and reduced to powder, they are given in drink to the patient. This preparation deserves a place beside the *oleum catullorum*.

There was a peculiarity of conformation in the bladder of this patient; it was bilobed and irregular, with fleshy columns on the right side. The stone was in the further cavity of the organ.

Case 2.—Mr. G——, aged 61 years, of nervous habit, had been for a very long time under treatment for catarrh of the bladder; his medical attendant suspected that the affection depended upon the presence of a stone, particularly as the patient had formerly passed small gravel, but could not prevail on him to be sounded. As he was experiencing no relief, he betook himself to the use of one of those advertised remedies *which cure all diseases*. Some little amendment, coincident with the commencement of this self-prescribed treatment, inspired him with great confidence in its efficacy. Long and scrupulously did he persevere in the use of his pills, of which he has assured me he often consumed four-and-twenty in a day, and grew worse.

He came to me, recommended by Mr. Heath of Nottinghamshire, upon whom I operated for stone about two years ago. The introduction of the sound, and the exploration of the bladder, gave considerable pain; the existence of the stone was ascertained, and the irritability of the urethra was relieved by the use of a wax bougie, introduced for a few minutes daily, for a week. During this time a mild regimen was prescribed, with the tepid hip-bath and opiates. In this manner some amelioration of the symptoms was obtained, and I was enabled to proceed with the operation. At the first sitting the patient manifested great anxiety lest the stone should prove too large for the grasp of the instrument, of this I promptly relieved him by telling him that it was now firmly fixed in the crotchets of the forceps. The stone was about the size of a small walnut, and friable. It was completely in fragments at the second sitting; and two more sittings sufficed for their entire removal. After the second sitting the irritability of the bladder ceased, indeed he might be said to suffer nothing more from his complaint; the urine, which had been very offensive, and loaded with mucus, had become clear; he ate and slept well, and seemed quite an altered man. The treatment of this case occupied only seventeen days from the commencement of the operation, and for the last ten of these he was able to walk, and attend to a little business. The gentlemen present at this operation were, Mr. Prater, Dr. Burke, Mr. Chapman, Mr. Ratray, Mr. Coleman, &c.

7, Parliament-street, Westminster,
January, 1833.

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REMARKS ON THE

MALIGNANT CHOLERA;

ITS PATHOLOGY AND MEDICAL TREATMENT.

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SPASMODIC CHOLERA has happily, at length, so far abated its fatal ravages in this island, as to allow medical men leisure from the harass, fatigue, and excitement, into which they have been thrown for twelve months past, to look around them, and review what has been done by themselves and their contemporaries in investigating this dreadful malady; consequently, we may now hope for some more satisfactory exposition of it than has yet obtained amid the contention of opposing factions, and the inveterate warfare between contagionists and anticontagionists. In every quarter of Great Britain, carefully-observed facts have been quoted, and a profusion of monographs have been published on the subject; some excellent, and all showing an ardent desire on the part of the profession to struggle successfully with this new pestilence. But, so far, no theory has received more than a partial assent, as accounting for the complex train of symptoms which accompany the disorder; nor has any principle of treatment been laid down, that furnishes a key to the operation of the various remedies which have been hitherto employed with apparent advantage. Both the nature and treatment of cholera are still involved in a mist of doubt and uncertainty: the most skilful research has failed to throw more light than that of probability on the one, nor has acute observation and experience, in many millions of cases, been more successful in establishing the other.* Under these circumstances, it affords gratification to the writer of this article to remark, that a careful perusal of much (it were vain to say all) that has been written on the disease, a rather extensive acquaintance with professional friends, who have had ample opportunities of observing and treating cholera, together with some personal experience, have tended strongly to confirm the opinions he advanced in a

* It was stated in THE LANCET, several months since, that twenty millions of persons have perished by cholera; but if we add up the statistical accounts, we shall find this far below the reality, eighteen millions were carried off in the East Indies alone. Journs estimates the total number of victims to the pest at two or three times that amount; therefore, averaging one-half of those attacked to have died, there must have been since 1817, upwards of one hundred millions of cases of spasmodic cholera!

paper read to the *Darlington Medical Society* on the 2nd of last January, and shortly afterwards to the *London University Medical Society*. The following observations contain a more explicit detail of the views he was on the above occasions induced to take of the pathology of spasmodic cholera; he is desirous to submit his ideas to the consideration of his professional brethren, through the medium of the valuable journal in which they now appear.

Though the nature of cholera is yet imperfectly understood, we are daily advancing toward a more correct knowledge of it. The minutest analytical induction has been applied to the disease, and the result has been the accumulation of so voluminous a mass of valuable evidence on all points connected with it, as was never brought to bear upon one subject since medicine became a science. Analytical reasoning is the main-spring of medical argument; but after a disorder has been so thoroughly analysed, that the collection of facts becomes cumbrous on our hands, we may be excused in venturing to resort to synthesis. If ever such a mode be allowable,—though speculative hypotheses too often take the place of legitimate deduction,—it is surely the case now, in the malady under consideration.

In order, then, to illustrate more strikingly the pathology of cholera, let us for a few moments dismiss the analysis of the disease entirely from our argument. Let us suppose some irritating, morbid, infectious, or epidemic influence, the effect of which shall be, through the medium of the bowels, on the ganglionic system of nerves. What, according to the principles of physiology and pathology, would be the train of symptoms produced by such an influence?

In tracing the answer to this query, we must bear in mind the following propositions, which are so well established that they may be regarded as axioms.

1. The ganglionic or sympathetic class of nerves, convey neither sensation nor mobility, but their action is referred solely to the functions of organic life, and chiefly to the various offices connected with secretion.

2. The spinal nerves are the sources of feeling and motion; they supply all muscular structure, including both the voluntary and involuntary muscles; and hence,

3. The muscular coats of the bowels, as well as the muscular envelopes of the bronchial tubes,* and the muscular fabric of the heart, are dependent on the motor branches of the cerebro-spinal system for their contractility; and, further,

4. The viscera derive their sensation (as

to pain) from the cerebro-spinal nerves, and their sensibility (to their own peculiar functional stimulus) from the ganglionic.

5. From the intimate anastomosis between every branch of the sympathetic nerves, and a corresponding part of the spinal or cerebral system, irritation of the one class is speedily transferred to the other; and the pneumogastric nerves, being the pair most extensively connected with the ganglionic, are uniformly involved in such disorder.

6. Irritation of a nerve of motion produces *spasm* in the muscular fibres supplied by that nerve. Irritation of a nerve of sensation causes pain, which is generally felt at the extremity of its ramification; but the only perceptible effect of irritation on a nerve of sense, or a nerve of organic life, is the diminution, or total loss of its functional power.

7. Secretion is a process of life by which the blood is converted into certain healthy products; and by affections of the sympathetic nerves, the secretions are altered in their nature. The only effect which we can conceive to be the result of a paralysis of those nerves, will be a conversion of the process of secretion into an *exudation* or *elimination*.

Some physiologists may hesitate to allow the third and fourth postulates; but I think it may be proved, that the functional sensibility of the involuntary muscles is derived from the sympathetic nerves; and that the impression received by them, which John Hunter aptly termed “the stimulus of necessity,” is communicated to the motor branches, by which the muscular action is performed. Just as in a paroxysm of sneezing or coughing, the involuntary action of the muscles is owing to irritation of a nerve of sensation. In the eye, the phenomenon of winking, though performed by muscular nerves, is the result of a “stimulus of necessity,” felt by the branches of the fifth pair in the conjunctiva. Numerous similar illustrations might be adduced. I decline in this place entering into a physiological discussion of the truth of the above postulates; but, being myself satisfied of their correctness, I am convinced of the soundness of the corollaries founded on them.

Taking these data as the groundwork of our argument, we will endeavour to answer the problem advanced; viz., what would be the train of symptoms excited by a given influence on the ganglionic nerves.

The immediate effect would be a disordered state of all the alvine secretions, inducing diarrhoea. Constipation could not be the result, as that would imply a tonic or astringent influence, which is contrary to the premises. The diarrhoea not being caused by irritating food, morbid biliary

* See an ingenious paper by Dr. Carson, junior (Liverpool), in *LANCET*, April 7, 1832.

secretion, or drastic ingesta, but merely by the alvine secretions being more watery than natural, there would be no pain, because the nerves of sensation (which are sparingly distributed over the intestines, with the exception of the rectum and the stomach, where only they are needed) are not acted on by any irritating matter within the bowels; but the motor branches distributed throughout the whole length of the alimentary canal are much more intimately concerned. From their universal anastomosis with the ganglionic nerves, there would arise a spasmodic action of the coats of the bowels and stomach; a violent and peculiar vomiting, with little or no concordant action of the diaphragm or abdominal muscles, merely a contraction of the organ itself, accompanied by incessant purging, not with tenesmus or tormina, but the evacuations would be expelled without effort, and by a sort of pumping action of the bowel. This state having lasted a longer or shorter period, the fecal matter of the bowels would be all carried off; but, the cause still remaining, the ganglionic nerves would become paralysed. Elimination would take the place of secretion into the intestines, and the watery parts of the blood be rapidly drained off; diarrhœa, or sickness, or both, still continuing to carry out of the body the evacuations, now become pale and serous, without smell or any of the characteristics of fecal matter.

Meantime the irritation would be extending along the whole of the sympathetic nerves. A change would take place in the secretions of all the glandular system. When the powers of organic life are weakened by fever, bilious diarrhœa, &c., we observe that, in proportion as the secretion of the bowels is increased, the other secretions are diminished; so, in the case we are supposing, we should expect a total suspension of them. The bile would be no longer thrown into the duodenum; the pancreatic juice would fail to supply its portion, whatever that may be, to the alvine discharges; the urine would be suppressed; the detrusor muscles of the bladder would be in a state of spasmodic contraction, while the viscus itself was empty; the throat, tongue, and mouth, would be dry, and the skin livid and without perspiration; or, perhaps, a cold moisture might exude from the capillary pores; the eyes would be devoid of tears, and, from the experiments of Mons. Dupuy,* we might expect them to sink in their sockets and become dim. The irritation would also have extended to the spinal column. Great pain and spasms would be liable to take place in all the mus-

cles. The pneumogastric nerves would be especially affected: hence much disturbance in the circulation and respiration would ensue. This would be further increased by the blood having been deprived of great part of its fluid constituents; the circulation would be almost suspended, and the heat of the body, consequently, fall to a very low degree; the blood would hardly be propelled through the lungs; of course the veins near the heart would be in a state of violent congestion; while, by means of the recurrent branch of the eighth pair, as well as from dryness of the larynx, the voice would become altered in tone and gradually lost. These symptoms would go on increasing with dreadful rapidity, and in a short time occasion death, without any disturbance of the mind, or any lesion of a single organ of sense; touch, perhaps, alone excepted.

These are all, and they are the only symptoms which we could calculate upon resulting from the cause we set out by stating, and it is unnecessary to ask the reader to compare them with the symptoms of spasmodic cholera. Any one who had the painful office of visiting a patient labouring under that horrible malady, cannot fail to recognise, in this supposed case, an accurate picture of the real disease.

Let us now turn to the analysis of cholera, and see how far it accords with our synthetical production. In all important particulars, *post-mortem* examinations, the most perfect of all our tests, confirm, to the letter, the correctness of the view here taken. It may, indeed, be objected, that dissection does not always show the ganglia to be unnatural in appearance, and a long list of cases might be quoted in support of this objection; but I cannot allow them to have much weight. We do not expect the scalpel uniformly, or even generally, to expose distinct organic lesion in cases of paralysis and other disorders of the brain; and why should a more palpable alteration of structure be demanded in a similar affection of the ganglionic nerves, the pathology of which is less perfectly understood than that of the brain? If physiological signs of disordered function in this obscure system are detected, we must not refuse to be convinced they exist, though a visible lesion of structure is not always perceived. It is, however, at the same time to be recollected, that positive disorganization has frequently been noticed.

The blood has been proved by Dr. O'Shaughnessy, Dr. Clanny, Mr. Rose, &c. to be vitiated during all stages of cholera. Though the results of their experiments slightly differ, yet they all agree in the main point, viz. that the serous, i. e. the watery and saline parts of the blood, are

* Quoted by Mr. H. Bell from the *Journal de Médecine*, tom. 37, p. 343.

wanting. Upon this cause the above chemists suppose the disease to depend; but they seem to have stopped short in their pathological reasoning when just on the verge of important truth. *How does this serious portion become wanting?* If all the secretory vessels were in a state of health, there would be no possible mode of escape for the serum, except by the natural secretions. Unless, therefore, we suppose that disturbance of the secretory vessels precedes the loss of serum in the blood, we must assume, that an effect takes place without a cause. It has been demonstrated that the blood in all fevers undergoes some disorganization, which probably is the case also in cholera, independently of this filtering. *Dr. Southwood Smith, Dr. Brown,* and others, have spoken of the latter disease as a species of fever, and they undoubtedly have many points of resemblance; but till the pathological condition called fever is more correctly ascertained and understood, the comparison throws little light upon the subject. The writer thinks that the state of the sympathetic nerves has not been sufficiently inquired into with regard to the latter class of diseases, fevers. He does not mean here to venture upon any hypothetical deductions as to their nature, but he wishes that the hint may be deemed worth attending to, by those who have opportunities for investigating it.

The view of cholera which is thus briefly sketched, occurred to the writer after a deliberate consideration of the subject when in *Paris* eighteen months ago. It will be seen that the opinions advanced by several of the talented pathologists who have intermediately written on the disease, approach in some degree, though, essentially, they differ from that which he has ventured to suggest. The theory of *Mr. Hamilton Bell* (one of the most plausible which has been published) does not satisfactorily explain the occurrence of premonitory diarrhœa, nor indeed of diarrhœa at all, as a primary symptom: besides the division of the nervous system into three "powers" is neither so simple nor so explicit as that which is so beautifully demonstrated by *Mr. Bell's* distinguished uncle, *Sir Charles*. If *Mr. Bell's* theory were the true one, an injection of stimulant fluid into the veins, in order to rouse the action of the heart, or inhaling oxygen for a similar purpose, would, along with venesection, at once cure the disorder. The remark that saline injections seldom prove of permanent benefit, applies also to the views of *Dr. O'Shaughnessy, Mr. Annesley,* and those who consider a morbid state of the blood as the primary source of cholera. The hypothesis of *Dr. Kennedy* and many others, that the seat of the malady is the spinal marrow, would

lead us to expect pain, cramps, and spasms, or palsy in the extremities, as invariable and early symptoms preceding all others, which is certainly not the case. Neither can it be allowed that vomiting and pain in the stomach necessarily precede diarrhœa, as might be presumed from *Mr. Lizars* having ascribed the proximate cause to inflammation of the eighth pair of nerves. If malignant cholera were a "gastro-enterite," as stated by *Mr. Christie* and many French writers, one would naturally infer that it might be subdued by those remedies which are found effectual in ordinary cholera morbus and bilious diarrhœa. That such is not the case in the malignant disease is evidenced by the various other modes of treatment which have been resorted to with advantage; and especially by the paradoxical fact, that a drastic cathartic so powerful as croton oil has the effect, in spasmodic cholera, of checking the diarrhœa and substituting mild evacuations of fecal matter for the "rice-water" discharges. Unless any hypothesis of the late unfortunate *Professor Delpech* be similar to the one at present under discussion, there is no view which has fallen under the observation of the writer that is free from these various objections, or which will explain the singular anomaly just alluded to, except that which he is endeavouring to substantiate.

It is immaterial to the preceding argument whether cholera be epidemic, infectious, or contagious. In the MS. before alluded to, similar opinions are advocated to those now generally prevalent among medical men who have seen much of the disease, viz., that it is usually epidemic, and occasionally but rarely infectious. The writer has also seen well-marked cases of malignant cholera, both endemic and sporadic. The atmosphere appears to have little share in producing or propagating the disorder, unless, as *Sir H. Davy* hinted, the air has "something more than its ponderable elements." Has the earth some influence on the powers of life which philosophy has not yet detected?

Let it not be said, however, that all this is speculative theory; for the great object of this paper is to elicit practical benefit in the treatment of cholera. So far various plans have been adopted, but few of them have been derived from any fixed principle. Most of them are recommended by experience alone; and experience, alas! has failed to speak much in favour of the most successful ones. In a large proportion of cases, where the patients' constitutions were very unhealthy, and the disease had got a firm hold, the most consummate medical skill appears to have been of little avail; but if those methods of treatment

which have been found most serviceable can all be reduced to one and the same principle, and that principle be found to agree with the most plausible theory of the pathology of cholera, it is fair to infer, that by modifying and adapting our remedies to fulfil the indications thus pointed out, we may arrive at the most beneficial practice which the nature of the case admits of.

As paralysis of the ganglionic nerves is a pathological condition which has seldom, perhaps, presented itself to the mind of the therapist, we can only draw the indications of treatment from analogy. In ordinary loss of power of a nerve of sense (or, indeed, of any nerve), it is usual to employ stimulants, and hence, in our view of cholera, we should be induced, *a priori*, to administer the only stimulants we know of, affecting the abdominal part of the ganglionic system, viz., aperients or purgatives, combined with calomel, perhaps adding a little opium at first, to allay the irritation of the spinal nerves. Let the following plans of treatment, decidedly the most successful on record, be my answer to the question:—

Mr. Searle's exhibition of large quantities of table salt.

Mr. Montgomery's employment of calomel, capsicum, and opium.

Dr. Kennedy's plan by bleeding and calomel.

Dr. Hacket (of Trinidad), and Dr. Teggart's recommendation of croton oil; a modified use of which, I have been informed, was successfully adopted during the late virulent irruption of the malady at Stockton.

Dr. Ayre's frequently-repeated doses of calomel, a plan which Mr. H. Thomas, of Sheffield, acquaints me, has been attended with marked success in that town.

Mr. McIntyre's (of Newcastle) treatment by castor oil and aromatics, with calomel,

small doses of opium, and copious enemata of warm gruel and turpentine, which was so successful in that gentleman's extensive colliery practice, that I am authorised, on his own information, to state, he and his assistants did not lose more than about 80 out of 700 cases.

With most of the above modes of treatment, early bleeding is insisted on. It is a point of great importance mechanically to relieve the loaded veins of a portion of their dark contents. And here it should be remembered, that a small pulse no more contra-indicates that practice in the malignant disease, than it does in enteritis; but for a similar reason in both cases (so far as the cause of the weak pulse is concerned) urgently calls for venesection. More or less diffusible stimulus, in order to rouse the faltering powers of life, is also generally recommended in spasmodic cholera.

A word on the saline injections into the veins. If the foregoing sketch be correct, it explains the curious train of effects following this novel course of practice. Fluidity is restored to the blood; but it is like water poured into a sieve,—the liquid soon runs off by the bowels, which are not at all acted on by the venous injection, and the patient too often sinks from a state of apparent revival into one of fatal relapse. As a measure of the success which has attended this plan, the writer has collected from various periodical journals, reports of 105 cases, in which fluid of various kinds, generally in solution of salts, had been injected into the veins of cholera patients. He has arranged them in a tabular form, showing the age of the individuals, the number of hours which elapsed from the time of attack before injection was resorted to, the quantity injected in a given time, and the event. The following is a summary of his list.

No.	Hours ill.	Injected.	Cured.	Convalescent.	Under Treatmt.	Died.
15	4 to 12	3/4 24 to 305	1	2	—	12
13	12 to 27	3/4 23 to 292	3	4	—	6
77	unknown	3/4 14 to 640	1	1	15	60
105		3/4 14 to 640	5	7	15	78

The method has not often been successful, and seems now pretty generally abandoned; but, combined with the administration of aperients, calomel, and other remedial measures, it might, perhaps, be of great advantage. It has been by relying on the injections as a permanent instead of a temporary relief, that much of the failure of that system is probably to be attributed.

A mass of further authorities might be adduced, to convince the reader that the view of cholera which is here briefly taken,

is *practically* worthy of attention. The writer hopes what has been said is sufficient for that purpose; and, though not so sanguine as to believe that he has solved all the intricacies of the subject, or that the sketch he has given is free from objections, he thinks it is calculated to remove some difficulties; hence, he shall esteem himself fortunate if his remarks advance us one step farther in the elucidation of this important department of medical science.

Stockton-on-Tees, January, 1833.