanding that the choleraic poison in an invisible and impalpable state may thus pervade, and be communicated through, the air, it had long been conjectured, and it is now perfectly certain, that (horrible thought), we may eat and drink the poison, and so obtain the disorder. That, as I shall have to tell you, is the case also with enteric fever; the discharges from the alimentary canal are at once the main outlet for the poison and the chief source of infection. The late Dr. Snow was the first to broach the notion that the poison may be *swallowed* with the food which we eat, or the water which we drink; and that its multiplication takes place within the system, whence, by the alimentary canal, a new and abundant stock of it is voided. He showed how easily portions of the rice-water excretions, colourless and but slightly odorous as they are, may, without our notice, come to adhere to our food during its preparation, or its consumption. And the horribly disgusting fact had been made too certain by the unchallengeable disclosures of the microscope, that the water which is supplied by the several water companies for domestic purposes to this great city habitually contained visible particles of human ordure. Some striking facts had been collected by Dr. Snow, which warranted the presumption that a most fearful outbreak of cholera in Soho was attributable to the water of a certain pump, contaminated from a neighbouring sewer. A remarkable converse fact had been reported by the late Sir William Lawrence. Bethlem Hospital, and an asylum for children called the House of Occupation, stand near together on an open space of ground between fourteen and sixteen acres in extent, lying in the parish of St. George, Southwark. Being dissatisfied with the filthy water then supplied by the Lambeth Company, the Governors some forty years ago sank Artesian wells on the premises, and the pure water thus procured is used exclusively in the two institutions, which between them number about seven hundred residents. There was not a single case of cholera in the Hospital or in the House of Occupation in any of the first three epidemics; although the disease prevailed extensively in the parish, and in the streets in their immediate vicinity.

The result of an inquiry suggested by the Board of Health into the effects of the consumption of impure water during the second and third cholera epidemics was favourable to Dr. Snow's theory. Mr. Simon reported that "the population drinking dirty water appeared to have suffered three and a half times as much mortality as the population drinking other water."

That cholera may indeed be contracted by drinking a mixture of choleraic discharges and water, is demonstrated with all the force, if not with the reality, of an experiment, by the facts thus stated by Mr. Macnamara, a gentleman practising in India:—"I may mention the circumstances of a case in which the most positive evidence exists as to the fact of fresh cholera dejecta having found their way into a vessel of drinking water, the mixture being exposed to the heat of the sun during the day. Early the following morning a small quantity of this water was swallowed by nineteen persons. (When partaken of, the liquid attracted no attention either by its appearance, taste, or smell.) They all remained perfectly well during the day, ate, drank, went to bed, and slept as usual. One of them, waking next morning, was seized with cholera; the remainder of the party passed through the second day perfectly well, but two more of them were attacked with cholera the next morning; all the others continued in good health till sunrise of the third day, when two more cases of cholera occurred. This was the last of the disease; the other fourteen men escaped absolutely free from diarrhoea, cholera, or the slightest malaise."

At the time of this remarkable occurrence there was no cholera in the neighbourhood, nor had there been for several years, nor, so far as Mr. Macnamara is aware, has there been since.

Mark here the period of incubation, varying from twenty-four hours to two or three days; mark also that the majority of those who drank the tainted water escaped unhurt; in other words, that some persons take the complaint more readily than others.

The epidemic of 1865-66 has illustrated in a very remarkable way the soundness of Dr. Snow's theory. The prevalence of the epidemic in this country was clearly a step forwards in the progress of the malady in its rapid advance from Mecca to Egypt, and thence to various places on the eastern and southern coasts of Europe, and in the basin of the

Mediterranean. Mr. Simon lays it down as an axiom to be generally accepted in State Medicine, that "contagions current on the continent of Europe must be deemed virtually current in England." The disorder first showed itself here in the autumn of 1865, as usual at a seaport—Southampton. Then, as usual, it slept apparently for a while, to reappear and diffuse itself, after fresh importations from the Continent, and at its customary rate of increase, in the spring and summer of 1866; when, in the middle of July, there occurred in the eastern part of London an increase of the disorder so sudden, vast, and rapid as to warrant its being spoken of as an explosion. This outburst was limited to a certain definite and remarkable area, the line of limitation having an obvious relation, not to *soils*, but to *houses*; it was contemporaneous over that area, and stopped short abruptly within and along the line of limitation. It had a week's duration only. Its cause began to act during the week ending on July 14, and ceased to act in the week following. On the sixth day of its increased activity cholera had appeared in every portion of the before-mentioned area; the rate of its increase, as compared with the previous week, was nearly seven times greater than in the rest of the metropolis; while in the subsequent week the rate of increase became virtually the same over the whole of London. It is worthy of remark that there had been no undue prevalence of diarrhoea in the affected area.

This strange and definite outbreak must have had some adequate and definite cause; and, upon careful search, there was found evidence only just short of demonstrative proof—evidence which I cannot stop to state in detail, but which you may study in the ninth Report of the Medical Officer of the Privy Council—that this local calamity was produced by the temporary distribution to the area in question of unfiltered and infected water from certain reservoirs of the East London Water Company.

The peculiar blue mist which was noticed in the epidemic of 1854 was present also in the last epidemic. Mr. Glaisher says of it:—"On some days no trace of the mist has been visible; on other days it has been seen for parts of the day only. It has extended from Aberdeen to the Isle of Wight. This mist increased in intensity when viewed through a telescope; usually no mist can be seen when thus viewed." As in previous epidemics, there was a marked deficiency of ozone in the atmosphere. In other respects the meteorological phenomena were in remarkable contrast with those which had occurred during previous visitations of the cholera, and the law of altitude was broken by the predominance of more powerful influences.

With respect to the mode of propagation of the disease, Mr. Simon uses this strong language:—"It cannot be too distinctly understood that the person who contracts cholera in this country is *ipso facto* demonstrated with almost absolute certainty to have been exposed to excremental pollution. Excrement-sodden earth, excrement-reeking air, excrement-tainted water—these are for us the causes of cholera."

He adds: "The local conditions of safety are, above all, these two: (1) that, by appropriate structural works, all the excremental produce of the population shall be so promptly and so thoroughly removed, that the inhabited place, in its air and soil, shall be absolutely without faecal impurities; and (2) that the water supply of the population shall be derived from such sources, and conveyed in such channels, that its contamination by excrement is impossible." And he concludes with the pious hope that "for a population to be poisoned by its own excrement will some day be deemed ignominious and intolerable."

Our knowledge of the *morbid anatomy* of cholera has become more complete and more exact during the last epidemic.

Drawing my conclusions not from any experience of my own, but from numerous and very careful *post-mortem* inspections made by Dr. Parkes, by Dr. Johnson, by Dr. Sutton, and by others, I believe it may be stated as a rule—a rule broken sometimes, no doubt, by disturbing but intelligible circumstances—that in cases of death *during collapse*, when the examination is made sufficiently early, the lungs are found to be shrunken, light, dry, and pale—in one word, unnaturally bloodless; the left ventricle of the heart is contracted and nearly empty; its right cavities, the trunk of the pulmonary artery, and the systemic veins, much distended with blood; the mucous membrane of the intestines free from congestion, and pale.

In some of these cases the lungs, though very light in weight, are of a dark colour, which gives them an appearance of congestion. This colour Dr. Johnson refers to a backward engorgement of the *bronchial* veins and capillaries, consequent upon the block in the pulmonary artery and its branches.

When death has occurred during incipient and imperfect reaction, the morbid conditions disclosed by dissection are the reverse of these. The lungs are congested, sometimes even inflamed; and the mucous membrane of the intestines is also loaded with blood.

These are points which bear closely upon the pathology of the dis-

case; and upon its true pathology rests its rational treatment. To these I now turn.

There are two conflicting theories as to the pathology of cholera; and there are two conflicting principles—which accord with and flow from these theories respectively—as to its proper treatment. Upon this momentous problem of treatment, the final appeal must clearly be made to experience.

It is acknowledged on all hands that the primary and special danger in cholera lies in its period of collapse. Now it was a very natural and plausible theory which attributed this state of collapse to a drain upon the blood by the profuse and repeated fluxes from the stomach and bowels, whereby the blood, being robbed of its more liquid ingredients, and made thick like tar or treacle, became incapable of flowing freely, if at all, through its natural channels; and thus the circulation coming ultimately to a stop, life stopped also. And the practice suggested, and put in force, as a direct corollary to this theory, was that of endeavouring to arrest the destructive flux by astringent drugs and by opium, to sustain or urge on the lingering circulation, and to restore the spent strength and the lost animal warmth by alcoholic and other stimulants. Upon similar grounds was advocated the dilution of the thickened blood by water injected into the veins.

It is affirmed, on the other hand, that the condition called collapse is not due to the excessive discharges from the body; that those discharges are really eliminative of the poison, or of the products of the poison, which caused the disease, and are to be permitted, or even encouraged, rather than checked; that to pen the poison and its products within the body is to fight against the conservative forces, and to do what art can do to ensure the mortal agency of the poison, and, therefore, that astringents and opiates can do no good, but are, on the contrary, positively hurtful.

Were the first-mentioned theory true, there must be a discernible relation between the alleged cause and its effect. The greater the amount of the intestinal discharges, the more certain and the more decided should be the resulting collapse. But no such proportion has, in fact, been observed. Nay, the very reverse not seldom obtains. The most hopeless cases are those of collapse after very scanty discharges, or with no discharges at all.

Again, if the collapse were indeed owing to the drain upon the blood effected through the intestinal discharges, it would be prolonged, deepened, and rendered more perilous, by the continuance of those discharges; whereas it is notorious that patients emerge from the state of collapse, and recover, while the evacuations nevertheless go on, and that the cessation of the evacuations during collapse is a fatal sign. "It may confidently be asserted," says Dr. Parkes, "that there is no one who has seen much of cholera who does not know that, exclusive of the mildest forms of the disease, a case with little vomiting or purging is more malignant and more rapidly fatal than one in which these are prominent symptoms."

Tested, then, by the evidence of acknowledged facts, this theory must be pronounced a failure, and the treatment founded upon it a mistake.

In truth, a fallacious analogy has been assumed between the collapse or exhaustion arising from a drain upon the blood and the collapse in cholera. In one single point—namely, the smallness and weakness of the arterial pulse (in other words, the defective circulation of the blood)—the two may seem to touch each other. In almost every other point they differ widely. A person exhausted by loss of blood, or by a long continued drain upon that fluid, is in a state that is very near to syncope. When the exhaustion is extreme, if he assume the erect posture, he faints outright, and becomes unconscious. To walk, to stand, or even to sit up, is simply impossible; whereas, in the collapse of cholera, a patient, with death stamped apparently upon his features, with no pulse to be felt at his wrist, with a blue and icy-cold skin, may be able to walk about the room, and to perform many of his usual functions. He does this indeed at the peril of his life; but the fact that he is capable of such an effort proves that there is an essential difference between cholera collapse and ordinary syncope. The exhausted man, if he recover, recovers slowly; the repair of his impoverished blood is necessarily a gradual process. The cholera patient rallies from his collapse at once, if at all. He may be in full collapse to-day, and convalescent the day after to-morrow, and apparently but little the worse for the terrible disorder through which he has so recently passed. "I have seen," says Mr. Grainger, "a man stand at his door on Wednesday, who on Monday was in perfect collapse." Again, the way in which remedies tell upon the two contrasted conditions is totally and instructively unlike. The coldness and faintness of exhaustion are relieved at once by a glass of wine or of brandy; the pulse instantly acknowledges the virtue of the stimulus. But alcoholic stimulants do not warm or invigorate, even for a moment, the patient in choleraic col-

lapse; rather, they seem to make matters worse. On the other hand, blood-letting has often brought marvellous relief under collapse; while to draw blood from a person who is fainting from exhaustion would probably ensure his death, and would certainly aggravate his danger. Take the following instance, recorded by Sir Ranald Martin, of the effect of venæsection. "On visiting my hospital in the morning, the European farrier-major was reported to be dying of cholera. I found that during the night he had been drained of all the fluid portion of his blood. His appearance was surprisingly altered: his respiration was oppressed; the countenance sunk and livid; the circulation flagging in the extremities. I opened a vein in each arm; but it was long before I could obtain anything but trickling of dark treacly matter. At length the blood flowed, and by degrees the darkness was exchanged for more of the hue of nature. The farrier was not of robust health, but I bled him largely; when he, whom not a moment before I thought a dying man, stood up and exclaimed, 'Sir, you have made a new man of me'. He is still alive and well."

The question has naturally been put, "Is it possible to reconcile facts of this kind with the theory that the collapse of cholera results from the loss of the liquid constituents of the blood?" If Sir R. Martin's hypothetical statement that his patient "had been drained of all the fluid portion of his blood" were an accurate expression of facts, can we conceive it possible that he could have "made a new man" of him by extracting largely the blood which remained in the vessels?

The main advocate in this country, and, as I think, the triumphant advocate, of what may be called the evacuant or cleansing practice in cholera, is your present able professor of physic, Dr. George Johnson. To him is justly due the great merit of having established, by his persevering efforts in the face of much opposition and discouragement, the worth and efficacy of that practice, although he was not the first to recommend or to adopt it. It was, in fact, tried, with favourable results, nearly half a century ago, by English practitioners in India; its professed object being that of getting rid of offensive morbid secretions. The practice thus vouched for by Dr. Johnson is directly in accordance with, and serves to confirm, that view of the pathology of cholera which, by a methodical display of numerous facts, and by a process of close and conclusive reasoning, he may fairly challenge as his own. Briefly, he holds, as many before him held, that the phenomena of cholera result from the entrance of a peculiar poison into the blood, where it probably undergoes, like that of small-pox, a rapid process of self-multiplication, and spoils certain of the blood-constituents, which are then ejected through the mucous membrane of the alimentary canal; that the feelings of general oppression and *malaise* sometimes experienced before the onset of the bowel symptoms, are indicative of blood-poisoning; that the copious discharges are expressive of the efforts of nature to throw off a noxious material, and really form, therefore, a necessary part of the process of recovery; and that, if the pouring forth of the vascular excretion be checked (as it can, perhaps, be by opium), the risk of fatal collapse is thereby increased. He declares that the results of his own practice, founded on these views, have amply justified them; and a considerable body of other evidence has now been furnished in support of the same plan of treatment.

It is plain that, if "elimination" be a condition of recovery, the method of elimination is Nature's method, which Art may help or hinder—help by the cleansing method, hinder by the astringent.

In discussing the principle of treatment I have shot ahead of several points in the novel, interesting, and, to my mind, satisfactory exposition of the general pathology of cholera by Dr. Johnson.

Remember the abrupt contrast seen, upon early examination of the body after death during collapse, between the anæmic condition of the lungs, and the gorged condition of the trunk of the pulmonary artery and of the systemic veins. What is the explanation of this sudden arrest of the stream of blood in the small arteries, just before it reached the capillaries? Were the arrest of motion due to gradual thickening in consequence of the continued abstraction of its liquid portion, it would be found stagnating in the capillaries, as well as in the arteries. Bear in mind that one characteristic symptom of cholera—that symptom which, irrespectively of the fatality of the disease, renders it truly a disease to be dreaded—consists in very painful cramps of the larger muscles of the body. These contractions, it may be assumed, are produced by the choleraic poison, just as we know they are producible by the poison of strychnine. Dr. Johnson supposes that a similar spasm or cramped state of the muscular fibres which embrace the minute pulmonary arteries, is caused by the same choleraic poison, and bars these slender channels against the advancing blood: that the stopcock action which I have so often explained to you, comes here into play. The thickening of the blood is a consequence, and not a cause, of the arrested circulation and the collapse. Precisely the same blood-thickening occurs as a result of the impeded circulation through the lungs which is associated with long

continued, extreme, and fatal apnoea, as I have explained to you in a former lecture.

The true explanation of the fact that mere diarrhoea, however profuse, does not thicken the blood, is probably, as Dr. Johnson suggests, that water is rapidly absorbed from the soft tissues to take the place of that which escapes from the alimentary canal. Acting on this principle of physiological hydraulics, we remove a dropsical accumulation by the action of a hydragogue purgative.

Surely the theory that I have now placed before you seems a reasonable theory. It is founded on a true analogy; it is consistent with the symptoms noticed during life, and with the conditions discovered after death. We may, therefore, legitimately regard it, until fairly refuted, as a sound as well as a most ingenious and important theory. In truth, it derives strong confirmation from the fact that it unlocks, like the right key, the whole of the pathological intricacies of the disease. Thus the emptiness of the systemic arteries accounts for the extinction of the pulse at the wrist, for the cadaverous sinking in of the eyeballs and falling of the features, for the blueness and coldness of the skin, and for the absence of syncope. The circulation stops, not from debility of the heart, as in exhaustion, but in consequence of a direct mechanical impediment to the onward course of the blood. We can understand the impotence of brandy against this condition; and how, on the other hand, bleeding may help, both by relaxing the spasm and by unloading the distended right heart, to restore the circulation. Into this explanation Dr. Johnson presses, plausibly enough, the singular effect of the injection of fluids into the veins of these patients. It appears that, to be most influential, the fluids must be hot; and he concludes that they act partly by diluting the morbid blood, but chiefly by relaxing, through their warmth, the spasm of the smaller arteries. The blood then flows on again, and the symptoms of collapse are for a time removed. Again, the husky whispering voice is owing, not to muscular weakness, but to the small volume of tidal air in the respiratory currents. As but little venous blood reaches the lung-tissue proper, there is but little demand for air to meet and decarbonise it. The respiration accordingly becomes shallow, and the vocal pipe, feebly blown through, refuses to speak. Under the temporary impulse of the warm injections, the voice regains its usual tone and note. Once more, there are chemical and less obvious changes which receive their explanation from this theory, and further attest its truth. The stream of blood through the pulmonary capillaries being greatly lessened, the supply of oxygen is proportionally reduced in quantity. Hence during the stage of collapse there is defective oxygenation of the blood and of the various tissues of the body, coldness and blueness of the surface, diminished exhalation of carbonic acid, and suppression, nearly absolute, of bile and of urine—carbonic acid, and the chief constituents of bile and of urine, being all results of oxidation. That this is the correct explanation of the suppression of bile and urine during collapse is rendered all the more probable by the curious fact that, when a nursing mother becomes the subject of cholera, and falls into collapse, the secretion of milk continues unchecked. Now the chief constituents of milk—casein, sugar, oil, and water—may be obtained from the blood without the addition of oxygen. They are *not* products of oxidation.

If the doctrines advanced by Dr. Johnson be well-founded, as I firmly believe them to be, it must be wrong to dam the choleraic poison and its products within the body. Even when those products have, in one sense, been separated from the system, they may produce highly noxious effects if they remain shut up in the stomach or bowels, there to ferment and decompose. Admitting, as we must, that a minute quantity of the morbid excretions swallowed with water may suffice to produce the disease, a large quantity retained, through weakness of the expulsive powers or otherwise, can scarcely be harmless. Rather may we expect that its expulsion will tend to liberate the patient from danger and discomfort; just as the opening of large abscesses, and the discharge of foul pus and imprisoned gases, are often seen to rescue, as if by magic, a sick man from apparently impending dissolution. Whatever may have been Dr. Johnson's earlier purpose, he does not now propose to *excite* discharges from the mucous surface of the digestive canal; but simply to facilitate the removal of matters lodged there. And this he would do by emetics, by draughts of tepid water or other diluents, or by castor-oil, of which the action is both speedy and gentle. The recommendation of the evacuant plan must, after all, lie in its comparative success, and its worth has already been put closely and extensively to the proof.

In the fiftieth volume of the *Medico-Chirurgical Transactions* there is a most instructive communication from Drs. McCloy and Robertson. They show that, of 375 cases of cholera admitted into the Liverpool Parish Infirmary in the last epidemic, 161 proved fatal—a gross mortality, under all the modes of treatment adopted, of 42.93 per cent. Of these cases, 91 were treated with astringents and stimulants, camphor

and iced water, applications of ice, and hypodermic (opiate) injections; and the mortality per cent. of these cases was 71.42. 87 cases were treated with castor-oil, and with a liberal use of food and alcohol; and the mortality was 41.37 per cent. 197 cases were treated with castor-oil only, and the mortality was 30.45 per cent. The authors of the paper declare that "recovery never occurred without the continuance of the intestinal discharges; or their restoration, if previously arrested."

The late Inspector of Prisons, Mr. Perry, had charge, in 1832, of the cholera patients in the Marylebone Workhouse. He told me that, though he had no specific notes to refer to, he distinctly remembered that about thirty patients were treated with castor-oil; and that they did better than any of the others.

When I last spoke on this subject in these Lectures, I stated that the few recoveries which I had witnessed had all taken place under large and repeated doses of calomel, but that I could not venture to affirm that the calomel cured them. At present, I am much disposed to believe that, by its cleansing action, the calomel may have helped the recovery; and, after all that I have since seen, heard, read, and thought upon the matter, I must confess that, in the event of my having again to deal with the disorder, I should feel bound to adopt, in its generality, the evacuant theory and practice; and to avoid alcoholic stimulants and opiates.

Now, if this theory and practice in respect of cholera be true and right, the practice ought to be right in respect of the associated diarrhoea also; and it is strongly affirmed by those who have largely tried it, that it is right, inasmuch as it is eminently successful. Dr. Johnson avers that he has found it so.

Hear the concurring testimony of Drs. McCloy and Robertson. "Our experience of diarrhoea was very extensive. Several thousand cases came under our observation in the different dispensaries connected with the West Derby Union and in the Liverpool Parish Infirmary. Among these were doubtless many which would have recovered under any mode of treatment, or by the *vis medicatrix nature* alone. But there were many, too, of a most severe choleraic type. The treatment adopted was generally evacuant in its nature; and consisted in the administration of castor-oil, calomel, rhubarb, or magnesia. In every case relief was afforded 'pleasantly, quickly, and safely.' It was but seldom that more than two or three doses of oil were required." The medical officers of the Bootle Dispensary depose to the same effect: "We certainly had less trouble with the evacuant mode of treatment. Our patients seldom gave us a third visit; two doses of castor-oil or rhubarb mixture being generally sufficient to cure the disease." "We never saw a diarrhoea patient, treated with evacuants from the commencement of his attack, require subsequent removal to hospital. In a large proportion of our cases there was 'premonitory diarrhoea' which had been treated, often for four or five days, with astringents. Diarrhoea patients undoubtedly recover when treated with astringents; but the recovery is not consequent upon the arrest of the discharges, as these are invariably restored before the patient feels well."

In the face of this and of much similar evidence, I feel bound to say that the rules laid down by Dr. Johnson for the treatment and prevention of diarrhoea and cholera, seem now to me safer and better than the less discriminating advice which heretofore I gave you, "whenever a suspicion arose that cholera was present in the community, not to try, in cases of diarrhoea, to carry off the presumed offending matter, but to quiet the irritation and to stop the flux as soon as possible, by astringents, aromatics, and opiates."

No doubt, the true indication of treatment is, to stop the flux as soon as possible; but this may sometimes be best effected (as also in "crapulous diarrhoea, and in the summer cholera of Sydenham") "by carrying off the offending matter."

POISONOUS SILK GLOVES.—Dr. J. T. Dickson writes in a daily paper: On Saturday last a patient called upon me and exhibited her hands covered with very irritable blebs or blisters. After minute examination and questioning for cause, she told me that during the week she had purchased in Marylebone a new pair of silk gloves for two shillings, had worn them during a journey to Manchester and back, and that her hands had borne these vesications ever since. She wore the left glove more constantly than the right, and the left hand is consequently much more affected than the right. I desired her to bring the gloves to me, and I found that they were of good quality spun silk, dyed of a light-brown colour, the dye apparently being an aniline dye of coal-tar origin. Spun silk takes the bright colours made from aniline very well, but the use of articles so dyed is dangerous. In the museum of the College of Surgeons are preserved some brightly-coloured children's socks, which gave rise to a similar affection on the feet of a child (I believe in France). I also believe that the late Marquis of Hastings was once similarly affected.