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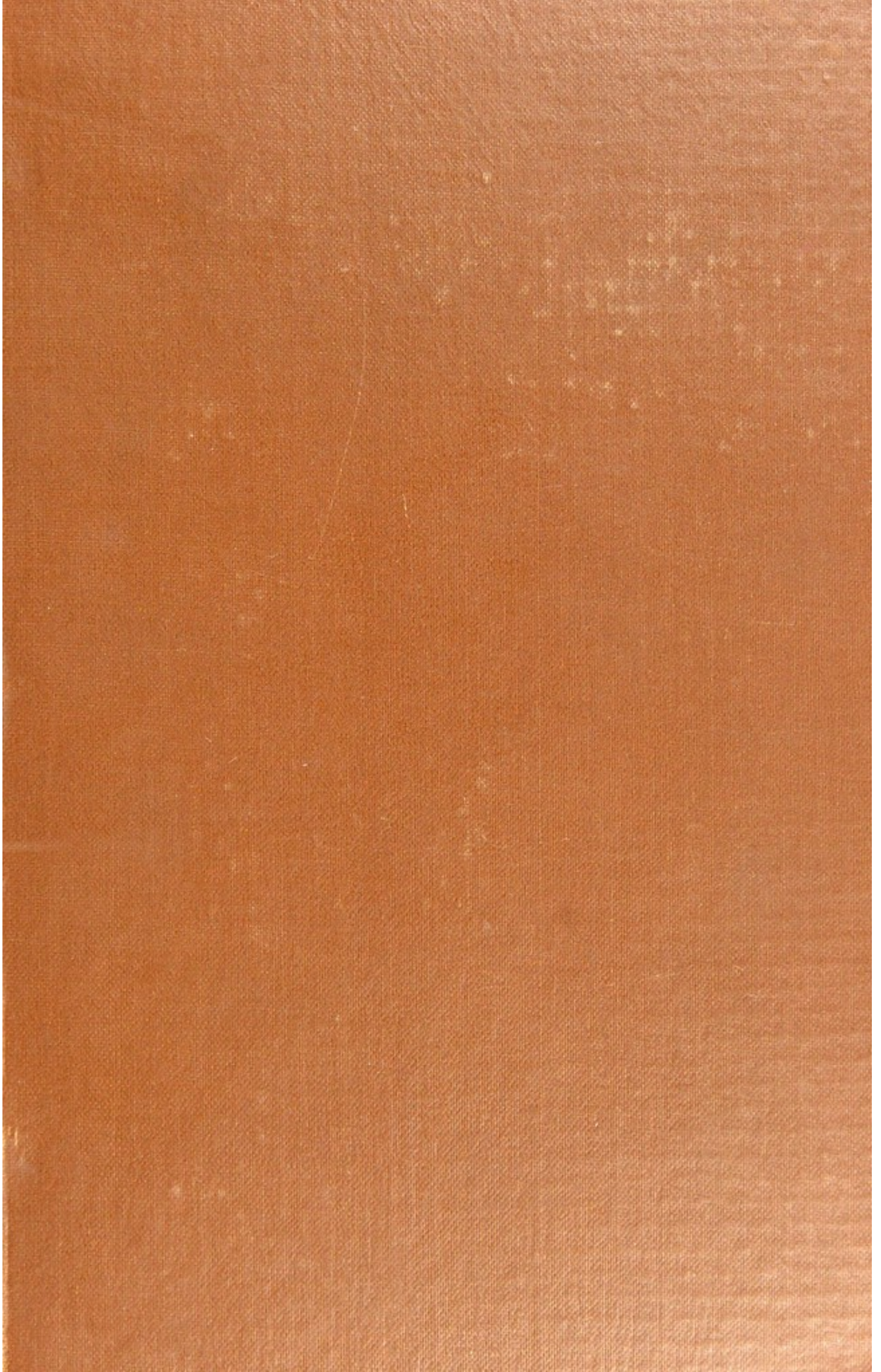
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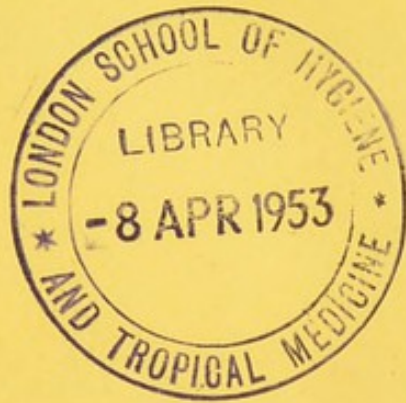


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EPIDEMIC CHOLERA.

BY JOHN PARKIN, M.D., F.R.C.S.,

FORMERLY H.M.'S MEDICAL INSPECTOR FOR CHOLERA IN THE WEST INDIES.

[Third Edition]

LONDON :

SAMPSON, LOW, MARSTON & CO., ST. DUNSTAN'S HOUSE,
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CHAPTER I.

SYMPTOMS.

It is necessary to remark, for the perfect understanding of the observations about to be made, that I divide the Epidemic Cholera into two distinct and different forms—the mild, which has received the generic name of Cholérine, and the severe, that of Cholera Asphyxia, or Blue Cholera. The latter I have subdivided into four different periods.

The premonitory,* or preliminary, Diarrhœa, so common and general a precursor of the other periods, forms, with me, the first stage of this disease. Again, that peculiar affection of the stomach, characterised by *malaise*, giddiness, faintness, nausea and vomiting of the contents of this organ—which sometimes precedes the diarrhœa, but more generally follows it, and ushers in the next stage; and which forms, in those cases wherein the preliminary diarrhœa is wanting, the first link in the chain of morbid symptoms—I have also placed in the same division.

* Premonitory, as Mr. Greenhow has remarked, is an erroneous term; it is not a premonition of the disease, but a concatenation of circumstances constituting the early period of the disease itself.

I designate as the second stage of the disease, that period of the attack when a fluid resembling *congee*, rice-water, or barley-water, is thrown up from the stomach, or evacuated from the bowels. Spasm may, or may not, be present at the same time; but the pulse is little, if at all, affected.

The state of collapse constitutes the third, and, in certain localities, as India, the last stage of the disease; while, in others, as cold climates, another stage is superadded, characterised by symptoms the reverse of the former—being a state of excitement, or fever—and termed the consecutive fever. This, therefore, when it occurs, forms the fourth, and the last, stage of the disease. With these cursory remarks, we may now pass on to consider, more in detail, the symptoms peculiar to these different stages.

The term Cholérine has been applied to that peculiar affection, which has sometimes ushered in the more severe attack, but which, in other instances, has prevailed with those who had no regular attack afterwards.

It is characterised by indigestion, flatulency, rumbling in the bowels, a feeling of sinking, or anomalous nervous sensation, referrible to the abdominal organs, and irregularity of the bowels, which are either confined or slightly relaxed. So common was this affection, during the first invasion of the epidemic cholera in Europe, that few persons were exempt. In fact, as a German writer remarked, individuals

who, previously, had not known where their stomach was situated, then complained of a feeling of uneasiness in this organ. In the subsequent visitations, cholericine has not been so prevalent, or so apparent. This particular affection must not be confounded with that which follows, and which, when present, almost invariably ushers in the more severe form of the disease.

FIRST STAGE.—*Premonitory Symptoms.* These are very uncertain, being sometimes absent, sometimes present, and varying with different individuals, and in different situations; while, when present, they are not easily detected, excepting by an experienced observer. In these cases, the person about to be attacked will have an expression of anxiety, and his complexion, an unnatural, earthy, appearance, while his eyes appear to be sunk in the sockets. If questioned, the stricken individual will deny that he is ill, but may say, that he has some uneasy sensations—that he is listless or depressed, and somewhat deaf, with some undefinable sensations of discomfort. On being further questioned, it will be found, that there is, either tormina of the bowels, or a burning sensation at the pit of the stomach, with or without nausea, a quick and *weak* pulse, with cold hands and feet—accompanied sometimes with slight blueness of the finger nails. If these symptoms be not relieved, they will be followed, sooner or later, by the supervention of diarrhœa, or of the second, or third stage of the disease.

Second Division of the First Stage ; OR, DIARRHŒA.

Although many writers refuse to acknowledge, that the diarrhœa forms the first link in the chain of morbid phenomena, there can be little doubt on the subject. Not only does this peculiar affection usher in the more severe form of the complaint, in the majority of instances, but the diarrhœa itself is characteristic. It is unaccompanied by pain, or griping, or spasm, as is usually the case with similar attacks ; it is a simple relaxation of the bowels—a looseness—similar to that which occurs from the operation of some depressing cause, either mental or bodily. It is, in fact, if we may employ the term, a *nervous* diarrhœa. Hence it is, that the diarrhœa is so frequently neglected by the public, and by those ignorant of their impending danger.

The duration of this stage is very uncertain, being much longer in cold than in warm climates, where the type of the disease is more severe. In intertropical regions, and also in temperate ones when the attack is severe, the disease passes at once from the first to the second stage. When this occurs, the symptoms are peculiar and characteristic. The call is sudden, and the motion copious, with a sensation as if the whole contents of the intestines had been emptied at once. This stool will necessarily be fœculent, but the next is generally serous. The second stage of the disease may then be said to have commenced.

SECOND STAGE.—When the diarrhœal stage has existed for some time, and when it has been accompanied by those general symptoms before described, the malady passes almost imperceptibly from one stage to the other. In this case, serous, or rice-water, evacuations will be the only characteristic symptom of the supervention of this stage of the disease, unless there be vomiting, when the fluid thrown up, after the contents of the stomach have been evacuated, will be white, or serous—the same as that discharged from the bowels. In general, however, the vomiting occurs toward the termination of this stage, and is thus one of the first symptoms to usher in the next stage.

THIRD STAGE, OR, STAGE OF COLLAPSE.—In addition to vomiting and purging of a serous fluid, one of the most constant and distressing symptoms at the commencement of this stage is spasm, more or less general. The spasms generally commence in the hands and feet, and spread up towards the trunk, which, however, they do not always reach. In some cases—exceptional ones—the muscles of the abdomen and back are principally affected; those of the extremities being comparatively or entirely free. Another symptom, characteristic of the commencement of collapse, is jactitation, or rolling from one side of the bed to the other. This phenomenon, although not painful to the patient, is scarcely less distressing to the beholders than the spasms.

No matter whether the premonitory and general symptoms, previously described, have been observed before or not, they invariably become developed at this period, in a more marked degree, and with great rapidity. The blueness, at first confined to the mouth and the finger-nails, soon spreads over the face and the extremities, and, eventually, over the rest of the body; at the same time, the skin of the extremities appears corrugated, as if steeped in hot water, while the surface of the body is covered with a cold, clammy perspiration. The sensibility of the skin is so greatly diminished, that vesicatories do not act, and boiling water produces no effect. Notwithstanding the icy coldness of the surface of the body, when a hand is applied to it, a patient will frequently complain of heat, or a burning sensation in the skin, and throw off the bed clothes.

At the commencement of the collapse, the pulse is weak, slow, and fluttering; but, after a certain time—the interval being greater or less in different cases—the pulse at the wrist becomes imperceptible. The heart, also, is found to have a feeble, slow, fluttering, beat; the breathing becomes slow and oppressed; the exhaled breath is cold; and the voice hollow, low and unnatural; the eyes, surrounded with a dark circle, are completely sunk in the sockets; while the whole countenance is collapsed, and the expression, or physiognomy, so altered, that the patient can hardly be recognised by his most intimate friends.

When these symptoms supervene, and especially when the pulse at the wrist becomes imperceptible, the collapse is said to be confirmed, or complete.

These symptoms, unless relieved by the efforts of art, continue without change until death takes place; excepting that the spasms generally cease previously to this. The discharges from the stomach and bowels, also, vary greatly in particular cases. Generally, the vomiting and purging are severe at the commencement of collapse, but they frequently cease before its termination. In other cases, they cease after two or three stools: and, occasionally, they are absent altogether, or occur only towards the termination of the attack.

The preceding is the order in which the symptoms arise, in the majority of cases, but, in other instances, this order is reversed. Instead of purging and vomiting, and the supervention of the other symptoms described, the patient may fall at once, and suddenly, into a state of collapse,—the vomiting and purging following, instead of preceding, the stage of collapse. I met with several cases of this kind in Jamaica; while in some, which terminated fatally, there was only one slight evacuation, unattended by vomiting, shortly before death—the disease, in these cases, commencing where others terminate.

So, again, in other instances—in those where the stages run into each other in the usual order—there will be a variation as regards the prevalence and in-

tensity of particular symptoms. The vomiting and purging will be more severe in some cases than in others; while the same may be said of the spasm. Not only do these anomalies occur in particular individuals, but in particular localities—constituting, what Mr. Scott has termed, “local epidemic visitations.” “Thus,” he remarks, “when the disease appears epidemically in a town, or district, or in the lines of a corps, or the camp of a marching regiment, it may, on one occasion, be distinguished throughout by the absence of vomiting, and the prevalence of purging; and, on another, by the excess of vomiting, and, though more rarely, by the absence of purging. Spasm may be generally present in one instance of invasion; in another, it may not be distinguishable.”* The Honourable Mr. Harris, describing an outbreak that occurred at Soonda, in India, said: “It commences by attacking the sufferer with an agonizing heat in the stomach, vomiting, purging, and lock-jaw; death ensues in two hours, and, often, more rapidly: it bids defiance to every remedy.”

In some rare instances, the attack commences with *Colic*, without either vomiting or purging. An instance of this kind will be given hereafter, while narrating the particulars of the outbreak in Jamaica.

But, whatever may be the order of progression, or the variety of the symptoms, there will always be in the fatal cases, as Mr. Bell has rightly remarked,

* Madras Report, p. xxi.

*“the collapsed countenance, blue lips and nails, shrunken fingers, the total failure of the usual secretions, deficient animal heat, suspension of the pulse, and remora in the venous system.”**

During the whole of the above period, when the patient presents the appearance of a “living corpse,” it is a remarkable fact, and worthy of particular note, that the functions of the cerebro-spinal system remain perfect and intact. It is this circumstance which forms so remarkable and characteristic a feature of the epidemic cholera.

In the next stage, this immunity does not continue, the functions of the brain being disturbed during the consecutive fever, the same as in all other fevers.

* Loc, cit. p. 17.

CHAPTER II.

CAUSE.

THE IMMEDIATE OR PROXIMATE CAUSE.—When we observe the symptoms which this singular malady presents, and compare them with those produced from various poisonous substances, more particularly septic ones; when those deductions are drawn, which are allowable, from the result of various methods of treatment; and when, as has been remarked by one writer,* all other theories, which have yet been broached on the subject, are insufficient to account for the morbid phenomena presented; the conclusion, that this disease is produced by the operation of a poison on the system, would seem as capable of proof as a mathematical problem.

As, also, a number of persons, no matter whether congregated in the same place or not, are always attacked simultaneously, we must infer that the cause is a general one; and, that the poison is generated

* Vide *Lancet*, "History of the Epidemic Cholera."

without, and not *within*, the body. Although it is immaterial, in a practical point of view, in what way the deleterious matter enters the system, still, when we witness the numerous changes which take place in the atmosphere, during the prevalence of the malady ; that the epidemic commences in one place on the setting in of particular winds, and subsides in another after heavy showers of rain, or, the reverse ; it is not illogical to infer, that the poison is contained in the air we breathe.

What the source is, whence this destructive agent is derived, it would be superfluous to attempt to show in this place—particularly as I can refer those interested in the inquiry to a work expressly devoted to the subject.* It is sufficient for the present to conclude, that the Epidemic, Malignant, or Blue Cholera is produced by a specific cause ; and this cause, the introduction into the system of an extraneous, deleterious, and poisonous substance.

Taking it for granted, then, that to this single cause we must ascribe the production of the disease under consideration, it only remains to ascertain how the poison acts, when thus introduced into the human body. This has been most ably accomplished by Mr. Bell, who has clearly shown, that the Epidemic Cholera is the effect of a derangement or suspension of those functions over which the sympathetic, or ganglionic, system of nerves presides.† As the circu-

* On the Remote Cause of Epidemic Diseases.

† On the Cholera Asphyxia.

lation and distribution of the blood, and all the different secretions of the human body, are directly under the control of these nerves ; and as branches from this system are distributed, not only to the heart, the arteries, the veins, and capillaries, but, also, to the organs of digestion and assimilation ; we should expect to find, that if, from any cause, the vitality of these nerves were diminished or destroyed, the action of the stomach and bowels would be deranged ; the secretions, perverted or suspended ; the heat of the body, diminished ; and the circulation of the blood, either partially or entirely arrested.

This is precisely what is observed during attacks of the Epidemic Cholera, which is neither an affection of the mucous membrane of the stomach and bowels, as some have supposed, nor a mere functional disorder of these organs, arising from depraved and vitiated secretions or other local causes. It is a disease which is characterised, not only by a derangement of the stomach and bowels, but, of the circulation of the blood, and, of all those functions so intimately related to this vital one. Hence, when the disease has become fully developed, the pulse ceases at the wrist ; blood no longer circulates in the arteries ; the process of respiration is interrupted ; and all the secretions are suspended. The liquid thrown up from the stomach or passed by stool contains neither pancreatic juice, nor bile, mucous, or excrementitious matter. The kidneys cease to secrete urine ; saliva no longer moistens the mouth ; the eyes are deprived of tears ;

carbonic acid is not given off from the lungs; and animal heat is no longer generated. This, therefore, is a disease which consists in the suspension of the organic functions—those functions over which the ganglionic system of nerves presides.*

The apparent anomaly of copious dejections, at a time when all the secretions are suspended, has been satisfactorily explained by an analysis of the fluid evacuated, which has been proved to consist of the serum of the blood; while an examination of the body, after death, has discovered the venous system full of thick, viscid, and black blood, entirely deprived of its serous part. The phenomenon itself has been thus explained by Mr. Bell, in the work referred to.

* The powers carrying on life, in the more perfect animals, may be divided into three: viz., the sensorial; the respiratory; and the sympathetic, ganglionic, or organic.

1. The sensorial influence may be suspended—as in apoplexy, injuries of the head, etc.—without affecting the other powers of the system.

2. That the respiratory power is independent of the sensorium has been shown above. It has also been proved, by Sir B. Brodie, to be independent of the circulating power. When the *Upas antiar*, which suspends this power, has been injected into the blood, the respiration continues long after the heart has ceased to beat. We observe the same fact in Cholera. On the other hand, the *Upas Tieutte* arrests the process of respiration, but leaves the sensorium unaffected. Cl. Bernard has also shown that the *Woorara* poison paralyses the motor nerves of the cerebro-spinal system, leaving the sensitive nerves unaffected, as, also, the organic nerves for a certain time—the action of these being also abolished eventually.

The Vena porta not being provided with valves, this part of the venous system becomes easily distended, while the retrograde motion of the fluid distends the capillaries, which, from the loss of their contractile power, allow the serous part of the blood to escape—thus giving rise to the evacuation, from the bowels, of a fluid resembling rice-water, which forms so characteristic a feature of the disease.

We may conclude that the disease is produced by a lesion of the grand sympathetic, not only in consequence of the derangement observed in the functions presided over by this system of nerves, but, also, from the integrity of those functions under the direct control of the cerebro-spinal system. Except slight giddiness, which sometimes occurs at the commencement of the attack, and which can be explained by the sympathy that exists between the stomach and the brain, the latter organ remains unaffected during every stage of the disease; and the intellectual faculties continue to perform their functions with the most perfect freedom, until the last moment of existence. The voluntary, or locomotive, functions also enjoy the same freedom, so that patients have been known to walk a considerable distance after the partial suspension of the organic functions; while it is a common occurrence for such persons to get in and out of bed, without assistance, within a few moments of their death, and after the circulation has been completely arrested for hours. We thus have the

anomaly presented to us, of a complete suspension of the functions over which the ganglionic system of nerves presides ; while those under the control of the cerebro-spinal system remain free and intact. This immunity, however, is only observed during the first three stages of the disease ; in other cases, and in certain latitudes, in which the consecutive fever supervenes, the functions of the brain are disturbed, the same as in other forms of fever. The cause of this variation, I shall not now attempt to discuss, but content myself with referring those interested in the subject to the physiological part of a former edition of this work, in which this phenomenon has been considered.

Not only may we conclude, that this disease is the immediate effect of a suspension of those functions over which the ganglionic system of nerves presides, but, also, that this effect is produced from the injurious operation of an extraneous substance in the body ; as we should be at a loss to explain the sudden and complete annihilation of the vitality of these nerves on any other hypothesis. More than this, we may also infer, that the poisonous element is contained in the circulating fluid ; not only from the conclusion before drawn, that the poison enters the system with the inspired air, but, also, because we should be unable to account for the production of all the phenomena witnessed in this disease, except on this supposition.

CHAPTER III.

RATIONALE OF TREATMENT.

As previously observed, in the introductory part of the Work, the treatment of the Asiatic Cholera has been more unsuccessful, and more empirical, than the treatment of any disease known to the present generation. Although this epidemic is a new one—a *nova pestis*—there is no reason why we should not treat it on the same scientific principles as other well-known and ordinary diseases. On the contrary, it is the very reason why we should do so, in order that, if unsuccessful, we may be assured, that our failure is not to be ascribed to empiricism, and a departure from those sound maxims that have caused the practice of medicine to be ranked among the other sciences. Had we acted thus, something like a rational plan of treatment might have been adopted; while the medical art would have been rescued from the charge, now brought against it, of being an uncertain and empirical, if not a useless, branch of science.

This lamentable result may be ascribed to the fact, that the treatment of the Asiatic Cholera has hitherto been, what may be termed, a Symptomatic treatment

—an attempt to relieve or suppress the more prominent symptoms, or effects—no regard being paid to the cause of these effects, or to its removal. If practitioners, instead of confining their attention to the effects, had directed their researches to a discovery of the cause, there would not then have been so many false theories respecting the pathology of the disease ; nor would so many remedies have been proposed, adopted, and extolled, during one visitation, merely to be abandoned and condemned, when tried, in the next.

Thus, one individual, observing that the type of the disease assumed the form, which is not uncommonly met with in tropical climates, of Spasmodic Cholera, makes use of antispasmodics ; and, meeting with some success, infers that spasm is the immediate cause of all the morbid phenomena, and, consequently, antispasmodics the principal or only remedies required. A second meets with the disease under another form, that of sudden and immediate collapse, and in which evacuations from the stomach and bowels are almost entirely or altogether wanting. He resorts to the lancet, and, finding it successful, immediately concludes that to venous congestion may be referred all the symptoms present ; and that venesection, therefore, is the best resource. A third remarking, more particularly in Europe, profuse evacuations from the stomach and bowels, being also the first symptoms which excite attention, and detecting, after death, what are con-

sidered, signs of inflammatory action, locates the disease in the intestinal canal; and, directing his treatment accordingly, pins his faith on emollient drinks, leeches, &c.; and, if not a disciple of Broussais, adds to them emetics or purgatives.*

These individuals, who were then treating the disease to the best of their ability, and according to the form which it assumed in these particular instances, should next have inquired, if the same symptoms were always the most prominent, and the most severe, in every case, or, if different remedies had not, in the hands of others, been equally successful, before they ventured to generalize upon isolated facts. Had this been done, or had their followers first inquired into the varieties of each individual case, or ascertained the type of the disease, which varies more in places than in persons, and considered what was the *modus operandi* of the medicines employed, something like a rational plan of treatment might have been adopted by the majority of the profession. But, instead of this, these different plans of

* Broussais, in 1832, in accordance with his doctrines, attempted to cure the patients admitted into the Val de Grace, by the application of leeches, and the use of emollient drinks. Having lost all his patients, he then adopted what he termed the *dangerously* stimulating system; and, being nearly as unsuccessful with this method as with the former, he abandoned both his patients and his practice, and retired in disgust from the profession. He is not the only one who has made shipwreck of his reputation, by the treatment of this inscrutable disease!

treatment, adapted only to particular cases and particular localities, have been recommended as capable of curing the disease under every variety of form, and in all its various stages. And what has been the result of this? Others, without reflection, and led away by their reputed success, have followed the same methods under different circumstances, and with an opposite result.

As an example, I may mention the opposite results that have been obtained by blood-letting. In India, where this remedial agent was first resorted to, and where it was employed to the greatest extent, the success attending the operation was certainly such as to merit the encomiums bestowed upon it, by most of the writers of that period. But, then, what was the type of the disease in these instances?—that of sudden and immediate collapse. Here then was a case, where abstraction of blood, by removing the great plethora of the venous system, and restoring in some measure the balance of the circulation, might be a powerful means, by producing a determination of blood to other parts, of removing the poison from the internal organs, and distributing it to less vital parts. But, what was the result of the practice in other instances, when venesection was resorted to in the stage of collapse, which had been preceded by premonitory diarrhœa of several days' duration, and by the profuse serous evacuations in the second stage of the disease; and when it was impossible to do more than empty the

adjacent veins of the all-but-coagulated mass, without having the effect of producing any derivation of the poison itself from the internal and vital organs? Its almost universal abandonment, even by some of its most zealous supporters, is a sufficient answer to the question.* So again, as the type of the disease has been found to vary, not only in opposite climates, but even in different places in the same country, we find that the operation has been attended with success in one town, and with almost fatal results in others. I could also mention instances, where this practice has been beneficial during one irruption of the disease, and prejudicial during another. The medical history of the malady, indeed, abounds with such examples.

The same results have been observed with nearly all the remedies, that have been employed for the cure of this modern plague.† Dr. Parry states, that the diacetate of lead was resorted to by him, towards

* Although occasionally beneficial in cases of sudden collapse, the following example will show that bleeding cannot be generally adopted with impunity: out of twenty patients, admitted into the cholera hospital of Her Majesty's 20th Regiment, in India, in 1828, the whole of whom were bled, seventeen died—being a mortality of 85 per cent.

† Referring to the different reports respecting the influence of calomel, Mr. Ross observes: "In India, small doses are useless, and large doses cure: in England, large doses are injurious, and small doses cure! Surely a man may be forgiven, if he venture to doubt." Lectures on the Asiatic Cholera.—*Medical Times*, November 18th, 1849.

the termination of one particular visitation of cholera, with success, but, with an opposite result afterwards—“ thus showing, that a remedy, which seems at one time to exercise a powerful control over the disorder, at another is found perfectly inert; and we have still to look forward to future research, or to future accident, for a specific remedy against this protean malady.”*

Not only have these opposite results been witnessed during different visitations, or years, but, actually, in the same visitations, and in the same localities. Hence the contradictory statements, respecting certain remedies or particular plans of treatment; and, hence, the observation of one writer, that what seemed a remedy in Whitechapel was no remedy in the Borough; and what was useful in the Borough was useless in St. Giles. Well might a talented reviewer exclaim; “ Amidst the variety of remedies presented to our notice, we feel like a hungry guest, with a splendid bill of fare—each article tempts, but which shall he prefer? One gentleman cures Cholera with cold water—another removes it with hot—a third puts it to flight with calomel and opium—a fourth exclaims that colomel and opium are poison, and drenches with salt water—a fifth exclaims against the absurdity of salines by the mouth, and throws them into the veins—a sixth—but why should we go

* Report on Cholera, in Her Majesty's 4th Regiment in India, 1840-1.

on?" It was certainly unnecessary to multiply examples, or to produce arguments, in support of the observation, that we were, in point of principles of treatment and practice, worse off on the termination of the epidemic in England than we were during the first year of the existence of the disease in India. And what else could have been expected from plans of treatment, whose only aim was the removal of effects, while the cause itself was left altogether out of the question!

In proof of this, we cannot do better than turn to the treatment of the epidemic Cholera by small doses of calomel, as proposed and adopted by Dr. Ayre. The theory of this writer is, that "in every case (of Cholera) there is a repressed secretion of bile, and in every case there is a necessity for calomel to restore it." And it is added: "The essence of the disease is an interrupted secretion of the bile; and the remedy is that which restores the secretion. Calomel, as possessing the specific property of promoting the secretion of the liver, is at once a medicine that claims, not merely to be united with others in the treatment, but, to be relied on as a specific in this complaint, and to be employed to the exclusion of all others."*

That the secretion of bile is arrested, during severe attacks of Cholera, is undoubted; but, then, this phenomenon is an effect, not a cause of the attack.

* *Lancet*, vol. ii., 1831-2, p. 461.

In addition to this, the suppression of bile is only one link in the chain of morbid phenomena. During the state of confirmed collapse, all the secretions are suspended, as already stated: the non-secretion of bile must therefore be a common effect of a common and antecedent cause. Why, then, should we confine our attention to the flow of bile, when there are so many other effects to be remedied? The biliary secretion is, no doubt, a very important one; but it is not more important than many others; while its suspension is not attended with such serious consequences as that of the kidneys: the latter cannot be suspended for any time without injurious, and, even fatal, results; but the biliary secretion is occasionally suspended for long periods, without giving rise to the same ill consequences. Unless we are enabled to restore the renal secretion, the means adopted to produce a flow of bile would be entirely useless; even supposing that we were able, by the exhibition of calomel, to effect this object. But such a result is not likely to be attained by Dr. Ayre's method, as, independently of administering the calomel in small and repeated doses, he restricts its employment to the state of confirmed collapse—at that period when the absorption of solid, if not fluid, substances must be entirely suspended. “There is,” remarks Dr. Ayre, “but one stage of the disease, viz., that of collapse, in which the practice I have recommended should be employed. In the premonitory, so given, it would be needless, and, in most cases, injurious; and, in the

third stage, viz., the consecutive, it would be most pernicious! It is only in the collapsed stage, that it can be given in small doses continuously." * It follows, therefore, that as the flow of bile does not take place, excepting in a few rare instances, until the collapse has been removed, it is impossible to say, whether the restoration of the secretion is to be ascribed to the administration of the calomel, or, to the removal of the collapse. As one critic truly observed, "Dr. Ayre's theory proceeds upon the necessity of exciting a flow of bile, and yet he informs us, this result cannot be expected, in cases of collapse, until the patient has emerged from that state, and is therefore virtually cured." †

That the calomel produces no effect on the system may be inferred from the fact, that salivation was rarely produced, although the quantity of calomel sometimes swallowed was enormous. This truth is acknowledged by Dr. Ayre, who tells us: "No other limit was found necessary to be set, to the quantity of calomel given, than that which the circumstances of the disease imposed; nor was any fear entertained, that the medicine would affect the mouth, for, *pending the duration of the collapse, no absorption of the medicine will take place.*" ‡ In a subsequent communication, Dr. Ayre remarks: "Greatly as salivation, with all its evil effects, might be apprehended, from the

* *Lancet*, Oct. 6, 1849. † *Medical Times*, March 18, 1849.

‡ *Lancet*, vol. i., 1831-2, p. 618.

enormous quantity of calomel that was given to each patient, but *four* instances occurred amongst fifty cases, and that in so slight a degree as not to merit notice." And it is then added: "It deserves to be noted, that it happened chiefly with those whose collapse was *the least intense*, at the commencement of the treatment."* The latter interesting fact is another proof, that the calomel is not absorbed into the system, and that it is inoperative during the collapse of Cholera.

Had the remedy been absorbed, the large quantity taken, varying from 20 to 500 grains, would not only have produced salivation, but even fatal results. Dr. Ayre admits this himself, for, after remarking that "no inconvenience is sustained from the medicine, however large may be the amount taken," he adds: "that calomel, so given to patients, in every other known disease, would infallibly *destroy them*." † Sobernheim states that a girl, aged eleven, took eight grains of calomel for an attack of tracheitis, and died in eight days, from inflammation and ulceration of the mouth and fauces. In another instance which occurred to Lesser, fifteen grains of calomel produced similar effects, with excessive salivation.

Then again it appears, that the favourable termination of the case did not depend on the quantity of the drug that was exhibited,—the fatal cases having taken the largest quantity, the recoveries the least.

* *Lancet*, March 10, 1849.

† *Ibid.*

Of the cases treated by Mr. Cox, on Dr. Ayre's plan, slightly modified, it appears, that the fatal cases were the longest time under treatment, and took the largest amount of calomel; the quantity varying from 380 to 480 grains, and the time of treatment from 10 to 20 hours: with the recoveries, on the other hand, the quantity varied from 36 to 168 grains, and the time from 2 to 16 hours. This shows, that calomel alone has no influence in the removal of the collapse, and that, when this occurs, it must be ascribed to some other and accidental circumstance. As, also, it is not until the removal of the collapse, that the flow of bile is usually observed, we must ascribe the latter result, not to the calomel, but, to the restoration of the circulation, due to some other agency.

That the removal of the collapse is not the effect of the restoration of the secretion of bile, even supposing that it were the first in order, we may also conclude from the fact, that we sometimes meet with cases in which the flow of bile is not suppressed, while, in others, the secretion is restored during the attack; yet the fatal termination is not thereby prevented!

It is thus apparent, from the preceding facts, that the attempt to restore the secretion of bile is perfectly useless, until the cause productive of this effect has been removed. The writer of the Madras Report, in reference to this practice, remarks, "Calomel has unquestionably a powerful effect in exciting the biliary system, and, in this

view, its exhibition is highly necessary; but the suppression of the excretion of bile being only a link in the common chain of symptoms; and its partial or occasional removal, or even its total absence, having been proved to be of little consequence in the general course of the disease, to attempt to excite it by particular means may be considered as premature and injudicious."

It is equally clear, that the restoration of the secretion of bile, were that possible, would be insufficient to remedy the other effects arising from the operation of the same cause, or, to remove the cause itself. These conclusions are confirmed by practical results. "The calomel plan," as we are informed by Dr. Sandworth, "in small and frequent doses, on his (Dr. Ayre's) own showing, must have been largely tried; and the awful registers of mortality prove, that it has largely failed."* Additional proof of this will be afforded in another part of the work.

A still more striking example of the inutility of attempting to remedy an effect, while the cause itself remains untouched, has been afforded by the operation of injecting fluids into the veins during the state of collapse.

As soon as it had been ascertained, that the discharges from the bowels consisted of the serous part of the blood, combined with a certain portion of those salts naturally existing in this fluid, it was at once

* On the cholera at Hull. *Lancet*, Oct. 27, 1849.

concluded, that nothing more would be necessary, to remedy the evil and to remove the disease, than the injection into the veins of a certain quantity of water or serum, holding in solution similar salts. The momentary effect of the operation, it is true, was little less than magical; the patient being shortly restored from the state of a living corpse to that of an animate being. But, mark the result in the majority of instances! The poisoned blood was either carried on, by the force of the propelling fluid, to the brain, producing by its presence there an increase of the previous congestion, and the patient died apoplectic; or, else, the state of collapse again returned, the stools became more profuse and frequent than before, the fluid thrown into the veins was speedily expelled, and the fatal termination, which had only been retarded, occurred as usual.* These unfortunate results, which might have been anticipated, instead of leading practitioners back into the right path, appear to have produced the very opposite effect.

Puzzled by these constant and repeated failures, and thinking that nothing but chance would extricate

* Dr. M'Cabe, in his communication to the Board of Health, justly observes, while speaking of the utility of saline injections, "the discharges from the blood must be occasioned by some morbid condition of the system; and we can scarcely suppose, that that morbid condition can be remedied, by merely replacing by injection into the veins, what that morbid condition had allowed, or, probably, had caused, to be effected." *Lancet*, July 28, 1832.

them from the labyrinth, other practitioners have adopted, instead of the former, or symptomatic, an empirical mode of treatment.

Among the empirical class, stimulants, as having been more largely employed than any other remedies, take the first place. As, however, the *modus operandi* of these agents will be considered hereafter, it is unnecessary to dwell upon their effect and influence in this place. It is only necessary to add, that their employment, when trusted to alone, has been more detrimental than any mode of treatment, with the exception of bleeding, and the salts of Dr. Stevens.

Next to stimulants, the remedy which has been employed to the greatest extent, and the most empirically, is opium. In a disease attended with such a prostration of the vital powers, as that witnessed in the epidemic cholera, the employment of stimulants can be readily understood, if it cannot be approved; but, to administer an agent—and this too in the largest doses—which produces a similar effect to that witnessed in the disease itself, would seem, at first sight, paradoxical and extraordinary. The only excuse, the only apparent reason, for the employment of opium is the existence of spasm. Without going now into the question of the cause of the spasm, which will be considered hereafter, or the probability of this state being relieved by such a remedy, I would only observe, that the exhibition of opium has invariably

failed to relieve this distressing symptom. With this failure, the administration of the drug should have been prohibited and abandoned ; but it has not been. As is familiar to every one, it has generally, nay, almost universally, been employed in the diarrhœal stage of cholera—being one of the ingredients in the anti-choleraic, or anti-diarrhœal, mixture of the General Board of Health, at each of the visitations in England.* The diarrhœa, we should remember, is merely an effect of an antecedent cause, and, that cause, if the arguments before used be sound, the presence of a poison in the system. If so, the super-vention of the diarrhœa may merely be an effort of nature to expel the poison ; notwithstanding that she fails in this effort, in the great majority of instances. But, then, she ought, it would appear, to be assisted in her endeavours, not checked, by the efforts of art. To act otherwise, or to arrest the diarrhœa, before an attempt is made to expel, or to destroy, the morbid matter, would merely be to lock it up in the system, to produce future mischief. The Negroes in Jamaica

* “The Board of Health in Dublin,” remarked the Editor of the *Lancet*,—and the same remark will apply to all other boards, English and Scotch,—“not content with ordering a dangerous familiarity with opium, have printed long lists of medicines and remedies to be used on the attack of the disease. Whether they intended them for the rich or the poor, or for the edification of the profession in Ireland, may be fairly questioned. It is, however, pretty certain that they will prove equally useless to all classes.”—*Decr. 2nd, 1848.*

acted more scientifically : they almost invariably, when left to their own resources, took a dose of castor oil at the first onset of the attack ; and so convinced was I of the propriety of this practice, that I did not attempt to interfere with it—those remedies on which I principally depend for the relief of the diarrhœa being scarce, and, therefore, reserved for the more severe cases. As to the chalk-mixture with opium, it not only failed to arrest the diarrhœa, in all the instances that came under my notice, but the next stages appeared to me to be of more than usual severity, and more difficult of removal. That the diarrhœa, *per se*, is not so injurious is shown by the fact, that the attack is always milder in those cases in which it is present, and more severe when it is absent. The most dangerous attacks are precisely those in which the collapse comes on without being preceded by either vomiting or purging.

But, if the employment of opium be injurious in the first or preliminary stage, what are we to say to its administration in the stage of collapse—at that period when all the organic functions are in a state of suspension, and the energy of the organic nerves all but annihilated ? To give opium, at this period, is merely to aid the morbid cause, and to increase the intensity of the attack. This is not all. Opium acts as a sedative on the cerebro-spinal system of nerves, as well as on the ganglionic ; so that, not content with increasing the depression of the latter, the

physician tries, at the same time, to suppress the functions of the former—the only system which remains intact during the collapsed state. If this be not, to use a vulgar expression, jumping out of the frying-pan into the fire, I know not what is!

In addition to the above, opium acts as a poison on the blood, turning even arterial blood from red to black—a state to which the stupor is, in part, to be ascribed. Can it, then, be a matter of surprise, that, in those countries in which the bodies were buried, as soon as the breath was, or was supposed to be, out of the body, individuals should actually have been buried alive. Several well-authenticated instances of this occurred in the West Indies; the particulars of which have been narrated in the Report sent in by me to the Secretary of State for the Colonies, on my return to England in 1855.

Opium is not the only poison that has been employed to cure a disease produced by the operation of a specific poison on the system. The acetate of lead—a direct sedative on both systems of nerves—has also been administered in attacks of the Asiatic Cholera. It was first adopted by Dupuytren, and subsequently recommended, as a remedy in this disease, by Dr. Graves of Dublin, who had employed it successfully in a mild, sporadic, visitation of cholera, or, rather, of diarrhœa, in Ireland. Since then, it has been resorted to by several practitioners, in the severe form of the complaint, and, in general, with

fatal results. During a visit that I paid to one estate in Jamaica, where this mode of treatment had been adopted by the medical attendant, and where a very accurate account had been kept of the cases, I learnt that, from the 20th June to the 9th July, 33 persons had been attacked: to these, the acetate of lead, combined with opium, had been alone administered. Of this number, *four* recovered, and *twenty-nine* died—being a mortality of 87.87 per cent! The same result would appear to have followed its use in India: “It signally failed,” as we are informed by Mr. M’Gregor, “in all cases, where the disease was severe.”

Nux vomica, another poison, has also been administered in this disease! as though the spasms were not sufficiently severe, and as if the congestion of the spinal marrow—an effect produced by the operation of nux vomica, the same as by the poison of cholera—were not sufficiently great before!

But, enough! I will not now attempt to enumerate the number of empirical remedies that have been resorted to, or waste the time of my reader in showing the injury which the science of medicine has sustained by this reprehensible conduct. It is confidently to be hoped, after so much melancholy experience of the inutility of plunging headlong into the abyss of darkness by which we were surrounded, that we shall henceforth belong to the class of rationalists, and not to the class of empirics; and that, if we cannot

meet with any specific for the destruction of this modern scourge, we shall, at least, treat the disease itself on the same just principles as are, or ought to be, our guide in the treatment of all other affections. And what are these principles? Starting on the well-known axiom, that there is no effect without a cause, let us first endeavour to ascertain to what agent the production of the disease is to be ascribed; and then, having satisfied our minds on this point, direct our treatment to the removal of the cause, before we attempt to remedy the effects of that cause; and thus strike at the root of the evil, instead of wasting our time and our resources in lopping only the branches.

Having endeavoured to prove, that the disease known by the name of the Epidemic Cholera, Cholera Asphyxia, or Blue Cholera, is caused by the introduction of a poisonous substance into the system, the plan of treatment which, in this case, ought to be pursued would appear to be, that which is adopted with persons who have taken, either by accident or design, any particular or known poison. In these instances, the plan of treatment usually adopted has had two objects in view: the rendering inert, or removing out of the system, the poisonous substance; and the alleviation of those effects which may have resulted from its presence in the stomach, or other organs.

This would seem to be the course, both from reason and analogy, that we ought to pursue with persons

labouring under the effects of the choleroïd poison— as far as our means of induction and proof enable us to proceed. As, however, the poison productive of Cholera is of so subtle a nature, that we have been, as yet, unable to discover or collect it, either in or out of the body, we are necessarily prevented from making any of those direct experiments so conclusive in other cases. We cannot ascertain, by analysis, what the nature, or composition, of this poison is ; and, consequently, what are the substances capable of combining with it, altering its properties, or destroying its virulence. But, although debarred from pursuing this direct and conclusive course, there are yet other satisfactory, though less certain, proofs to be obtained, by experiments conducted within the body. We must endeavour to ascertain, whether any substance which is administered, either with this view or any other, and which, by its nature or composition, may be placed among the class of remedies termed antidotes, removes all the effects previously witnessed ; and under circumstances that oblige us to refer the beneficial change to the operation of the medicine, not to the efforts of nature, or any accidental circumstance. If so, and if, also, the same result is obtained in a sufficient number of instances, we may reasonably infer, that the remedy is an antidote to the poison. This conclusion, however, can only be drawn when neither vomiting, nor purging, or other sensible effect, is produced, after the exhibition of the remedy ; for

otherwise, as Orfila has justly remarked with respect to emetics given to persons who have swallowed any poisonous substance, we are not certain but that the restoration of the patient may depend on the expulsion of the poison—upon which the chemical re-agent has exerted no influence.

These remarks apply to a host of remedies, and particularly to calomel, which, we are told by Dr. Ayre, “is at once a medicine that claims, not merely to be united with others in the treatment of, but, to be relied on as a *specific* in, this complaint, and to be employed to the exclusion of all others.”*

That calomel acts on general, and not on specific, principles, has been already shown—a conclusion confirmed by other facts. One is, and it is the most important of all, that the saturation of the system by mercury does not prevent an attack of cholera. “A lady,” remarks Mr. Bilkett, “under the full influence of mercury—as evidenced by the mouth—given by another medical man, for some other complaint, was seized with vomiting, purging, and sudden collapse, and sank in a few hours.”† Mr. Anderson, also, surgeon to the York Cholera Hospital, remarks:—“Two patients, who died in the collapsed stage, were brought to the hospital in *profuse salivation*, to which they attributed the attack: and Mr. Hay, a short time since, mentioned to me a similar case, which

* *Lancet*, vol. 2, 1831-32, p. 461.

† *Ibid*, Sept. 22, 1849.

proved fatal.”* In addition to the above, the treatment of the Asiatic cholera by calomel alone has been worse than useless. In one of the numbers of the *Journal Universel et Hebdomadaire*, a letter was inserted from M. Londe, the President of the French Commission, then in Poland, in which we find the following remarks, respecting the administration of calomel in that country:—“It was employed here, in the hospital of Bagalette, by Mr. Searle, an English practitioner. In this hospital,” adds the writer, “the mortality was frightful.”

After such facts as these, what, we may ask, becomes of the “specific” of Dr. Ayre? It is clear that it can possess no specific properties; if it were a specific in the disease, it would also be a prophylactic against it—a property possessed by all true specifics. This is a conclusion at which we might have arrived from other facts—from those previously detailed, while considering the power of calomel in producing a flow of bile. The treatment of Dr. Ayre has, it is true, sometimes been successful, otherwise it would never have been recommended: nor would Dr. Ayre have ventured to call it a specific. This success, as will be more particularly shown hereafter, is to be ascribed, *not* to the calomel, but to the remedies with which it was associated. Dr. Ayre, while deceiving others, had been himself deceived: hence, the promulgation of a false theory, and, hence, the

* *Lancet*, vol. 1, 1832-3, p. 72.

lamentable results which followed the adoption of this mode of treatment, by individuals who administered the calomel alone, and without any adjuvant. Unfortunately, errors of judgment in medicine are not like those in law ; the medical client is generally placed *hors de combat* before he has an opportunity of appealing to another tribunal !

We may draw the same conclusions respecting the powders of Vivorera, extensively employed in some parts of Spain, as a remedy and specific in the epidemic cholera. These powders were originally prepared from an old receipt which fell into the hands of D. José Melgarejo, Pharmaceutist of Murcia, since which they have been employed as a specific, in that part of Spain, in cases of poisoning from venomous reptiles and insects, and also in hydrophobia.* Of their virtue in these affections no doubt can exist ; but, that their efficacy is due to some general, and not to a chemical, or specific, action in the system seems also equally certain—for they appear to have produced the like good results in cases of mineral and of vegetable poisoning. As their action in the economy is that of a stimulating diaphoretic, to this effect must be ascribed all the cures that have been obtained by their administration. We cannot suppose, that the same combination would neutralise the

* They are composed of equal parts of “*Eryngium campestre* ; *Echium vulgare* ; *Alyssum spinosum* ; and a species of *Nepeta*, or Cat mint.”

poison of reptiles or other animals, and, at the same time, destroy the irritant and corrosive properties of cantharides, arsenic, and corrosive sublimate. The same remarks will apply to the epidemic cholera; for although these powders were proposed as a specific in this disease, and, in the hands of certain practitioners, almost entirely trusted to at the commencement of the epidemic, they were subsequently abandoned as useless, and their place supplied by other agents. This is a fate that must attend all remedies which act only on general principles; when trusted to, singly and uncombined, in every stage of a disease which presents various, if not opposite, effects at each separate period.

Many other substances have been recommended as specifics in the epidemic cholera; but, independently of the remedy which will hereafter come under consideration, I am not aware that any substance has been hitherto given which could be said to exert an antidotal power on the choleroïd poison. The *modus operandi* of the whole of them can be explained on other and different principles: while, very few have been shown, on trial, to be worthy of the name, even, of a remedy.

But this is not the only method of treating such a disease. If unable to effect the neutralisation of the poison, our only resource, then, is to endeavour to remove it out of the system, by those means usually resorted to on other occasions, or, such as experience

points out as most eligible in this particular disease. Many different remedies, from their known action in the economy, and which have been employed by various practitioners, tend to produce such a result; and have, doubtless, effected it, when either of these methods proved beneficial. The success which has attended the exhibition of calomel, emetics, certain purgatives, and diaphoretics, can only be explained on this principle.

The remedies which have acted with the greatest success, in the expulsion of the poison, are, as we might have presumed *a priori*, the class of evacuants; and, of these, emetics have been the most beneficial. This will be evident by a reference to the Table which follows (Table A, p. 44), by which it appears, that the best result has been obtained by the exhibition of tartar - emetic, and, after this, of salt and water. Ipecacuanha, so valuable in dysentery, does not give a favourable result—the rate of mortality being above the average. What the exact cause of the difference is, it would be somewhat difficult to say, without more precise knowledge on the subject.*

Cold water, without any adjuvant, has also been employed as an emetic—it having been found that water, taken in large quantities, at a particular period of the attack, was immediately rejected. It was in 1832, that Dr. Shute first made the profession ac-

* The Ipecacuanha being given in other localities, and in different cases, presenting a variety of type, would at once account for the variation.

quainted with this method of treatment; at which period, and, in his hands, it proved highly successful, We are unable to ascertain, what the general result of this mode of treatment has been, for the want of statistical data. Dr. Shute, in his Report to the General Board of Health, stated that, of 48 patients treated by him, 11 had died, while 9 remained under treatment: deducting the latter, the rate of mortality would be 28 per cent. This is a very low rate, when we find that these cases were all cases of collapse—the treatment being more particularly applicable to that stage of the disease. It is, in fact, at the commencement of collapse that the vomiting is, in general, first experienced; while it continues more or less obstinate for some time after. The act of vomiting, therefore, is more easily excited at this period than at any other by artificial means.

Although not to the same extent, purgatives have also been successful, in the treatment of the severe form of Asiatic cholera, as well as of the diarrhoeal stage. This method of treatment was resorted to during the first invasions of the disease in India, as will be apparent by a reference to the Reports of the three Medical Boards, published at that period. Calomel, croton oil, and the stronger purgatives were principally employed, and, in many instances, with success, judging from the detail of particular cases. In England, calomel, in large doses, so as to act as a purgative, has also been employed, but, as it has

usually been in combination with other remedies, it is not easy to ascertain its exact value.

The other and ordinary purgatives have been seldom resorted to in this country; arising, probably, from the fact that, in a cold climate, the purging is more severe, and of longer duration, than in inter-tropical regions. Their use, therefore, would not appear to be so much indicated as in those cases in which the collapse comes on suddenly, without being preceded by purging; and, in which, consequently, the congestion of the venous system is much greater and more apparent. Added to this, the opposite mode of treatment—that by astringents—has been the prevalent one in England, as previously remarked.

The above are the only remedies deserving consideration on the present occasion, although there are two other classes—viz. diaphoretics and diuretics—which tend to produce the same result, viz. the elimination of morbid matters from the system. But the influence of these agents must be very slight in a disease which runs its course so rapidly, and in which the renal and the cuticular secretions are entirely suspended. We have, in fact, few examples respecting their employment, and none respecting their efficacy.*

There is one agent which, if it has not a specific action, enjoys a sort of universal reputation in cases

* I ought, perhaps, to except sulphur, which was employed by Mr. Grove, in the visitation of 1849, and, as he states, with consi-

of poisoning—being alone trusted to for the expulsion of the syphilitic poison. But, as it takes some time—usually weeks, and occasionally months—to effect a cure, we could hardly expect that any benefit would be witnessed in a disease like the Epidemic Cholera. Its use, therefore, would seem to be restricted to that of a purgative, and in single, not in repeated, doses. This conclusion is not only confirmed by what has gone before, while speaking of Dr. Ayre's method of treatment, but by the experience of other individuals, who have tried both methods.*

The preceding arguments and conclusions will be rendered more apparent by a reference to the following table, by which it will be seen that the most successful method of treatment is that by tartar-emetic; the least successful, by the salts of Dr. Stevens—the *modus operandi* of which will be considered in a subsequent chapter.

derable success. But, then, he administered it in combination with carbonate of soda, which invalidates his evidence to a certain extent. It can be readily understood, however, that such a remedy would be beneficial, in the first stages of the disease, in the absence of a better and more specific one.

* An anonymous writer in the *Lancet* observes : "I beg to offer my experience, as most positively in favour of the larger quantity (of calomel), which seldom requires to be repeated, and, never more than once, or at the most twice, whilst it is shown that, in what appears to be the mildest treatment (by small doses), the quantity taken varies from two drachms to an ounce."—A. B. M., *Lancet*, Sept. 1, 1849.

Table A.—Comparative view of the various modes of treatment adopted in cases of Cholera in Europe, in 1832 and 1849, with their results.

Modes of Treatment.	Deaths per Cent.
Tartar Emetic	19
Salt and Water	20
Cold Water and Ice	30
Ipecacuanha	57
<hr/>	
Average	31
Calomel and Opium	57
Opium	58
Bleeding, Calomel, and Opium	59
Calomel	62
Stimulants	66
Bleeding alone	85
Dr. Steven's Salts	88
Ditto, by Injection into the Veins	90
<hr/>	
Average	70

NOTE.—The above table has been formed from those of Mr. De Grave and Mr. Fergus, giving the result obtained in the City of London and in Vienna, in 1832, and, from that of Mr. Ross, obtained from a variety of sources, in 1849. These tables were inserted in the *Lancet*, and in the *Medical Times*, at the periods referred to.

In the majority of diseases, the above method of treatment—that by expulsion—would not only be the best, but, it would also be attended with considerable suc-

cess ; for this is, after all, the ordinary method of treating diseases. It is the allopathic, as well as the homœopathic, mode of treatment ; and is as old as the science of medicine. The only novelty in homœopathy is, the infinitesimal doses of medicine, and the infinite amount of—humbug!—the one swallowed by the public as complacently as the other. In Cholera, the same result cannot be obtained as in other diseases by this method of treatment, for very good and substantial reasons.

If the same favourable result as that given in the table, from the administration of tartar-emetic, could always be obtained, there would be little more to wish for, in the treatment of this usually intractable malady. But this will never be the case. It is only in temperate climates, and in those cases in which the duration of the collapse is long, that emetics can be continued for a sufficient time to effect the object in view. It is by the repeated and continued action of vomiting that the poison becomes expelled ; not by two or three discharges ; otherwise this result would be obtained by the efforts of nature, independently of those of art. At Greville-street Hospital, 10 patients, who were restored, drank 225 gallons of water—rather more than 22 gallons each—in 78 hours ; while one patient, who died, took 40 gallons in 96 hours—4 days. When days are reduced to hours, there would evidently be no time for the beneficial operation of such agents.

Then, again, it is sometimes impossible to produce emesis in the severe form of the complaint, and during the state of confirmed collapse, by the administration of the strongest emetics. This I in vain attempted with some of the cases in Jamaica—and there were many of them—that came under treatment, for the first time, in a state of confirmed collapse. The same result has followed the exhibition of large draughts of water, given to produce vomiting.

As regards water, it is not only useless as an emetic in the instances named, but it cannot be taken with safety in large quantities—in quantity sufficient to produce emesis—in warm climates. In India, according to the united testimony of practitioners there, the imbibition of water is considered to be injurious, and, when taken in large quantities, fatal. The truth of these conclusions I had an opportunity of ascertaining when in Jamaica. A lad, in a state of confirmed collapse, got out of bed, during the momentary absence of his mother, and emptied a pitcher, containing about a quart of water. Arriving immediately afterwards, I administered a strong emetic—obtained at the Overseer's house, close by; but neither the emetic nor the water was rejected, and the patient died suddenly an hour after.

It is thus apparent, that the treatment by emetics, valuable as this treatment has proved, at particular times, and in particular instances, cannot be always

adopted, or with safety, for the cure of the Asiatic cholera. The same conclusion must be drawn with respect to purgatives. Although, next to emetics, they are more valuable than any other general remedy, their use is limited in some cases, and, contra-indicated in others.

It is in the diarrhœal stage, and, when this stage is wanting, or only of short duration, at the commencement of collapse, that the administration of purgatives would appear to be the most called for, and the most to be depended on. After this, or when the collapse becomes confirmed, no benefit is likely to accrue from their exhibition. On the contrary, much injury might follow from their employment—more particularly in those cases in which the discharges are profuse, or have been of long continuance. Were it possible to expel the poison by this channel, in such instances, it would be effected by natural means: not being effected, to increase the discharges would only be to run the risk of producing an uncontrollable catharsis, and thus to hasten the fatal termination. This result has been witnessed by me in numerous instances. As, therefore, purgatives can only be employed at the commencement of an attack, and as, in severe cases, there is seldom time to repeat the dose, there is little chance of effecting a cure by the employment of purgatives alone. Like emetics, they are valuable agents at particular times, and, in particular cases,

but they cannot be considered as general and certain remedies, applicable to all patients and all circumstances.

In addition to the above, it is very doubtful whether purgatives, administered in the stage of collapse, could effect the object in view—the expulsion of the poison. Purgatives, before producing their specific effect, must be assimilated, if not decomposed, and be absorbed into the blood. Hence, a certain interval always elapses—several hours—before they act on the bowels. But the processes of assimilation and absorption, as we have a right to infer, are suspended during the state of confirmed collapse. Independently, therefore, of the want of time, to enable these remedies to produce a depurating effect on the blood, we may conclude that they would neither be decomposed nor absorbed during a state of confirmed collapse. Their action, then, in all probability, is entirely local; that of a stimulant, or irritant, on the mucous membrane of the stomach and bowels—the very effect that ought to be avoided at this stage of the disease.

As the preceding are the only remedies, among this class, likely to be productive of any benefit, we may infer, that the treatment by expulsion, although the best that can be adopted, in the absence of a more specific method, can only be of very partial success, when generally trusted to for the cure of this rebellious disease. Hence the necessity of a specific re-

medy for the Asiatic Cholera—the same as for many other diseases, at present incurable, or nearly so.

Were it otherwise, however, or were the Asiatic Cholera less intractable, a specific method of treatment is always to be preferred, as the most certain, if not the most scientific. Unfortunately, it is seldom that we possess a specific for any disease—the majority of the so-called specifics being either no specifics at all, or, else, acting on general principles. Mercury, for instance, is considered to be a specific for Lues Venerea; but it is not so in reality. Poisons, when imbibed into the system, are frequently expelled without producing any ill result, by what is termed the efforts of nature, or, in other words, by means of the different secretions. As mercury has the property of increasing all the secretions—as evinced by the great increase of the salivary secretion—there is thus a greater chance of the poison being expelled than when left to the efforts of nature, or of chance; and hence the reputation which this drug has acquired in syphilitic as well as in similar complaints. But, then, the same results can be obtained by other agents, which produce the same effect: as such, the action of mercury is a general, not a specific, action. A specific, properly speaking, is a remedy which, administered in those diseases that are produced by the operation of a poisonous agent, acts, not on the body, but on the poison.* As the term, however, has been

* “ All toxicologists agree, with the few exceptions to be pre-

otherwise employed, it will be desirable, in future, to substitute the word *antidote* in the instances under consideration; the same as for those substances that have the property of neutralising well-known poisons out of the body, or, which are taken into the stomach, in cases of accidental poisoning.*

Not that such a word has been required up to the present time; for although the discovery of an antidote for the ills that flesh is heir to has been the dream, and has occupied the time and attention of philosophers and doctors, from the earliest ages to the present time, no such agent has been discovered for a single disease! It was not the transmutation of metals, or the conversion of lead into gold, that alone occupied the time, and wasted the lives of the alchemists of old—labours and researches that laid the foundation of that science which is now so perfect; it was also the hope of discovering an *Elixir*

sently stated, that there is no substance at present known which possesses a *specific* effect, in removing *the symptoms* produced by poison. The so-called antidotes (specifics!) act upon the poisons, and not upon the body; their action is, therefore, strictly of a chemical nature." *Taylor; On Poisons*, p. 73.

* "By employing the term antidote, it must not be understood that we mean a *specific* for any individual poison. The operation of antidotes is purely chemical, and their great use depends, first, upon their effects being either immediate or extremely rapid; and, secondly, on their action being local, *i.e.*, on their producing an alteration in the nature of the poison by mere contact." *Loc. cit.*, p. 73.

Vitæ ; an agent which, if it did not render man immortal, would, at all events, save him from the inroads of those diseases, that so terribly shorten the span of human life, and prevent his arriving at the human goal—the three score years and ten. Alas! for the limitation of human knowledge, as well as of human life! Nearly 6,000 years have now passed away, without the coveted antidote, like the coveted gold, having been discovered in the alembic of the chymist, or in the brain of the physician. But this is no reason why we should despair of accomplishing this grand object, compared with which all others are as nothing. What, indeed, would be a mine of gold, if balanced with the possession of an antidote for a single disease ; especially for a disease like the Asiatic Cholera. The one would not snatch a single victim from the jaws of death : the other might be the means of saving millions of lives, should the Epidemic Cholera continue to return, at shorter intervals and with increased intensity—as there cannot be a doubt will be the case—for the next two or three centuries.

This *desideratum*, this great boon to suffering humanity, unless my previous deductions be erroneous, and unless I and others have been led away by an *ignis fatuus*, luring us on to our doom, does exist for the Asiatic Cholera—that disease which, in the short space of half a century, has swept away several millions of the human race. As this is not the first time

that such an announcement has been made, it is only necessary to add, it will be my endeavour again to prove, in these pages, that Carbon, in its simple and compound forms, is that antidote.

Having formed a particular theory respecting the Epidemic Cholera, I was induced, during the first invasion of this disease in England, to administer the different forms of carbon, for the purpose of confirming or refuting the truth of my doctrine. The result of the first trials, with this remedy, not only confirmed the expectations I had formed respecting its efficacy, but led me to conclude, even at this period, that Carbon and Carbonic acid, *but more particularly the latter*, remedied the effects witnessed in the Epidemic Cholera; at the same time that they removed, by their specific action, the cause also. This will, it is hoped, be apparent to others, by a recital of the results obtained from the exhibition of these remedies.*

* As it happens, there are few diseases in which it would be so easy to establish the truth of such a conclusion as in the Epidemic Cholera. The suddenness of the attack; the rapidity with which it proceeds; the intensity of the symptoms; and its fatal termination, when unchecked by the efforts of art, render the beneficial operation of any remedy so apparent, that it is impossible for any one to be mistaken on the subject; unless we allow, with some persons, that recoveries from attacks of Cholera are sometimes due to the remedial powers of nature, as well as to the efforts of art. Such a conclusion, however, is alike contrary to reason and experience; for, as the writer of the Madras Report truly re-

In detailing the curative virtues of this agent, it is right to refer, in the first place, to the more common form of the remedy, viz., the simple Carbon, or Charcoal. Although my own experience with this preparation has been very limited, in comparison with the other, or gaseous, form, I can, fortunately, refer to the evidence of several other practitioners, and particularly of Dr. Wilson, of Xeres, in proof of its efficacy. This gentleman, who saw a notice in one of the English medical journals,* that the employment of Carbon had been recommended by me, was induced, in consequence, to resort to this remedy in the severe and fatal irruption of the Epidemic Cholera in that part of Spain. Having resided at Xeres for some years, and enjoying a considerable reputation among the Spanish population, as well as among the English residents, Dr. Wilson not only attended a greater number of patients himself than any other practitioner, but he was most ably assisted by several of the resident English merchants, who nobly volunteered their services on the occasion. The latter confined themselves, almost exclusively, to the administration of charcoal; it being only a simple and innocuous remedy like this that could be safely employed by non-professional persons. Not so Dr. Wilson, as he

marks, we may consider *death* as the ordinary termination of Cholera; and there is, in truth, very little variety in the course which the disease pursues towards it.

* Med. Chir. Review, Jan., 1833.

was obliged, for the reasons that subsequently induced me to resort to other forms of the same remedy, to abandon the *exclusive* employment of Carbon in some cases, and even its partial administration in others. These were, the difficulty of inducing many persons to take it, in sufficiently large doses, by the mouth, on account of its homely and apparently disagreeable nature; the impossibility of superintending its exhibition by injection, in the majority of cases; and the fact, that an additional, or a different, agent is required in the state of confirmed collapse.

Still, the cases treated by Dr. Wilson, with Carbon *alone*, were sufficiently numerous to enable him to form a correct opinion of its efficacy in every stage of the disease; while, as regards his assistants, I was afterwards informed by one of them, that he considered several thousands were cured by the administration of charcoal, as they had taken no other remedy. Dr. Wilson states, in the MS. which he forwarded to me, detailing the history of the disease in Xeres, that "the Carbon was given, from the commencement to the termination of the epidemic, with such general good effect, as to have impressed me with the firm conviction, that, in all stages of the disease, it is a most beneficial adjuvant; and *anterior to collapse*, and in the stage of reaction, most eminently curative;" or, in other words, that it is a specific remedy for the Epidemic Cholera, when administered previously to the state of confirmed collapse. The

cause of its failure, at the latter period, will be explained presently.

These results agree with those previously witnessed by me, on a more limited scale; for while this gentleman confined himself exclusively to this form of the remedy, I, on the other hand, was induced, after the first trials, to employ it only as an adjuvant, and to depend, as exclusively, on the gaseous form of the same preparation,—a fact that Dr. Wilson was in ignorance of until my arrival at Cadiz, after the subsidence of the epidemic in Xeres. Had he been acquainted with the superiority of carbonic acid gas, it would, doubtless, have prevented his resorting to other adjuvants in the treatment of the disease; and thus negating, to a certain extent, his otherwise important evidence.

As it is, however, we may fairly conclude, from the facts already stated, not only that Carbon is highly beneficial in attacks of the Epidemic Cholera, but that it also possesses specific properties, as there is no way of accounting for the result, excepting on the supposition that it neutralises the poison productive of the disease. In fact, we know that this agent combines with, and renders innocuous, putrefactive and other matters injurious to animal life, when added to them, either in or out of the body. This conclusion is strengthened by the result obtained from the administration of carbonic acid gas; a form of the remedy to which the same objections do not apply

as to the simple Carbon; while the effects witnessed are not only more apparent, but, also, more prompt and decided. This will be evident by a reference to the results that have been *invariably* observed by me, after the exhibition of carbonic acid gas.

When this remedy has been given, in those cases in which symptoms denoting derangement of the stomach are alone present, the effect of the medicine, according to my experience, has been to relieve the symptoms almost immediately. The nausea is speedily dissipated; the giddiness and faintness disappear; and the sensation of burning, or of heat, at the pit of the stomach, is no longer felt or complained of, after two or three doses of the medicine.

But the most remarkable circumstance is the result which has followed the employment of the same agent during the stage of diarrhœa—that relaxation of the bowels which is, almost always, the precursor of the severe form of Cholera. Of the numerous cases in which I have given the carbonic acid at this period, it has invariably arrested, with some few exceptions to be explained hereafter, the morbid process; at a longer period, it is true, but still at a regular and certain interval.

Neither is the remedy less useful in what has been termed the evacuant, or second, stage of the disease, characterised by vomiting and rice-water evacuations. In these cases, the irritability of the stomach is speedily relieved, and the vomiting ceases soon after the first,

or, at most, the second dose of the medicine ; while the relaxation of the bowels is also arrested, with as great, if not greater, celerity than in the former instance.

But it is at the commencement of collapse, that the efficacy of the remedy is best observed, and its *modus operandi* the most apparent. Not only is the vomiting arrested, as in the previous stage, but the thirst, heat, and burning sensation at the pit of the stomach disappear almost as speedily. During a repetition of the medicine, the spasms, which generally prevail at this period, are effectually relieved ; the evacuations from the bowels become less abundant and less frequent ; the depression of the system is removed ; and the other symptoms, characteristic of this stage, vanish by degrees ; so as frequently to leave the patient comparatively free from all ailment, after the administration of only five or six doses of the medicine. These remarkable and, hitherto, unexampled circumstances have frequently excited the attention and admiration of others, and are dwelt on with peculiar emphasis, by Drs. Ardevol and Pascual, in the reports which will be inserted in another chapter.

It now only remains to ascertain the influence which the remedy exerts, in the state of *confirmed* collapse. When it has not been previously administered, its employment, at this period, is not constantly followed by any immediate effect, or sensible result. This, however, is only what might, and what ought to, have been expected, from the employment of

such a remedy at such a period; there being several causes in operation, either of which would, it is probable, satisfactorily account for the apparent failure in this instance.

In the first and second stages of the disease, and, generally speaking, in the commencement of collapse also, we should have been led to infer, *à priori*, that no obstacle exists to the complete neutralisation of the poison; an inference confirmed by an extended experience in the administration of the remedy now under consideration. But in the state of confirmed collapse, when the circulation is entirely suspended, the same result can hardly be expected to follow; at least by the introduction of the remedy into the stomach. It is then more than doubtful if absorption can take place, except to a very limited extent; as the experiments of Magendie on animals have clearly and satisfactorily demonstrated.*

* In stating my belief that carbonic acid is an antidote to the poison of Cholera, and that, when introduced into the system, it is capable of neutralising the morbid matter productive of the disease, I presume that no objection will, or can, be offered to such an inference. All doubts on this point would appear to be solved by the result of the experiments of Drs. Lawrence and Coates, as detailed by Dr. Copland, in his Notes to Richerand's Physiology. In reasoning upon the subject of absorption, the question has frequently arisen, observes this writer, whether the articles found in the living fluids exist there as chemical substances; or, have their chemical nature altered and animalized, by the action of the vessels through which they have entered the system. It was

But, although we were certain of accomplishing this most desirable object—the neutralisation of the poison—it is uncertain whether we should, without the assistance of other adjuvants, be also able to remove the state of collapse, when it has become confirmed, or has been of long continuance. It is clear that, at this particular period, two things are necessary; first, to remove the cause, and then to remedy the effects of that cause—both which are probably to be effected by opposite and different means. It is not likely that the agent which neutralises the poison, the injurious operation of which on the system is the cause of the collapse, will also be able to remove the collapse itself—unless it were a compound substance, possessing, besides its specific properties, those of a stimulating nature, or such as act directly on the nervous system. In cases of partial collapse, the mere removal or neutralisation of the poison has been, of itself, sufficient to restore the

therefore deemed a curious subject of inquiry, whether artificial chemical changes can take place in the fluids, while they continue to circulate in living vessels, and the ordinary actions of life go on. With a view of ascertaining this point, they commenced by throwing prussiate of potash into the cellular substance, and green sulphate of iron into the abdomen, in order to try whether the well-known result of their admixture, prussian blue, would be produced in the vessels. On performing this, they were gratified by the striking result of a distinct and beautiful blue in the thoracic trunk and its contents; and in nearly the whole substance of the lungs.

patient—as the history of numerous cases tends to prove. But in the majority of instances of confirmed and long-continued collapse, the same effects can hardly be expected to follow similar or other more specific plans of treatment. When this state has existed for a certain length of time; when the blood has ceased to circulate in the body, perhaps, for hours; when every vital function has become suspended, and the energy, or vitality, of the nerves all but annihilated; the organic life of the individual may then be said to have become extinct, and that portion of the system as lifeless as the body which has ceased to breathe. If such is the state of a patient in the state of confirmed collapse, can it be expected that the mere removal or neutralisation of the poison, were we able to accomplish this, will be sufficient also to remedy the effects produced by the long-continued presence of the deleterious agent in the human body? Reason and analogy both lead us to answer in the negative. When a man, after receiving a severe blow on the head, has a portion of his skull fractured and depressed, the pressure on the brain may deprive him of consciousness and voluntary motion. If, however, the skull be trephined, and the depressed portion elevated, soon after the accident, the mere elevation of the bone will be adequate to the restoration of the patient. But, if this pressure has been continued beyond a certain time, the energy of the brain will have become so far destroyed, that

the application of the trephine, and the use of the elevator will be insufficient to restore him to consciousness or voluntary power ; and he sinks, the victim of delay. So, again, when any of the various and numerous classes of poisons have been taken into the stomach, either by accident or design, if they are immediately neutralised or evacuated, the effects resulting from their exhibition also disappear, with the neutralisation or removal of the morbid matter. But if a particular interval has elapsed, before these desirable objects are attained, we shall then have to remedy the effects which have resulted from the longer presence of the poison in the system ; effects but too frequently followed by the extinction of life.

In conclusion, I will only add, that we shall never arrive at a fair and impartial decision respecting the antidotal power of any remedy in this disease, if we limit our view of its operation to that period, when the collapse of the system is complete. We cannot then judge, except indirectly, and by comparison with other cases, in which the antidote has *not* been administered, whether the poison has become neutralised or not ; as the mere neutralisation of the poison may not be sufficient to save the life of the individual. In using other means and other remedies, as is usually the case, to combat symptoms—the effects of the poison on the system—we run the risk of mistaking the action of these remedies ; and confounding those which tend to remove the cause with

those which are only able to remedy the effects. We are consequently necessitated to restrict our view, in seeking for an antidote, to the first periods of the known operation of the poison on the system ; or, in other words, the first two stages of the disease, and the commencement of collapse.

As facts are better than hypotheses, and as I depend entirely on the former, in proof of the truth of the conclusions now drawn, it is immaterial what explanation be offered of the way in which the different forms of Carbon act, so as to render noxious substances innoxious. But, as the agents are so different—the one being solid, and the other gaseous—some explanation would seem to be required.

As regards carbonic acid gas, observing, when it comes into contact with the gaseous products of putrefaction, that the offensive and noxious qualities of the latter are destroyed, we can only infer that these gases become chemically united, and that a compound is formed of an innocuous nature. At all events, the noxious gases lose their peculiar property, whatever may be the nature of the compound : in the same way as the alkalies lose their corrosive quality, and become converted into a mild carbonate, by their union with the same gas.

The exact action of the solid Carbon is not so readily understood. It may however be explained in two ways. In the first place, Carbon has a great affinity for all gaseous substances, some of which it

will absorb in large quantities. It has, also, the power of decomposing many substances; and if it acts in the same way with the gaseous products of putrefaction and the noxious elements of disease, their destruction can be readily accounted for. In the next place, charcoal, when recently prepared, contains a large quantity of carbonic acid, which it imbibes during the combustion of the wood. The presence of this gas in the charcoal, or its extrication into the surrounding space, would also act beneficially, by combining with, and neutralising those noxious products with which it came into contact. Hence the reason why the charcoal should be recently prepared, when employed medicinally. If exposed for any time to the air, its pores might become filled with various matters—gaseous and non-gaseous—and thus lose their absorbing property; while the carbonic acid gas would necessarily have been extricated at the same time. In this state, it might be, to a great extent, useless as an antiseptic agent.

We can also understand why the charcoal, although sufficient to cure the disease in the first stages, is insufficient in the stage of collapse. As it is not, and cannot be, absorbed, it could not come into contact with that portion of the poison existing in the blood; the greater part of which, it has been inferred, is contained in the venous system. The quantity of carbonic acid gas contained in its pores, even if absorbed, would also be insufficient to produce a

beneficial result—it being necessary to administer the gas almost continuously at this stage of the disease. We thus learn, what is also taught us practically, that the gaseous form of the remedy can alone be depended on in the state of confirmed collapse.

If these inferences be correct, we can hardly fail to conclude, after what has been said respecting the effects of carbonic acid gas in the first stages of the disease, that this chemical agent is an antidote to the poison productive of the Epidemic Cholera. At least, it appears to me to be impossible to explain its *modus operandi* on any other supposition; while a slight consideration of the subject will, I trust, convince every unprejudiced person of the truth of the conclusion.

Putting aside the cessation of the vomiting, which may be referred to a local action of the remedy, to what, I would ask, are we to ascribe the relief of the other symptoms, particularly the arrest of the purging? Not to any astringent property of the remedy, as I have often ordered this medicine with success in cases of constipation; thus showing that if it exert any action on the large intestines, it is as a relaxant, rather than as an astringent. As, also, the common saline effervescing draught has usually been administered, tartrate of potash or soda, a purgative medicine, has been taken at the same time. The quantity thus taken would not, it is true, have been sufficient, in every case, to produce a purgative effect with persons in health; yet, as even an excess of ripe fruit, or

vegetables, at the epidemic period alluded to, is so generally followed by relaxation, it may be asked how it happens that the contrary result is obtained in this instance?

Again—to what are we to ascribe the relief of the spasm? Not to the antispasmodic property of the medicine, for this remedy has no action in common with the greater number of those belonging to the class of antispasmodics.

And, lastly, to what is the removal of the depression of the nervous system and of the state of collapse owing? To the stimulant property of the remedy? Assuredly not; because it possesses, if any action, that of a sedative, as is well known when this gas is inspired in any quantity. Neither can we ascribe the removal of one set of symptoms to the relief afforded to any of the others; for these various phenomena can only be common effects of one common cause—being sometimes present, sometimes absent. Besides, I have experienced the same result when vomiting, or purging, or spasm, or collapse, was alone present, and the remaining symptoms altogether absent.

How then, we may ask, can a remedy act, which arrests the most severe and long-continued vomiting at the first or second dose, and this, too, after every other calmant and narcotic has failed; which, possessing no astringent property, puts a stop to the most profuse purging; which, endued with no known anti-

spasmodic virtues, instantly relieves the severest spasms; and which, although acting as a slight sedative, produces reaction, and removes the depression of the nervous system, at a time when some of the most important of the vital functions are almost entirely suspended? A remedy which produces such results, and which has no sensible or direct action in the economy, can only act in one way; that is, by removing the cause of these various phenomena; and as that cause has been shown to be the presence of a poison in the system, the agent which removes it, or the effects produced by its injurious operation, must combine with, and render innocuous, the deleterious matter. The different forms of carbon, more especially the gaseous form, may therefore be considered SPECIFICS in the Asiatic Cholera, and ANTI-DOTES to the poison productive of it.

This conclusion is confirmed by another fact, which is, that when the gas has been given in sufficient quantity, and at a proper time—before the state of confirmed collapse—the attack has never, as far as my experience goes, been followed by consecutive fever. This is a remarkable circumstance, particularly in Europe, where the proportion of deaths has sometimes been greater in the consecutive fever than in the collapsed stage. “To give a notion of the importance and danger of the Cholera Fever, a most intelligent physician, Dr. Weimer, of the Merchant Hospital, informed us, that of twenty cases, treated under his

eye, who fell victims to the disease, seven died in the cold stage, and thirteen in the consecutive fever."* In England, and other parts of Europe, the proportion of fever cases was less than the above; but these were sufficiently numerous to cause the above-mentioned result to form a remarkable exception to the general one. Even in the most successful of the ordinary methods of treatment—that by emetics—the consecutive fever was not prevented. On the contrary, the administration of cold water, in emetic doses, was abandoned partly in consequence of its appearing to promote reaction, and to increase the intensity of the consecutive fever. The opposite result—the prevention of this stage—can only be explained on the supposition, that the gas acts, not only by remedying the effects, but, also, by removing the cause—the presence of an extraneous and poisonous substance in the blood. As, also, carbonic acid cannot expel the poison—not possessing any evacuant property—it must unite chemically with the morbid matter, and render it inert. Hence, if given in sufficient quantity, it will not only remove the effects then present, but it will also prevent the supervention of others. It must therefore be a true antidote, and only requires to be brought into immediate contact with the deleterious agent, in order to obtain the results previously described. As, however, the poison

* Report of Drs. Russel and Barry, to the Privy Council.—1830.

is contained in the blood, and as the human body is not an inert vase, or a chemical test tube, the admixture of the antidote and the poison can only be effected in a particular way, and to a certain extent at certain times and under peculiar circumstances. What these circumstances are will form the subject of the next chapter.

CHAPTER IV.

ANTIDOTAL TREATMENT.

As it is absolutely necessary, that the medicine should be taken in a proper manner—for, otherwise, the patient will only be swallowing a simple solution of tartrate or citrate of soda, instead of a certain portion of carbonic acid gas—it may not be superfluous to point out what I consider to be the best mode of preparing the effervescing draughts.

Thirty grains of the powdered bicarbonate, or sesquicarbonate, of soda or potash should be put into a large tumbler, with a wineglassful of water; to which is to be added a dessertspoonful of any simple syrup, mixing the two ingredients together so as to form a homogeneous mass. Then take twenty grains of citric or tartaric acid, and dissolve them in half a wineglassful of water, when the solution is to be poured on the contents of the tumbler, and the mixture drank off immediately, *before the effervescence has subsided*. If more convenient, or when to be obtained, lemon-juice may be substituted for the citric and tartaric acid—in the proportion of two table-spoonfuls of lemon-juice to the same quantity of soda or potash.

As the object in giving the syrup is to render the mixture more tenacious, and to prevent the gas escaping as rapidly as would otherwise be the case, it is not necessary, when the lemon-juice is used, to add any syrup.

Instead of the saline effervescing draught, Soda or Seltzer water may also be administered. One objection, however, which applies to these, and all other kinds of bottled, aerated liquors is, that during moments of anxiety and sickness, unless the patient and the attendant are well accustomed to the administration of such drinks, it is seldom that the draught is taken before a considerable part, or nearly the whole, of the confined air has escaped from the containing fluid. In this case, the remainder would be, in a great measure, useless ; as, unless under pressure, water absorbs and retains but a small quantity of carbonic acid gas. This inconvenience may be obviated, to a certain extent, by putting a teaspoonful or two of syrup into the tumbler, before the bottle is uncorked. When a stimulant is required, a teaspoonful of wine or brandy can be used instead. For children and infants, the quantity will necessarily be less, and must be proportioned to the age. With the latter, and also with the former, when they hesitate or refuse to take the bubbling liquid, the solutions of soda and tartaric acid can be given separately—one after the other. The soda water should also be used for common drink, after the

effervescence has subsided, as water will always retain its own bulk of gas.

In addition to the above, both the gas and the carbon can be employed in the form of enema, in the manner pointed out hereafter—the quantity being, of course, proportioned to the age of the patient.

At the first onset of the attack, when those symptoms are present which denote derangement in the stomach, without being preceded by diarrhœa, any of the preparations which contain carbonic acid may be given, and be repeated every hour, until all unpleasant symptoms are entirely dissipated. The first dose has always, with me, given immediate relief, and the third, at most, removed every symptom but that of a peculiar sensation of lassitude and languor.

In the preliminary diarrhœa, a dose of carbonic acid gas should be taken every two hours. This, in general, is sufficient either to arrest the diarrhœa or to change the character and appearance of the evacuations. Should the relaxation continue, after the medicine has been taken three or four times—which is sometimes the case with particular individuals, suffering from debility, or exposed to external causes, as cold or damp weather—it will then be advisable to substitute the simple carbon, which ought to be continued until the purging has entirely ceased. The best and most efficacious way of administering the carbon, at this stage of the disease, is by enema—two or three table-spoonful of the powder being mixed

with any convenient fluid, and suspended in it by means of the white of an egg; or, if preferred, a tablespoonful of the same preparation may be given by the mouth, and be repeated every two hours, until the purging ceases. When, however, the charcoal cannot be obtained, or the patient objects to take it, which frequently happens, we may then substitute the carbonate of soda, given in half-drachm doses every hour, or the prepared chalk, as in the common *mistura cretæ*. These preparations never fail to remove the relaxation, *after* the administration of carbonic acid gas to the extent already advised.

This difference in the effects of the remedy, at these two periods, may, perhaps, be ascribed to the circumstance, that the carbonic acid does not always reach the large intestines in sufficient quantity to arrest the morbid process—the principal part of the gas being absorbed from the stomach, and conveyed onwards to the lungs, where it will escape with the expired air. The greater benefit derived from the soda or the chalk may therefore depend on the carbonate not being decomposed entirely in the stomach, or the whole of the gas liberated, until the remedy reaches the large intestines. The astringency of the lime will also tend, at this period, to effect the object which we have in view; for although I have but too often witnessed the inefficacy of pure and simple astringents, when given alone, they may be advantageously employed, if only intended to remove the

simple relaxation consequent upon the previously depressing operation of the poison; that is to say, remedy an effect, the cause which produced it having been removed by other and different means. The superior efficacy of the common carbon may also be explained on similar grounds—the non-decomposition of this substance in the stomach, and its arrival in the large intestines in the same, or nearly the same, state as when first introduced into the above organ.*

But it is not sufficient to remove all the morbid symptoms, which may have existed in the above divisions of the first stage of the disease; it will also be necessary to continue the administration of the gas, only at longer intervals, until we suppose that all danger of the supervention of the other stages has entirely passed. This period will, of course, vary with different individuals; but it cannot be difficult for a professional man, conversant with the *reigning type* of the epidemic, and acquainted with the usual march of the disease in other cases, to form a fair criterion of the probable time when the collapse would have supervened, had the disease run on unchecked to that stage. As, also, no harm can

* In making use of this remedial agent, it is of the utmost importance, for the reasons previously assigned, that it should be recently prepared—otherwise, little or no benefit will attend its exhibition. In fact, it ought to be prepared at the moment, or at least for the occasion, and be preserved from the contact of the external air in glass-stoppered bottles.

result from the employment of the remedy for weeks, much less days, administered every three or four hours, it will be better to err on the right side ; and to give the patient the benefit of any doubt that may arise on this point, by continuing the medicine for a longer period than may be considered absolutely necessary for his safety or recovery.

In the second stage of the disease, characterised by rice-water evacuations, as the malady has then arrived at a point from which it always proceeds, at an accelerated pace, to the next stage, it will be requisite to administer the remedy at shorter intervals, as every half hour, until not only the vomiting, but the purging also, is entirely arrested. If the first dose, as sometimes happens, be rejected, it should be immediately repeated without waiting for the regular interval ; and this course is to be pursued as long as the medicine is rejected, although it is a very rare occurrence for the second draught not to be retained. So, again, if the purging continues to any extent, after four or five doses of carbonic acid have been taken, it will be advisable to administer the simple carbon, either by the mouth or by injection—giving a tablespoonful every hour in the former case, and three or four tablespoonsful in the latter. The patient must endeavour to retain the enema as long as possible ; but, if passed immediately, it should then be repeated, as well as after each evacuation ; or, at

least, as long as the fluid preserves its peculiar character, and contains neither bile nor excrementitious matter.

State of Collapse.—In the *commencement* of the stage of collapse, if no carbonic acid has been previously administered, a draught should be given every quarter of an hour, until three or four doses have been taken. If these have been retained, if the vomiting has ceased, and the spasms and other symptoms have become ameliorated, the medicine may then be continued every half hour, until the purging has ceased, or the character of the evacuations has changed, and they have become excrementitious; and until, also, the symptoms of collapse have yielded to the means employed, and the reaction is fully established. When the purging is severe, or of long continuance, it will be advisable to administer the simple carbon, either in the form of injection or by the mouth, the same as in the previous stage, and in the manner there pointed out.

As far as my own experience and observation go, the above treatment, if adopted at *the commencement* of the stage of collapse, is all that is required; the majority of the patients, to whom the antidote has been given at this period, having recovered; while the supervention of the next stage has been invariably prevented. This will be satisfactorily shown hereafter. Sometimes, as in inter-tropical regions, where the different stages run into each other with such

rapidity, the exhibition of the remedy at the commencement of collapse, or even in the previous stage, has been insufficient to prevent the collapse becoming complete. When this occurs, a certain time should be allowed, in order to ascertain the effect of the medicine; after which, if the patient does not rally, the measures hereafter explained must be had recourse to.

Occasionally, I have met with patients in whom— notwithstanding that the vomiting and purging have been arrested by the above treatment, and the state of collapse partially relieved—the reaction has yet been so slow, that it seemed desirable to assist the efforts of nature by the administration of some stimulant. The best that can be used at this period, in my opinion, is the carbonate of ammonia, which may either be given alone, or be added to the soda of an effervescing draught; according to the number of doses that have been taken of the soda, and the necessity there may appear to be for the repetition of the gas. On this point, no general rules can be given, as much will depend on the state of the patient, the effect that has been produced, and, lastly, the previous state of the bowels—as, when the evacuations are profuse and frequent, we have a right to presume that a great part of the remedy escapes out of the body, by this channel, without being absorbed.

But we must be prepared for cases of sudden collapse, without being preceded by the other stages of

the disease; and in which the situation of the patient is so dangerous, and the period allotted for the efforts of art so short, that nothing but the most active and vigorous treatment will be sufficient to raise the sinking powers, or to keep alive the flickering flame. Our only resource, under these circumstances, will be—in addition to the means already pointed out, for the treatment of a patient in the commencement of collapse—to resort immediately, if the symptoms are very urgent, or, else, *after the first few doses of the gas*, to stimulants and antispasmodics—as the different preparations of æther, the essential oils, carminatives, and ammonia. The selection of these must be left to the judgment of the attendant practitioner, as they will necessarily vary under different circumstances; for a remedy that would act with great energy with particular persons, and in particular countries, might be, to a certain extent, inert and useless in others. In other instances, the patient may come under treatment before the collapse is complete, in which case the stimulants and antispasmodics should be omitted. These attacks of sudden collapse, *if incomplete*, although requiring prompt aid and energetic measures, yield more readily to remedial means—when they are properly directed—than those cases in which the collapse comes on more slowly, and in which it has been preceded by the previous stages. When re-action takes place, or has been established, and symptoms supervene or remain characteristic of

the other stages of the disease—for, in these instances, the collapse is sometimes accompanied, and sometimes followed, by vomiting and purging—the treatment of the case will then be the same as if these effects had preceded the stage of collapse, and the disease had followed its usual course.*

Fortunately, these cases of sudden collapse are comparatively rare, except in inter-tropical regions: in colder climates, or in Europe, the state of complete collapse only supervenes, in general, after being preceded for several days by preliminary diarrhœa, and

* It has been stated by certain writers, that the severe attack is always preceded by diarrhœa. To show how fallacious this conclusion is, I may add the following history:—

During the invasion of cholera, in the town of Lucea, Jamaica, I received a pressing message one morning, while leaving the house, to visit a young woman—a European—close by. On going there, I found the patient on the floor, having fallen from the chair while at breakfast—the half emptied cup of tea, and the bread and butter, partly consumed, in the plate testifying to the suddenness of the attack. Although she had only vomited once, and although ten minutes had not elapsed from the first seizure, the pulse was weak and fluttering, the patient partially blue, and fast sinking into a state of complete collapse. After giving the necessary directions, I left, promising to call again shortly, but was unable to do so until two or three hours after. On entering the room, I found it vacant; and, on inquiring for the patient, was told that she was dead and—buried! And yet, I was assured, both by the patient and her mother, that there had been no previous relaxation of the bowels—no ailment whatever! But this was not a solitary instance; I met with many other cases of the same kind during my visit to the West Indies.

after being ushered in by the symptoms characteristic of the second stage of the disease. There is therefore, under ordinary circumstances, abundance of time to arrest the progress of the disease, before the patient arrives at the state, which has been aptly compared to that of a living corpse; and when, for the reasons before given, so many obstacles exist to the successful administration of any remedy. Unhappily, the treatment hitherto pursued has been insufficient, in the majority of cases, to prevent the supervention of collapse, even when resorted to in the first stages; while, as regards the remedy under consideration, more evidence will, I fear, be required, before the generality of practitioners can be induced to resort to it at the commencement of the disease, or until their own means have failed—even supposing the deductions drawn by me be valid. As therefore—either from a failure in the ordinary means employed, or from accidental circumstances—cases of confirmed collapse will occur in practice, it is a duty incumbent on us to be prepared for the supervention of this state, and to ascertain what measures can be adopted for its removal.

Although endeavouring, in the previous chapter, to impress on the mind of my readers the important inference, that if we wish to judge of the effect of the remedy, we must resort to it previously to the state of confirmed collapse, it does not follow that no good will result from its administration at this period. On

the contrary, although we cannot then judge of its antidotal property, it is plain that we ought, having established this point by other evidence, to place our chief dependence on carbonic acid gas at this period of the disease, the same as in the previous ones. There is, however, this important difference; that whereas the administration of the antidote is sufficient, of itself, to remove the disease in the first stages, we may be obliged, in the stage of collapse, to combine it with some adjuvant—as a stimulant—in order to remedy the effects produced by the long-continued operation of the deleterious agent—the primary cause of the attack. But, notwithstanding that it may be desirable to resort to stimulants, we must not conclude that this class of remedies is alone necessary at such a time; or that their exhibition would be attended with any good result, unless preceded or accompanied by some antidote, or an agent capable of expelling the morbid matter out of the system. As the stagnation of the circulation, and the suspension of those functions termed vital, or organic, are produced by the presence of a poison in the animal economy, the state of asphyxia can only be an effect of a particular and antecedent cause. The death of the individual also is due, not to the effect, but to the cause; for the extinction of life is but an effect, or the sum total of all the effects, produced by the same cause. How, then, could we hope to save life, if our views were limited to the remedying an effect—the state of collapse—with-

out having any regard to the removal of the cause—the presence of a specific poison! To attempt to arouse the nervous energy, at a time when another agent is in operation which tends to depress it, without taking any means to rid the system of its noxious presence, would be like pouring oil, instead of water, on the fire which we were endeavouring to extinguish. Even should we succeed in exciting the dormant energy of the nerves, and restoring the circulation, the patient would again fall into the same state, if the means resorted to were not such as to cause the neutralisation of the poison, or its expulsion out of the system. As, however, this proposition is so self-evident, I shall not attempt to pursue the subject further, but merely add, that the proper and only scientific plan to adopt, under these circumstances, would be, before resorting to stimulants, and, afterwards, in combination with them, or at the same time, to administer the antidote, the same as in the previous stages.

Independently of analogy and reason, I can confidently state, as the result of my own experience, that the antidotal treatment is infinitely more beneficial than any other method in the state of confirmed collapse; a conclusion verified by other practitioners, as will be evident by a reference to the reports added hereafter. We are therefore bound to resort to the antidote at this period, the same as at the others, and to persevere in its use, until either

reaction takes place, or all hope has entirely vanished. Should we fail, we shall have the satisfaction to know, that our want of success is due, not to the wrong measures adopted, but to the insufficiency of the means for the end desired; and we shall learn, that there is, in this disease as in all others, a limit to human skill, and a power, which presides over life and death, greater than that possessed by weak and finite man.

When, therefore, the collapse has become confirmed, it will still be necessary to continue the employment of the different forms of carbon; with this only difference, that they must be exhibited at shorter intervals, and in somewhat less quantity at each time. This is necessary, not only because time is precious, but, also, because the absorption of remedies, taken into the stomach, can only be effected to a very limited extent during the state of complete collapse. The experiments of Majendie have clearly shown, that liquids are not absorbed *at all* when the venous system is in a state of great plethora and congestion: and it is to this circumstance, that we must attribute the failure of all ordinary remedies in the collapse of cholera; as well as the apparent inefficiency of the different forms of carbon, when compared with their effect at other periods. Not that the gaseous form of the remedy is incapable of being absorbed, as such a conclusion would be erroneous. Although not nearly equal to the results

obtained in the other stages of the disease, the recoveries are sufficiently numerous—as will be satisfactorily shown in the next chapter—to convince us that the gas must be absorbed in part, otherwise no beneficial effect would be produced.* Still, as the absorption will be limited, the only plan is to administer the antidote more frequently, and without intermission, until reaction takes place.

In these cases, the gas should be administered every ten minutes, or quarter of an hour, either in the form of an effervescing draught, or in that of soda water. If the former, 25 grains of soda and 20 of citric or tartaric acid will be sufficient for each draught; while half a bottle of soda water will probably be as much as the patient ought to take each time. As the whole cannot be absorbed, it is desirable not to oppress the stomach with too much liquid. If, however, the patient has not taken any form of carbon previously, the remedy ought then to be taken in the usual doses, and more frequently.†

* “I never,” observes Mr. Beckett, “have found saline effervescents to aggravate purging, and I believe that their use is followed by their rapid absorption into the circulation, rather than by any tendency to excite increased exhalation from the mucous surfaces.”—*Lancet*, Sept. 22, 1849.

† Dr. Niddie remarks: “I have had ample experience of the total uselessness in cholera of large doses of medicine, given at long intervals; and am fully convinced of the great value of calomel and carbonic acid gas, constantly and perseveringly administered, at short intervals.” And in another place, while referring

One plan, which has appeared to me to be more beneficial, in some cases, than taking the gas into the stomach, is to inject it into the bowels. Either a solution of the gas in water, of a proper temperature, can be injected by the aid of a common syringe; or the gas itself may be introduced by means of a large bladder and an enema-pipe. If the solutions of soda and acid be introduced separately into the bladder, the two solutions being divided by a string, the latter can be untied at the moment of introducing the pipe; when, the two solutions coming into contact, the gas will be liberated, and pass into the bowels without difficulty. Should the discharges from the bowels continue profuse, and the above plan be insufficient to check them, an enema of charcoal may be employed as well.

If the means now pointed out fail to effect the desired object, or re-action, we may then resort to some stimulant. The stimulating antispasmodics, to the effect of these agents, in the state of confirmed collapse, he adds: "To take effect, they must be administered most perseveringly, and, almost forced into the system. . . . If rejected by vomiting, they must be repeated *every five minutes*, for several hours, and their use ought only to be limited by their effects, and not by the quantity administered. As the symptoms of collapse disappear, the period between each dose must be extended."—*Lancet*, February 10th and Sept. 1st, 1849. As regards the efficacy of the calomel, that may be surmised from what has gone before: while its actual value will be tested hereafter, by a comparison of the result obtained when employed alone, and when combined with carbonic acid gas!

before referred to, are uncalled for at this period, and would, probably, prove useless; there being little or no spasm after the collapse has lasted some time. Besides, their effect is too evanescent to be productive of any benefit in a state of permanent depression. The diffusible, and more permanent, stimuli, as wine and brandy, are most likely to be beneficial at this period. As good an agent as any, if it can be obtained, is champagne, which can be taken with less risk of injury than any other stimulant. It has the advantage, also, of being an antidote as well as an adjuvant, in consequence of the quantity of fixed air which it contains. In addition to champagne, there is another stimulant, which possesses a *specific*, as well as a general, action in the economy. This is coffee. That coffee possesses antiseptic properties, we might infer *à priori* from the simple fact, that, when burnt, it becomes converted into a carbonized substance. This inference is confirmed by practical experiments. If 3 or 4 ounces of freshly roasted coffee be placed in a room, or other confined spot, the effluvium from the decomposition of animal and vegetable substances, dung-pits, etc., will be instantly destroyed. If such are the results obtained out of the body, we may conclude, that coffee would also neutralise and destroy noxious elements within the body, in common with the ordinary forms of carbon. This has been found to be the case; powdered coffee, when burnt, having produced the same results as

those witnessed after the exhibition of charcoal, with the exception of the stimulating effect on the nervous system. It may therefore become a valuable resource in the treatment of the epidemic, whenever we wish to combine the antiseptic, or the antidote, with a stimulant. But for this purpose, the powder must be taken as well as the infusion. Young children might be induced to take this, although they refuse all other remedies—particularly the common charcoal. With adults, who could take the ordinary forms of carbon, it should only be employed as a stimulant, as the quantity of the powder required to act as an antiseptic would be so large, that it might be too exciting. To adults, therefore, the infusion may be given alone: to children, the powder as well as the infusion.

We can thus understand why coffee is a popular remedy in Germany in bilious diarrhœa, which it has been for a long time; and why, also, it should have been recommended by Dr. Pickford in infantile cholera. This practitioner states, that he found it to succeed in arresting the vomiting and diarrhœa after all the more usual remedies had failed.

Whether these or any other class of stimulants be resorted to, they ought to be administered cautiously and by degrees, so as not to excite the nervous energy too powerfully in the first instance, and thus cause, subsequently, as sudden a depression. It is, in fact, of the utmost importance not to exhaust the remain-

ing vitality of the body prematurely; it being impossible to say what length of time a patient may remain in a state of complete collapse. Dr. Kellet relates a case, where the pulse was gone within three hours of the attack; yet the man lived, in that state, from the 3rd of October, at 4 p.m., to the 6th of October, at 2 p.m.—70 hours.

As stimulants have a tendency to increase the evacuations from the bowels, especially if taken in excess, it will be desirable to discontinue their use whenever the alvine discharges are profuse and of long continuance.

Although thus appearing to give my consent to the employment of stimulants, I should regret much if my readers supposed this has been done from an idea that their addition would be productive of any good. On the contrary, my only motive in the preceding recommendations has been to restrain and defend, rather than to permit and recommend, their use,—feeling that it would be useless to prohibit their exhibition altogether. It is, in fact, almost a work of supererogation to attempt to persuade the friends and attendants *not* to administer stimulants to an individual suddenly struck down by the operation of some unknown cause,—with one foot already in the grave, and gradually changing, before their eyes, from a living and healthy body into a breathing and nearly inanimate corpse. Yet this should be attempted, as there can be no doubt that stimulants are useless,

if not injurious, in the state of confirmed collapse. When trusted to alone, they have given, with the exception of the salts of Dr. Stevens, a higher rate of mortality than by the employment of any other remedies.

Independently of these objections to the use of stimulants, it is not improbable that one of the benefits derived from the administration of carbonic acid is, that it produces no stimulating effect upon the nervous system. The vital powers, therefore, run no risk of having the depression increased by overstimulation; and will, consequently, last the longer. The same conclusion has been drawn by other writers. Mr. Beckett, referring to the action of effervescent salines, remarks: "One reason why their effect is so valuable, in extreme adynamic conditions, is, that as their action depends so little upon the vital power, and, in fact, chiefly upon chemical laws, their power is felt when the system is scarcely capable of being influenced by any other therapeutic agent."

For these reasons, it would be better, whenever possible, to refrain from the use of stimulants altogether; or, at least, to restrict their exhibition to the cases of sudden collapse before described. As their employment under these circumstances is only temporary, they cannot be productive of much injury; while they may prove beneficial, by aiding in restoring the balance of the circulation thus suddenly disturbed,—the nervous system not being, at this moment, in a state of very great depression.

While thus obliged to confess that stimulants, although so much resorted to, and apparently so much indicated, are almost entirely useless, we are no less bound to conclude, that there are no other remedies, no other adjuvants—or, at least, internal ones—that can be depended on for the removal of the effects witnessed in the collapse of Cholera. Independently of statistical facts, and the reasons before assigned for the failure of the majority of remedies, there is another reason for the non-activity of ordinary medicines during the stage of collapse. In addition to the non-absorption of solid and fluid substances into the blood, many drugs—as calomel—require to be digested, or decomposed, before they can produce a specific effect. But the process of assimilation, like all the other organic functions, is arrested during the suspension of the circulation. Of what use, then, can it be to administer ordinary medicines and ordinary drugs under such circumstances? Statistical facts, irrespective of arguments and analogical deductions, have already answered the question. We are therefore restricted, at this period, to the employment of a gaseous substance which, like carbonic acid, is capable of being absorbed; or, else, to those substances which, like stimulants, act directly on the nervous filaments, distributed to the different organs and textures of the body.

There is only one method, beside the specific one now under consideration, that would appear to hold out any hope of success, during the collapsed stage of

Cholera. This is by the employment of cold water as an emetic. But there are several reasons why this method can only be of very partial benefit. In the first place, emesis, as before stated, cannot always be produced by draughts of cold water; and this, too, precisely in those cases in which an adjuvant is most required—in those, in which the disease assumes its most severe and malignant form. In the next place, no effect would be produced with those patients to whom the antidote had been previously administered; the tranquillizing effect of this agent being so great that the largest draughts of water can be taken without being rejected. To employ the adjuvant, we should thus be obliged to reject the antidote—take the least valuable, and leave the most valuable, agent.

In addition to the above, this method of treating the disease, even when capable of adoption, is not unattended with risk. Subsequent experience, and a more extended trial with this simple agent, proved that the reaction which took place, after recovery from the collapsed stage of the disease, was generally greater and more dangerous than when this practice was not adopted. Nor is this all. According to Dr. Shute's report,* two patients, treated by him (with large draughts of cold water), manifested symptoms of mental derangement; and a patient of Dr. Waytes, treated in the same way, exhibited such strong indications of mental aberration, and became so

* The *Lancet*, Vol. ii. p. 776, 1832-3.

ungovernable, as to require the strait-waiscoat, and coercive confinement to bed.* The cause of this is readily understood. Instead of being expelled, the poison was driven, during the act of vomiting, from the venous centres—the abdominal and thoracic organs—in which it is probably contained during the stage of collapse, to the brain, producing, by its presence there, an effect similar to that observed in the consecutive fever. The same remarks will apply to the employment of emetics, although we might be able to produce vomiting with these, when cold water alone would be insufficient.

It is for these several reasons that I have hitherto refrained from employing any adjuvants, excepting stimulants, in the collapsed stage of Cholera. It would be, doubtless, of great advantage, if there were an agent capable of restoring the suspended circulation with certainty and celerity; the disease would then be as easy of cure, after the supervention of collapse, as it now is previously to the arrest of the circulation. Wanting such an agent, we must be content with the result that can be obtained, at present, without its aid.

In addition to internal stimulants, external ones have been employed, and, if possible, to a still greater extent than the former. The external stimulation has been effected by means of embrocations, frictions, sinapisms, the hot and vapour bath, and heat applied

* *Lancet*, Vol. i. p. 80, 1831-2.

in a variety of ways. It will be desirable, therefore, to ascertain the value of these agents; especially as the internal ones have been shown to be all but useless.

And, first, as to frictions. If we consult only experience, and set aside all preconceived opinions and theories on the subject, we shall be forced to confess, that this operation is, to say the least of it, useless; and not very unlike the attempt to wash the black man white. This, however, is not all: the resorting to this process in the early part of the stage of collapse has often been productive of much injury, by exhausting the energy of the system, and diminishing, instead of increasing, the subsequent temperature of the extremities and external surface. Considering that the generation of animal heat is all but impossible, at a period when the functions of the lungs are entirely suspended, nothing should be done which can possibly tend to exhaust prematurely the small quantity of caloric, that may yet be retained in the nearly inanimate body. Such a result must certainly be expected to follow the application of strong liniments and spirituous embrocations, perseveringly used by three or four assistants.*

For these reasons, I have always opposed the

* "The undue attempt to arouse the nervous system," remarks Mr. Ross, "must certainly exhaust its powers; and the principle, upon which the largest amount of stimulation has been applied to the lowest amount of vital power, is as false as any paradox the human mind can conceive."—*Loc. cit.*

employment of frictions, embrocations, &c., to the extremities, and restricted their use as much as possible, not only at the commencement of collapse, but during its confirmed state. Besides, these adjuvants are unnecessary at the commencement of collapse, as the administration of the gas almost invariably relieves and dissipates the spasms, in common with all the other effects observed at this period. This must be evident by what has gone before. When the collapse has become confirmed, the result is different.

In this case, the exhibition of the gas is not always sufficient to relieve the spasms: at least, the relief is so slow, that it seems desirable to make use of some other, and more apparent, if not more speedy, means, to satisfy the anxiety of the patient, and the impatience of the attendants. It is not always possible to restrain the latter, or prevent their applying heat and friction to the extremities; but their exertions should be restricted to the use of the hand, moistened with a little oil.

Instead of the extremities, I have usually applied the heat and the embrocations to the spine, and for this reason: the cause of the spasm is not local, or in the muscle; it is in the nervous system, or in the spinal marrow. The spasms come on, almost invariably, at the commencement of collapse; at that moment when the pulse begins to fail, the circulation to be retarded, and the blood to be thrown back, by a retrograde movement, from the arterial into the

venous system—as shown by the blueness of the skin. Such being the case, it will not be difficult to understand the cause of the supervention of spasm. As the plethora and stagnation of the blood in the venous system will be extended, as a matter of course, to the vessels of the spinal marrow, a greater or less amount of pressure must be exerted on this organ—the result of which would be spasm of the muscles, which generally occurs during an asthenic, not during a sthenic, state of the system. That this is a correct view of the subject, we may conclude, not only from the arguments now adduced, but, also, from the following circumstance.

On my arrival in Valencia, during the prevalence of the Epidemic Cholera in Spain, I was requested to visit the sister-in-law of an English merchant living there. This lady had just recovered from a severe attack of Cholera, excepting that there remained a contraction of the left leg, rendering it impossible to straighten the knee. To the surprise and amusement of the patient, I requested to be allowed to examine the back, which, being granted, I discovered, on pressure, considerable tenderness in the centre of the spine. After some persuasion, the patient consented to apply a dozen leeches to the spine; fomentations and a warm poultice being directed to be used afterwards. Nothing more was done. When I called the next day, the lady was able to straighten the leg, and, in a few days, was walking about quite well.

It is right to add, that this patient had suffered from spasm, during the previous attack, to an unusual extent.

These are the reasons that have induced me to direct my attention to the spine, and not to the extremities, whenever the spasms were severe, and did not yield to the means previously employed. It has also appeared to me, that the application of heat to the spine has been more beneficial in removing the other effects, or the state of collapse, than when applied to the extremities. It is, in fact, in the internal and central organs that the plethora of the venous system is the greatest; and it is to these organs, and not to the extremities, that our external and local efforts, to relieve the stagnation of the circulation, should be directed, in the absence of those general ones which will be pointed out hereafter.

A plan of treatment, in accordance with these ideas, has been lately proposed by Dr. Chapman—the only difference between us being, that he applies cold, or ice, to the spine, instead of heat; and, that he trusts, or appears to trust, to this agency alone for the cure of the disease. This is in consequence of a particular theory entertained by this writer, respecting the immediate cause of Cholera. It may be as well, therefore, to consider briefly what this theory is.

Dr. Chapman states, that “the *primary* cause of Cholera is, the excessive heat of hot climates, and of temperate climates in summer, when Cholera prevails.”

The consequence of this high temperature is, in the human subject, vascular excitement, terminating in plethora and congestion of the venous system, and—an attack of Cholera. “The hyperœmia of the spinal and sympathetic nervous centres produces contractions of the general arterial system,” and, hence, the arrest of the circulation, and all the other effects observable during the state of collapse.*

Without waiting to inquire whether indigenous, or endemic, Cholera can be produced by such a cause, I would merely observe, that heat, alone, can have nothing to do with the production of the epidemic Cholera. If heat could produce such a malady, how is it, that the Asiatic Cholera was never observed before 1817?—there being no record of such an affection having prevailed during historical periods. Then, again, there are certain localities, as on the west coast of Africa, situated on, and close to, the equator—and where, consequently, the temperature is the highest—that have never yet been visited by the epidemic Cholera. On the other hand, this disease commenced its ravages, both in England and in Russia, in the middle of winter. It is clear, therefore, that heat, *per se*, cannot be the primary, or remote, cause of the epidemic Cholera.

As regards the proximate, or immediate, cause, Dr. Chapman is no less in error. That there is a state of congestion of the blood vessels at the nervous

* *Medical Times and Gazette*, July 29, 1865.

centres—at the cerebro-spinal, at least, for the ganglia of the sympathetic are but sparingly supplied with blood vessels—during the state of collapse, is undoubted; but, then, this state is an effect, and not a cause, of the attack. Independently of the arguments before used, in order to show that this disease is the effect of another and a different cause, I would merely observe, that we sometimes meet with cases which run their course without this peculiar or hyperæmic state being present. I met with several cases in Jamaica, in which there was no congestion of the venous centres, no arrest of the circulation, and no failure of the pulse, until a few minutes before death; and in which the only clue to the nature of the attack was one characteristic stool and slight vomiting, half an hour before the fatal termination. The poison, in these instances, appeared to have expended its fury on other portions of the nervous system, leaving that which regulates the circulation of the blood intact. This form of the complaint has sometimes been met with as the prevalent type, in certain localities and at particular visitations. At Punderpore, in the East Indies, persons were attacked so suddenly, that they were seen, to quote the words of the narrator of the event, “tumbling over each other in the public street, as if struck down by lightning,” or, by a dose of prussic acid. In these cases, there could have been no previous hypæremia, no failure of the pulse, no arrest of the circulation: death must

have arisen from the direct action of some powerful agent on the nervous system.

Then, again, there is no arrest of the circulation, no state of congestion, in general, during the first two stages of the disease; at that period when it is, in the second stage, at least, if not in the first, fully developed. It is not until the commencement of the stage of collapse, that the signs of a congestive state of the venous system become manifested. Congestion, or hyperæmia, therefore cannot be the cause of an attack of cholera: it is not only an effect of the same cause as that which produces all the other effects, but, more than this, it is one of the ultimate, not one of the primary effects of the operation of that cause. So far from the congestion of the nervous centres being the cause of the arrest of the circulation, the probability is that the plethora of the venous system is to be ascribed wholly and solely to the want of power in the heart and arteries to propel the blood through the capillary system. Like so many other practitioners, Dr. Chapman has mistaken an effect for a cause.

Allowing that we could, by the application of ice to the spine, act upon the nervous system, and restore the balance of the circulation, when this is deranged or suspended, we should not be able, by this means alone, to effect a cure, or to save the life of the patient. If the poison, the cause of all the morbid phenomena, be not neutralised or expelled, the same

result would be witnessed as that which followed the injection of salts into the blood; the patient would again fall back into his previous state. And yet, Dr. Chapman recommends others, as far as I can understand his views, to trust to the external application of ice for the cure of this formidable disease.

It does not follow, however, that because Dr. Chapman has drawn false conclusions, or errs in his practice, that the employment of ice would be useless in the epidemic cholera. It is not necessary to refer to the theory of this writer, in order to understand the *rationale* of the application of ice to the spine. Its *modus operandi* can be readily understood by what has gone before: it acts, when beneficial, by removing the congestion of the vessels of the spinal marrow; and, in this way, may prove a powerful adjuvant under certain circumstances, and at particular periods of the disease.

That the application of ice to the spine will prove beneficial at *the commencement of collapse*, we might have inferred *à priori*; but, then, this is precisely the period when it is not required—there being other agents which act with more certainty and promptness, and which will produce results that cannot be obtained by the employment of a mere adjuvant. The application of ice, therefore, being unnecessary at this period, even as an adjuvant, it is desirable not to resort to it; as, when adjuvants are employed, we are frequently in doubt whether the cessation of the

symptoms is to be referred to the removal of the cause, or, simply, to the temporary relief of an effect. We might then be induced to relinquish the exhibition of the antidote before the whole of the poison had been neutralised.

It is different in the state of confirmed collapse: at this period, as already remarked, the cessation of the spasms, and the removal of the other effects of the disease are neither so certain nor so prompt as when the remedy has been employed previously to the supervention of this stage. What the effect of the application of ice may be at this period, and in the severe form of the disease—it having been hitherto employed in sporadic cases only—yet remains to be seen. Reasoning from analogy, I should not expect any great benefit from its use. Although somewhat paradoxical, at first sight, it is yet well known, that precisely the same results are sometimes produced by directly opposite means—by heat and by cold. They are both excitants of the nervous system, the only difference being that the one acts directly, and the other indirectly. But in order to produce a stimulating effect by cold, a certain amount of nervous energy is required, otherwise no re-action will take place; and it is this re-action to which the stimulating effect is to be referred, not the primary effect of the cold, which is that of a sedative. If an individual take a plunge into a cold bath when the system is in a state of unusual depression—as from

over fatigue—the result is generally serious, sometimes fatal. For the same reason, I should argue, that to apply ice, even to the spinal nerves, at a time when the ganglionic system of nerves is in a state of complete depression, must be an erroneous, if not a dangerous, practice. The cerebro-spinal system is the only system which preserves its energy and vitality in the state of complete collapse; and it is to the integrity of these nerves, or the functions under their control, that the individual owes his existence. Destroy that integrity, or depress the energy of the spinal nerves, and you might hasten, rather than prevent, the final catastrophe. At such a time, it would be, in my opinion, more in accordance with sound therapeutical principles to apply heat; it being a well established fact that heat will excite and raise the nervous energy, when cold would depress and destroy it. In cases of drowning, when the vital powers are suspended, we do not apply cold, either locally or generally, but we put the individual into a warm bath, or place him before a fire and apply friction. This is the course which I should pursue, in addition to the internal means already pointed out for the relief of the spasms, in the state of confirmed collapse—excepting that the heat ought to be applied to the spine, and not to the extremities, for the reasons previously given.

It is not for the spasms alone, that we require such an agent: it is still more necessary to try and re-

move the state of collapse, and to aid in the restoration of the circulation. For this purpose, we may apply heat, not only locally or to the spine, but generally, and to the whole surface of the body. External heat has been applied in the form of the hot-air and the vapour bath, at particular times and under certain circumstances, from the first year of the prevalence of the disease in India to the present time. The vapour bath, which was extensively tried in Russia, does not appear to have produced beneficial results; but more favourable reports have been given respecting the hot-air bath. The variation is, perhaps, to be ascribed to the circumstance, that the heat is produced more gradually in the latter; while it is continued for a longer period than when the vapour-bath is employed. Hence there is not that chance of the patient experiencing the subsequent depression, so common a result of the use not only of the vapour, but of the warm, bath, and thus increasing, rather than relieving, the state of collapse. If employed in a proper way, the hot-air bath may prove a valuable adjuvant; and I have resorted to it myself, in preference to all other general means, when internal ones have failed, or have appeared to be inadequate.

The method which I have adopted has been as follows. A cradle, reaching from the patient's armpits to the feet, should be placed in the bed, and be covered over with two or three blankets or rugs. A

tin vessel, containing a spirit lamp, with an open door, and terminating in a horizontal chimney—a foot or two long—may then be placed on a chair, or stool, at the foot of the bed. The end of the chimney being introduced under the bed-clothes, and the lamp being lighted, the heated air will pass into the open space under the cradle, and gradually raise the temperature to the required standard—the test being the feelings of the patient, and the effect on the circulation and the pulse. It should be continued for some time—generally speaking for two or three hours—and until the circulation has been fully restored. As soon as the pulse can be felt at the wrist, the antidote may be given internally; although it is to be presumed that a portion of the gas—carbonic acid—generated by the combustion of the spirit, will be absorbed by the external exhalents; a result that invariably occurs in a healthy state of the system, when this gas is brought into contact with the skin. It is to this circumstance, perhaps, that the efficacy of the warm-air bath is mainly to be ascribed, and hence the failure of the vapour bath and the warm bath,—the latter having also appeared to me to be inefficient and useless during the stage of collapse.

Although no inconvenience is experienced by the absorption of carbonic acid gas into the venous system, excepting in very large quantities, care ought to be taken to prevent its introduction into the arteries. For this purpose, the *under* blanket should be drawn

tightly round the chest under the armpits, and the *upper* blanket be tucked in closely round the neck.

Sometimes the application of heat is highly distressing; even although the body, to the hand of an observer, appears to be icy cold. The bed-clothes, also, are oppressive; and it is with difficulty the patient is persuaded to retain even a sheet, so anxious is he to expose his body to the contact of the external air. These symptoms, although apparently a bar to the employment of external heat, are not so in reality; they will be soothed and relieved, as soon as the temperature of the surface becomes more equalised, and the natural transpiration has been partly restored.

The preceding are the only means which, according to my experience, are likely to prove beneficial for the removal of that most distressing of all states—the state of collapse. But there are some other symptoms which require a few remarks.

As, in consequence of the great thirst in this disease, and the burning sensation in the stomach, it is with difficulty that the patient can be debarred from partaking freely of whatever fluid is within his reach, and as so much difference of opinion exists on the subject, some explanation will naturally be expected on this point. In England, as previously shown, copious draughts of cold water were employed as a remedial agent; while no limitation was placed to the use of water as a diluent; it not appearing that any injurious result followed from this licence

and permission. In India, if we credit the united testimony of all the practitioners there, when cold drinks were allowed, and when the patient, in spite of the instant rejection of the fluid, persisted in its use, the result which has been so frequently witnessed in England,—the recovery of the patient—was seldom observed there. On the contrary, as Dr. Young has remarked, if this craving for cool drinks was indulged in, the frightful state of collapse soon succeeded, and all the bad and dangerous symptoms were immediately increased. It is not surprising therefore, after witnessing such results, that a total abstinence from all liquids was strictly enjoined by the majority of the profession in that country; and only administered by others in the smallest possible quantity, as a table-spoonful at a time, until the worst symptoms had disappeared.

With such different results before us, and such a diversity of opinion, it might be somewhat difficult to give a safe guide, or a general rule, on the subject. But being fully convinced, that the great and urgent thirst can be relieved by more certain and less doubtful means; and believing that it is not requisite or desirable to resort to such a remedy, on account of its emetic effect; the necessity for any rule is, in this case, entirely done away with. In administering carbonic acid in this disease, the burning heat in the region of the stomach, and the great and urgent thirst are not only more effectually relieved than by any

other means, but these sensations are *speedily and entirely removed* by this simple and single remedy. In giving or withholding any liquid, I have been guided entirely by the patient's own wishes and feelings; not having seen any harm result from the use of simple diluents, as toast-and-water, as soon as one or two doses of the medicine have been exhibited. On the contrary, in the evacuant period of the disease, and more particularly after the cessation of the intestinal discharges, it is advisable and necessary to encourage the patient to partake freely of liquids; in order to supply the place of the discharged serum, and to restore the lost fluidity of the blood.

Lastly, as regards the treatment of the case after re-action has taken place. Generally speaking, the recovery is so perfect that nothing more is required than to turn the patient over to the cook—with a caution not to overload the stomach, nor to take indigestible articles for some little time.

As, however, the different secretions, after being suspended for some time, as in the severe attack, cannot be in a very healthy state, it will be advisable to order the patient a dose of calomel, to be followed by an aperient draught, or a dose of castor-oil. This will be more especially necessary if the stools do not present a healthy appearance, or if there appear to be a deficiency of bile.

In some cases, the renal secretion remains suspended for some time after re-action has taken place.

In one patient, that I met with in Jamaica, the suppression of urine continued for 108 hours *after* reaction had taken place, and for 12 hours previously—thus making 120 hours in all, or 5 days! In such cases, I would recommend the oil of turpentine, given in small, and frequently repeated, doses, to act as a diuretic—in combination with, or in addition to, the ordinary means resorted to under such circumstances.

Although, as already remarked, the consecutive fever has never been observed by me, in those cases in which the antidote has been administered in sufficient quantity, previously to the state of confirmed collapse, still, as this remedy is not yet of universal use, such cases will occur in practice, and in a large proportion in temperate climates. A few remarks, therefore, would seem to be required, in this place, respecting the treatment of such cases.

As this fever cannot be referred to excess of reaction, as, in this case, it would be observed in warm, as well as in cold, climates; and as it also presents peculiar and characteristic symptoms; we must regard it as an effect of the same cause as that which produces the other stages of the disease. This conclusion is confirmed by the fact, that the symptoms are very similar to those observed in attacks of typhus fever—a disease generally acknowledged to be produced by the operation of a specific poison. It will therefore be necessary, in addition to the ordinary

remedies resorted to in fever, to administer the antidote in this stage of the disease, the same as in the previous one. The gaseous form will be the best, and a dose should be given every hour; until some effect is produced, and the fever, with all its attendant symptoms, is entirely dissipated.

We sometimes meet with cases in which a tonic is required after an attack of Cholera. But in the majority of instances, particularly when the treatment now recommended has been had recourse to in the first stages of the disease, the recovery is so rapid and so perfect, that such a remedy would be, if not useless, unnecessary. Persons who are in a state of debility or of disease previously to the supervention of the attack of Cholera should, of course, be treated in the usual way, and the same as after recovery from other diseases; unless, from the mal-administration of the medicine in the previous stages, or other cause, we deem it advisable to continue the administration of the antidote until the debility and symptoms of depression have been removed, and all fear of a relapse has passed away.

CHAPTER V.

RESULT OF THE TREATMENT.

It was in 1831, on the first irruption of the Epidemic Cholera in London, that the different forms of carbon were proposed by me, as remedies in this disease, in a printed paper that was presented to the General Board of Health, through the late Sir David Barry. A copy was subsequently forwarded to the different local Boards, and to many practitioners in those towns in which the disease appeared. It is of importance therefore to ascertain what evidence has been collected respecting the efficacy of these agents during this long interval. And, first, with respect to the ordinary form of carbon, or charcoal.

In the narrative of the Cholera, published in the *Lancet*, we find this observation, in the chapter on the treatment. "An exceedingly simple remedy was used; and it is said, with unprecedented success, on board the ships belonging to the United States. A common bottle cork was burnt, and the powdered coal given in a little milk or water. The third dose, at most, was sufficient to allay the urgent symptoms; and we are assured, that it has, more than once, saved patients almost in the agony of death."

Again: by a letter from M. Moreau de Jonnes, inserted in the same journal, we learn that Dr. Gavardan, of Arras, had administered a tablespoonful of wood charcoal, in an opiated enema, during the prevalence of the Cholera in the Pas de Calais. In twelve cases, of which he gives the details, the cure was complete and immediate. In several others, he omitted the opium with the same success.*

Subsequently to this, we received accounts of the success which an eccentric and unknown individual had met with in Canada, by the employment of the same remedy. It appeared, from the particulars then given, that he prepared the carbon in the presence of his patient, using certain forms and ceremonies, which, although ridiculous in themselves, were not altogether without benefit; as, however we may laugh at his ceremonies and incantations, it was allowed by all who have given us any particulars respecting him, that he lost few or no patients by this method of treatment. The true charm consisted in the preparation of the remedy at the moment of its administration; carbon, as was before remarked, when recent, possessing properties which it loses by age.

As stated in a previous chapter, carbon was extensively employed by Dr. Wilson during the severe visitation of the epidemic cholera at Xeres, in the south of Spain.

In the MS. memoir, which Dr. Wilson kindly pre-

* Nov. 19, 1832.

sented to me subsequently, are the following remarks on the curative virtues of charcoal: "As a prophylactic, it gained a speedy reputation; but, as a remedy, the whole faculty were opposed to it; and the people, from its novel and homely character, and from the required largeness of the dose, could not readily be brought to regard it favourably as a remedial agent in the disease. Ultimately, therefore, it was only in occasional cases, that I continued its exhibition by the mouth; but, from first to last, I persevered with it, and met with no opposition to it in enema. Altogether, however, it was given in numerous cases by the mouth, from the first appearance to the close of the epidemic period, and with such general good effect as to have impressed me with the firm conviction, that, *in all stages of the disease*, it is a most beneficial adjuvant, and, *anterior to collapse*, and in the stage of reaction, most *eminently curative.*"*

"It was stated in the *Semaphore* of Marseilles, that M. Alphonse Gay, a physician of that town, had employed, with success, the following simple remedy for the cholera—viz.: half an ounce of charcoal,

Dr. Wilson, at the commencement of the attack, administered olive oil—a remedy that was much resorted to in the south of Spain, during the prevalence both of cholera and yellow fever. A cupful was given two or three times, at intervals of ten or fifteen minutes, so as to produce full vomiting. It also acted on the bowels, but generally occasioned a good deal of pain in these organs; while the irritation of the stomach was not so readily subdued as with other patients.

reduced to an impalpable powder, mixed in 2lbs. of water, administered as an injection; and a quarter of an ounce of the same powder in a glass of warm water, taken as a draught." *

A correspondent of the *Times*, in a letter to the Editor (Sept. 20th, 1848), makes the following statement: "A friend of mine in Bristol (not a medical man), lately had a child so ill with the Cholera, that the medical man considered it a hopeless case, and left nature to take its course. Collapse had come on, and the child could take nothing but water, of which she was allowed to take as much as she could. What induced my friend to make the experiment, I do not know; but he got a few corks, charred them in the ashes of the fire, and mixed the charcoal so made with the water; and, to the surprise of all, the child was fully restored. In reply to my query, he said, he used cork charcoal, because it is more *impalpable* than any other charcoal, and mixed more freely with the water."

In the same journal (Nov. 15th, 1848), is another Letter, dated Zamdzin, Kieffe, Russia, and dated October, 1848. The writer says: "I have upwards of 300 people at work, and the Cholera all around and amongst us: sixty of my people were taken ill. I gave immediately to those who were seized one glass and a half of spirits of wine, in a glass of water, with four or five tea-spoonsful of powdered charcoal,

* *Hampshire Telegraph*, September 2, 1837.

and three drops of oil of mint ; and the patient took violent exercise until a strong perspiration was induced, and *all* my patients recovered. To those who were weakly, I administered the charcoal in a fresh egg beat up with a little water, milk warm. For myself, as an antidote, I take every morning a fresh egg, beat up with a tea-spoonful of charcoal. I made the charcoal from white beech, but any wood free from resin, will do."

Mr. Hood, in a letter to the Editor of the *Daily News* (Sept. 3rd, 1849), remarked: "When the Cholera was so prevalent, about the year 1832, a manufacturing chemist of Bolton, in Lancashire, with whom the writer was acquainted, administered powdered charcoal to a great number of the working classes in the neighbourhood, who were seized with this dreadful complaint, with invariable success, *when collapse had not taken place.*"

Mr. Evans, surgeon at Swansea, also observed: "Since my last communication to the *Lancet*, on the administration of freshly powdered charcoal in Cholera, I have used it alone, in many cases of violent diarrhœa and vomiting, and with invariable success; and a case of incipient dysentery yielded to it almost immediately."*

Independently of the ordinary form of carbon, the hydro-carbons have also been successfully employed in the treatment of the Asiatic Cholera. Of these,

* Jan. 6, 1848.

Naphtha is the best and the most powerful ; and it is the one of which we have had the most satisfactory account.

At a meeting of the Medico-Botanical Society of London, held June 15th, 1848, Mr. Gutherie read a communication which he had received from Dr. Andreoski, respecting the successful employment of naphtha in the Russian Army, in the Caucasus. The naphtha employed was obtained from Bakü, on the borders of the Caspian Sea. Dr. Andreoski says : “Naphtha is an infallible remedy against the Diarrhœa Cholericæ, which prevails during certain seasons ; and is given in the dose of from four to eight drops, in a little brandy, white wine, or mint tea, taken cold. A single dose usually suffices to arrest the complaint. Even in many of the advanced cases, naphtha alone had proved successful. As to simple diarrhœa, during the existence of Cholera, there was not a single case in which naphtha did not effect a cure, when resorted to immediately.”*

These are precisely the results that we should have expected, *à priori*, to follow the exhibition of such an agent. Containing a large proportion of carbon—85 per cent.—it might be inferred that it would be beneficial in the Epidemic Cholera, if the conclusions before drawn be valid. As, however, the carbon is not in a simple form, but is united with another substance, viz., hydrogen, thus forming a binary com-

* *Lancet*, July 1, 1848.

pound, it would, we may presume, have to undergo decomposition before producing a specific effect. But the processes of assimilation and absorption are arrested, as previously remarked, during the state of complete collapse: no decomposition, therefore, could take place, at this period, after its ingestion. We can hence understand why the remedy should be so beneficial previously to the supervention of collapse, and inefficacious after. The same remarks will apply to other compounds of carbon, as creosote, which has been successfully employed, by many practitioners, to allay the vomiting. Dr. Mackenzie states, that this remedy was tried at Archangel rather extensively, and is looked upon by Drs. Condry and Snee as something approaching to a specific.

We can also understand why olive oil should have been employed so extensively in the South of Spain, as a remedy in the first stage of the disease; and why, in consequence of its apparent benefit, it soon became the popular remedy. Although not a pure hydrocarbon, as it contains a small quantity of oxygen—the proportions being 78 of carbon, 13 of hydrogen, and 9 of oxygen—it will furnish a large supply of carbon to the system, after its digestion and assimilation. As previously observed, the oil was employed as an emetic in Spain; consequently, only a small portion could have been retained. In addition to this, that quantity would have to be digested and absorbed before producing a specific effect. Hence ew

should expect to find that this agent would be beneficial in the first stages of the disease, and almost entirely useless afterwards. This was precisely the result which followed its employment, as far as my observation and inquiries went.

In addition to the above, the alkaline carbonates have been employed, and with undoubted success. As the secretions of the stomach are acid, and particularly so in the first stages of the disease, a certain portion of carbonic acid gas will necessarily be liberated when these substances are administered. The same result, therefore, ought to follow the employment of these agents as of the pure gas, excepting that, the quantity of acid present in the stomach being limited, there would also be a limitation to the evolution of gas: as such, the effect would be less than when the gas was administered in the ordinary way, and in unlimited quantity. Still, many practitioners have formed a highly favourable opinion of the alkaline carbonates, even when employed alone.

Thus, Dr. Lewis, who concluded that an acid in the stomach is the immediate cause of an attack of Cholera, administered antacids—as the carbonate of lime, the sesquicarbonate of soda, and of ammonia, *in a state of effervescence*; or, in other words, an effervescing draught with an excess of alkali. He remarks: “Having had opportunities of testing the above treatment, on an extensive scale, at one of the

largest metropolitan dispensaries, with uniform success, I can confidently and conscientiously state, as the result of my experience, that *simple as the treatment may appear to heroic practitioners, it is the only safe and certain method of arresting the progress of Cholera*, as it prevails in England.”* The carbonate of soda without any adjuvant was employed by Mr. Torbeck, surgeon, Sunderland, with considerable success. The history of one severe case which recovered—the remedy having been taken every hour, in drachm doses—was inserted in the *Medical Gazette*, Jan. 7, 1832.

Dr. Maxwell, also, in a letter dated Hydrabad, August 1849, addressed to Dr. Prout, speaks thus of the employment of the carbonate of soda. “I do myself the pleasure to communicate to you the important fact (in which you will rejoice) viz. that the carbonate of soda is a speedy and effectual antidote to the poison of Cholera. I have just ascertained this, in the treatment of Cholera here. I give a tea-spoonful of it the moment a patient comes to me, in a cup of very hot gruel, or water. It relieves the pain and burning of the stomach and bowels, produces sleep, and restores the pulse and heat in a very short space of time.”† ‡

* *Lancet*, Oct. 20, 1849.

† *Ibid.*

‡ In a review of my work in the *Englishman* newspaper of Calcutta, in 1837, it was stated that Dr. Maxwell had been going about, *soda-water bottle in hand*, like another David, trying to slay the modern Goliath with this new weapon. I do not know

With these remarks, we may now pass on to consider the evidence that exists respecting the efficacy of carbonic acid gas. In consulting the practice of different individuals, and perusing the documents transmitted to the Central Board of Health, it will be discovered, that carbonic acid gas, after the first appearance of the disease in England, came to be more and more generally adopted, to allay the irritation of the stomach and to check the vomiting, not only in the first stages of the disease, but in that of collapse also.

Thus, soda-water, solution of soda, and effervescing draughts were used, in the treatment of the disease, by the surgeons at Cawood, near Selby; and by the medical practitioners at East Retford.

Two cases of *confirmed* collapse, successfully treated by Mr. Radcliffe with carbonic acid gas, were inserted in the *Lancet* (Sept. 29th, 1832); while the same remedy, as I have since learnt, was extensively employed in Devonshire,—particularly at Plymouth—and with such success as to have attracted the attention of non-professional persons.

This, in addition to my own experience, and that of several friends in the profession, on a small scale, or with a few isolated cases, was all the direct evidence to be obtained, after the first invasion of the

whether these two personages are one and the same individual, but another letter was inserted in the *Medical Times*, the year previous to the above (July 22, 1848), from a Dr. Maxwell, of Calcutta, in which the writer states, that he had cured himself of an attack of Cholera with *effervescent draughts* alone.

disease in England. But, as the Epidemic extended to Scotland and Ireland, and, subsequently, to America and Canada, to which places a printed circular was sent, recommending a trial of carbon and its compound, I was in hopes that some valuable evidence would have been derived from these sources. But in this expectation I was, if not altogether, in a great measure disappointed; for although it is certain that the antidote came to be more and more generally employed, the evidence derived from this source is only indirect. The antidotal agent, instead of being given alone, was combined with other remedies; so that the evidence became, to a great extent, invalidated. All that could be ascertained was this: from an examination of the different reports that were subsequently published, respecting the treatment of the disease, it appeared that the most successful method was that to which carbonic acid had been added. This, so far, is an important link in the chain of presumptive evidence. If many different plans of treatment are adopted, and various remedies used, at the same time that a particular medicine is common to all, it is more philosophic to refer this effect to one particular agent than to many different ones—provided we have other proof that this cause is sufficient for the purpose. This, at least, was my opinion; and I was about drawing up a Memoir in order to show these facts more clearly, when the intelligence reached England of the outbreak of the Epidemic Cholera in Spain.

Being dissatisfied with the amount of evidence then obtained, and wishing to have another opportunity of testing the value of the agents proposed by me, I was induced to proceed to that country. Prevented, by the political state of the country, or the Carlist war, from entering by the northern boundary, and thus meeting the epidemic on its march from the south to the north, I was obliged to take the more circuitous route by sea to Cadiz.* Finding that the disease had subsided, or nearly so, in this town, in Seville, and in the surrounding districts, and being unable to proceed by land, on account of the absurd and impolitic quarantine regulations, no resource was left me but to proceed by sea to Groa, near to Valencia, in which town the disease was then prevailing. Unfortunately, the vessel was bound to Alicante as well, and the captain having heard, from a schooner we met, that the cholera was raging in the town, he put his helm down, and ran into a bay near to Carthage. Here we remained four days; and we should have stayed much longer had not I, and two of his crew, who had served in English ships for some years, risen in rebellion, and obliged him to sail for his destination. Although detained for nearly two days at Alicante, I did not land; as the disease was then

* The Epidemic Cholera commenced in the southern extremity of Spain, and then proceeded directly north by two distinct routes—one through the centre of the country, intersecting Madrid; and the other, along the eastern coast.

subsiding, and I should have been unable to embark again, or to proceed by land to Valencia—some of the intervening towns on the route being uninfected. The consequence of all this delay was, that the epidemic had nearly ceased on my arrival at Valencia. Finding that the pestilence had broken out at Barcelona, I pushed on to this town by land, and arrived there shortly before the epidemic had arrived at its height—having thus traversed near the whole of Spain, from south to north, before I was able fairly to confront the enemy, that thus appeared to be flying before me. But having brought him to bay, I was compensated by the result of the encounter, not only for my long journey, but for all my previous troubles and anxieties. Through the influence and kindness of Mr. Montagu—the acting English consul—I was introduced to some of the principal physicians in Barcelona, all of whom, after a trial of the remedy in one particular, and, apparently, desperate case, adopted the same mode of treatment during the subsequent prevalence of the epidemic in this town. The result will be best ascertained by a reference to the following documents.

In answer to some observations which were made in a newspaper of Barcelona, relative to the effect of carbonic acid in one particular case, the attendant physicians, Dr. Ardevol, Dr. Frau (Professor of anatomy and surgery), and Dr. Sauch (Physician to the General Hospital), the first individuals in that

town who adopted that mode of treatment here pointed out, express themselves in these terms:—
“The Spanish physicians, resident in Barcelona, who had the honour of meeting Mr. Parkin, admitted from the beginning, with cordial consent, the opinions of this gentleman; since, although they were not entirely ignorant of the good effects of carbonic acid gas, to this English practitioner (whose name already belongs to history, adorned with the most grateful remembrance of mankind) is to be ascribed the glory of having established the administration of this medicine. This chemical agent is *a specific remedy* for the cure of the Asiatic Cholera, in its first and second periods; administered by skilful hands and with medical tact. We have had the satisfaction of saving, with this remedy, scores of individuals who were brought to the gates of death.”*

The former of these gentlemen, in a subsequent communication †, and after a more extended trial, offers the following strong testimony to the efficacy of this remedy:—“The carbonic acid gas is a chemical agent which positively neutralises the morbid poison of Cholera. Its effects are observable in the first, second, third, and fourth periods of the disease. In that of the preliminary diarrhœa, it modifies the morbid impression in a perceptible manner; changes the nature of the discharges, giving rise to bilious secretions; and produces a speedy alteration in the

* *El Vapor*, Oct. 1834. † *El Catalan*, No. 44, Novem. 13, 1834.

choleroïd physiognomy of the patient, who soon acquires his normal appearance."

"*The efficacy of this medicine,*" continues the writer, "*is most visible at the commencement of collapse, when the blueness appears, and when there is an alteration and sinking of the voice. This medical metamorphosis appears most remarkable in these otherwise desperate cases; since the physician observes with pleasure the speedy effects of the neutralisation of the poison, by the disappearance of the anxiety; the recovery of the voice; the animation of the physiognomy; the return of the pulse; and the removal of the state of depression; and finally remarks, that the patient passes to the period of reaction with a free development of the pulse, confirming thereby the hopes of the sufferer for the re-establishment of his health. In the algid state, with blueness, loss of pulse, suppression of urine, and visible depression, if the patient preserves his intellectual faculties, its good effects are also observable to all who have sight and wish to see. And I will say, finally, that in the actual state of our knowledge, the materia medica, in this case, has no other agent which can replace it.*"*

This document, which I appreciate as the testimony

* The name of Dr. Ardevol must be familiar to many of the English surgeons stationed at Gibraltar during the prevalence of the yellow fever in that fortress. He died, I regret to add, of an affection of the heart, some months after the subsidence of the Epidemic—the event being hastened, in all probability, by the fatigue and exertion he underwent at that period.

of an individual for whose memory I shall always entertain the greatest respect, and to whom I was indebted for much valuable co-operation during my stay at Barcelona, will not fail to make an impression on the mind of every unprejudiced person, coming, as it does, from a gentleman of reputed talents in his profession, and who to his general experience, acquired in different parts of the world, added the advantage of having witnessed the fatal irruption of the Epidemic Cholera in the capital of France.

But it is to the medical practitioners of Mataro, the Brighton of Catalonia, that I am most indebted for the assistance they afforded to that cause in which I was then engaged. This will appear evident by a perusal of the following Report, which was sent to the Supreme Board of Medicine in Spain, and subsequently inserted in one of the medical journals of Madrid.*

The writer, Dr. Pascual, commences thus:—

“The continued announcement of specific remedies for counteracting the effects of this destructive disease; and their inefficacy, inutility, and detriment in the hands of the physician who has trusted to the unbounded praises with which they have been enhanced, will, perhaps, cause this

* Boletin de Medicina y. Cirujia, No. 35, Jan. 29, 1835.

These documents, together with a copy of the Spanish, French, and Italian editions of this work, are lodged in the Library of the Royal College of Surgeons, London; where they will be open to the inspection of any individual who may wish to peruse them.

new remedial agent to be viewed as another of the numberless advertisements which have adorned the corners of the streets; filled the columns of the newspapers; and, lastly, barbarously trafficked with the credulity and ignorance of the vulgar, always easily beguiled and never tutored by experience. Impressed with a conviction of the necessity and obligation, by which the clinical observer and historian is bound, to use the language of sincerity and freedom; and impelled solely by a desire to be useful to humanity and science, I am going to present the result of the administration of carbonic acid gas in the Cholera Morbus of the town of Mataro, from the 8th day of October to the 12th of December, 1834.

“In the first days of the appearance of the disease, all the patients were treated with the method entirely anti-phlogistic; but three or four persons of note, who were subjected to this treatment, having perished, it lost credit, and caused bleedings to be regarded as the passport to the other world.”

It was at this period that I placed myself in communication with the physicians of Mataro, who, having heard that the administration of carbonic acid had been attended with considerable success in Barcelona, were easily induced to try its effects in that town also. “This,” continues the narrator, “happened in the period of the ascent of the disease. But what was our pleasing surprise, when we really saw all the patients, who summoned us before the state of collapse became developed, change, *as by enchantment*, their morbid condition, under the immediate influence of the gas! You may be assured, gentlemen, that I do not magnify the facts: but it was necessary to have seen the effect in order to believe it. The several practitioners mutually recounted to each other the pleasing change which, with satisfaction, they observed in the sick; so that we were able to prognosticate,

with all the probability of which the science of medicine is susceptible, that a patient who took carbonic acid, before the period of asphyxia, generally recovered speedily and securely.

“ The most immediate effects which I have constantly observed, from the introduction of the gas into the stomach, have been the following :—If the patient is labouring under suspicious diarrhœa ; loud rumbling of the bowels ; small pulse ; slight alteration in the physiognomy and voice somewhat changed, the third dose usually suspends altogether the diarrhœa, and the other symptoms disappear successively—the course of the disease, consequently, being arrested. If the patient has that continued mortal oppression at the præcordia, with burning heat in the epigastric region ; vomitings and rice-water evacuations ; remarkable alteration in the physiognomy ; without having altogether lost the temperature of the extremities ; it may be said that *the carbonic acid*, in these cases, *acts miraculously*. At the third or fourth dose, the patient experiences a calm and indescribable ease, which makes him exclaim, with all the vehemence imaginable, ‘ *Give me more of this draught, for it restores me to life.*’ Truly, it was surprising to see the rapid amendment of a patient, a little while before in so dangerous a state. The purging either ceased, or was converted from albuminous into excrementitious ; the voice, the pulse, the physiognomy, recovered themselves remarkably, and the physician inwardly rejoiced at the promptitude, security, and gentleness, with which he had just snatched from the jaws of death an idolised father, a beloved son, or a mother surrounded with presumptive orphans.

“ Do not suppose, gentlemen, that I overcharge, in any respect, this medical recital. Fourteen practitioners (the whole in the town) would, I am certain, put their signatures

to any testimonial which might be required of them. Persons of the first rank in this city proclaim openly the advantages which they experienced from this remedy; and I, also, should consider myself unworthy of the noble profession which I exercise, if I had been capable of distorting, in the least, a relation, whose object is no less than the good of humanity, and the advancement of medical science. All the objections which can be made against the especial efficacy of carbonic acid, in the Cholera Morbus of this city, are thrown down at the feet of the appreciable multitude of practical facts that I, the other practitioners, and, in particular, Dr. Rabassa, physician to the hospital, have collected, unless it should be proved that we have not seen that which we have seen."

In addition to this document, two very strong reports were also drawn up by the practitioners of the towns of Salamanca and Balleccas, in favour of this remedy, but I was unable to obtain copies of them previous to my departure from Madrid; as they had been mislaid or abstracted from the Archives of the Academy of Medicine in Madrid, by some individual, and thus rendered useless both to me and to others.

The following report, which was not inserted in the first edition of my work—not having been then received—is equally favourable. As it emanates from a public body, similar to the Colleges of Physicians and Surgeons in England, or the Faculty of Medicine in Edinburgh, and was drawn up by the express order of the Spanish government, it necessarily becomes a most important and valuable document.

REPORT of the Commission appointed by the Royal Academy of Medicine and Surgery in Barcelona, agreeably to the order of the Supreme Board of Medicine and Surgery in Spain, that it should make known what plan of Treatment had produced the best effects in the Epidemic Cholera, in those Towns situated in the districts under the jurisdiction of the Academy, and which had been attacked by the disease.

After a few introductory remarks, as to the nature of the duties which the commissioners had to perform; their obligations; the contradictory accounts that had been given respecting various remedies, and the failure of those that had been most vaunted, the report thus proceeds:—"After the first cases which presented themselves, the following plans of treatment were adopted. General and local bleeding; abstinence from all solids—the patient's diet being reduced to only toast and water or rice water; the external and internal application of refrigerants; sudorifics; antispasmodics and opiates; hot and vapour baths; the powders of vivorera (of Murcia) so strongly recommended; water acidulated with sulphuric acid, named, *par excellence*, the anti-cholera mixture; derivatives and blisters, varied in a thousand ways; the moxa; cupping; the sulphate of quinine, in large and gradually increased doses, both in powder and in solution; frictions of various kinds, and other agents which increase the heat of the body; camphor; musk; ipecacuanha; oil of olives; the sulphate of copper and iron; the oxide of bismuth; the aristolochia, etc. But, what cause for lamentation! *nearly all the patients that had the disease at all severely perished*; notwithstanding that, in the employment of the above means, the requisite attention was paid to the state of the patient, so as to determine the proper occasion for their

administration, and which is necessary with every remedy.

“This truth, lamentable as it is, the commissioners have had the sorrow to experience; but they hope to lessen its bitterness, by submitting to the judgment of the Academy a plan of treatment which, according to their ideas, in addition to being the most appropriate, *offers great simplicity*, and is the same, with a slight difference, which many practitioners have adopted with *the most happy results*, according to the official communications made to the Academy.” After drawing a distinction between the bilious diarrhoeas which prevailed at the same time, and which were found to yield to the ordinary plans of treatment, the writers add, that “the indication was entirely changed when the excretions were composed of the true choleraic fluid; and when, in addition to the rice-water evacuations, similar to those of dropsical patients, there was an alteration of the physiognomy, and a certain morbid condition of the surface of the body (blueness), which is common to all in that state, in spite of the number or diminution of the other symptoms. *When this is the case (i.e., when collapse has supervened), the employment of carbonic acid gas produces wonderful effects*—administered according to the method of Dr. John Parkin, an honorary fellow of this Academy, either in combination with water, or, as you may say, by forming the gaseous acidulated waters, obtained by the decomposition of the bi-carbonates of potash or soda, by means of the citric or tartaric acids. This treatment should be continued, even in the state of complete and confirmed collapse, on account of its being *the most efficacious and direct of all the plans that have been tried*; and it is certain that, if it is used at the commencement, and the patients do not neglect to call the medical attendant, after the disease has once manifested itself, *there will be few*

who come to this state of atrophy and wretchedness. As auxiliaries, blisters may be employed, and enemata of starch or linseed; and, for the cramps, frictions with common oil, either with or without laudanum, or, what is better, with assafœtida and camphor dissolved in vinegar." After a few more cursory remarks on some unimportant subjects, as the allowance of cold water, the treatment of the febrile stage, etc., the writers conclude their report in these words:—"The commission, in giving this opinion, has endeavoured, *without any other object than the truth and the future progress of science*, to reject every doubtful and fanciful theory which leads only to error, and which might be perpetuated if, through delicacy and ill-judged consideration, it had remained silent. The commission has made it a duty to conform entirely to this fundamental axiom: that facts, well attested and separated from all theory, are, in the treatment of disease, the only durable acquisition."

(Signed)

JUAN FRANCISCO DE BAHÍ, *President.*

"The above document was remitted in the name of the Academy to the proper authorities, who approved of the report in all its parts, and of the opinions given by the commission, specially named by them, and of which I was secretary."

(Signed)

B. SAUCH.

In a note which I received, at the same time, from Dr. Sauch, the then Medical Secretary to the Academy, the writer says, in answer to some inquiries of mine on this particular point; "It is impossible now to ascertain the proportion of cases of collapse, on account of the little regularity observed by the different writers in their communications to the Academy

—but it is clearly deducible, from the above report, that there were plenty, since, at the commencement of the epidemic, nearly all the patients attacked with Cholera died. I can also assure you, that I attended a great many patients in a state of *confirmed collapse*, whose recovery was entirely due to the administration of carbonic acid gas. In fact, from what I then saw in my private practice, somewhat extensive, I can affirm that the treatment of Cholera with carbonic acid is truly scientific; the gas in this case is the *real antidote to the disease*, and, consequently, the supporter of life. Up to the present time, it is *the only therapeutic agent* which, taken scientifically, is capable of destroying the organic lesion, by directly changing the morbid action which produces the Cholera. Such admirable results we may in vain expect to obtain from any of the other methods which have been adopted, the whole of them being, more or less, unsuitable or useless.”

On my way to Valencia, as was before mentioned, I touched at Alicante, where the disease was then prevailing; but not being allowed to land and embark again in the same vessel, which I was anxious to do in order to proceed on to Barcelona, where the Epidemic had just commenced, I could only obtain one short interview with the practitioners of the above town, several of whom very kindly walked down to the end of the pier for the express purpose,—an attention for which I am indebted to Mr. Williams, the

then acting Consul, in the absence of Mr. Waring, Her Britannic Majesty's Consul. During the conversation which ensued, I explained to these gentlemen my object in visiting Spain, and the reasons I had to be so satisfied with the remedy which it was my wish to recommend; leaving them also, on my departure, a copy of a Memoir on the Cholera which had then been translated into Spanish, but not published.

During a second visit to Alicante, some months afterwards, I learned that the plan of treatment then recommended had been adopted in both the hospitals established there; and that it was principally trusted to for the cure of the disease, during the latter part of the period of its prevalence. That it was found successful may be presumed, when it is stated that, in one hospital, in which previously the powers of Vivorera had been almost wholly trusted to, no sooner were the effects of the carbonic acid witnessed than the above remedies were abandoned, and their place supplied by effervescing draughts. This is no small proof of the efficacy of the latter remedy; more especially when, in addition to what has been already advanced respecting the good effects of these powders, it is added, that a commission of medical men was then in Alicante, having arrived from Murcia with the express view of making known their virtues, and recommending their adoption.

Although the Cholera only prevailed in Spain,

during the following year, in a few towns in the north of Catalonia, the carbonic acid was employed in one of them, and with its usual success; as evidenced by the following extract from a letter inserted in the *Vapor* newspaper of Barcelona, No. 261, announcing the cessation of the disease as an epidemic, and the removal of all restrictions—the Te Deum having been previously sung. It concludes thus:—“The plan of treatment recommended by Dr. Parkin, whose treatise I obtained out of mere curiosity when I was in Barcelona, has effected wonders, since it has cured all with whom it has been tried.” “Would to God,” exclaims the writer, “that it had been adopted, even by one of our Physicians, at the commencement of the disease, to the same extent as was the case afterwards, for we should then, most assuredly, have had no victims.”*

It might have been supposed that the preceding evidence would have been sufficient to satisfy all unprejudiced and impartial persons, and even the most sceptical. This was not the fact. Several of the reviewers of the first edition of my work professed doubts of the value of the remedy, if not of the results that had been obtained in Spain. One critic said: “It is utterly impossible that so simple a remedy can cure so dreadful a disease—one that has baffled the united skill of the profession in this and every other

* This letter was from the English Vice-Consul at Rosas, to the Consul at Barcelona.

country." To this I replied: "It is this very circumstance which ought, in my opinion, to command attention; for it may be confidently said, that in the treatment of disease, as well as in many other sciences, 'Simplicity is the perfection of Art.'"

If, however, by simple, they mean *single*, I would answer that Nature produces the most important results by means, apparently, the most simple—never employing *two* agents when one will suffice. We may therefore conclude, that in the production of disease, as well as in all her other operations, the same rule is observed; while I may add, such an inference is confirmed by the fact that certain diseases are known to be produced by a single, or specific, cause. If therefore the *cause* be single, why should not the *remedy* be single also? Were *Nature* the physician, such, doubtless, would be the case; for we may be sure that she would never employ *two* remedies to remove a disease produced by a single, or specific, agent. Nor should *we*, unless from ignorance, or from an inability to obtain, by a single remedy, the result that we seek for—sometimes successfully, and sometimes fruitlessly—by the employment of a number.

But this agent, after all, is not so inoperative as many suppose: it is endowed with most important chemical properties. It not only renders putrefactive and other matters injurious to animal life innocuous, but it will actually preserve meat and other

articles, excluded from the contact of the external air, untainted for centuries—as long, in fact, as we can prevent the escape of the antiseptic gas. Can such an agent, therefore, be a simple one, or unlikely to be attended, I would ask, with beneficial results? especially when compared with the greater number of those employed in the treatment of disease; and which, so far from being possessed of antidotal properties, may themselves be placed among one or other of the different classes of poisons.

Under the idea that the antidotal agent is a simple one, and totally inadequate to the production of the results detailed, another critic observed: “To (*sic*) those who have witnessed any cases of this disease, when epidemic in this or other countries, or who have merely read of the melancholy and hopeless powerlessness of almost all the methods of treatment employed, must assuredly be not a little surprised at Mr. Parkin’s confidence, and alleged success..... It is obvious that Mr. Parkin is far from being justified, by the result of his own cases, in pronouncing effervescent saline draughts to be the antidote of Cholera; and in reference to the Spanish evidence, in the absence of specific documents, and with the ample experience we had in this country of the extreme difference of the epidemic in different places, is it not infinitely more probable, that the particular epidemic, treated with such happy results by the Spanish physicians, was one

naturally mild and tractable—one that would have yielded to the remedial powers of nature—than that *the terrible Cholera*, which, in many parts of this country, and in some parts of Spain, produced such havoc, could have been subdued by a few saline draughts? It is impossible to come to any other conclusion.”* So said the critics in Harvey’s time. It is impossible, they argued, that blood can circulate in the arteries! And why? Simply because they had found them void after death. So, instead of plunging a lancet into the artery of a living animal, in order to satisfy their doubts, they wrote treatises for 40 years after Harvey’s immortal discovery, in order to disprove what was a matter of ocular demonstration! Thus it has been with my critics. Instead of waiting to ascertain, by practical experiment, whether the facts mentioned by me were true or false, they quietly seated themselves in their arm-chair in their studies; and then pompously announced to the world, that to obtain such results by the means indicated was impossible. Impossible! I had thought previously that this word had been long since expunged from the scientific vocabulary; particularly after we had seen the mighty Atlantic traversed by means of a little boiling water, contrary to the dictum of a modern philosopher; and after having held ‘sweet converse’ with every distant land, and sent our messages from pole to pole by the aid of a few copper

* *British and Foreign Medical Review*, January, 1837.

wires, and that simple, invisible, agent—electricity. But we may let that pass, and come to facts, not opinions and conjectures, by briefly examining the validity of the reviewers' doubts and objections.

With respect to the first of these, I have only to remark, that it could never have been my intention to rest the decision of this question on the result of the treatment of the few cases added in the Appendix. Those cases, it was distinctly stated, were merely given "as illustrative of the effects of the remedy in the different stages of the disease," and with the view of showing, that the same agent was sufficient to arrest the vomiting at one period, the purging at another, and to remove the state of collapse at a third. I rest the claim which this medicine has to the attention of the profession, on the *invariable* result that has attended its administration, in all the cases which have fallen under my observation—a result which has been particularly dwelt upon in another and a different part of the work.

As regards the Spanish evidence, I may be allowed to say, that there are no persons so blind as those that will not see, or so deaf as those that will not hear. So, because we had hitherto been unsuccessful in the treatment of the disease, and because, forsooth, practitioners in this and other countries had been unable to arrest its destructive tendency, it follows, as a natural consequence, that no remedy can hereafter be found of any value in the treatment of the disease. Well, in-

deed, has it been observed by one writer, that “the scepticism of the learned is sometimes not less injudicious and indiscriminate than the credulity of the savage; and he who should resolve every extraordinary event into ‘a cunningly devised fable’ would not be less reprehensible for want of candour than the untutored rustic, who yields his assent to every alleged miracle, is to be taxed with want of discrimination.”

In order to render some apparent support to his puerile reasons, the reviewer affected to despise not only the evidence, but the individuals who brought it forward; for he says, that the second class of documents consists of short fragments of letters and reports from *three or four* physicians.* Now these letters and reports happened to be from a number of practitioners in England and other countries; from three of the first physicians in Barcelona; and from fourteen practitioners of Mataro (the whole in the town); for, although the report was written by Dr. Pascual, it was at the request and with the sanction of his brother practitioners, as is evident from a passage contained in that report.

Fortunately, I am enabled, in this instance, to bring

* It is right to add, that the Report of the Academy of Medicine in Barcelona was not given in the first edition of my Work. The Report was first forwarded to the Supreme Medical Board in Madrid; and it was not until after it had received their approval that I was favoured with a copy.

forward, in addition to my own, the opinion and the testimony of another individual—a disinterested witness. Dr. Dunal, of Montpellier, the translator of the French edition of my work, referring to the adoption of the remedy in Barcelona, added: “He” (the author) “arrived in that town at the moment when the epidemic was at its height, and frankly communicated his opinions, which were adopted and put into practice by *several very distinguished* practitioners; among others by Dr. Sauch, Physician to the General Hospital, and Secretary to the Royal Academy of Medicine and Surgery in Barcelona. The epidemic was not long in manifesting itself at Mataro. Mr. Parkin repaired thither, and his plan of treatment was adopted with *wonderful success*. These particulars, *the accuracy of which may be depended on*, I received from Dr. Campdéra, a pupil of the Medical School at Montpellier, who has practised his profession with success in Catalonia for twenty years.” That there were no other, or statistical, returns, must be, of course, a subject of regret to me, as well as to others. But the fault was not mine. As I refused to attend patients, excepting in conjunction with the medical attendant of the patient, and at his express desire, the consequence was, I seldom saw a patient twice; and thus became entirely dependent on the voluntary and casual evidence of other practitioners. Statistical details were promised me, on my departure from Spain; but they never arrived.

From the comments of the reviewer, a person would suppose that no data had been given as to the type of the disease in that part of Spain. But this is not the fact. Not only was the gross mortality in Barcelona and Valencia then stated, but the proportion of deaths to cases in the latter city was also given. According to the official lists then published, there were in Valencia 3,854 deaths out of 5,115 attacked; being a ratio of 75 per cent.—25 above the average in Europe. In truth, I can state, from my own observation, as well as from statistical facts, that in no country in Europe, if we except the fatal irruption at Paris, was the epidemic more severe, or its ravages greater, than in Spain.

In Valencia, where the treatment now under discussion was not adopted, except in a few solitary cases—as I did not arrive until the subsidence of the epidemic—the mortality amounted to 6,000, out of a population of about 50,000; the remainder having fled on the outbreak of the malady. This amount is nearly one-third more than the number of deaths in London, with a population—a resident population—of a million. In Barcelona, again, where the administration of the remedy proposed by me was commenced shortly before the epidemic had arrived at its height, and when from 100 to 200 were dying daily, the mortality was calculated at from 3,000 to 4,000, out of a population, then remaining in the town, of about 70,000. At Mataro, it is true, the mortality was very

low—not more than three per cent. of the number attacked; but, then, was this to be ascribed to the mildness of the epidemic, or the superiority of the treatment adopted? If any faith is to be placed in the report of Dr. Pascual, we must ascribe it to the latter; while also I may add, that in the neighbouring towns, attacked at the same time, the disease appeared to have lost nothing of its intensity or virulence—at least, if we are to judge by the proportion of deaths. Thus, at Suria, two-thirds of those attacked died: while at Manresa, a village between Barcelona and Mataro, and which was invaded by the disease shortly before the latter, the whole of the cases proved fatal. I would only further remark, that, in these towns, the treatment recommended by me was not adopted.* But independently of individual opinions and statistical facts, the reviewer ought to have known that the difference in the mortality in different countries—possessing the same physical qualities or temperature—has not arisen so much from a variation in the type of the disease as

* There were 1,000 cases and 30 deaths in Mataro. Of these, 15 died before the adoption of my remedies, and 15 after. As every patient, after the first 15 deaths, took carbonic acid gas, and no other remedy, the rate of mortality by the antidotal method amounted to 1.52 *per cent!*

Having only remained in the town two days, I am unable to give any particulars as to the general type of the disease. I can only state, that, of six patients in the Cholera hospital, five then were, or had been, in a state of collapse!

from the greater or less extent of the ravages, *geographically considered*. Thus, although the mortality was so small in London, when compared with other places, the cause was not to be ascribed to the mildness of the epidemic, or the superiority of the treatment adopted, but solely to the limitation of range of the morbid cause; the ravages of the disease being principally confined to particular parts of the town, particular streets, or particular houses; while it was also shown, that *the number of deaths to the number attacked* was the same as in other countries in Europe, viz. about one half.

Again: the reviewer supposes that the patients in Spain, treated by this particular method, might have recovered by the remedial powers of Nature, instead of by the efforts of art. This conclusion, however, is contrary to all experience; for not only must we attribute every case of recovery *entirely* to the efforts of art, but we must also, with the writer of the Madras Report, consider *death* as the ordinary termination of Cholera. In fact, it is on record, that in India and elsewhere, where the population was without medical aid, *every person attacked died*.

In a village, near Meerut, the inhabitants of which could not be persuaded to use any remedies, every individual attacked died.* Dr. Consour also informs us, that, among the numerous sects of Russia, there is one, the adherents of which are bound to refuse

* Bengal Report, p. 175.

medical aid. "Among these fatalists, as it were, the confirmed Cholera has been extremely destructive, as nearly all the patients died."* With these facts before us, we are bound to conclude, that the recoveries in Spain by the treatment under review are to be referred, not to the remedial powers of nature, but to the antidotal powers of the different forms of carbon. That this conclusion is a correct one, we may infer, not only from what has gone before, but from certain facts to be gleaned from the medical records of this country.

As previously mentioned, carbonic acid came to be very generally employed as an adjuvant in England, towards the termination of the visitation in 1832; and still more during the outbreak in 1849. This shows that it was found to possess some peculiar and beneficial property. More than this; the most successful method of treatment appeared to be that to which one or other of the different forms of carbon had been added. But this is not all. There were certain modes of treatment recommended and extolled as highly efficacious by particular individuals; but which, in the hands of other practitioners, were found to be completely useless and inefficacious.

As an example, I may mention the different results that have been obtained with different individuals, by what has been termed the saline treatment. Dr. Stevens and several other practitioners affirmed that

* Bulletin Général de Thérapeutique.

this treatment was more successful than any other that had been adopted ; while, on the contrary, many who had tried the same remedies considered them either useless or worse than useless. To what, then, are we to ascribe this great discrepancy ? Not to the perversion of facts or a biassed judgment ; for, notwithstanding a dispute ensued, which became, to say the least of it, acrimonious and personal, we have no reason to question the honour or the motives of either party ; much less of the talented individual most interested in the issue of the question. The solution of the problem I consider to be this : carbonic acid was administered in one case, in combination with the salts, while, in the other, the salts were employed alone.

As the subject is an important one, both to me and to the public, it will be desirable to pause for a short time, in order to examine the facts on which this conclusion reposes. Such a statement ought not, indeed, to rest on individual opinion, or mere assumption ; the saline treatment having been extensively adopted, and still having its zealous supporters, and as warm opponents.

This mode of treatment was first brought to the notice of the profession at the commencement of the outbreak in England, in 1831 ; a paper on the subject having been read by Dr. Stevens, at a meeting of the College of Physicians. Previously to this, the neutral salts had been proposed and employed in the

fevers of the West Indies, Dr. Stevens having practised, for some years, in the Island of Santa Crux. His opinion of their *modus operandi*, and of their value in these diseases may be gleaned from the following extract:—"They (the salts) enter the circulation, and not only redden the colour of the whole current, but remedy its diseased properties; adding, at the same time, to its power of stimulating the vascular organs much more effectually than either bark, brandy, or opium. But, independently of their power of reddening and adding to the stimulating quality of the blood, there are, also, many facts which induce me to believe that some of the active salts possess a specific property, by means of which they are capable of *neutralising* and preventing the action of *the aerial poisons* that cause fevers."*

Dr. Stevens drew the same conclusions respecting the action and the efficacy of his remedies in the epidemic Cholera; and as his opinions were embraced by Dr. Prout, and several other physicians of note, the result of the employment of the neutral salts was looked forward to with more than usual interest.

The first trial was made in the Coldbath Fields Prison by Dr. Stevens himself; and the following is the result of the treatment, as furnished by Mr. Wakefield, the surgeon of the prison.

Speaking of the first visitation in the prison, Mr. Wakefield remarks, in a letter addressed to the editor

* On the Blood, p. 310.

of the *Medical Gazette* (April 22, 1832): "There have been nearly 100 cases, where individuals have been, more or less, evidently labouring under the influence of the cholera poison. Twenty-five of these assumed the malignant character of the disease, and three died." Excluding the former, the mortality would be 12 per cent. In a subsequent visitation, when the patients were under the care of Dr. Stevens and Mr. Crook, there were, from the 28th June to 28th August, 187 cases, 175 recoveries, and 12 deaths—the ratio of mortality in this instance being only 6.3 per cent. But this rate included all the cases—mild and severe.

After this remarkable success, and the promulgation of the fact, the saline method of treatment was adopted by a large number of practitioners. But great was the surprise expressed by several of these individuals, when it was found that their result was entirely different to that of Dr. Stevens.

In the *Medical Gazette* (August 11, 1832) some cases are reported, treated on the saline principle.* *The whole* of these cases proved fatal. "In seven cases," observes one writer, "the saline treatment was employed, not only without mitigation of any one symptom, but with injurious effects. In not one case did the saline treatment produce a recovery." †

* The remedies employed were : muriate of soda and carbonate of soda, 30 grains each, with 5 grains of oxymuriate of potash, at each dose.

† Report of patients admitted into St. Pancras Cholera Hospital.

Again: Mr. Anderson, surgeon to the York Hospital, informs us, that "the saline treatment was tried in most of our first cases, till we resolved to abandon it as a most useless remedy. In our hands, at least, it proved so."* Mr. Robertson, also, surgeon to the Cumberland Convict-ship, Chatham, remarks: "The saline treatment recommended by Dr. Stevens and his advocates was fairly and fully tried. Their cases, and mine, however," he adds, "must have differed widely in their nature; for, assuredly, never was mode of treatment more unsuccessful. I deeply lament ever having trusted to it."†

Drs. Casey and Bullen, physicians to the North Cholera Hospital, Cork, say, in their report: "We have employed, in the treatment of several cases of Cholera, in this hospital, the saline powders recommended by Dr. Stevens; and have not observed them to produce any good effect. They have, in some instances, caused a great increase of vomiting, but without advantage. We do not think them entitled to any degree of confidence; and have rejected them from the practice of the hospital, as entirely useless, if not injurious."‡

By a reference to the Table previously inserted (page 44), it will be seen that salines, *i.e.* the neutral salts, employed alone and without any adjuvant, give

* *Lancet*, Vol. i., p. 72, 1832—1833.

† *Ibid.*, Vol. ii., p. 558, 1831—1832.

‡ *Ibid.*, Vol. i., p. 51, 1832—1833.

the least favourable result of any method of treatment. This is not all. Salines are sometimes injurious, as well as inefficacious. In addition to the tendency which they have to increase the irritation of the stomach, and to keep up the vomiting, they act as irritants on the mucous membrane of the bowels. "The danger," remarks Mr. Ross, "of administering saline remedies is in the bleeding diarrhœa to which they are apt to give rise—a symptom which is generally fatal."*

These results, and the extraordinary discrepancies between the statistics of Dr. Stevens and those of other practitioners, gave rise, as previously remarked, to a warm and personal discussion—those who had adopted this mode of treatment, and failed, imagining that the figures of Dr. Stevens were erroneous, or, else, that his cases were not genuine and decided cases of cholera; while Dr. Stevens concluded that his remedies had not been fairly and properly administered.† Both parties were wrong. Not only were the statistics of Dr. Stevens correct, and his cases genuine cases of cholera, but his remedies,—as far, at least, as they were indicated by his theory,—

* *Medical Times*, Feb. 23, 1850.

† "As we predicted," remarks the editor of the *Lancet*, "professional scepticism and displeasure, or, in the language of Dr. Stevens's friends, slander, envy, and detraction are busily concerned in the discussion of the character of his statements."—*Lancet*, Vol. ii., p. 43, 1831-2.

were properly and faithfully administered by other persons; the subject having been since fully investigated by Dr. Turley and Mr. Ross, and these facts clearly ascertained. We must therefore seek for some other cause of the discrepancy; and this will readily be found, if we examine the accounts furnished of the various remedies that were employed in these respective instances.

Turning to the plan of treatment pursued at the Coldbath Fields Prison, as described by Dr. Stevens in his work, we find the following detail:

“Premonitory stage:—On admission, a seidlitz powder was administered, and, if sinking was felt, but without bowel complaint, more active purgatives were then employed; or, three or four tea-spoonsful of Epsom salts were added to the seidlitz powder. Thirst was relieved with *seltzer*, *soda*, or *pure water ad libitum*.

“Next stage—characterized by cramps, coldness, or sinking of the pulse:—The non-purgative salts were administered every half hour, or more or less frequently, according to the severity of the symptoms, and in the following dose; muriate of soda 20 grs.: carbonate of soda 30 grs.: chlorate of potass 7 grs. When the patient complained of heat or burning at the stomach an additional quantity of *the carbonate of soda* was added to the saline powder.

“When collapse had supervened, a strong solution

of the same salts, at 100, was thrown into the bowels. In extreme cases of collapse, this latter method," it is added, "succeeded far better than the injection of the vital electric salts into the veins.

"When the stomach was extremely irritable, it was found that the carbonate of soda, given by itself, or the tartrate of soda, *in a state of effervescence, was the most effective remedy* that could be employed for allaying the irritation, so as to enable the stomach to retain the stronger salts. At this period of the disease, the alkaline carbonates are of infinite value. For example, by exhibiting the carbonate of soda, the fixed acids of the stomach are immediately neutralised: *a large quantity of carbonic acid is evolved* by the mouth, and the irritation of the stomach disappears almost as fast as *if it had been removed by a charm.*"* In addition to this, Dr. Stevens has remarked, in another place:—"When the stomach is irritable, which it generally is in Cholera" (who could have doubted it?) "*the saline effervescing draughts are of great value; and I feel confident that the mortality from this disease would be greatly lessened, even if we were to trust, almost entirely, to this simple remedy.*"†

It is thus apparent that carbonic acid was admi-

* On the Nature and Treatment of Asiatic Cholera. By William Stevens, M.D., 1853. Pp. 38 and 39.

† *Medical Gazette*, August 25th, 1832.

nistered by Dr. Stevens, in the largest possible quantities, in all stages of the disease, and especially in the first stage. That the benefit derived in these instances is to be ascribed to this adjuvant alone, we may conclude, not only from what has been advanced in a previous chapter, while detailing the effects of the administration of carbonic acid gas, but, also, from the fact, that when the salts of Dr. Stevens were employed alone, and without any adjuvant, no benefit was derived from the treatment.

There is another circumstance which we shall be able to understand by a reference to the result obtained, when carbonic acid has been given alone and uncombined in the first stages of Cholera. When asked by Sir David Barry, on his official visit to Coldbath Fields Prison, where his cases of collapse were, Dr. Stevens replied, that his treatment prevented the supervention of that stage. This answer, which appeared inexplicable, and a mockery to many, was, nevertheless, a true one; as there can be no doubt that patients who had an unlimited allowance of soda water in the first stages of the disease never would fall into a state of collapse.

It also appears that carbonic acid has been employed in combination with the salts of Dr. Stevens by other practitioners, and with the same beneficial results as at Coldbath Fields Prison.

Thus, in the cases treated by Mr. Bossey, at Wool-

wich, on this system, the remedies employed were as follows: "A salt-water emetic was first administered. This was followed by a dose of the saline mixture, or Dr. Stevens's salts every half hour; and by *an effervescing draught*, with an excess of soda, every hour."* An enema, composed of carbonate of soda, muriate of soda, and starch with barley water, was also administered every hour. †

Of 65 patients thus treated, 56 recovered, and 9 died. Of the recoveries, 16 had premonitory symptoms, leaving 40 cases of decided Cholera, of which 10 had partial, and 30 complete, collapse. This would give a ratio of 13 per cent., if the slight cases be included, and of 18, if they be excluded—a ratio somewhat higher than that at Coldbath Fields Prison. The difference is easily accounted for. The patients of Mr. Bossey did not, like those of Dr. Stevens, have an unlimited allowance of soda water.

Mr. Tucker, of Sligo, referring to Dr. Stevens's method of treatment, adds:—"I feel convinced that it is the most efficacious and successful of any practice, that is yet known. In every instance, where the patient was put under treatment before collapse set in, the case did well; while many patients in the

Medical Gazette, vol. x., p. 729.

† In addition to the above, the patients were allowed barley water *ad libitum*; to every pint of which *two drachms of carbonate of soda* were added.

last stage, who appeared to be beyond all hope, recovered under its use."* No mention is made of the remedies employed; but we may be certain that carbonic acid gas was administered at the same time, as it appears that such was invariably the case, whenever the saline treatment was successful.† Thus, Dr. Mair, of Kingston, Canada, who resorted to the salts of Dr. Stevens, during the prevalence of Cholera in that country, in 1839, states that "the powder was given with tartaric acid, *in a state of effervescence*, for cases of premonitory symptoms. I can hardly entertain a doubt," he adds, "that benefit was derived from this medicine as a prophylactic. . . . I frequently had recourse to it, when fatigued and suffering from a sinking sensation in the stomach. Its use was instantaneously followed by a pleasant, exhilarating feeling pervading the whole system, from the centre to the extremities."‡

The following case reported by Dr. Turley — the great advocate of Dr. Stevens, and of the saline

* *Medical Times*, Oct. 20th, 1849.

† Mr. Ross, who analysed the evidence respecting the saline treatment, but who failed to discover the cause of the discrepancy, tells us, that "a most agreeable and useful mode of administering salines is in the form of an *effervescing draught*, when it is given in conjunction with carbonic acid, and tends to allay the excessive uneasiness at the stomach."—*Lectures on Asiatic Cholera: Medical Times*, Nov. 20th, 1849.

‡ *Medical Times*, Nov. 24th, 1849.

treatment,—affords a good illustration of the preceding conclusions.

“ Mr. E. T., aged 47, of robust and rather plethoric frame, was in good health at 10 o'clock in the morning. Immediately after this he was seized with pains and cramps in the stomach and bowels: these soon extended to the limbs, so that the knees were drawn up to the shoulders, and the nails were nearly driven into the palms of the hands. A chemist gave him a draught with aromatic confection, and 25 drops of laudanum.”

When seen by Dr. Turley, “ the patient was in a state of great prostration—cold clammy skin, blueish cheeks and mouth, hands blue and shrivelled, the nails purple, the pulse about 40, very feeble and intermittent. He was ordered a tea spoonful of common salt, and half that quantity of carbonate of soda in a glass of potass water *highly effervescent*. This remained, and afforded him immediate comfort. In about a quarter of an hour, I gave him,” adds Dr. Turley, “ *in the same vehicle*, one of the powders of Dr. Stevens. The cramps and sickness left after the first dose; and warmth and proper colour supervened gradually after the second dose. He took only two more doses, and was well, after experiencing slight fever, in a few days.”*

With these different and opposite results, it is not

* *Medical Times*, Sept. 29th, 1849.

surprising that there were two parties in the profession: the one condemning Dr. Stevens's treatment, and saying it was worse than useless; the other extolling the remedies, and praising them to the skies. Among the last is Dr. Turley, who, in 1849, wrote a series of papers in the *Medical Times*, in defence of Dr. Stevens's method of treatment. He also stated, that it was his intention to occupy himself with a work "On the history of *the opposition* to the first introduction of the saline remedies in Cholera, *by which thousands of lives have been lost.*"

In order to prove to Dr. Turley that he is in error, and to show to him and others, that the salts of Dr. Stevens are, and must be, useless in attacks of the Asiatic Cholera, we have only to turn to the theory of this writer, and to inquire into the *modus operandi* of his remedies. The theory of Dr. Stevens is contained in the following propositions:—

1. "The fluidity of the blood depends, not on the presence of an alkali, but to the circumstance of the solid ingredients being held in solution by a saline fluid."* But blood, when drawn from the arm, immediately coagulates, although the quantity of saline matter, which it contains, is the same after as before coagulation.

2. "Pure alkalies, all acids, and carbonic acid, give a dark colour to the colouring matter of healthy blood." As regards alkalies and acids, the conclusion may be

* Observations on the Blood, p. 6.

granted: not so the action of carbonic acid on the colouring matter, the result that has been obtained by certain experimentalists leading to a very different conclusion. If the black colour of venous blood be the effect of the presence of carbonic acid, the separation of this gas from the blood ought to restore its florid colour. "To ascertain this, a portion of healthy black crassamentum was placed in the receiver of an air-pump, in a small vessel communicating with another holding lime water. On exhausting the receiver, the crassamentum gave out carbonic acid, and *retained its black colour.*"* The reverse operation is no less conclusive. "On passing carbonic acid through *red* crassamentum, no change of colour was observed."† The presence of carbonic acid cannot therefore be the cause of the black colour of venous blood.‡

3. "The whole of the neutral salts, and, also, the sub-carbonate of soda, when added to venous blood, change the colour from a dark Modena red to a bright arterial colour." Hence, Dr. Stevens infers that arterial blood owes its red colour to the salts contained in it; and that "arterial blood contains a larger proportion of saline matter than venous."

4. "The colour of the blood becomes red in the

* *Lancet*, Sept. 8, 1832.

† *Ibid*, August 25, 1832.

‡ "After the inhalation of carbonic oxide, and some of the hydrocarbons, the blood is of a brighter and more scarlet hue than natural."—Nunnerly, on Anæsthetic Substances. The same result has been observed by me after the administration of carbonic acid gas.

lungs, because the carbonic acid is removed, which permits the salts to resume their reddening agency."

5. "Oxygen only influences the production of the red colour by removing the carbonic acid." 6. "Blood becomes red when exposed to air, or oxygen, only when it contains saline matter."

That salt, when added to blood out of the body, has the property of changing it from a dark, or a black, colour to a bright red is a phenomenon familiar to every nurse; but we must not hence infer, that the red colour of arterial blood is due entirely to the presence of the salts. In the first place, it has been just shown that the dark colour of venous blood cannot be ascribed to the presence of carbonic acid: its extrication, therefore, from the lungs cannot be the cause of the change of colour from the venous to arterial. Nor can this change be referred to an increase in the quantity of saline matter, as no alteration of this kind occurs at the moment of the conversion of venous blood into arterial. Besides, it has been shown by Prosper Denis in his work—*Sur le Sang Humain*—that venous blood contains as much saline matter as arterial. Hence, if carbonic acid be not the cause of the dark colour, venous blood, according to the theory of Dr. Stevens, ought to be red; but it is—black! We may therefore conclude that the red colour of arterial blood is not owing to the saline matter which it contains.

As regards the agency of oxygen, it has been

before shown that the abstraction of the carbonic acid, *in vacuo*, does not change the dark colour of venous blood: if, therefore, oxygen gas exert any influence in the change from venous to arterial—or, from black to red—it must be directly, not indirectly. The following experiment would seem to prove this:—“If you immerse the *red* clot,” remarks the writer in the *Lancet* before referred to, “in distilled water, and place the vessel in the chamber of an air-pump, bubbles of gas will rise through the water, and, as this takes place, the crassamentum *becomes black*.” It is immaterial whether this gas be oxygen or carbonic acid, as the result, in either case, is fatal to the theory of Dr. Stevens. We may however conclude, that the change takes place from the abstraction of oxygen: an inference confirmed by the following operation—the *experimentum crucis*. “Our Dublin contemporary, the *Dublin Medical Journal*,” observes the reviewer, “goes a step further, and, on re-admitting atmospheric air to this black *saline* blood, discovers that it forthwith again becomes florid. The experimental evidence is here complete, and overturns the whole of Dr. Stevens’s theory, from the dome to the foundation.” We cannot ascertain what the result would be, if the crassamentum were deprived of its saline ingredients; it being impossible to separate the whole of the saline matter, even by repeated washings in distilled water. It is clear, however, that this matter must play a very subordi-

nate part in the arterialisation of the blood ; the change that takes place in the lungs being, apparently, the effect of the absorption of oxygen into the circulation. No matter what the operation may be, when this agent is absent no change takes place ; while its abstraction changes red blood to black, and its addition transforms it again into its original colour—the proportion of salts being the same in both instances.

As to the change of colour which occurs out of the body, when salt is added to black blood, although ignorant of the exact cause of the transformation, we must yet remember that the phenomenon is only observed in the presence of oxygen. Looking at the preceding facts, we shall be justified in inferring, that the salt merely acts by producing some change in the blood—such as rendering it more fluid—and which, by assimilating it to the condition in which it exists in the body, enables it the more readily to absorb oxygen from the air. Be this as it may, it is evident, from what has gone before, that the salts exert but a small and insignificant influence on the transformation of colour that takes place within the body ; and, consequently, that the foundation on which Dr. Stevens has raised his superstructure is false and untenable.

Having arrived at this conclusion, we may now pass on to consider the practical deductions that Dr. Stevens has drawn from the promulgation of his theory, and which, as a matter of course, form the key to his treatment.

“In malignant fever,” remarks Dr. Stevens, “as the disease advances, the colour of the whole mass of blood, both in the arteries and veins, changes from its scarlet or Modena red, to a dark black.” . . . “In these fevers, the saline matter, as well as the fibrin, appears to be exhausted faster than it enters the circulation; so that the black colour of the blood is a certain proof of *the entire loss*, or, at least, of the great diminution, of the saline ingredients.”

That the blood, in the majority of fevers, is darker than usual is an undoubted fact; but the conclusion, that this dark colour can be referred to a loss of saline matter, is a gratuitous assumption. There is no proof whatever, that the salts of the blood are diminished in quantity in ordinary fevers; more especially at the commencement of the attack. And yet the blood will be found changed and darkened, in the majority of cases, even before the paroxysm commences. The loss of saline matter cannot, therefore, be the cause of the dark colour of the blood in fever.

But Dr. Stevens extends his theory to Cholera, as well as fever. Here, there would appear to be some foundation for his inferences, as, during one part of the attack, there is a great loss of saline matter. Hence, it was not very irrational to infer, that the dark colour of the blood in Cholera was owing to the loss of the salts. Yet this conclusion, like the former, will be found to be untenable.

In the first place, the blood has been found to be quite black, when drawn, in those cases of sudden collapse that have not been preceded by purging, or by serous evacuations; cases in which there could have been no appreciable loss of saline matter.* There must therefore be another cause in operation to which the black colour of the blood is to be referred in Cholera, the same as in fever.† That cause, there can be no doubt, is the presence of a specific poison. This is allowed even by Dr. Stevens to be the case, at the commencement of an attack of fever, although not afterwards. "The dark colour of the blood," he remarks, "which we observe in the beginning of pestilential fevers, is the effect of the poison on the vital fluid; but the blackness in *the last stage* of these diseases is produced by the loss of the saline ingredients."‡ And it is added, in another place, while

* We find by the investigations of Dr. O'Shaughnessy, first, that the blood of healthy persons resident in infected districts or houses is not chemically changed; secondly, that no alteration of the blood takes place either in the premonitory stage, or in that characterised by cramps and slight evacuations; and thirdly, that the alterations of the blood, consisting in loss of water, and saline matter, only occur in the collapsed cases, which have been preceded by excessive rice water evacuations.—*Lancet*, Vol. i., p. 704. 1832-3.

† Dr. O'Shaughnessy tells us he inferred, from his experiments, that the alterations in the blood were *secondary effects* of the original and unknown cause of the disease.

‡ *Opus Cit.*, p. 441.

referring to the poisons productive of Cholera and Yellow Fever: "both poisons produce their effects by their absorption into the circulating current; causing a morbid condition of the living blood, by which, after a short time, the vital fluid becomes divested of its saline ingredients; and is thereby rendered incapable of carrying on the circulation, and supporting the life of the material frame."* It thus appears that Dr. Stevens refers the black colour of the blood, in both diseases, to the operation of two different agencies—the one at the commencement, the other at the termination, of the attack. It would be, in my opinion, more philosophical, and more in accordance with all the facts, to refer the effect to one and the self same cause; particularly as we have no evidence that there is any loss of saline matter during attacks of fever. Were it otherwise, it would be no proof that the black colour of the blood was owing to the loss of saline matter; or that the introduction into the system of a fresh quantity would change the blood to its normal condition. The following experiment affords presumptive evidence of the truth of this conclusion. Dr. Stevens states, that "he added a *very large* proportion of muriate and carbonate of soda to different portions of the blood, drawn from a person in *the incipient stage* of fever; and though they reddened the colour, still, they did not produce the same beautiful arterial ap-

* On the Nature and Treatment of the Asiatic Cholera, p. 17, 1853.

pearance which they invariably give *to blood in health.*"* The same result was observed with some patients of Dr. Skey, labouring under typhus fever. We may therefore conclude, that the administration of the neutral salts in fever would not be productive of much advantage, or change the dark colour of the blood to its healthy standard. But this is immaterial. The question at issue now is, would the administration of the salts in Cholera be productive of any advantage; or would they effect all the beneficial results stated by Dr. Stevens, and others?

"The evacuation of the serum," observes Dr. Turley, "occasions great thirst, and the plentiful supply of the artificial fluid is readily imbibed into the blood; the heart is thereby invigorated; its natural action restored, and the blood then acquires its wonted colour."† Setting aside the question, whether fluids are, or can be, absorbed into the venous system during the stage of collapse, let us inquire whether these results are likely to follow the introduction of saline matter into the blood.

There are necessarily but few facts which enable us to ascertain the changes that take place in the system, or in the blood, after the ingestion of particular substances. Fortunately, one experiment that has been made satisfactorily shows what the

* On the Blood, p. 229.

† *Medical Times*, March 24th, 1849.

result has been of the introduction of the neutral salts into the blood, during attacks of the epidemic Cholera—these salts having been injected into the veins by certain Experimentalists. In these cases, when the blood has been examined after death, it has been found to be as dark as in other bodies! In one case, reported by Mr. Tweedie, this writer remarks:—“It is strange, that though 120 ounces (nearly a gallon) of fluid passed into the vein, the blood in the *right auricle* of the heart, and, indeed, everywhere else, should, nevertheless, have been so thick and dark as it was. This is the more remarkable, when it is remembered, that twelve of these ounces were thrown in during the last eight or ten minutes of life—in fact, while the patient was dying.”*

In two cases mentioned by Dr. Venables—in one of which the patient died ten minutes after the operation, while, in the other, he lived for some hours—the blood in the heart, lungs, and aorta, was also found in the same state—dark, firm and clotted.

It is thus apparent, that the black colour of the blood in Cholera is not the effect of the loss of the saline matter contained in it; nor could the introduction of a fresh quantity into the system change the morbid condition. The administration of the salts would therefore appear to be entirely useless during attacks of the Epidemic Cholera, merely for the purpose of changing the abnormal colour of the

* *Medical Gazette*: June 9th, 1832.

blood. But they have been resorted to for other purposes: Dr. Stevens concludes that they possess *specific* properties. In that case they ought, when administered, to give a better result than by any other, or ordinary, method of treatment. But the contrary has been the fact, as already shown. It may possibly be inferred that the salts, in these instances, were not employed in a proper way, or to a sufficient extent to ensure a successful result. It might also be imagined, that the cause of the failure, when the salts are taken into the stomach, is to be ascribed to the non-absorption of the remedy into the blood; being carried out of the system by the intestinal discharges. But no such inference can be drawn, when these agents have been injected into the veins; and when, in consequence of the quantity of fluid in which they were dissolved, they must have been intimately and completely mixed with the whole mass of the blood. And yet, as has been already shown, no beneficial effect is produced, while the colour of the blood remains unchanged. We have thus proof, independently of the practical results, that the salts of Dr. Stevens do not possess antidotal properties; for as the dark colour of the blood is merely an effect of the presence of a poison in the system, were that poison neutralised, or destroyed, the effect would cease, or be removed, as a natural consequence.

It is even doubtful if the salts possess a stimulating

property when introduced into the blood ; as the few recoveries that have taken place—9 or 10 per cent.—may possibly be referred as much to the warm fluid as to the salts contained in it. In fact, the injection of distilled water, in the hands of Dr. Laurie, produced reaction, the same as when the salts were introduced.*

Were it otherwise, or did the salts possess a stimulating property, no benefit would attend their administration, if employed alone. The state of collapse, like the black colour of the blood, is merely an effect of a particular cause—the presence of a poison in the blood. Unless therefore we were able to neutralise or expel the morbid matter, no permanent effect could follow the employment of a mere stimulus. As the salts of Dr. Stevens do not possess either the one property or the other, their administration would be useless in the collapse of Cholera ; even if we grant, what has not yet been shown, that these agents act as stimulants to the blood. In addition to these facts, we may also learn from others—opposite ones—that the neutral salts possess no remedial virtue. The alkalies, which Dr. Stevens tells us produce the very opposite effect on the blood to that of the salts, have been employed in the treatment of the Cholera, and with more benefit than by the administration of salines. Thus, Mr. Blackhall, of Exeter, treated the disease successfully with lime-water and milk, and the aromatic spirit of

* *Medical Gazette*, August 25th, 1832.

ammonia : in fact, as this writer remarks, most practitioners unite in applying alkaline remedies, in some form or other, to the cure of this disease.*

We thus learn from the preceding facts, that the theory of Dr. Stevens—medical as well as chemical—is an erroneous one. As such, we might have inferred, *à priori*, and irrespective of all practical results, that the treatment of the Epidemic Cholera, as deduced from that theory, would be inefficacious and useless. This granted, we can only infer that the success, which has sometimes attended the employment of the salts, is to be referred to some other agent, or some other cause,—an inference that has been already drawn, while considering the results obtained with the saline treatment.

The establishment of this fact affords us a very instructive lesson. We here find an author proposing a particular remedy for the cure of a violent and intractable malady, founded on a particular theory; and who, in his own practice, combines another agent with it—the action of which agent, so far from being indicated by his theory, is, on the contrary, actually opposed to it. According to Dr. Stevens's theory, carbonic acid not only blackens the blood, but its presence in this fluid prevents the action of the salts, and induces certain morbid changes.

“The carbonic,” observes this writer, “like all the

* *Lancet*, Vol. i., p. 176, 1832-3.

other acids, blackens the blood, and when this is added to the venous circulation, it not only darkens its colour, but *changes its properties*, and renders the fluid which contains it so impure, that it is totally incapable of supporting life." * And yet, the gas was resorted to by this theorist—not occasionally, but regularly, and in large quantities, *ad libitum*—in all the cases treated by him! Can it then be a matter of surprise that such false theories have been promulgated, and erroneous methods of treatment adopted; or that charges of empiricism have been so frequently brought against that art which ought to be placed on a sure and solid foundation? †

To those who can look back on the previous history, and review the facts connected with the adoption of the saline treatment, it must appear extraordinary that the light never once flashed across the mind of Dr. Stevens; and that he did not himself discover the cause of the discrepancies before referred to. This will appear the more singular, when we turn to certain statements of this writer respecting

* On the Blood, p. 38.

† "The spirit of pure theory," justly remarks one writer, "has been as powerful a narcotic to the science of medicine, as the spirit of barleycorn has been to the physical constitution of man. Under the intoxicating influence of theories, therapeutical knowledge has been diverted from the straightforward path which it is her duty to follow, and sent abroad, reeling, like a drunkard, through the sloughs and quagmires of error."—*Lancet*, August 25th, 1832.

the employment of the salts alone, and in combination with carbonic acid gas. When asked by Sir David Barry, "if he had ever seen *even one case* of blue collapsed Cholera saved, that had been treated by 20 grain doses of culinary salt, and by *nothing else*, he candidly confessed that he never had." *

On the other hand, he observes, while referring to a particular case in which his remedies had failed: "Every physician, who forces a strong saline fluid into an irritable stomach, at a time when it cannot be retained, must expect to be unsuccessful. . . . The result, however, would probably have been different, even in these cases, if he had first *quieted the stomach with saline effervescing draughts*, and then administered the stronger salts, when there was some chance of their doing good."

Had Dr. Stevens employed these agents merely for the purpose of relieving the vomiting, and only at one period of the disease, the case would have been different; but when he and others continued to administer them to the termination of the illness; and when he observed that these cases were successful, while in others, in which the salts were employed alone, the result was the reverse; it does seem to be a mystery, that this writer should have failed to ascertain the relative value of the salts and of the adjuvant.

* *Medical Gazette*, July, 21st, 1832.

It is also somewhat strange that Dr. Turley, after devoting so much time to the subject, failed to discover that he and Dr. Stevens had been employing an adjuvant not resorted to by many other practitioners ; and that this agent, so far from having any connection with the theory of Dr. Stevens, was, in fact, contra-indicated by that theory. Having, however, in the work published by me at that period, repeated the explanation that had been given in the first edition, of the cause of the success and of the failure in these instances, we have heard nothing more of Dr. Turley's intended work, or of any attempt to substantiate Dr. Stevens's theory.

Similar results and similar discrepancies were observed by the adoption of Dr. Ayre's method of treatment, with small doses of calomel. This treatment was adopted and recommended by Dr. Ayre in 1832, during the first visitation of Cholera in this country ; but it was not followed by other practitioners, to any extent, until 1848-9 ; at which time it was more particularly brought to the notice of the profession. Writing in 1832, we find Dr. Ayre making use of these strong and emphatic expressions : "Calomel, given in minute and frequently-repeated doses, is endowed with a property of controlling and arresting it (the disease), which no other medicine, or combination of medicine, has ever had assigned to it, or will, if I mistake not, be ever found to possess." *

* *Lancet*, Vol. ii., p. 461, 1831-2.

These conclusions were not only reiterated in 1848, but their soundness was admitted by other writers; while the success of his treatment was considered to be undoubted. "Since the present occurrence of Cholera in this country," remarked the Editor of the *Lancet*, referring to Dr. Ayre's method, "the same treatment has been put in force; and, as we learn, with signal and like success—*scarcely a case having been lost* under such treatment, if applied in proper time; and if the patient has not been previously plied with too many drugs."* The statistical facts adduced by Dr. Ayre appeared to confirm the truth of the opinions thus strongly expressed. In one of the communications of Dr. Ayre, it is stated that of 76 patients treated by him, in 1832, 16 died and 64 recovered, 5 being then under treatment.† This, if the latter cases be excluded, would give a mortality of only 15 per cent. We are told, in another communication, that, of 219 cases, the deaths amounted to 43, being a mortality of 19 per cent. No information has been afforded of the type of the disease, or the proportion of cases of collapse; but, allowing that they were all genuine cases of Cholera, the rate of mortality is a very low one.

In the following Table, this information has been afforded; while we also learn, that the success of the treatment, at this period, was greater than in 1832.

* *Lancet*, August 4th, 1849. † *Ibid*, Vol. ii., p. 451, 1831-2.

Table C. Return of Diarrhœa and Cholera cases, which came under the care of Dr. Ayre, and Messrs. Hay, Gibson, and Archbald, between the 11th July and the 6th August, 1849.*

	Cases.	Deaths.	Reco- veries.	Under Treatment.
Diarrhœa (premonitory),	149	None.	129	29
Incipient Cholera	47	„	44	2
Entire collapse	34	10	24	None.
Total Cholera	81			

The rate of mortality, deducting the cases of diarrhœa, is 12·34 per cent. ; and 29·41 for the cases of collapse.

After such results, and their publication, it cannot be a matter of surprise, that this mode of treatment came to be adopted by a considerable number of practitioners. But the wonder was great, when, after its adoption, the result, in numerous instances, appeared to be diametrically opposite to that previously obtained by Dr. Ayre. Thus, Dr. Favell states that “a patient, admitted into the Cholera hospital at Sheffield, on the 16th of August, 1832, took two grains of calomel every five minutes, from ten o’clock in the morning till five in the evening (amounting to

* *Lancet*, August 11th, 1849.

168 grains), without the slightest amendment in the symptoms. On the contrary, during the whole of that period he rapidly got worse. Moreover, he took the same quantity of calomel, at the same interval, till the 19th (three days), without the slightest apparent effect being produced by it." * According to this, the patient must have taken 1,600 grains of calomel—upwards of *three ounces!* "We tried," remarks another writer, "Dr. Ayre's plan—small and frequently repeated doses of calomel, with a small quantity of laudanum—in three bad cases with fatal results." † Dr. Hughes, also, speaking of this method of treatment, adds: "I believe that all, or, at any rate, almost all, the patients so treated have died. So common indeed has been the result, that, latterly, when, on entering the sick-room, I have seen some dozens of powders on the dressing-table; and have found the patient's tongue coated with white, as from so much chalk, or carbonate of lead, I have at length assumed that his fate was almost certainly sealed. I regret to express my belief, that I have been very rarely mistaken." ‡ And it is added: "The result, then, of my observation of the now general method of treating Cholera (almost solely by calomel) has been so mournful, so wofully depressing to my mind and spirits, that I feel assured I should fail in

* *Lancet*, Vol. i., p. 712, 1832-3.

† Report of the Belfast General Hospital.—*Medical Times*, Aug. 25, 1849.

‡ *Medical Gazette*, Sept. 21, 1849.

my duty to our common humanity, if I did not make it known to the professional public at large."

Dr. Hughes further states: "A medical friend and neighbour was induced to try the plan of small, frequently-repeated, doses of calomel. They were given, as he believes, constantly and regularly, and, as he truly observes, there is no art or mystery in their administration: nothing can be easier. They were tried in twelve consecutive cases. *All died*, and, he says, died more rapidly than ordinarily, although the patients were of varying ages, and in different stages of the complaint, when the treatment was commenced. Again: a gentleman, who, perhaps, in this present epidemic has seen as much or more of Cholera, in this sadly infected district, than any other person, reports: 'I have tried calomel, in large and repeated doses, in thirty cases, brought into the workhouse in a state of collapse. Not one of these recovered, and as to the small, frequently repeated, doses, I had tried them before, and found them equally inefficacious.' " *

Dr. Golding Bird, also, at a meeting of the Medical Society, London, remarked: "With respect to the calomel treatment—that by small and often repeated doses—he had seen much of it both at home and abroad: he had tried it himself carefully, assiduously, and constantly, and he had come to the conclusion, that it was of no benefit whatever. He could only reiterate the opinion of his colleague, Dr. Hughes." †

* *Loc Cit.*

† *Ibid*, Oct. 27, 1849.

It also appears that it does not check the progress of the disease, even when employed previously to the supervention of the stage of collapse. "So far as my experience goes," says Dr. Sandworth, "calomel alone will not, especially in small doses, restrain the rice-water discharges, and this is one of the capital defects in Dr. Ayre's plan. I have been called in to cases under that treatment, which were rapidly sinking into collapse, from an *unchecked* rice-water diarrhœa."*

As a natural consequence, these melancholy results gave rise to a feeling in the mind of the Profession, either that the cases of Dr. Ayre were mild ones, or, else, that the character of the epidemic was different in Hull to that observed elsewhere. This was the opinion expressed by Dr. Bird at the meeting before referred to. He observed:—"With respect to Dr. Ayre's treatment, if that treatment is successful in Hull, the diseases of Hull and of London are very different things. I never saw it do one particle of good. Some, indeed, have recovered, but they rather got well in spite of the remedy than in consequence of it." Dr. Hughes also added: "There must be a mistake somewhere; either the disease is much more virulent and less controllable here than it is in Hull, and in other places, in which the small doses of calomel are found to be so wonderfully efficacious, or

* *Lancet*, Oct. 27, 1849.

there must be some other source of fallacy to be discovered.”*

That the failures elsewhere could not be ascribed to a variation in the type of the disease may be inferred from the fact, that precisely the same unsuccessful results were observed in Hull. Dr. Ayre having stated, “that no one in Hull had ever abandoned his mode of treatment, after once employing it, or failed to become converts to the practice, when once they had attended in it with him,” fourteen practitioners in that town signed a letter, which was inserted in the *Hull Packet*, to the following effect: “We, the undersigned, beg leave to dissent from the truthfulness of it (the above assertion), so far as relates to ourselves.” † Dr. Horner, a Physician at Hull, stated in addition, that “when the Cholera visited Hull, in 1832, he requested Dr. Ayre to let him see his success in the first twelve cases he was called to. Speedily, I was summoned to four, which were treated by Dr. Ayre himself; three of these died, and I was never asked to any others! . . . At that time Dr.

* Dr. Favell, of Sheffield, while referring to the treatment of Cholera by calomel, remarks: “I cannot account for the wonderful discrepancies which we observe in the statements of different Medical Practitioners, except on the supposition that the cases which they treated were not identical. If calomel cures Cholera at Hull, how is it that it does not at Sheffield?”—*Lancet*, Vol. i., p. 710, 1832-3.

† *Lancet*, October 6th, 1849.

Ayre professed, that he cured all his patients."* That he did not do so can be shown by other facts.

In 1849, Dr. Ayre was appointed Physician to the Sanitary Committee of the Board of Guardians of the town of Hull—Messrs. Day, Gibson, and Archbold being associated with him. "Our duty," remarked Dr. Ayre, "was to take charge of *all the cases among the poor* of this large town, with above 50,000 inhabitants." And he added: "As the official attendants of the poor, and having been widely advertised as such, my colleagues and myself have necessarily had *the greater number* of the patients attacked."† Such being the case, we might have inferred, if the treatment of Dr. Ayre were so efficacious, that the mortality in this town would have been below the average. But the contrary is the fact. Dr. Sandworth informs us, that there were in Hull *in one week*, in September, 1849, nearly *double* the number of deaths which occurred during the entire period of its visitation in 1832."‡ According to one of the local papers, there were about 1,200 fatal cases of Cholera in Hull in the space of two months; "very nearly four times the number of the deaths which occurred in 1832, from the same disease." "This," observes the Editor of the *Medical Gazette*, "shows that the treatment by small doses of calomel, if resorted to, has very little effect in staying the ravages of the disease."

* *Lancet*, October 16th, 1849.

† *Ibid.*

‡ *Ibid.*, October 29th, 1849.

We are also informed by a Practitioner in Hull, that these deaths occurred *in spite of the treatment* by small doses of calomel; *not* as a consequence of its non-adoption. "It is well known," remarked Mr. Sleight, "that the mortality in this town has been exceedingly great; under the calomel treatment, it is thought that there has not been a recovery of one case in twenty."*

With these facts before us, it is impossible to infer that the discrepancies remarked in the treatment of the Asiatic Cholera, by small doses of calomel, are to be ascribed to a variation in the type of the disease. Nor can they be referred to the mild character of the cases treated by Dr. Ayre; the statistics furnished in the table previously inserted (table C. page 172) negative such a conclusion. It is there apparent, that of the seventy-one patients treated by Dr. Ayre, thirty-four were cases of collapse—rather a large proportion. We must therefore seek for another, and a different, explanation. As it is impossible to suppose that there was any wilful misrepresentation of facts, or that the member of a God-like profession could ever, in a question of life and death, pervert the truth, merely to prop up a favourite theory, we cannot err much in inferring, that Dr. Ayre, like Dr. Stevens, has been himself deceived. We may even go a step further, and conclude, that the cause of the deception was the same—that Dr. Ayre had been employing, in

* *Lancet*, October 6th, 1849.

combination with his favourite remedy, some adjuvant, to which the success is to be ascribed. A rigid inquiry into the history of the cases will show that this was actually the fact.

Although Dr. Ayre states, that calomel "is, at once, a medicine that claims, not merely to be united with others in the treatment of, but to be relied on as a specific in, this disease ; and to be employed to the exclusion of all others," we are yet informed by Mr. Ross, that Dr. Ayre allowed his patients to take as much cold water as they desired.* Dr. Ayre also stated, that "no inconvenience appeared to arise from an *unrestrained* allowance of cold water."† Now water, as previously shown, is sometimes a valuable agent, given in large quantities, during the stage of collapse—at that period when the calomel, according to Dr. Ayre's method, is principally administered.

But as this mode of treatment requires to be continued, regularly and continuously, for a long period ; and as the pills, if the vomiting were kept up, could not be retained ; the employment of water alone will not account for all the recoveries, although it may have acted beneficially, as an adjuvant, in many cases. Besides, there were forty-four recoveries from incipient Cholera—cases to which this mode of treatment is not applicable, for the reasons previously stated. These must also be accounted for, and in a different way.

* *Medical Times*, November 18th, 1848.

† *Lancet*, Vol. ii., p. 460, 1831-2.

The natural question then is, did Dr. Ayre make use of any other remedy, or any other adjuvant? The reply to this question is, that he did.*

According to the detail of two cases, reported by Dr. Ayre, it appears that effervescent salines were administered in combination with the calomel. In the first, treated by his friend Dr. Chalmers, of Hull, "*precisely in accordance,*" adds Dr. Ayre, "*with the principles and practice which I have advocated in this letter,*" the calomel was given every half hour, with a *saline effervescing draught*, containing three drops of tinct. opii. In the next, attended by Dr. Ayre, in conjunction with Mr. Sharpe, the calomel was given without any opium, but "with an *effervescing draught*, and a teaspoonful of brandy, *every half hour.*" This case, as we are informed by Dr. Ayre, was one of the severest kind; but, "being treated *according to the mode already adverted to* (by him), was cured with a facility which greatly surprised the gentleman who saw the case with me."† Effervescent salines were not administered by Dr. Ayre alone; we have proof that they were resorted to by other Practitioners in Hull. Dr. Horner states, that "even the medical gentle-

* Dr. Ayre states, "*I scarcely used a single auxiliary means of any kind. I neither bled, nor gave stimulants, nor used the air baths, or frictions, except to relieve the cramps; nor did I resort to any but the ordinary means for supporting the temperature or strength of the system.*"—*Lancet*, October 28th, 1848.

† *Lancet*, Vol. ii., p. 110 and 273, 1831-32.

men, who are associated with Dr. Ayre on the Board of Guardians here, no longer, as two of them informed me, give it (the calomel) as he did; whilst some, if not all of them, *give salines in conjunction with it*. Not that they had so much faith in the salines, but that so many had died, when taking the pills, that *the poor absolutely refused them, if offered alone.*"*

This method of treating the disease was not confined to Hull; the same combination, and the same adjuvant have been resorted to in other localities, and in other instances. Thus, Mr. O'Shea, of Lambeth, states, that he found the treatment of Dr. Ayre beneficial, and then adds: "The calomel must be given alone, to the utter exclusion of stimulants, especially alcoholic; and only admitting occasionally, during its use, *an agreeable effervescent* of citric acid and soda, with syrup of oranges."† In a number of the same Journal (September 15th, 1849), a case of Cholera is reported by Mr. Pickop, of Blackburn, "as proving the power and efficacy of calomel in subduing Cholera in its worst form, *when administered as recommended by Dr. Ayre.*"

* *Lancet*, October 6th, 1849.

† Not having any theory to support, Mr. O'Shea is not quite so much to blame as Dr. Stevens. Yet, as he tells us in one sentence, that the calomel is to be given alone, and in the next, that an effervescent draught is to be added to it, the gas, it is apparent, with Mr. O'Shea and the majority of Practitioners goes for nothing.

The patient, when first seen (September 1st), was complaining of severe pain in the bowels and legs; haggard expression of countenance; voice almost inaudible; pulse very feeble; cold, livid, condition of the skin; extreme thirst, with incessant vomiting and purging of the peculiar rice-water evacuations. He was ordered a grain of calomel every five minutes: simple *effervescent draughts*, with three drops of tinct. opii, *every hour*; toast and water and barley water *ad libitum*. He began to rally nine hours after the commencement of the treatment; but reaction was not fully established until the 2nd, when he passed urine for the first time. On the 3rd he had two bilious motions, and, on the 5th, he was well enough to return home. "I have also," continues Mr. Pickop, "given calomel in grain doses in three other cases, of less severity, every fifteen minutes, with *simple effervescing draughts* every hour, containing two minims of tincture of opium; and, in each case, the vomiting and purging have been arrested, *as if under the influence of a charm!*" In the treatment of the Cholera at Cawood, near Selby, by Messrs. Weightman and Clarke, "soda water, solution of soda, and effervescing draughts were combined with sudorifics, antispasmodics, and calomel in small doses." Of eighty-seven cases treated, the deaths amounted to seventeen—being a ratio of mortality of nineteen per cent. only.*

* *Lancet*, Vol. i., p. 82, 1831-2.

Dr. Niddrie, Surgeon, R.N., states that, in 1832, he was attached to the Dover Cholera Hospital ship; at which time the disease proved to be so unmanageable that all treatment appeared to be equally useless. He adds, however, that more extended experience, subsequently, led him to observe the vast value of *calomel and carbonic acid*. "I have witnessed," he adds, "the failure of a long list of medicines, and of much variety of treatment. Stimulants, given to the extent of a bottle of brandy in 24 hours: extreme doses of carbonate of ammonia: the homœopathic use of strychnine and other medicines: cold water dashed on the body; bleeding during collapse—all proved equally useless, or injurious, and I now feel assured, that *calomel and carbonic acid are the only remedies to be trusted to.*"* In a subsequent communication, Dr. Niddrie observes: "Under this treatment, purging ceases, or becomes less frequent, although vomiting may occasionally recur for some time: the expression of the countenance gradually changes, the temperature of the breath and tongue rises; the pulse gets stronger, and the skin warmer; cramp becomes slight, and extreme collapse is followed by little or no febrile excitement.† ‡

The above are the principal instances, that I have

* *Lancet*, Feb. 10th, 1849.

† *Ibid*, Sept. 1st, 1849.

‡ These results, it may be added, are precisely the same as those described by the Spanish physicians; while they read as if copied almost *verbatim* from them.

met with, of the employment of carbonic acid in combination with the remedy of Dr. Ayre. There is no doubt, however, that both soda water and effervescing draughts were employed in many of the other cases reported—in all, probably, that proved successful—without the fact itself being stated. This was the case in one of the metropolitan hospitals that I visited in 1849; and in which the calomel treatment was said to have proved highly successful. On entering the ward, I saw the nurse giving a pill to one of the patients; and, immediately after, a bottle of soda water. On asking, if this was the regular practice, and being answered in the affirmative, I needed no further evidence to be assured, that this treatment would, and must, prove successful. As the dose was repeated every quarter of an hour, a considerable quantity of the gas would necessarily be taken.

Why the gas was added in these instances has not been stated: the reason assigned by Dr. Horner, for its employment in Hull, would not apply to other places and other cases. We may presume that it was given to lessen the irritability of the stomach, and thus enable it to retain the pill, which would otherwise have been rejected.

That the calomel alone would not, and could not, cure the disease has been previously shown, while discussing the theory of Dr. Ayre, in a previous chapter. An attempt was then made to prove, that the theory of this writer is false, and the practice deduced from

it irrational, unscientific, and erroneous. This is not my opinion alone: it is that of others also.

“Although some persons,” observes Dr. Favell, “have hastily asserted, that the disease depends on a want of secretion of bile, we have the most direct evidence to show, that it does not owe its origin to any such cause. Two facts prove this: first, in many instances of diseased liver we have no bile secreted, although (and yet!) we do not observe symptoms in any degree resembling those of the disease in question; and, secondly, we have the most direct evidence on *post-mortem* examinations, that the secretion of bile has been by no means deficient.”* The gall-bladder, in fact, is frequently found full of bile, and with patients, too, who have not taken a grain of calomel.

With these facts before us, we are bound to conclude that the benefit derived from the administration of calomel is to be referred, in the majority of instances, to the employment of carbonic acid gas as an adjuvant. Water, if taken in sufficient quantity to produce emesis, and at the proper time, may have been the curative agent in some instances; but it could have had no influence in those cases in which the gas was administered at the same time. The reason is, that the gas almost invariably arrests the vomiting, at the first or second dose: as such, the irritability of the stomach being relieved, water can

* *Lancet*, Vol. i., p. 710, 1832-3.

be taken in almost any quantity without being rejected. As it is only when acting as an emetic, that the exhibition of water can be said to prove beneficial, or curative, the whole of the benefit must be ascribed to the gas, whenever this agent is employed.

In addition to the above, we shall find, if we carry this inquiry farther, that the same result has been obtained, whenever carbonic acid gas was added to other remedies, no matter what these remedies may have been. Thus Mr. Beckett, surgeon to the Hull and Sulcoates Dispensary, after remarking that he had valuable opportunities of testing those therapeutic agents which are most frequently relied on in this disease, adds, that he divided it (the disease) into three periods—viz., the premonitory diarrhœa, incomplete and complete collapse. “In the former, he gave the acetate of lead and opium (remedies which have been previously shown to be so injurious in attacks of Asiatic Cholera); combining them with *effervescent draughts*, when the stomach was too irritable to retain the pill.” In the next variety, Mr. Beckett found, he tells us, “more advantage from another plan, viz., *small doses of calomel* and opium, with *an aromatic effervescent* often repeated—say twice or thrice an hour. This,” he adds, “I have found to suit admirably a large proportion of anxious cases, where frequent vomiting and pale, flocculent dejections are rapidly prostrating the patient.” *

* *Lancet*, September, 22d, 1849.

Numerous examples of the same kind might be adduced, but the preceding will suffice—especially as they are so abundant and so complete. That the benefit derived, in all the instances that have been quoted, is to be referred to the gas, we are bound to conclude, not only from the facts that have been adduced, but for another and substantial reason. If we regard the results obtained, or, rather, the effects produced, when the calomel has been combined with the gas; and then compare them with those described in the previous chapter, when the carbonic acid was alone employed; it will be found that they agree exactly. Hence, in addition to the failure of the calomel when employed alone, we have direct proof that the results observed are to be ascribed wholly and solely to the adjuvant.

We can now explain what before appeared a mystery, viz., that both Dr. Stevens and Dr. Ayre should persist in calling their remedies specifics, in the face of all the facts adduced by others in opposition to the conclusion. It was the adjuvant, not the remedy, that possessed these specific properties. How it happened, that this discovery was not made by these authors is somewhat enigmatical; and can only be explained by the fact, that carbonic acid has not hitherto been considered to possess any remedial virtues; except that of allaying the irritability of the stomach, in cases of nausea and sickness. Still, when it was found that effervescent draughts removed one

of the effects of the disease, and that a very troublesome one—the vomiting, it would not have been very illogical to have referred the subsidence of the other effects of the disease to the influence of the same agent. That this was not done will appear the more singular, as a similar inference was drawn by certain individuals whom I addressed, recommending a trial of effervescent draughts, and extolling their virtues. In these instances, I was met with the objection, that the benefit in all probability, was to be ascribed to the neutral salt, combined with the gas, and not to the gas. Such an objection might have held good, had effervescent draughts been alone employed; but when it had been shown that the simple solution of the gas in water—uncombined with any salt—produced precisely the same result, the objection fell to the ground. Added to this, it had been proved that the neutral salts, administered alone, were actually useless, if not injurious.

The intellectual blindness of these writers will also appear the more singular, when it is remembered, that carbonic acid gas had been previously recommended by me as a remedy, and an antidote, for the Asiatic Cholera. It is very possible, that Dr. Stevens and Dr. Ayre were in ignorance of this fact in 1832; but this could hardly have been the case in 1849—two editions of my work having been then published. In both, an allusion was made to the discrepancy arising from the employment of salines, and an ex-

planation offered of the cause. Had these writers taken the trouble to investigate the subject, and to analyse the facts, they would probably have been led to see the fallacy of their theory, and of the practice emanating from it. They might then, perhaps, have ceased to recommend the employment of agents which, in the hands of numerous practitioners, led only to failure, and to the loss of many valuable lives.

Although the evidence thus adduced is only indirect, it is perhaps more valuable, in some respects, than direct evidence. By the employment of these agents, what is termed a *crucial experiment* has been made—one that it would be impossible to make directly. For instance, we could not be so inhuman as to administer the gas to 10, 20, or 50 patients, and then leave the same number without any medicine, in order to ascertain what difference there would be in the result. And yet, this is precisely what has been done, although unintentionally, by those who have adopted the two methods of treatment now under consideration. They have, in one case, administered remedies that had been proved to be useless, or worse than useless—the effect of which would be the same as though no medicine had been taken; while, in other instances, they have given the same agents in combination with carbonic acid—the result, as a matter of course, being precisely the same as if the gas had been administered alone.

By these experiments, another question has been

elucidated and cleared up. It has been insinuated by certain critics, when referring to the employment of carbonic acid gas, that the patients, in these instances, might have recovered by the remedial powers of Nature; although such a conclusion is in opposition to all the facts with which we are acquainted. Here, however, we have seen that when the patients took carbonic acid gas the majority recovered: when they did not, the majority, or the whole of them, as invariably died. We are thus enabled to ascertain, in the most satisfactory and conclusive way, what the real value of the agent is; at the same time that evidence is afforded of the folly of trusting to the powers of Nature, for the cure of this most rebellious of all diseases. Had it been necessary, we might have rested in perfect confidence, and without any fear of error, or deception, on the evidence that has been thus adduced—although that evidence is indirect, not direct. Fortunately, this is not necessary; as a considerable amount of direct and valuable evidence has been already furnished; while still more will be given in the following chapters.

CHAPTER VI.

TREATMENT OF THE EPIDEMIC AT ST. BARTHOLOMEW'S HOSPITAL.

No other, or direct, evidence had been obtained by me up to the time of the breaking out of Cholera in England, in 1849. Not being then in practice, and being anxious to have a public opportunity of showing the value of the agents now under discussion, I addressed a letter to the heads of Her Majesty's Government, asking for this opportunity, and, at the same time, for the appointment of a Commission to report on the result. In answer to this application, I was referred by the Prime Minister to the Board of Health; but the Board, when written to, informed me that the treatment of the disease did not fall within their province, or jurisdiction. The Home Secretary also referred me to the same Board, although the disease was then prevailing to a considerable extent in some of the jails, and it was, soon after, thought advisable to incur the expense of removing the prisoners to other localities.

The Poor Law Commissioner, also, when applied to, considered that he could not interfere with the

Board of Guardians; and the latter were not disposed to permit clinical experiments to be made in the unions, or workhouses, by a stranger; while those who held the appointments feared, or hesitated, to give up their patients to one not officially appointed, without the sanction of their superiors.

Thus the matter ended, as far, at least, as these parties were concerned, by verifying the old adage, that what is everybody's business is nobody's. As wards had been opened at a few of the metropolitan hospitals for the reception of cholera patients, I applied to the physicians at St. Bartholomew's—my *alma mater*—for permission to treat some of the cases admitted there. This, I was informed, it was not in their power to grant; but they offered to try the remedies themselves, in order to ascertain their value. As the trial would be, to a certain extent, a public one, I accepted the offer; thinking it preferable to the seeking out private cases, which, although satisfactory to myself, would have been useless, or nearly so, to the profession.

By the arrangement at this hospital, the three physicians took charge of the cholera wards by alternate weeks; and it so happened, that Dr. Roupel, whose week it then was, had already employed the carbonic acid gas—in the form of effervescing draughts—not exclusively, but in conjunction with other agents. As Dr. Roupel appeared to think that it was unnecessary to do more than continue

the same mode of treatment, although the carbonic acid was not administered precisely in the manner and to the extent recommended by me. The treatment adopted in the majority of the cases was as follows:—The patient, on admission, was put into a warm bath, after which he took an emetic of sulphate of zinc. When the effect of this had passed off, effervescing draughts and soda water were administered. Having only visited the hospital towards the termination of the week, I was unable to take notes of the cases, and must therefore content myself with giving the general result of the treatment.

During this week, from August 2nd to August 9th, there were 39 patients admitted: of these, 9 died and 30 recovered—being a ratio of mortality of 23 per cent.

In the following week, from August 9th to August 16th, the patients were under the care of Dr. Burrows, who not only expressed his willingness to give the remedies a trial, but who, at the same time, appeared anxious to carry out my wishes and intentions. Notwithstanding this, there were certain circumstances, as will presently appear, that prevented the result being as favourable as would otherwise have been the case.

In this week, 52 patients were admitted, of whom 30 recovered, and 22 died—the rate of mortality consequently being 42·30 per cent. But 9 of these must be excluded by me, as not having taken the antidote at all, or, else, as having taken it in

a manner, and to an extent, that rendered its employment useless. These cases being excluded, the rate is reduced to 30·23 per cent.

Of the cases excluded, two may be said to have had no treatment, as they only took an emetic and had a warm bath. In four of the others, an agent was resorted to that not only interfered with the administration of other remedies, but which, in my opinion, produces an injurious effect in attacks of the epidemic cholera. I allude to the inhalation of chloroform. As a matter of course, the administration of other remedies is abandoned, not only while the patient is under the influence of the anæsthetic agent, but for some time after. Now, most of these patients inhaled the chloroform two or three times, and one, no less than eight times. It is evident, therefore, that there could be no time for the administration of effervescing draughts, although ordered to be taken.

Independently of these reasons, chloroform, when inhaled, acts by depressing the whole nervous system—cerebro-spinal and ganglionic—producing, at the same time, a suspension of the cerebral functions, on which its peculiar efficacy as an anæsthetic agent depends.

According to Mr. Lamprey, chloroform was tried, on two occasions, in Berlin. “The first case derived a temporary benefit from it, as it completely subdued the spasms, and afforded tranquillity for about three hours; but its effects were not permanent, as these symptoms were renewed as soon as its

effects were over. From the partial success in one case it was tried in another ; but, unfortunately, immediate death was the consequence.* Although the same result, immediate death, did not occur with any of the patients to whom it was administered at St. Bartholomew's, chloroform, in my opinion, acted injuriously in all the cases ; while it failed to effect the object contemplated—the relief of the spasms. This will be apparent by the following extract from one patient's card, that of David Heban :—

“ 1.30 p.m. : Cramps very violent still : has had chloroform *six* times with only temporary relief.” It was again inhaled at 3.30 and 6.15 p.m., but “ although the patient soon got under its influence, and remained so a quarter of an hour, it only produced *insensibility* to the cramps, *did not relieve them.*” Again, in another case, it is said : “ the chloroform gave temporary relief, but the cramps are now as severe as ever.”

In addition to the primary effect of chloroform, or its action on the brain and spinal marrow, there is a secondary effect produced, very similar to that observed during attacks of the Asiatic Cholera. Mr. Nunnerly, who has made a great many experiments, in order to ascertain the effect of anæsthetic agents when introduced into the animal system, states that after recovery from apparent death, “ there is always a loss of heat, the vital powers are evidently depressed, and the animals are all cold, and for a time appear shrunk.” A lady, who had inhaled chloro-

* *Medical Times*, February 3, 1849.

form for a spasmodic affection, was found by Dr. Jackson with a feeble pulse and with diminished temperature of the extremities. "She remained," adds this writer, "cold and nearly *pulseless* for forty-eight hours."* Mr. Nunn mentions the case of a lady who inhaled the vapour of æther, and who died on the following day.† She recovered her senses, but did not rally after the operation. "In addition to numbness in the feet and legs, there was *suspension of all the secretions*"—the very state that forms the characteristic phenomenon of the epidemic Cholera. If such are the effects of the inhalation of chloroform, and other anæsthetic agents, with persons whose nervous systems were not previously in a state of depression, it cannot excite surprise that the same operation should be productive of injurious and fatal results in attacks of Asiatic Cholera. Suspend the functions of the cerebro-spinal system, which alone remain intact, and the individual must perish.

As regards the remaining cases, they have been excluded, in consequence either of the short period which the patient lived, after his admission, or else, of the long intervals at which the remedy was given; so that the patient, instead of taking three or four draughts every hour, only took the same number between the time of his admission and his death. No benefit could have been expected from such an agent

* American Journal of the Medical Sciences, 1849.

† *Medical Gazette*, vol. 39, p. 738.

under such circumstances: in the stage of collapse it would be perfectly useless.*

Similar causes were in operation, although not to the same extent, in the majority of the other cases—those not excluded by me. For instance, the draughts were given at too long intervals, generally every two hours; although the objection to this manner of giving the remedy was obviated, to a certain extent, and in some of the cases, by the use of soda water. But, then, as this drink was employed to relieve the thirst, or to check the vomiting; and as these symptoms are not present at all periods, and admit of relief by the employment of these very agents; while even the use of soda water was dependent on the caprice and inclination of the nurse and the patient, this substitute for the effervescing draughts could not always be depended on.

In addition to these circumstances, all the patients had a warm-bath on admission, and then an emetic: after the operation of which, and *not until then*, the effervescing draughts were commenced. Without entering into the question of the value of the bath and the emetic, it is sufficient to add, that their employment necessarily occasioned the loss of much time in the exhibition of the draughts. How injurious

* Dr. Niddrie, whose evidence as to the efficacy of carbonic acid has been previously quoted, observes: "Calomel and carbonic acid seem to be as powerless as other means, unless thrown constantly into the stomach."—*Lancet*, Sept. 1, 1849.

this loss was must be evident from the arguments advanced in a previous chapter, while referring to the *modus operandi*, and the effects of carbonic acid—particularly when it is added that the majority of the patients either came into the hospital in a state of collapse, or fell into that state shortly after. Under these circumstances, we could hardly expect that the result would be a very favourable one, if the success of the treatment depended on the employment of carbonic acid gas.

The following history will afford a good illustration of the plan of treatment pursued in the majority of the cases :

James Summers, aged 50, admitted August 9th, 1849. 8 a.m. History : “ was found lying on the floor of his room this morning, having been attacked the day before with diarrhœa, which was followed, in the night, with vomiting and severe cramps.” On admission, the following were the symptoms :—“ Countenance sunken and dusky : eyes sunken : skin moist and warm : tongue clean, moist and cool : complains of great thirst : pulse 34, very soft : is constantly vomiting : bowels open continually : passed urine last night, none since. Ordered, a warm bath. Mustard poultice and an emetic. 1 p.m. Effervescing draughts and soda water, *pro potu*. August 10th. Is much better, but has not yet passed any urine.” On the following day, the 11th, some urine was voided, and he then gradually improved, was convalescent on the 14th, and discharged, quite well, on the 16th.

Remarks :—As is apparent, there was an interval of *five*

hours before the effervescing draughts were administered—a delay that would have been fatal had the symptoms been more severe, or the collapse complete. “In Sylhet, in 1818, if nothing curative was attempted for an hour, the case was then considered hopeless.”—*Bengal Report*.

This is not all. Mr. Wood, the apothecary to the hospital, under whose charge the patients were in the absence of Dr. Burrows, who only visited the hospital once daily, had, unfortunately for me, prepossessions on the subject of treatment. One was, that the effervescing draughts increased the sickness; and the other, that the patients would recover as well without medicine as with. These opinions were not only prejudicial to me—for the draughts were sometimes ordered to be discontinued, in consequence—but they were erroneous in fact.

The effervescent draughts, administered at that period when the vomiting is most severe, were, no doubt, sometimes rejected, in common with all other liquids; but, then, this arose, not from any property inherent in the draught, but simply because the gas had not been given in sufficient quantity, or repeated often enough, to allay the irritability of the stomach. The facts previously adduced will prove this.

It has been, in fact, principally on account of their power in allaying the irritability of the stomach that effervescent draughts have been resorted to by so many practitioners. “Soda and effervescent draughts,” says one writer, “we gave largely, and

with benefit, *checking vomiting, and allaying the urgent thirst, better than any other remedy besides*: they did not in any degree aggravate the diarrhœa.* It will also be seen, presently, that the draughts were employed in this very hospital, at a later period, to allay the vomiting when all other means had failed. As to the other conclusion, that patients would recover without medicine, it is in opposition to all the experience that has been acquired up to the present time: whilst its fallacy was also shown in the wards of St. Bartholomew's, as will appear hereafter.

As Dr. Burrows's time was necessarily limited at his daily visits, and as I hesitated to discuss such matters in the ward—before the pupils and the patients—I wrote to him pointing out the preceding errors in the employment of my remedies. In his answer, he observed: “Pray be assured that I should, most readily, have given your suggestions, upon the treatment of Cholera, a further trial, had the opportunity offered; but, as is my usual custom, I resign the care of the patients within the hospital to my Assistant Physician, for three weeks, commencing from to-morrow. I trust it may please God to have removed those causes, predisposing to this malignant disease, within the metropolis, ere I resume my duties in the last week of September. I fear there will be great difficulty, at all times, in carrying out your

* Report of the Belfast General Hospital. — *Medical Times*, August 25th, 1849.

views in the treatment of the *advanced* stages of Cholera in the wards of St. Bartholomew's Hospital. Most of our patients are of the lowest class; many are stupid, and very many dislike the effervescing form of draught so much, that they do not, as you justly state, imbibe any considerable quantities of the gas." "You suggest giving the gas in larger quantities at the commencement of the attack, but, if you inquire into the histories of our cases at St. Bartholomew's, you will find the majority came in at advanced stages of the disease, and scarcely any at the commencement."

Two observations in this note require an answer. Dr. Burrows states that many of the patients dislike the draughts—an opinion entirely in opposition to my own experience, and to that of other practitioners. With the exception of children and a few ignorant persons, who have never seen an effervescing draught, the patients are ready and anxious to take them—especially after the exhibition of the first draught. The cry of the patients at Mataro, according to the testimony of Dr. Pascual, was: "Give me more of this draught, for it restores me to life." Dr. Burrows also observes: "You suggest giving the remedy at the commencement of the attack;" and then adds, very properly, that this is impossible in hospital practice. What I said, or, at least, meant to say, was, that the remedy should have been given at *the commencement of the treatment*, or, on the admission of

the patient, and before, not after, the bath and the emetic. These errors, which were of some importance, would, no doubt, have been corrected, had Dr. Burrows resumed his charge of the Cholera wards: and, it was, therefore, with much regret that I learnt he had transferred his duties to Dr. Black.

Dr. Hue, who took charge of the patients after Dr. Burrows,—that is to say, from the 16th to the 23d August,—agreed to continue the same treatment. The remedies were given in the same way as before, except that the animal charcoal was used in enema, instead of the vegetable, to which latter I should always give the preference. All the patients had a warm bath on admission, but the emetic was omitted, in the majority of the cases—a mustard cataplasm being applied to the region of the stomach instead. This change was an advantage, for the reasons already stated.

Of 26 patients thus treated, 20 recovered and 6 died—being a mortality of 23 per cent.—the same ratio as that obtained by Dr. Roupel.

Two of these cases were claimed by Mr. Wood, or, rather, by Dame Nature, no medicine having been ordered for them; and it being considered that they had recovered by the unaided efforts of nature. These patients were pointed out to Dr. Hue and myself by Mr. Wood; and it so happened that, as I was passing through the ward, shortly after, I saw the nurse giving some soda-water to one of these patients—

William Haring, a lad of 12 years of age. On my remarking to her that the soda-water was not put down on the card, she replied, "No; but as medicine had not been ordered, she had given him the soda-water *to allay the vomiting* and to relieve the thirst." On my asking her, if he had taken any before, she replied: "Oh, yes! ever since he came in." It also appeared that the other patient had taken, not only soda-water, but effervescing draughts as well. Instead, therefore, of giving the credit of the cure, in these instances, to Dame Nature, it must be referred to another doctress or agent.

That this inference is a correct one may be inferred from what occurred with another patient, for whom no medicine was ordered, and to whom neither soda-water nor effervescing draughts were given. As they are instructive, the following are the particulars of this patient's case, as copied from the card:—

Augt. 18th.—William Elliott. "Was attacked, early this morning, with slight sickness and purging; but *there is no sickness now, only diarrhœa*: no cramps. On admission, the pulse was 187 and full: the skin warm; the countenance good; the evacuations brown, and the urine voided as usual.

"Ordered a warm bath. Was sick while in the bath.

"*Augt. 19th.*—Seems quite comfortable this morning, and only complains of slight diarrhœa.

"*21st.*—*Convalescent.*"

On my visit to the hospital the following day, at 2 p.m.,

I found this patient in a state of complete collapse, and learned, that he had been attacked in the middle of the night with coldness, which was speedily followed by the other symptoms characteristic of collapse. From this he never rallied. Although one of the few cases that were admitted in the first stage of the disease; and although three days elapsed before the fatal attack came on—an interval sufficient to have cured three such cases by appropriate treatment—it is evident that rest, diet, nursing, and nature alone will never cure an attack of Asiatic Cholera.

Augt. 23d to Augt. 30th.—It now came to Dr. Roupel's turn to take charge of the patients, or, rather, of the new admissions; as each physician continued to attend the patients first admitted by him, until their discharge or death.

This week a modification was made in the treatment. A large proportion of the patients took the nitro-muriatic acid, first recommended by the late Dr. Hope, of Chatham. The result was very unsatisfactory; the acid appearing to keep up the irritability of the stomach, and to increase the sickness; while the majority of the patients, so treated, died. Several were evidently saved by the discontinuance of the acid, and the substitution of effervescing draught. Others, again, especially towards the end of the week, were treated in the same manner as during Dr. Roupel's former week; viz, by the employment of the warm bath, and an emetic, and then by the exhibition of effervescing draughts. I am unable to say, what the

exact proportion was of those patients to whom the effervescing draughts were administered in combination with the acid; but I have the notes of twelve cases, in which the effervescing draughts were taken without any other medicine. Of these, two died and ten recovered.

This week, there were 47 admissions, 24 deaths, and 23 recoveries—the rate of mortality being 51 per cent. Deducting the 12 cases treated with the carbonic acid alone, the ratio would be 63 per cent.: while it only amounts to 16 with those to whom the gas was administered—a difference of 47 in 100. The preceding rate would have been still higher, if those cases had been deducted that recovered, as I infer, solely by the substitution of carbonic acid for the nitro-muriatic acid. Of such cases, the subjoined will afford a good illustration;

Charles Edwards, æt. 22, was admitted into the Cholera wards, on the 26th August, in the first stage of the disease. Ordered a warm bath; an emetic, a mustard-poultice, and the nitro-muriatic acid, a dose every hour.

27th.—“The vomiting has continued incessant until now.” A blister was ordered to be applied to the abdomen, and an enema of starch, with thirty drops of opium, to be given.

30th.—On my visit this day, the following note was made by me:—“Soda water and effervescent draughts were ordered for this patient on the 27th, since which, the vomiting and all the other symptoms have disappeared.”

In the next case, in which the preliminary treatment was different, the result was precisely the same.

Charlotte Bruce, æt. 35, admitted August 23rd, having had vomiting and purging for the previous ten days. Ordered, a warm bath; a mustard cataplasm; an emetic; and an enema of broth; and, in addition, Pil. Hydr. c Cret. gr. x, nocte.

24th.—A blister was ordered to-day, and the mist. cretæ, with ten drops of the tinct. opii, three or four times a day.

25th.—“The sickness has continued incessant until now.” The other medicine was discontinued, and effervescent draughts ordered instead.

27th.—The sickness has entirely ceased.

29th.—Convalescent.

Remark:—It is thus apparent that when all other means failed, the effervescent draughts invariably relieved the sickness: although they had been previously accused of producing the very effect, to relieve which they were now had recourse to. So much for prejudice!

In the next week, from August 29th to September 6th, Dr. Black took charge of the wards, in the absence of Dr. Burrows, as previously mentioned. I had no communication with this gentleman respecting my remedies, but found, on my visit to the hospital, a few days after, that the plan of treatment adopted was what the French call “*la médecine expectante*”—the do-nothing system, or, as we might more properly term it, the good-for-nothing system, in a disease like the Asiatic Cholera. For instance: the patient was ordered a warm bath on admission, an enema of beef tea, but no medicine.* When

* With the exception of calomel, and a mixture of sulphate and carbonate of soda, which were given to a few of the patients.

symptoms of collapse came on, stimulants were resorted to—wine, brandy, ammonia, &c. As might have been expected, there were very few recoveries by this method of treatment. Another agent, therefore, was called into requisition to aid and assist the efforts of Nature. This agent, as may be surmised, was carbonic acid gas, in the form of soda water, by the aid of which several patients would appear to have been snatched from the jaws of death. How many, I am unable to say, not having visited the hospital regularly at this period. That this conclusion is not a mere surmise, or individual opinion, the following case, the particulars of which were taken from the patient's card, will clearly show:—

Ann Reid, aged 50, admitted August 31st, at 1 a.m.

History: "She was taken at 5 p.m. (the day previously), with purging, and vomited freely after some medicine which she took: cramps came on at 11, and have continued ever since: passed urine at 4 p.m."

Symptoms on admission: "The countenance characteristic: skin cool and moist, and the tongue coated." (No mention of the state of the pulse, of the stomach, or of the bowels.) Ordered a warm bath, a mustard poultice, and an enema of broth.

Sept. 4th. On my visit to the hospital to-day, I remarked "soda water" put down on the patient's card, and ascertained that the vomiting had continued incessant up to this time. The purging had not been so severe.

Sept. 5th. Was informed to-day that the vomiting had ceased in the night, and had not since returned.

Sept. 6th. "Mutton chop."

Sept. 7th. "Convalescent."

No medicine, it will be observed, was ordered previously, or subsequently, to the employment of the soda water!

With these remarks, we shall be the better able to judge of the result of the treatment adopted by Dr. Black. During this week, 39 patients were admitted; 20 died, and 19 recovered—being a mortality of 51 per cent. This rate would have been much higher but for the circumstances before mentioned—the employment of carbonic acid gas in a certain number of cases, and the fact that the majority of the patients admitted were in the first stage of the disease.

In the following week, Sept. 6th to 13th, when the patients came under the care of Dr. Hue, the treatment was again modified. The charcoal was still employed in enema, but, instead of the effervescing draughts, the following mixture was given:—"Carbonate of soda, 10 grs.; sulphate of soda, 10 grs.; aromatic spirit of ammonia, 10 grs.; camphor mixture, one ounce: for a draught to be taken every hour."

This change in the treatment was anything but beneficial. There were 40 admissions, 22 deaths, and 18 recoveries—the ratio of mortality being 55 per cent. This is more than double that previously obtained by Dr. Hue, when he trusted principally to effervescing draughts. Even this rate would have been higher, but for the fact, that soda water was also resorted to in some of the cases. The following history will prove this:

Richard Heely, who was admitted on the 12th Sept., in the first or diarrhœal stage of the disease, had been treated in the manner now described, excepting that the charcoal and enema had been omitted. He was considered to be convalescent on the 13th; but, on the 14th, he was suddenly attacked with severe sickness—the fluid thrown up being white and characteristic—great oppression at the chest, and all the symptoms of the near approach of collapse. Being in the ward at the time, the wife of the patient asked me to order her husband some medicine; but not having the power to do this, I told her she must wait until Dr. Hue or Mr. Wood came. I added, however, that if she applied to the nurse, the latter might be able to give her some soda water, for the moment, to relieve the sickness. This she did, and I left her giving a bottle to her husband. On the next day, I found all the bad symptoms had subsided, and I learnt that the soda water had not only relieved the sickness, but, by its continuance, the oppression at the chest, and all the other dangerous symptoms. Another case was that of a boy, named Neville, who was admitted on the 7th September. He was treated in the same way as the others, without any relief; and, on the 9th, he fell into a state of incomplete collapse, with cramps and constant vomiting. In order to relieve the latter symptom, the nurse, of her own accord, gave him some soda water. This had the desired effect; and, on the next day,

when I saw him, reaction had taken place, and all the dangerous symptoms had entirely vanished.

So conscious was the little fellow of the benefit he had derived from the soda water, that he kept a bottle constantly under his pillow, taking a portion from time to time until the 14th, when he was convalescent. He had, I presume, made love to the nurse, and thus obtained this favour, which, to him, was the saving of his life. So, at least, I should conclude.

Having thus given the result of the treatment at this hospital during six weeks, with different physicians, and under a variety of circumstances, I shall now close this part of the subject with a general summary of the cases, from the commencement to the termination of the epidemic.

According to the Treasurer's and Almoner's Report, published at the time, there were admitted into the hospital, from the 17th June to the 6th Oct., 478 cholera patients. Of this number, 199 died; being a ratio of mortality of 41.65 per cent. If, however, we deduct the cases in which carbonic acid was employed, as previously given, the numbers will be, of admissions, 358, and of deaths, 172. This done, the ratio will be raised to 48.04 per cent. On the other hand, if we calculate the per centage of deaths of the cases treated by my method, it will be found to be 22.50—there having been 120 patients so treated, of which number 27 died. The mortality of the other

cases is, therefore, more than double that obtained by the employment of the different forms of carbon. A glance at the following table will show this more clearly :

Table D. Return of the Cholera Patients admitted into St. Bartholomew's Hospital in 1849, with the number of deaths and recoveries, and the treatment adopted.

	Admis- sions.	Recove- ries.	Deaths.	Per cent- age of do.
General treatment . .	358	186	172	48.04
Antidotal „ . .	120	93	27	22.54
Difference	25.54

This difference would have been still greater had it not been for two circumstances. The first is, that many of the patients grouped under the head of general treatment took carbonic acid gas, to which the recoveries of some were undoubtedly to be ascribed. The next circumstance is, that the patients, treated by the antidotal method, did not take the gas in sufficient quantity to derive the full benefit from its employment. Hence the rate of mortality by this method of treatment was higher than it would otherwise have been.

These inferences will be confirmed, if we turn to the result obtained by the methods of treatment that were adopted, previously to the employment of car-

bonic acid gas. For the want of the necessary documents, I am unable to give the exact, or statistical, results ; but the following extract, from a report published in the *Lancet*, will afford sufficient information for my purpose :—

“St. Bartholomew’s Hospital,—In the outset, the saline plan, as recommended by Dr. Stevens, was tried, but completely failed. Salt and carbonate of soda, with chlorate of potash, were given by mouth, and injected into the rectum. In one case, the liquid was injected into the veins, but this procedure was speedily followed by a fatal termination. A second plan, that of calomel with opium, in the proportion of five grains of calomel and one of opium, repeated every two or three hours, was equally unsuccessful. The acetate of lead in large doses, with opium and without, was given with a view to check the diarrhœa, and certainly exerted some effect in doing so. The evacuations from the bowels after the first three or four doses were usually blackened ! Several cases, with the view of testing popular remedies, have been treated empirically—one with the wet sheet, another with olive oil—but with no results that would induce us to continue the plans. That which seems to have been attended with most success is the exhibition of an *emetic* of sulphate of zinc, repeated quickly ; if not followed by reaction, a warm bath, mustard cataplasms to the epigastrium, and, *an hour after the emetic, an effervescing solution of the citrate of soda*, has been

given *at intervals of two or three hours*, ice and iced water being administered to allay the thirst. Stimulants and ammonia have had scarcely any effect in rallying patients." *

That the emetic had little, if any, influence, in producing a beneficial result, will be clear from the facts previously adduced; for when the emetic was given without the effervescing draughts the course of the disease was seldom, if ever, arrested. On the other hand, when the emetic was omitted, as in Dr. Hue's week, the treatment being the same, the rate of mortality was lowered.† That the recoveries at St. Bartholomew's Hospital, in those cases in which the different forms of carbon were resorted to, are to be referred to these agents, and not to any other, we may learn from what occurred at another hospital, King's College.

Previously to applying to the physicians at Bartholomew's, I called on Dr. George Budd, and expressed the same wish. In reply, he informed me, that he had already adopted my mode of treatment at King's College Hospital; having been made ac-

* *ancet*, August 11th, 1849.

† Mr. Searle stated, at a meeting of the Medical Society, "that in intense cases of the Cholera, emetics are fatal to the patient." He added, that he had, at first, a high opinion of them, and accordingly treated the first eight cases at Warsaw with the muriate of soda. They got over the cold stage, but "they all had typhus after the temporary amendment, sunk, and died, every one of them."—*Lancet*, Vol. i., p. 448. 1831-2.

quainted with it through his brother, Dr. Budd, of Plymouth, whom I had occasionally met in consultation there, some years before. Finding this to be the case, I contented myself with a single visit to the hospital, in company with Dr. Budd, and knew nothing of the result of the trial until it met my eye in the following table, inserted in the pages of the *Lancet*:

TABLE E.—“KING'S COLLEGE HOSPITAL.”

“The following table exhibits, in a concise manner, the cases, the treatment, and the results, of all the Cholera patients admitted into this hospital, up to the present time:”—*

Name.	Age.	Sex.	Admitted.	Treatment.	Result.
Richard F.	36	M.	July 5	Vegetable astringents; <i>effervescing saline mixture</i> ; ammonia and wine; ice.†	Discharged cured, July 16.
Sarah D.	40	F.	— 13	Ditto.	Died, July 14.
Catherine P.	16	F.	— 20	Ditto.	Discharged cured, July 25.
Mary H.	32	F.	— 20	Ditto.	Discharged cured, July 25.
Robert B.	18	M.	— 24	Ditto.	Convalescent.
Denis D.	30	M.	— 25	Ditto.	Cured, August 3.
J. J. D.	10	M.	— 25	Ditto.	Convalescent.
Elvin D.	41	F.	— 27	Ditto.	Died, July 28.
E. H.	49	F.	— 29	Ditto.	Died, July 30.
F. W.	31	M.	Aug. 1	Ditto.	Died, August 1.

* *Lancet*, Aug. 11th, 1849.

† Iced water was given *ad libitum*, and effervescing saline mixture during the whole time. (Bicarbonate of potash, two scruples; water, two ounces. Citric acid, thirty grains; water, one ounce.) During the secondary fever, a few minims of aromatic spirit of ammonia were added to the effervescing mixture, and large quantities of weak broths and milk were allowed.

Name.	Age.	Sex.	Admitted.	Treatment.	Result.
H. M.C.	66	M.	Aug. 1	Vegetable astringents; <i>effervescent saline mixture</i> ; ammonia and wine; ice.	Convalescent.
H. T.	6	M.	Aug. 4	Ditto.	Died, August 7.
E. T.	2	F.	Ditto.	Ditto.	Convalescent.
M. T.	12	F.	Ditto.	Ditto.	Died, August 5.
G. D.	37	M.	Aug. 5	Ditto.	Died, August 6.
C. M.	38	F.	— 7	Ditto.	Died, August 7.
					Total, 16; deaths, 8.
T. P.	34	F.	July 19	Chloroform.*	Died, July 20.
W. P.	7	M.	Ditto.	Ditto.	Ditto.
M. P.	3½	F.	Ditto.	Ditto.	Ditto.
T. P.	5	F.	Ditto.	Ditto.	Ditto.
E. S.	48	M.	Ditto.	Ditto.	Ditto.
					Total, 5; all died.
M. R.	52	F.	July 26	Greville-street mixture.†	Died, July 27.
E. B.	50	F.	July 28	Ditto.	Died, July 29.
J. C.	32	M.	Ditto.	Ditto.	Died, July 28.
T. R.	72	M.	Ditto.	Ditto.	Ditto.
					Total, 4; all died.
A. T.	57	F.	Aug. 4	Turpentine.	Died, August, 4.
C. M.	38	F.	— 7	Ditto.	Died, August, 7.
					Total, 2; both died.
Thomas T.	7	M.	Ditto.	Salt emetic and effervescent mixture, with ammonia.	Under treatment.
John T.	40	M.	Ditto.	Ditto.	Ditto.
Isabella G.	36	F.	Ditto.	Ditto.	Ditto.

* *Chloroform* was given to anæsthesia; and, in two of the cases, ten minim doses, dropped in sugar. Iced water and salines were also given.

† Formula of the *Greville-street mixture*;—Chlorate of potash, eight grains; carbonate of soda, two scruples; chloride of soda, two drachms; sufficient water for a draught; to be taken every quarter of an hour. Iced water *ad libitum*; hot-air bath and friction; sinapisms.

It is here seen, that the only recoveries which were obtained in the above hospital, during the period specified, were those of persons to whom carbonic acid gas had been administered. *All the other patients, although subjected to three different modes of treatment, perished!* A more striking example could not be adduced, in proof of the conclusions before drawn, while referring to the employment of the same agent at St. Bartholomew's Hospital.

The result—a mortality of 50 per cent.—if we exclude the three under treatment, may not be considered a particularly favourable one; while it is double that obtained at St. Bartholomew's. This may be referred to one of two causes, or, possibly, to both. The majority of the patients, there treated, may have been extreme cases, or in a state of collapse before the commencement of medical treatment. On the other hand, the comparatively high rate may have been owing to the non-exhibition of the remedy in sufficient quantity, or at the most proper time, to derive the full benefit from its employment.

That the majority of the cases admitted into the wards of a Cholera hospital are severe and extreme ones, is not only well known, but it is a natural consequence of the prevalence of a disease that runs its course as rapidly as the Asiatic Cholera. The interval between the commencement of the second and third stage, or the stage of collapse, is usually very short; and as it is not until the severe symptoms set in that patients think of applying for admission, the

consequence is, that a large proportion of the cases admitted into a Cholera hospital are in a state of collapse at the moment, or fall into that state shortly after. Hence the mortality in Cholera hospitals is always much higher than in private practice. Of the first 100 patients admitted into the Hotel Dieu, Paris, in 1832, it is not pretended that more than *four* were cured—and of these, three were not decided cases of Cholera! As such, there was only *one* recovery in 97 cases! If, also, we turn to the cases at King's College Hospital, not treated *specifically*, we find that the result is still more appalling—the recoveries being 0, and the mortality 100 per cent. ! Although the rate of mortality of the cases in which effervescing draughts were employed will appear high, when compared with the evidence previously adduced,—and especially the Spanish evidence,—it may be a very low rate under the circumstances. It is, in fact, the same in one respect as at St. Bartholomew's,—only half that obtained by the other modes of treatment.

This evidence, like the indirect evidence before adduced, is also valuable in another point of view—more valuable, perhaps, than that obtained when the specific agents have been alone employed, no matter how successful these may have been. As is apparent, when these agents have *not* been resorted to, the highest rate has been obtained: while the lowest has invariably followed their employment—the success of the treatment being in exact ratio with the extent of their administration.

Such were the facts connected with the trial of my remedy at the above hospitals; and I have been induced to enter into this detail for two reasons. In the first place, I was anxious to render up an account of my stewardship; and to be able to give an answer to those who may say, "What were you doing in 1849, and what was the result of your labours?" In the next place, I feared it might be thought, if not said, that the trial of my remedy at St. Bartholomew's had been a failure; neither of the physicians there, unlike those in Spain, having given the least evidence on the subject. I asked these gentlemen, on the termination of the epidemic, to make the result public: and was told that their reports would be sent in to the Committee of the College of Physicians in London. This Committee was appointed to inquire into the different methods of treatment that had been adopted during the outbreak of Cholera in 1849, and to report on the result. The Report of the Committee was brought forth in due season; but not a word was said of my experiments at St. Bartholomew's, or of the employment of carbon and its compounds. The cause of this I am entirely ignorant of, although I have, of course, an opinion on the subject: to express that opinion might be considered prejudice or egotism on my part. I will therefore only add that, in this case as in many others, good came out of evil.

Disappointed in my hopes, if not disgusted with the fruit of my labours and exertions, I determined

not to depend on others, during future visitations of the Epidemic Cholera, in order to obtain fresh evidence of the efficiency of the remedies proposed by me for the cure of this modern scourge. Fortunately for me, or, rather, for the cause in which I am engaged, the desired opportunity was soon afforded; and I embraced it the more readily, because, in addition to the prospect of obtaining a large field of observation, I should have an opportunity of treating the disease in those localities in which it assumes its most severe and malignant form—in intertropical regions. The occasion referred to was the outbreak of Cholera in the Island of Jamaica in 1850; the particulars of which must be reserved for the next chapter.

CHAPTER VII.

THE OUTBREAK OF CHOLERA IN THE ISLAND OF JAMAICA.*

THE Epidemic Cholera commenced, in the Island of Jamaica, at Port Royal, on the 8th of October, 1851, and, in a few weeks, carried off a third of the population. The troops stationed here were removed, after a time, to Stoney Barracks; but, before this was effected, 80 out of 300, or rather more than a fourth, had been cut off. It had been prevailing at Chagres, and on the Isthmus of Panama, for some time previously; and it was stated that the disease had been imported into this island by one of the American steamers, which touch here on their voyage from the above port to New York. For this opinion, no good or sufficient evidence has been adduced; while the facts that have been collected, during the prevalence of the

* The following detail has been taken from a pamphlet, published by me in 1852, on my return from Jamaica:—*Statistical Report of the Epidemic Cholera in Jamaica*. Being now out of print, an epitome of the facts it contains has been added on the present occasion, these facts being of some importance.

disease in other parts of the world, tend to negative such a conclusion. Be this as it may, the disease soon spread to Kingston, on the opposite side of the bay ; carrying off 6,000 out of a population of 40,000.* It prevailed in this town, in a severe form, for about six weeks ; and, during this period, spread with unusual rapidity to other parts of the island. The first fatal case in Kingston was on the 11th October ; on the 19th there were three deaths in the adjoining parish—St. Catherine's—and, on the following day, Dr. Palmer, of Spanish Town, who, in conjunction with four other practitioners, had made a *post mortem* examination of one of the bodies, was attacked, and died in a few hours. On the 20th there were thirty deaths in this town, and the disease continued its destructive career until the end of November—few towns having been more severely visited than this, the capital of Jamaica. It appeared at St. Thomas's, in the East, thirty miles from Kingston, on the other side, about the same time as at Spanish Town ; and from these different points it spread east, west, north, and south. By the end of November, it had

* Among these was the lamented Dr. J. M'Fayden, who has acquired for himself, as a botanist, a European reputation ; while he was esteemed as highly in Jamaica for his talent as a physician. Nine more medical men died of cholera, subsequently, and during the first irruption of the disease ; and one, of fatigue or exhaustion—making eleven in all. This proportion—amounting to 9 or 10 per cent.—is much larger than has been observed in any other quarter of the globe.

reached every part of the island, excepting the parish of Manchester, in the centre, and the parishes of Westmoreland and Hanover, situated at the western and north-western extremity of the island. It was lingering, however, on the boundaries of these parishes, having already attacked Montego Bay and the adjoining districts, in the North, and St. Elizabeth and Black River, in the South. This river forms, as it were, the Eastern boundary of the great alluvial plain of Westmoreland; and it was therefore expected, that the epidemic would thence have spread, with unusual rapidity, over the whole parish. Such, however, was not the case; for the disease extended no further in this direction—the parish of Westmoreland being attacked, seven months afterwards, by another and a different route, as will subsequently appear. Continuing its course along the sea-coast, from Montego Bay, the epidemic invaded the parish of Hanover early in December, and reached Lucea, the principal town, the end of this month.

From this spot, the disease gradually spread towards Green Island, where it appeared on the 14th of February, 1851; having previously attacked Lance's Bay, Cousin's Cove, Davis's Cove, and the intermediate estates. The disease only continued at the former spot between two and three weeks; and was then confined entirely to the new town, situated on the eastern side of the Bay—not a single case having occurred on the opposite side. The epidemic, how-

ever, had been prevailing in several of the rural districts—more particularly Fish River, at the Western extremity of the Island—previously to this; and continued in some after the disease had subsided in the town. By the middle of April, all these districts were free of the scourge. This, therefore, may be said to have been the boundary of the disease during its first irruption. Isolated cases, however, continued to occur, from time to time, in the immediate neighbourhood of Green Island; and, more particularly, in the hilly, or mountainous, districts between Lucea and the latter town. From these points, and after an interval of about six weeks, it suddenly spread, the end of May, to the contiguous districts—Flamstead and Glasgow; the latter being on the boundary line between the parishes of Westmoreland and Hanover. Kendal, also, situated to the westward of Flamstead, and between Green Island and Glasgow, was invaded at the same time. In the course of a week, the artificial boundary, which separates Hanover from Westmoreland, had been crossed—many cases occurring, simultaneously, on the estates and settlements immediately adjoining to Glasgow. After this, by one of those leaps so characteristic of the disease, it suddenly burst out, with extreme violence, in the middle of the parish; leaving the intermediate district untouched and uninfected for the moment. The epidemic reached the town of Savanna-la-Mar on the 8th June, when the first death occurred; but it did not prevail generally for ten days after. Its con-

tinuance in this town was not more than six weeks; but it lingered in the country districts until the end of August, when it ceased to prevail epidemically—excepting on one or two estates in the south-western corner of the parish, which had not been before invaded.

With these preliminary remarks, we may now proceed to consider a few of the points connected with the type and peculiarities of the disease. And first, as regards its rate of diffusion.

DIFFUSION.—There are, as will be apparent, few countries where the epidemic has diffused itself over the surface to the same extent, and with the same rapidity, as in Jamaica. In the short space of four months, the pestilence had extended over every part of the island—with the exception of the parishes of Manchester and Westmoreland. Even this immunity did not last long, for, after a short interval of two months, the epidemic suddenly re-appeared; and, commencing at the point where it had before ceased, spread over the whole parish of Westmoreland.

What the rate of diffusion has been, or what proportion of the population has been attacked, it is impossible to ascertain, as no general returns have been made; but, judging from the number of deaths, we may conclude that one-sixth of the population was attacked. This proportion is much greater than what was observed in Europe; and is only exceeded by the results obtained during the prevalence of the disease in some parts of the East.

In Europe, the attacks have varied from 1-10th to 1-300th, in those parts invaded by the disease—the average being about 1-150th of the population. Thus, while in Europe only one person out of every 150 has been attacked, in Jamaica 1 in every 6, or, at all events, 1 in 7, of the inhabitants, was brought under the malign influence of this invisible cause, during one single irruption.

In India, the proportion, during the first irruption, was calculated at 1-10th of the inhabitants of the infected districts; the disease having taken three years to spread over the whole Continent.

In the countries subsequently invaded by the epidemic, and situated between the confines of India and Russia, the rate was much higher; although the actual proportion is unknown. Judging, however, from the number in the towns, it must have amounted to a fifth, a fourth, a third, or even two-thirds, as in the Caucasus. It was only in the latter countries, therefore, that the rate of diffusion was as high, or higher than in Jamaica. Even in India, the place of its origin, the numbers attacked, in proportion to the population, was less by nearly one-half.

Having thus ascertained this important fact, we may now inquire what has been the intensity of the disease, or the proportion of deaths—for the rate of mortality does not always bear an exact proportion to the rate of diffusion.

INTENSITY.—Although no official Report has been

published, it was calculated at the time that the deaths from Cholera did not fall short of 40,000, during the first irruption. Now the population of Jamaica was estimated at 400,000; so that one-tenth of the inhabitants must have been cut off. There has been nothing to equal this in any part of Europe, while even in India the rate was not so great. According to Moreau de Jonnes, derived from official Reports, the sixteenth part of the population perished in India, in those parts invaded by the disease.

We have no means of ascertaining what the rate of mortality was, as regards the entire population, in those countries subsequently visited by the epidemic; judging, however, from the mortality which occurred in the towns, and in particular districts, it must have been very high. Thus, in Arabia, 1 in 3; in Mesopotamia, 1 in 4; in Armenia, 1 in 5; in Persia, 1 in 6; and in Syria, 1 in 10, of the inhabitants of the towns perished. In Egypt, also, the mortality was very great. According to Clot Bey, the French physician, who was present, 50,000 died in Cairo out of a population of 150,000. At Tiflis, the ratio of deaths was equally great, for 1 out of 3 of the inhabitants perished in the town; and in the province of the same name 8 out of 11. The proportion of deaths was little less in the mountainous regions of the Caucasus; but after reaching the alluvial plain of Russia, the ratio of mortality was very sensibly diminished. In the infected districts, 1 in 350 of the

inhabitants perished ; but taking the whole country—not more than one half of this immense empire having been invaded—the 700th part only of the entire population perished.

In Europe generally the mortality has varied from 1 in 20 to 1 in 700 of the population—the average being about 1 in 350. In England, the proportion was 1 in 332. In particular spots and in certain towns, the ratio of mortality has been much above that average. Thus, in London it was 1 in 161 ; in Paris, 1 in 42 in 1832, and 1 in 57 in 1848 ; but in Moscow, 1 in 25—the highest rate observed in Europe.

Compared with this quarter of the globe, the epidemic exhibited a great and awful difference in Jamaica,—being for the whole island, and the whole of Europe, as 35 to 1. Had the same rate of mortality occurred in England, 1,758,669 would have perished, during one visitation, instead of 53,293, the actual number. Or, to state the case in another way. In all England, 30 persons died of Cholera to 10,000 living ; but, in Jamaica, there were 1,000 deaths to the same number of inhabitants.

The comparison is still more striking if we take the mortality in particular towns, instead of the whole island. At Kingston, the deaths were very little more than an eighth of the population ; but at Port Royal and Falmouth, they amounted to a third : at Lucea, to rather more than a fourth, or thirty-one per

cent.* At Port Maria, two-thirds of the population were cut off, or 600 out of 900. "At first," writes the Rev. Mr. Simpson, "the epidemic was mild in its type, and yielded easily in most cases to the treatment of our medical men; but, on the 1st of Dec., it burst on the town like a flood, carrying off 400 (nearly half the population) in the short space of ten days."

In many of the small villages and settlements, the mortality was as great, sometimes much greater. Thus, at Houghton Court, an estate close to Lucea, 30 or 40 died in the course of a week or ten days, out of a population of about 100; when the remainder, being panic-struck, fled. Of these, a great many died subsequently in Lucea and at Johnson's Town; so that the half, or two-thirds, must have perished. At Orange Cove, an estate between Lucea and Lances Bay, from 70 to 80 out of a population of 100 were cut off—the overseer being one of the victims. But at Bachelor's Hall, 70 *out of* 73 residents died—or 95 per cent. As there was a medical man residing at the latter estate, when the disease broke out, we may presume that these individuals had

* Had the proportion of deaths to population been as great in London as in the above towns, *half-a-million* persons would have perished; whereas, only 14,137 died in 1849. In 1832, the proportion was still less. And yet Lucea is one of the healthiest towns in Jamaica; so much so, that the late Dr. Warren recommended the invalids and convalescents, among the troops, to be sent there from other parts of the island.

medicines sent to them, if they did not receive personal aid. On the other estates, the patients were attended, and medicine administered, by the Overseers. While, therefore, in the towns in Europe, the mortality has not been, in general, more than 1 per cent., and has only, in some rare instances, been as high as 5; in some of the towns and villages in Jamaica, 30, 40, 50, and even 90 per cent. of the population perished. We have no examples like this, even in the East, excepting in places in which the population was without medical aid; or where, as among the natives of India, the sufferers have refused to avail themselves of the resources of the healing art. In Arabia, where the want of medical men was more particularly experienced, and where the proportion of deaths was so much greater than in India, the highest rate of mortality recorded, with the exception of Mecca and a few other localities, is 33 per cent. The same cause was in operation in Jamaica, but only to a very trifling extent; for the negro is willing to take medicine, while it was supplied to him, liberally and gratuitously, by the Boards of Health, or the parishes, by the proprietors of estates, and by private individuals.

In no part of the world, therefore, has the gross mortality, or the ratio of deaths to population, been greater than in Jamaica; and, as a consequence of this, and the high rate of diffusion, there are few in which the intensity of the operating cause has been

as great. Having arrived at this conclusion, we may now pass on to a consideration of the type, or character of the disease.

TYPE OF THE DISEASE.—As might have been expected, the type of the disease in Jamaica was much more severe than what had been witnessed in Europe; and was only equalled by the irruption of the epidemic Cholera in intertropical and other warm climates. That the type of the disease was of the severest, or most malignant, form, we might infer from the awful mortality that occurred in Jamaica. It is desirable, however, to adduce other proof in support of this conclusion; and the best which can be furnished is that derived from the duration of the attack in individual cases. If we make this the standard to which to refer, it will be found that the severity of the disease was as great in Jamaica as in any part of the world, in which such facts have been recorded. Contrary to what might have been expected, there are no *general* statistical data to refer to, in Europe, of the duration of the disease in individual cases. It was not until 1849, that we were furnished with an accurate and complete digest of the age, sex, and duration of attack, of all the fatal cases that had occurred in one particular country. This valuable information was given to the profession by the Registrar-General, in the Tables which are published, weekly, of the births, deaths, and marriages in England. These Tables were afterwards published in a

separate form, and added to the Report on Cholera, which has been drawn up with so much care, labour, and research, by Dr. Farr. This Report, forming the most valuable record of facts connected with this or any other disease, that has been published in any country, must henceforth form the basis of all calculations on these particular points.

In India, with the best medical staff, perhaps, in the world, under the direction of three medical boards, these facts escaped notice altogether; although reports, invaluable in all other respects, were sent in to the Government, by every surgeon in the service of the Hon. E. I. Company. The mind of the observer was then so completely absorbed by the probable origin of this *nova pestis*, the peculiarities of the disease, and the question of contagion, that other points were passed over, or disregarded. And yet, it is only by a knowledge of the duration of the attack in the fatal cases, that we can ascertain the type of the disease; and the probable efficacy of different modes of treatment. Although the disease has prevailed in India from 1817 to the present time; and although many valuable reports have been published during the interval, I have only met with a few isolated examples from which such facts could be obtained. They will, however, be sufficient for our present purpose; more particularly as the deaths, inserted in the Table, occurred during one of the

severest visitations that have been observed in India, of late.* They may therefore be taken as an example of the type of the disease in that quarter of the globe, in its severe form. I except, of course, that form of Cholera, termed by the French "Cholera Foudroyant," in which the patient is attacked suddenly with collapse, and carried off in the space of 15 or 20 minutes; and which has not only prevailed in India in particular instances, but has also formed the prevalent type in certain towns, and during particular visitations. This form of the disease, fortunately, is an exception, and a rare exception to the general rule. In Jamaica, I only met with two instances, in which the disease assumed this peculiar type. But notwithstanding this circumstance, it will be found that the duration of attack in Jamaica has not been, on the average, greater than in India; while, as compared with Europe, there is a very marked and striking difference. The instances that I shall adduce, however, in proof of this assertion, are few in number, as no facts of this kind have been collected, that I am aware of, by other individuals in Jamaica; while those derived from my own experience can only be received to a limited extent. In fact, it is only by a reference to those cases not

* For particulars, see the very able and lucid Report of the Cholera at Kurrachee, in Her Majesty's 86th Regiment, by A. Thom, Esq., *Medical Times*, May 28, 1848.

attended by me, or in which the same mode of treatment has not been employed, that a positive inference can be drawn; as there can be no doubt that, even in the fatal cases, the lives of individuals were lengthened to a much greater extent than by any other method. In the following Table these facts have been thus arranged :—

TABLE F. DURATION OF ATTACK. DEATHS.

Hours	Under 6.	Under 12.	Under 18	Under 24	Under 48	Above 48	Total.		
							Under 24	Above 24	At all Periods.
A. Jamaica Cases not treated at all	4	7	1	1	13	...	13
Or, per cent.	30·76	53·88	7·69	7·69	100	...	100
B. Jamaica Cases treated by others: Cousin's Cove.	...	5	4	...	3	...	9	3	12
Or, per cent.	41·66	33·33	...	25·00	...	75·00	25·00	100
* Port Royal	2	6	1	1	10	...	10
Or, per cent. ...	20·00	60·00	10·00	10·00	100	...	100
C. Cases treated by the Author.....	5	18	20	11	17	13	54	30	84
Or, per cent. ...	5·95	21·42	23·30	13·9	20·23	15·47	64·28	35·72	100
D. Cases treated at Kurrachee, India	...	57	...	117	32	32	174	64	238
Or, per cent.	23·94	...	49·15	13·44	13·44	73·12	26·88	100
E. Cases treated in London, in 1849.	212	1759	3239	1447	2466	3838	6657	6304	12,961
Or, per cent. ...	1·63	13·41	24·99	11·16	19·02	29·61	51·31	48·69	100

* Reported by Mr. Watson, Surgeon, R.N.

MEAN DURATION OF THE ATTACK.

	Hours.
Cases not treated at all	8·
Ditto, treated by others*	16·50
Ditto, treated by the author	24·64
Ditto, in India	25·09
Ditto, in London	50·01

If we examine these Tables, it will be seen, that of the cases not treated at all, the whole died under 24 hours; and the majority under 12 hours. Precisely the same result occurred at Port Royal, with patients that had the best and the most prompt medical aid: but, at Cousin's Cave, 25 per cent. of the patients died between 24 and 48 hours. Among my own patients, 64 per cent. died under 24 hours, and 35, above that period: 15 per cent. having lived beyond 48 hours! Nearly the same result was obtained in India, although not, probably, from the same cause—the superiority of the treatment adopted. In London, the greatest per-centage was above 48 hours.

In England, the period antecedent to death has been sometimes lengthened to 28 days, and, in India,

* We are informed by the Rev. J. Campbell, that, at Goshen, “death, in many instances, has ensued three hours after the attack, and seldom does the patient suffer more than twelve hours.”—*Missionary Record*, Feb., 1851.

In the first Report sent in by Mr. Murphy (Assistant-surgeon, R.N.), to the Westmoreland Board of Health, the duration of the attack, in two of the fatal cases, is mentioned, viz., five and ten hours.

to 12 days. But in Jamaica, the longest period during which an attack lasted, among my own cases, was 96 hours, or 4 days: while, of the patients treated by others, none lived beyond 72 hours, or 3 days.

Taking these and the preceding facts, we must conclude, that the type of the disease was not only much more severe in Jamaica than in Europe, but, that it was not surpassed by that which is met with in India, where its ravages have hitherto been the greatest.

We may now pause, for a short time, in order to ascertain what has been the character, or the peculiarities, of the disease in this part of the world.

PECULIARITIES OF THE DISEASE. — As a natural consequence of the intensity of the disease, and the rapidity with which it ran its course, all the deaths occurred during the stage of collapse. At least this was the case, as far as my own experience and observation went. I heard, certainly, of cases, attended by other practitioners, that died comatose, before and after reaction had taken place; but, then, I have no hesitation in asserting, that the result was due entirely to the treatment adopted: viz., the exhibition of opium, or the improper and too early employment of stimulants. To prove this, it is only necessary to add, that, of all the cases treated by me, I only met with one in which the symptoms approached those common to the consecutive stage in Europe. Fever, it is true, preceded and followed attacks of Cholera, in numerous instances; but then the fever, in these cases, was

unaccompanied by coma, suppression of urine, and the other symptoms characteristic of the consecutive fever of Cholera. In fact, it appeared to be produced simply by an excess of action, and subsided rapidly, in the generality of cases, in a few hours; instead of being prolonged, as in Europe, to as many days. This peculiarity, therefore, assimilates the disease, again, to the form met with in Asia, rather than to that observed in Europe.

Another peculiarity was the absence of the preliminary diarrhœa in a large number of the cases; the attack being ushered in by the characteristic symptoms, as rice-water-purging and vomiting.

In other instances, and in one particular locality, the attack commenced with severe pain and griping in the stomach and bowels, followed, in a short time, by rice-water-purging and vomiting. In some few cases, the characteristic symptoms were preceded by diarrhœa, which thus formed the connecting link between the attack of Colic and Cholera. When present, however, the diarrhœa was of comparatively short duration—being seldom more than a few hours. This form, or type, of the disease has been observed occasionally before, in some parts of the East; but I am not aware that it has been met with in Europe, as the prevailing type in any particular locality; isolated and individual instances, however, have occasionally occurred.

To the preceding circumstances and the rapidity of

its course, either in recovery or death, may no doubt be ascribed the other and minor variations which the disease presented. Thus, there was not the same amount of cold, clammy perspiration, so peculiar a feature of the disease in Europe; nor that remarkable change in the physiognomy and general appearance of the patient—the consequence, doubtless, of the shorter duration of the state of collapse. Still, the essential symptoms of the Epidemic Cholera were, with few exceptions, present in all the *fatal* cases that I saw in Jamaica. These were the collapsed countenance; blueness of the body, particularly in Europeans, but scarcely perceptible in the negro; shrunken fingers; wrinkled, shrivelled skin; total suspension of all the secretions—particularly the biliary, fœcal, and renal—the non-generation of animal heat, with icy coldness of the extremities and tongue; arrest of the circulation, and total cessation of the pulse.

LIABILITY TO ATTACK AS REGARDS AGE. The same law, which has been observed in all other countries visited by the epidemic, appeared to prevail in Jamaica also. This was that the strong, the robust, the middle-aged—those, in fact, in the prime of life—were more liable to attacks of the disease than the young, or the aged. This will be evident by a reference to the following Table:—

TABLE G. LIABILITY TO ATTACK, AS REGARDS AGE.

Under these years	5.	10.	15.	25.	35.	45.	55.	65.	75.	85.	95.	Total.
Recoveries	30	38	39	115	175	81	83	28	19	10	4	622
Deaths	7	8	11	13	20	9	9	7	10	8	...	102
Total	37	46	50	128	195	90	92	35	29	18	4	724
Deaths in London, in 1849	2048	1338	700	1378	2106	2185	1891	1487	984	374	35	14524

In London, the greatest per-centage would appear to have been from the age of 35 to 45; the next, between 25 and 35; and, after this, under 5 years. But in Jamaica, the highest per-centage is between 25 and 35; the next, between 15 and 25; and then, between 45 and 55—among the recoveries: while, with the fatal cases, the liability is—1st, between 25 and 35; 2dly between 15 and 25; and 3dly between 10 and 15.

This calculation, however, can only be considered as an approximation to the truth, on account of the difficulty of ascertaining the age not only of the African negro, but the Creole also. The lower orders in Jamaica, in fact, never know their own age; and a mother, not even that of her own child, if the period extends over more than a few years. As the majority of the negroes on the estates had lived there in the time of slavery, or had been born during that period, their age was easily obtained by a reference to the books; but, in other instances, the information was only to be ascertained by reference to some intelligent

individual, who was acquainted with the parties, and could determine the probable age.

Having thus given a brief outline of the spread and type of the Epidemic Cholera in Jamaica, I may now refer to my own position during this period. When the news reached England of the outbreak of the disease in this Island, I was induced, for the reasons already stated, to proceed by the following packet to Kingston, where I arrived on the 26th December, 1850. After obtaining the necessary information, I was about leaving Kingston for Black River, under the supposition that the disease, which was still prevailing there, would thence have spread into Westmoreland—the epidemic being, apparently, on the decline in all the localities previously invaded. The news of the commencement of the disease in Lucea induced me to alter this intention, and to start for this town, where I arrived January 6, 1851.

Scarcely had I entered the hotel, when a message was sent to me to attend a lady; and I was soon after requested to go round and visit the patients, under the charge of the district visitors. On asking where the resident medical men were, I was told that one was ill in bed, and that the other two slept at their country-houses, and only came into the town for a few hours in the day. One of them, however, was obliged to sleep there, when it was his turn at the hospital—three days out of nine. The army surgeon, who had taken charge of the Cholera Hospital, alter-

nately with two of the private practitioners in the town, confined himself to this duty, and the attendance of the sick at the Fort, with its immediate neighbourhood. The patients in the town, therefore, were left almost entirely to the care of the district visitors, non-professional persons. As to the causes which led to so lamentable a state of affairs, I have neither the wish nor the intention to enter into now; the fact itself is sufficient for my purpose.

On the next day, when the Board met, the offer of my services was, of course, readily accepted; but I was told, in answer to my inquiry, that, as the services of the other medical men were not available, the *whole town*, not a district, and all classes of patients—high, low, rich, and poor—would fall to my share. Although such a charge could neither be undertaken nor discharged, properly, I determined to exert myself to the best of my ability, and commenced my duties at once. This was on the afternoon of the 7th. On the following day, the cases appeared to have increased twofold; the deaths, also, were greater than on any previous occasion—the disease being of the most malignant type. The consequence was, that the majority of the severe and rapid cases were only seen by me once. In fact, I was arrested at almost every door; while all that could be done, in the majority of instances, was to give a hasty opinion and pass on. On this day, viz., the 8th, 32 persons died—the population being between 1,000 and 1,100. On

the 9th, there were only 17 deaths; on the 10th, twenty-two; on the 11th, eight; and on the 12th, twenty-five. After this, the numbers decreased very gradually until the termination of the epidemic, the middle of February. On the last-mentioned day, the 12th, the disease assumed a peculiarly malignant form—nearly all the cases being those of sudden and complete collapse.

Having received a request from the inhabitants of Green Island, through the Custos of the parish, to proceed on there, some cases having occurred in the immediate neighbourhood, I left Lucea on the 30th January. Finding, on my arrival at the above place, that the disease was prevailing principally, and in its greatest intensity, at Cousin's Cove, I took up my quarters there. This estate is on the coast, between Lucea and Green Island, and midway between Davis's Cove and Lance's Bay. The latter town, or settlement, is on the road between Cousin's Cove and Lucea; and it was evidently by this route that the epidemic reached the above estate, several deaths having occurred there previously; while the estates between Lance's Bay and Lucea had been severely visited, almost depopulated. Here I remained until the middle of February, when I removed to Green Island,* and took charge of the new town, in addition to

* This is a misnomer, the town of this name being situated on the main land, and on the margin of a bay in which there is a small, verdant island. Hence the name.

the above district. I returned, however, at the end of a fortnight, and continued there until the termination of the epidemic—the beginning of April.

On the re-appearance of the disease, the end of May, I was induced, at the request of the Hanover Board, to take charge of Glasgow, and the settlements at Flamstead and Kendal. Three weeks afterwards, the adjoining district in Westmoreland was added to the above. I had then a circuit of four or five miles in every direction; while the majority of the people, in the Hanover portion of the district, lived on the adjoining hills—some of them almost inaccessible to either man or beast. On the subsidence of the disease in Hanover, and its increase and extension in Westmoreland, I removed to the Mint Estate, contiguous to Mount Eagle; and remained there until my departure, the beginning of September.

Although particularly pressed by the Westmoreland Board of Health to proceed to Savanna-la-Mar, on the outbreak of the disease there, I was reluctantly obliged to decline the invitation. The disease not having then subsided in the Hanover district, I could not, consistently and with honour, resign the charge I had undertaken, against the expressed wish of the Hanover Board—although my facilities for treating the disease would have been so much greater in such a town than in a country district; while I should thus have had a greater chance of accomplishing the object

I had in view, in proceeding to Jamaica. Having, however, during this visit had an opportunity of treating upwards of a thousand patients; and having adopted a particular mode of treatment in the majority of these; it only remains now to make the result known, for the satisfaction of the profession, and in order to show, by statistical facts, that the opinions advanced by me and others are not erroneous.

RESULT OF THE TREATMENT.—As the treatment was the same in nearly all the cases—at least in those which exhibited the characteristic symptoms of the malady—no column has been left in the Table, that will be hereafter added, for particulars under this head. A few remarks, therefore, will be necessary in this place.

This treatment, as may be surmised from what has gone before, consisted in the administration of the different forms of carbon. The form almost exclusively adopted, in this instance, was that of carbonic acid gas, produced by the admixture of the bi-carbonate of soda and lime-juice; as the simple solution of the gas in water, commonly termed Soda, or Seltzer, water, could not be obtained in the districts where I was engaged. This was the occasion of much inconvenience to me, and, sometimes, of injury to the patient—particularly in those cases in which there existed great irritability of the stomach; for the presence of the neutral salt, produced by the decomposition of the soda, and the union of the base with

the acid, is sometimes productive of irritation, and thus tends to keep up the morbid action of the alimentary canal. In addition to this, I was obliged to depend, almost entirely, on the fresh juice for the above object; it being impossible to obtain either the citric or tartaric acid in a solid form. In consequence of this, I was not only unable to obtain a sufficient supply, at particular times and in particular cases, but it was often made an excuse, with idle and careless attendants, for not administering the draughts as frequently and as regularly as ordered. To remedy these inconveniences, I was sometimes induced, when time and opportunity were afforded me, to administer the gas, *per anum*; and also to resort to the pure carbon, both by the mouth and in enema.

With respect to the premonitory diarrhœa, as so many practitioners refuse to acknowledge that it is an effect of the same cause as that which produces the more severe form of the disease, although, in my humble opinion, there can be little doubt on the subject, I made it a point not to treat this stage in the same way as the others. Generally speaking, the patient took five or ten grains of calomel, followed by a dose of castor-oil; and then, if the purging continued, and the stools became watery, either the common carbon or the effervescing draughts were resorted to. If, however, the stools were watery and abundant, although of a dark, or brown, colour, the draughts were employed from the commencement. In these

instances, the remedy seldom failed to afford relief, and to prevent the supervention of the next stage of the disease.*

In some cases, the calomel and oil were sufficient to check the diarrhœa, and to prevent the further progress of the malady; but in others, and this was the general result, the patient, after an interval, varying according to circumstances, would be suddenly attacked with the severe form of the disease, without being preceded by diarrhœa. Sometimes the purging continued until the stools changed from bilious to serous, followed by vomiting, cramp, &c. Not having visited many of the patients at this period of the attack, I am unable to state what the exact proportion of such cases was.

The same mode of treatment was adopted for the colic as for the diarrhœa, excepting in one particular locality, Mount Eagle, when the plan was altered for the following reasons:

Although the attack had been, in a great many

* I do not recommend others to adopt or follow the same plan, particularly in countries in which the preliminary diarrhœa is of short duration. In such instances, the common carbon might be employed, or even one of the hydrocarbons, as naphtha. The success which has followed the administration of this agent, in Russia and other places, in the first stage of the disease, points it out as a valuable substitute for the other forms of carbon, when the latter cannot be resorted to. These agents, however, cannot be depended on in the other stages, or in the severe form of the disease.

instances, preceded by colic in other localities, still, the examples were rare, and formed the exception to the general rule; while, in the majority of cases, diarrhœa was the connecting link between the colic and the severe form of the complaint. At Mount Eagle, however, the reverse was the case; as, generally speaking, the severe form of the disease was immediately ushered in by an attack of colic,—there having been, out of 110 patients on this estate treated by me, only 6 labouring under diarrhœa. The interval, also, between the attack of colic and the supervention of the next stage of the disease was so short, in nearly all the cases, that there was not time for the calomel and oil to produce any effect. The consequence was that, instead of bilious, serous motions were produced, followed by vomiting, spasms, &c. Anxious not to break through the rule I had laid down for my guidance; and believing that the calomel and oil, under such circumstances, were injurious; I was induced to exhibit antispasmodics, as sulphuric æther, &c. These agents generally relieved the pain and griping, but they neither prevented nor retarded the onward course of the disease. Having tried the experiment in one instance with success, I determined, in order not to lose valuable time, to treat the cases *specifically* from the first, by administering the effervescing draughts. In this resolution I was strengthened by Dr. Jelly, who had been induced to adopt the same mode of treatment as myself, and

who, like me, had only resorted to the effervescing draughts, when the characteristic symptoms appeared. He found, however, that all the patients attacked with colic, who were treated according to the ordinary routine, *died*; and he was, therefore, induced to resort to the draughts for the Colic, as well as the Cholera. Now, it is a remarkable circumstance that, from the time when this alteration was made, until I gave up the charge of the district, that is to say, from July 16th to September 1st, *not a single death* occurred, although 11 persons out of a population of 132 had died previously.* This favourable result cannot be ascribed to the cessation of the disease, as cases continued to occur during the whole of the above period; while I may add, that there were two deaths *the week after* I gave up the district—the patients having been attended by another practitioner.†

* Nearly all these were cases of Colic.

† Dr. Makertienne, who practised at Tiflis during the irruption of the Cholera in that part of the world, informs us that, in Persia, the attacks were frequently ushered in by severe pain at the epigastrium, and more particularly at the navel, which was almost immediately followed by vomiting and rice-water purging. According to the observations of M. Angelin, made in Syria, during the voyage of the *Active*, sloop of war, the disease manifested itself there, also, by a sudden, severe, and excruciating pain in the epigastric region; being followed, as in the former instance, almost immediately by the characteristic dejections and by vomiting. These cases were severe and rapid in their course, many of them terminating fatally in three hours. We must look upon this varia-

It remains now to ascertain the result obtained in Jamaica, and in this severe form of the disease, by the adoption of the mode of treatment recommended by me. Before entering into this part of the subject, it may be as well to observe, that there were many circumstances which prevented my obtaining as favourable a result in Jamaica as in other countries, independently of the type, or severity, of the disease. These were the apathy and prejudice, or superstition, of the Negro, who frequently has more faith in the Obeah or Myal man, and his charms and incantations, than in the Doctor and his remedies. The dread of the disease, and a disinclination to approach the patient were also serious obstacles to the successful treatment of the case; increased by the general feeling that, when the severe symptoms set in, nothing could be done for the relief of the sufferer. In the worst cases, therefore, the patient would be found on the floor of the hut, sometimes with an attendant, and sometimes without; the relatives and friends having left, as soon as danger was apprehended, to obtain a coffin, or to dig a grave. In general, however, the coffin would be found standing at the door, and the grave ready to be dug, therefore, in the mode of attack, as indicative of a severe form of the complaint—the most severe, perhaps, of any, next to that of sudden collapse.

I may also remark that every person on this estate would appear to have been attacked: as, of 132 residents, 110 had cholera, or colic and cholera, 17 colic alone, 2 diarrhœa, and three fever—not the consecutive fever of Cholera, but remittent fever.

receive its victim. Favourable results, therefore, could hardly be expected under such circumstances, as individuals who had already consigned the patient to the grave would be very unlikely to follow, with perseverance and regularity, the means of cure that might be proposed; for, when hope is extinguished in the human breast, exertion ceases. I must add, however, in justice to the negro, that although I invariably had these obstacles to encounter on my arrival in an infected district, they were generally removed, after a time, by example, by persuasion, and, more particularly, when it became known that, instead of dying, the majority of the patients recovered. As to the Obeah man, a few days were generally sufficient to cause him to desert his post, and to resign his office.*

In addition to the above, and the want of proper means and appliances for treating a severe case of Cholera in the huts of the lower orders, another great drawback was, the antipathy of the negro to leave his home from sunset to sunrise. However urgent the occasion might be, it was seldom that he could be induced to go for medicine after dark; while, if a

* The African idolatry—Obeahism (from Obi, the black-art) and Myalism—has been traced to that ancient cradle of idolatry, Egypt. They are supposed, by the Rev. Mr. Simpson, to correspond to the Ob and Sidoni of Scripture, and to have a common relation to the Sabian worship, of which Baal, or Moloch, or the Solar Fire, was the head.—*Missionary Record*, October, 1851.

person was attacked in the night, the friends rarely attempted to obtain assistance until the morning. To such an extent is this superstitious feeling carried, that I have often been unable to obtain an answer at the door of a hut, when doubtful of my way at night. On one occasion, not being able to find the opening of the fence, as it was very dark, I was obliged to make a considerable *détour*, and gain admission to the hut on the other side of the hill; although the inmates heard me calling to them, and although the husband and two adult females were sitting up with the patient. But, as the distance from the gate to the hill was greater than usual, they imagined that, if the door had been opened, Bogey might rush in and seize them, before I could arrive to the rescue.

These circumstances were serious obstacles in a rural district, and in a disease "which commences," as has been remarked, "where many others usually end—a disease in which time is everything, and for the treatment of which, consequently, minutes are as precious as hours, or even days, in most other diseases."* In Sylet, in 1818, as we are informed by the writer of the Bengal Report, if nothing curative was attempted for an hour, the case was then considered hopeless. Taking, therefore, these circumstances into consideration, and remembering also the difference in the type, or intensity, of the disease, it will not be surprising if the result obtained by me in

* Dr. Adams on the "Cholera in Glasgow."

Jamaica is less favourable than that which followed the same mode of treatment in Europe; or which those who have read the Reports of the Spanish physicians might have been led to anticipate. With these few remarks, I will now proceed to show what the rate of mortality and recovery has been in Jamaica; and, in so doing, shall first state what the result was in each locality visited by me, and then calculate the general result of all the cases that I attended.

As already stated, my labours commenced in the town of Lucea: but as the epidemic had prevailed in this town for two weeks previously, many of the patients that I visited had been prescribed for by the district visitors in the absence of the resident medical men. Under these circumstances, it was not possible, in all cases, to change the mode of treatment, or to employ my own remedies. There were, however, 100 patients who took the effervescing draughts through the whole, or the greater part, of the attack. Of these, 84 recovered, and 16 died; being a ratio of mortality of exactly 16 per cent. "In the above calculation, those patients that I saw and prescribed for at the dispensary, amounting to 60 or 70 of rice-water purging, have been omitted, as no book was kept there for the entry of such cases. As the probability is that I should have been sent for, had the disease proceeded to the next stage, as was the fact in some instances, we may conclude that the majority of these cases were recoveries. If so, another

50 ought to be added to the amount of recoveries before given; which would diminish the rate of mortality again, making it 10 instead of 16, and raise the proportion of recoveries to 90 instead of 84.”* As, however, the actual number of recoveries and deaths is uncertain, I shall take the former ratio as the basis of my calculation, instead of the latter.

In order to ascertain if the above result be favourable or unfavourable, it is desirable to have some standard to which to refer; derived, not from other towns or other visitations, but from the very spot in which the disease prevailed. As, however, the majority of the patients were attended by the district visitors who made no returns, and as those sent in by the other medical men were imperfect, it is impossible to ascertain what was the general proportion of deaths to cases. But as 31 per cent. of the gross population perished, and as we may presume that two-thirds of those attacked died—a calculation within the mark, for there were, I believe, but few recoveries before my arrival—the rate of mortality would amount to 66 per cent. “There is, however,” as was remarked in the Report just referred to, “another standard to which perhaps it would be fair for me to refer, viz., the Cholera Hospital. Now it will be found, by a reference to the hospital returns, that there were 185 admissions, 124 deaths, and 61 discharges. Of these

* Extract from the Report sent in by me to the Hanover Board of Health.

8 appear to have been taken out again uncured, or before the treatment was concluded, so that the actual number of recoveries amounted to 53. According to this calculation the ratio of recoveries would be 30, and of deaths, 70 per cent." Compared to that obtained by me, under what I consider to be nearly similar circumstances, the difference amounts to 44 per cent.*

At Cousin's Cove, the locality next visited by me, there were 50 cases and 5 deaths. As such, the ratio of mortality is 10 per cent.—4 per cent. lower than at Lucea. This difference must be ascribed to the fact that, the majority of the huts being near to the house of the proprietor, I was enabled to attend all the cases myself; while they were seen, in general, at an earlier period of the attack. There were, however, two cases that I did not see until they were in a dying state, and another that died before I arrived at the house; but such accidents will occur, at all times and in all situations.

That the variation, in the rate of mortality, did not arise from the lesser intensity, or the type, of the

* In making this comparison, I have taken into account 10 cases that ought to have been excluded, as some of these were in a dying state when first seen, while the remainder had taken other remedies before the antidote was administered. Although the calculation is a fair one, when taken as a comparison with the Cholera Hospital Return, it does not give the exact ratio of mortality by the antidotal treatment.

disease may be concluded from the following extract, contained in my Report to the Board of Health:—
“On my visit here, on the 30th ult., it was stated to me that there had been, up to that period, 10 deaths and *no recoveries*.* There were, however, 8 patients then under treatment, of whom one (John Crooks) has since died.” In addition to this, I may observe that the type of the disease was as severe, and the rate of mortality as great, during the last as during the first period of my stay there.

At Lance's Bay, there were 24 cases of Cholera and 2 deaths. Although the majority of the patients lived at some distance from my house, and I was seldom able to visit them more than once a-day, the ratio of deaths is only 8·33 per cent. The success of the treatment, therefore, must be ascribed to my assistants, one of whom resided on the spot. That the favourable result was due to the treatment adopted, and not to the mildness, or type, of the disease, I would infer from the simple fact that, in other instances, in which a different mode of treatment was adopted, the majority of the patients *died*.

* These patients were all carried off in the course of a few days; the first death having taken place on the 20th. The treatment adopted was the Liverpool mixture (Sp. Terebinth. Sp. Camphor. Sp. Lavand. Co. ā ā drs. iij. Ol. Menthæ. P. m. xxx.—a teaspoonful every 2 hours); or the chalk mixture and opium, in the preliminary diarrhœa, and calomel and the saline mixture of Dr. Stevens, in the severe form of the disease.

Thus there had been, previously to my first visit to this settlement, 14 deaths; and there were 6 more, not attended by me, subsequently to this. Several deaths occurred, also, after I ceased visiting the district. Three of these were attended by Dr. Bremner, and the others by an individual who acted under the orders of this gentleman, and afterwards as my assistant.* He assured me that they all had medicine—the remedies employed being principally calomel and opium, or calomel alone.

At Davies's Cove, the cases amounted to 17, and the deaths to 2—the ratio of mortality, consequently, is 11.17 per cent.† As there had only been one death prior to my visit, it is not possible to draw any local comparison as regards the result. I may add, however, that the type of the disease was quite as severe here as in the other portions of the district; while the disease itself, in both the fatal cases, was of a very malignant form, as evinced by the short duration of the attack in these cases, viz., ten and fifteen

* One was the aunt of a patient that I attended. She was quite well at my visit in the middle of the day, but was attacked at ten p.m., and died at three a.m. the next day—five hours only. She had medicine sent to her immediately by Dr. Bremner, who was near there at the time, but he did not visit her. On the following day, the husband was attacked, and he also, although attended by Dr. Bremner, died in the course of a few hours.

† At this small settlement, situated about a mile from my residence, I had no assistant located.

hours. In addition to this, of four cases attended by another practitioner, after I left the district, two died and two recovered—being a mortality of fifty per cent.

In the Green Island district, which I next took charge of, there were no deaths among the patients attended by me. The whole of the cases, however, which occurred previously—five in number—proved fatal. They were attended by Dr. Lyon, and the treatment pursued was nearly the same as that at Cousin's Cove, already detailed. Some of them had effervescing draughts administered to them as well; but they were not resorted to until collapse had supervened, and the case had become hopeless.

In the Glasgow district, there were 103 cases and 12 deaths. As such, the rate of mortality is 11·65—being 1·65 per cent. higher than at Cousin's Cove. This is to be accounted for from the fact, that the population was scattered over a wider space; while the majority of the patients lived on hills, almost inaccessible at night, and occasioning the loss of much time during the day. As there were only two deaths before my arrival, and as they did not receive proper medical aid, no comparison can be drawn as respects the above rate.

At Kendal, my cases amounted to 66, and the deaths to 8: the rate of mortality consequently is 12·12 per cent. Here, there had been, previously to

my assistant taking charge of the district, 8 deaths within four days. I saw three of these patients, but it was then too late to attempt any kind of treatment. One, in fact, died ten minutes after I entered the house, and the other two within half an hour. They had been attended by the district visitors—a self-appointed Committee—and the remedies employed were the turpentine mixture, calomel, and the saline mixture of Dr. Stevens. There were not, that I am aware of, any other cases attended by the above individuals during this period; if so, *all* the persons attended by these visitors *died*.

At Flamstead, where the cases amounted to 81, and the deaths to 12, the ratio of mortality is higher than in the other two portions of the district—being 14·62 per cent. There had been four deaths previously to my arrival, but I am unacquainted with the number of recoveries: no comparison, therefore, can be drawn with my own cases in this locality. As regards the higher rate of mortality in these two districts, it may be observed that I had an assistant in each district, but I resided entirely at Glasgow. The patients, therefore, were more under my immediate control in the latter locality than at Flamstead and Kendal.

In the Mint district, of which I had charge subsequently, there were 167 cases and 16 deaths. As such, the rate of mortality is 9·58 per cent. This rate, although higher than that obtained at Lance's Bay, is below

that of all the other districts, contrary to what might have been anticipated.* Here, as elsewhere, no opportunity was afforded me of drawing a comparison with the result obtained by other modes of treatment; for although Mr. Murphy had charge of the district for a fortnight previously, I did not ascertain what had been the number of the cases. I only know, that during this period there had been eight deaths—about half the number that were subsequently carried off, during a period of two months. This greater relative mortality could not have been caused by the greater intensity of the disease at the commencement of the epidemic, as I had no death until a week after taking charge of the district. It was *then* that the disease put on its most destructive and malignant form.

At Mount Eagle, there were 110 persons attacked with Cholera, and 11 deaths, being a ratio of 10 per cent. Although the disease in this locality was of the most malignant type, the rate, nevertheless, is below that obtained in some other districts.† This favourable result must be ascribed to the alteration in the treatment—

* Nearly all the patients lived on a low, marshy, or wet soil, forming part of the plain of Westmoreland.

† That the disease assumed its severest form in this instance we may conclude, not only from the circumstances before referred to, but, also, from the shortness of the attack in the fatal cases, the majority of the patients having been carried off within twelve hours.

the antidote having been given in the first, or preliminary, stage of the disease as already mentioned.*

Although unable to make any comparison in this particular district, I may be able to draw sufficiently correct inferences from the result obtained by other practitioners in other parts of the parish—the elevation, soil, &c., being nearly the same in all. The parish of Westmoreland is, in fact, one vast alluvial plain, abounding in swamps and morass—intersected by only a few hills, and these at no great elevation. There are, nevertheless, certain circumstances which prevent my drawing a comparison with *the general* result obtained in this parish, and for the following reasons.

In the first place, two of the medical men adopted the same mode of treatment as myself. In making a comparison therefore with other modes of treatment, it is necessary to withdraw them entirely from the calculation. There were others, again, who adopted a modification of my own treatment—either employing effervescing draughts, in combination with other remedies, or, else, agents which, as I infer, produce the same effect; such as the carbonate of soda, and

* It is right to observe that, of the recoveries, three had relapses, and died—one on the following day, another a fortnight after, and the third three weeks after. They had, however, removed, previously to this, into Mr. Murphy's district, and were attended by him or his assistant.

the other alkaline carbonates. At the Cholera Hospital, also, there was a variation in the treatment. The remedies employed, at the commencement of the epidemic, were the Liverpool mixture in the first stage of the disease, and calomel and opium in the subsequent ones. In consequence, however, of the apparent failure of these agents, and the injurious effect of the opium—the majority of the patients dying comatose—they were abandoned, and the saline mixture and calomel, without the opium, substituted for them. In addition to this, one of the gentlemen who adopted the same mode of treatment as myself was appointed to the hospital, at the height of the epidemic, in conjunction with the two practitioners who had previously attended. These circumstances would necessarily cause a difference in the result, at these two periods. For these reasons, I have, in drawing up the following Table, divided the returns into three sections, or classes, according to the treatment adopted; so as to enable others to form their own opinion, and their own conclusions on the subject.

TABLE H.—OFFICIAL RETURN OF CHOLERA CASES
IN THE PARISH OF WESTMORELAND.

	Cases.	Deaths.	Deaths Per Cent.	Treatment.
1. Cholera Hospital, to July 6.....	69	49	70·1	{ Liverpool mixture. Calomel and opium. Acetate of lead and opium. Calomel and opium.
2. Dr. A. (rural district)...	215	145	67·90	
3. Dr. B. " "	92	58	63·54	
Average rate			67·15	
4. Dr. C. (Sav.-la-Mar) ...	104	60	57·69	Mixed treatment.
5. Cholera Hospital, to July 14	32	16	50·	" "
Total cases, from June 23 to July 22.....	163	93	57·5	" "
6. Dr. D. (rural district)...	688	319	46·36	" "
7. Dr. E. " "	214	70	32·71	" "
8. Dr. F. (Sav.-la-Mar) ...	192	38	19·79	Effervescing draughts.
9. Dr. G. (rural district)...	503	90	17·89	" "
10. Dr. Parkin " " ...	277	27	9·74	" "

NOTE.—According to the returns, there were only 965 deaths; but, according to the "Colonial Standard," there had been 1,500 up to the same period. As the parish was divided into districts, and a medical man appointed to each, I am at a loss to understand the cause of the omission. I can safely affirm, that not a single person died in my own district, whose name was not returned to the Board. There were only 5, in addition to those attended by me or my assistants, thus making 32 in all. In the town of Savanna-la-Mar, also, there were between three and four hundred deaths, but by the above Reports only 191 are accounted for. There was another practitioner in the town, unconnected with the Board of Health, and who, therefore, sent in no return, but this circumstance is insufficient to account for the difference.

In drawing a comparison with my own mode of treatment, I am necessarily restricted to the examples

inserted in the first division of the Table. If we take the average of these results, and then compare it with mine, the difference amounts to 57·41—within a fraction of $57\frac{1}{2}$ —per cent. Compared with that obtained by the individuals who employed the same remedies, the result is less favourable; but, even in these instances, the difference amounts to nearly 50 per cent. in favour of the mode of treatment proposed by me. This difference will be still more apparent, if we compare the result with particular modes of treatment. For instance, I have assumed that in the return (No. 2) the acetate of lead and opium were the only remedies employed; whereas I have reason to believe, that the above result does not give a correct idea of the mortality which occurred under this mode of treatment. During a visit which I paid to one estate in this gentleman's district, and where an accurate account had been kept of the cases that occurred, I learnt that, from the 30th June to the 9th July, 33 individuals were attacked with Cholera. Of this number 29 died—being a mortality of 87·87 per cent. The whole of these patients were treated alike—the acetate of lead and opium being the only remedies employed—and they were all attended by the above-named practitioner or his assistants. Either, therefore, the type of the disease changed, or the treatment was modified towards the termination of the epidemic—otherwise the general result would have

been still more unfavourable than that inserted in the Table.*

Having thus ascertained the rate of mortality in the different districts under my charge, we may now consider what has been the average rate with all the patients treated by me.

In the following Table, the total number of cases, of deaths, and of recoveries in the different districts before-mentioned, and which were attended by me, or by my assistants, has been given. The patients not attended by me, and those who died without receiving any medical aid, have been excluded.

The cases in this Table have been separated into classes, according to the symptoms, excepting that the cases of partial collapse have not been specified. It was, in fact, impossible to ascertain the exact number of such cases, visited, as the majority of them necessarily were, at long intervals. If, however, we were to take the half of those in the third column, suffering from rice-water purging, vomiting, and cramps, the calculation would not be very far from the truth.

* At Savanna-la-Mar, also, there was, it would seem, a much greater variation in the ratio of mortality, at the commencement and termination of the epidemic, than what the returns indicate. According to public rumour, there were no recoveries at the commencement of the outbreak in this town; while the Correspondent of the *Daily Advertiser* stated that up to July 12, there had only been 6 recoveries out of 230 cases.

TABLE I.—ABSTRACT OF CASES, DEATHS, AND RECOVERIES,
IN THE BEFORE-MENTIONED DISTRICTS.

Names of Places.	Rice-water Purging.	Do. and Vomiting.	Do. do., and Cramps.	Collapse.	Total.	Deaths.	Recoveries.	Per-centage of deaths.	Cases of Diarrhœa.
Lucea	18	27	8	47	100	16	84	16·00	...
Cousin's Cove	22	8	5	15	50	5	45	10·00	28
Lance's Bay.....	10	2	8	4	24	2	22	8·33	7
Davis's Cove	5	3	3	6	17	2	15	11·17	15
Green Island	15	4	6	3	28	...	28	48
Glasgow	24	23	25	31	103	12	91	11·65	26
Flamstead	8	29	16	28	81	12	69	14·62	29
Kendal	4	46	5	11	66	8	58	12·12	10
The Mint.....	68	29	34	36	167	16	151	9·58	90
Mount Eagle	40	29	18	23	110	11	99	10·00	2†
Total.....	214	200	128	204	746	84	662	11·26	255

* I cannot state what number of cases of Diarrhœa there were in this town, as no book was kept at the Dispensary, or lists made by the District Visitors. In the other localities, the numbers inserted in the Table only include those that I prescribed for; they do not include the whole in each locality.

† There were 17 cases of colic that did not proceed any further, the symptoms being arrested at the onset.

By a reference to the above Table it will be seen, that the total number of cases of actual Cholera attended by me, exclusive of the Dispensary cases at Lucea, is 746. Of this number 84 died. As such, the average rate of mortality is 11·26, and of recoveries, 88·74 per cent. Including the Dispensary cases, it would be 10·52 and 89·48 respectively.

Such being the result of this particular method of treatment, it becomes an interesting subject of inquiry to ascertain what the general result was in

Jamaica with other, or the ordinary, methods. Although it is notorious that the rate of mortality was very high in this island, there are, unfortunately, few general or statistical facts to refer to in proof of this assertion. Daily and weekly returns were sent in to the local boards at first, but they were speedily abandoned—the medical men, in general, having little time to attend to such details. In only one instance, with the exception of Westmoreland, referred to before, do they appear to have been continued, or at least published, regularly and faithfully from the commencement to the termination of the epidemic. This was in the parish of St. Thomas-in-the-East—30 or 40 miles from Kingston. These returns were published in the local newspapers at the time, and from them I have constructed the following Table:—

TABLE K.—ST. THOMAS-IN-THE-EAST: CHOLERA RETURNS.

Places.	Cases.	Deaths.	Recoveries.	Ratio of deaths per cent.
Golden Grove, to Nov. 22	97	79	18	81·44
Do., to Nov. 29.....	216	168	48	77·77
Average.....				79·60
Plaintain Garden District, to Nov. 22.....	268	230	38	85·82
Do., to Nov. 29.....	575	503	72	87·65
Average.....				86·73
Total for the two districts, to Dec. 10	1049	750	290	71·40
Average for the three periods ...				79·26

It thus appears, according to this Table, that the ratio of deaths to cases was less towards the termination of the epidemic. Whether the difference arose from a variation in the type of the disease, or the adoption of a more successful mode of treatment, I am unable to say, no mention being made of the remedies employed. There were three medical men in the district, and we have the assurance of one of them, that all the cases were regularly and properly attended. This we can readily believe, inasmuch as the rate of mortality, high as it may appear, is lower than that obtained in many other localities, possessing the same advantages as regards medical aid. Thus, at Rio Bueno, which I passed through shortly after the termination of the epidemic, it was stated to me that there had been 73 deaths, and only 90 cases—being a ratio of mortality of 81.11 per cent. At Port Maria, where, as was before stated, two-thirds of *the population* died, the rate must have been still higher—not 50 persons being left in the town on the termination of the epidemic. At one particular settlement, near Caron Hall, “one of the highest and best cleared places in the neighbourhood,” observes the Rev. John Cowan, “in spite of medicines and precautions, the exertions of the doctor and my own, the infection spread like an uncontrollable fire. So deadly was the disease, that only 1 in 20 recovered.”* The Rev. J. Campbell, also, writing from Goshen, says: “So

* *Missionary Record*, March, 1851.

virulent is the type of the disease, that by far the greater number of those who have been attacked—probably 3 out of every 4—have died. It seems to defy the power of medicine. Indeed, the greatest exertions of the medical man, who has been appointed for the district, have been almost wholly useless.”*

With such facts as these before us, we cannot err much in concluding that three-fourths of those attacked—or 80 per cent.—were cut off in Jamaica. More than this, we may also infer that this high rate of mortality is to be ascribed to the intensity of the operating cause, not to the improper application or the absence of remedial means. That such was the case I would infer from the fact, that the result is not so materially different to that which was obtained in Europe, by the modes of treatment generally resorted to in Jamaica. This will be apparent by a reference to the Table previously inserted (Table A, p. 44), and to the lower division of that table: the rate for those methods of treatment being 70 per cent.

In 1832, the average mortality in Europe, including

* “Without venturing to question the purposes of the Most High, which no man dare venture to do, we may inquire whether any terrestrial, household, or personal causes exist, thus to sustain and propagate an epidemic of such fearful virulence and fatality, in a manner seldom, if ever, before witnessed in any part of the globe. . . . The question we have put is deserving the utmost attention of all interested in the welfare of mankind—what is their fate to-day, may be ours to-morrow.”—On the Cholera in Jamaica: *Medical Times*, October 25, 1851.

all methods of treatment—the most efficacious as well as the most inefficacious—was 50 per cent. But in the second invasion, when the disease assumed a more severe form, the rate, in particular instances, was as high as in Jamaica. Thus we find, by a Report presented to the Municipal Council of Brussels in 1849, that, of 1,244 cases, there were 1,013 deaths—being a ratio of 80 per cent. At Toulon in the south of France, the ratio was as high as 91·74 per cent.—1,260 persons having died out of 1,373 attacked. At Moscow, also, during one period, the deaths amounted to nine-tenths; but, afterwards, sunk to two-thirds.

In Asia, there are no statistical returns to refer to of the proportion of deaths to attacks, excepting in India; while these are for the troops, not for the native population. To draw a comparison between these men and the negroes would not be a fair one; it being certain that the mortality is always less with soldiers than with the civil population. The principal reason for the lower rate of mortality with European soldiers when compared with other classes is, that they are seen at an earlier period of the attack, and have the benefit of the best medical aid and resources. Not only are they admitted into hospital for the slightest ailment, but they generally apply for relief at the earliest period; either from a dread of the disease, or in order to escape their duties, which, at such a time, are frequently increased. As these

anomalous pseudo-cases are returned under the head of Cholera, in the official Reports, their addition, as Dr. Parkes has remarked, swells considerably the list of recoveries. Had these anomalous cases, or those of diarrhœa, been separated from the others, a different result would have been arrived at.

Thus, of the patients treated by Dr. Parkes in 1846, 56 per cent. died. So, again, in the fearful outbreak of the Cholera at Kurrachee in 1846, the mortality in Her Majesty's 86th Regiment was 58·5, and in the 88th Regiment, 59·36 per cent.* Among the European women and children, the rate was 61·67 per cent. During the visitation to which Her Majesty's 4th Regiment was exposed at Bellary, in 1840 and 1841, the ratio of deaths among the men was 60, and with the women and children 100, per cent.† In 1862, the rate of mortality from Cholera in the 93d Highlanders, of the cases treated, was 80 per cent. with the officers, and 64 with the men.‡ In 1863, the average ratio was 75 in Bengal.

There is another circumstance that renders a comparison between the European or Hindoo soldier and the negro difficult, if not unfair. Although enjoying an almost total exemption from fever—the

* This was exclusive of diarrhœa, the cases of which amounted to 171. The total cases of cholera were 410.

† Report by Dr. Parry.

‡ *Army Medical Report* for 1862, p. 421.

endemic of his country—he is more than usually susceptible of other diseases, when, by removal or other causes, he is brought within the sphere of their operation. Thus it appears, from the army medical returns, that in Ceylon, where there was a regiment of African blacks, the deaths from fever only amounted to 1 per 1,000 annually; while the other coloured corps, including Malays and natives, suffered a loss of from 4 to 11 per 1,000. On the other hand, while the loss among the native corps from diseases of the liver and abdominal organs amounted only to a fraction, the deaths among the African troops, from these causes, were as high as among the former from fever. The compiler of the above Report states, also, that, in the Mauritius, the negro suffers from liver complaints to a greater extent than the whites, or Europeans.

As such, we cannot be surprised to learn that the same individual, during visitations of the Epidemic Cholera, should suffer more than any other class. In Ceylon, although the returns are imperfect, the difference during one particular visitation was as 10 to 1, when compared with the native troops. But at the Mauritius, in 1819, out of 1,327 deaths, 997 were blacks, 162 coloured persons, and 168 whites; or, $75\frac{1}{2}$ per cent. of the former, and only 12 and $12\frac{1}{2}$ of the latter. We know nothing of the ratio of cases, but it is evident from the large proportion of deaths, when compared with other classes, that the negro is pecu-

liarily predisposed to the operation of the cause, whatever that may be, productive of the Epidemic Cholera.

Assuming these inferences to be correct, we arrive at a very important conclusion: this is, that the high rate of mortality in Jamaica cannot be ascribed to the absence of remedial means, nor yet to want of skill and experience in the practitioners of that island; for the rate is not materially different to that which was obtained, by the same methods of treatment, in Europe and in India. The difference can easily be accounted for by the greater intensity of the disease, and the want of that vital energy in the negro to resist the operation of the malign influence, which Europeans appear to possess. It was necessary to ascertain this point before drawing any deductions, with respect to the result obtained by other practitioners and myself in Jamaica.

It has been before inferred that the average rate of mortality in Jamaica, of the cases treated, was 80 per cent. Granting this, the difference between that rate and mine is 68·74 per cent.: or, in other words, out of every 100 patients treated, 68 more were saved by my method of treatment than by the ordinary methods.

Having thus ascertained the general rate of mortality, it will be instructive to compare the proportion of deaths in the severe form of the complaint, or, in the stage of collapse. By referring to the

Tables inserted in my Report—Statistical Report of the Epidemic Cholera in Jamaica—it will be seen that, of the 84 deaths, 40 were in a state of collapse when first seen, and the remainder in the second stage of the disease,—the proportion being 47·62 of the former, and 52·38 per cent. of the latter. Again, out of the 664 recoveries, 120 passed through the stage of collapse—34 being in that state before the commencement of medical treatment, and 86 in the previous stage; or, 28·33 per cent. in the former class, and 71·66 in the latter. The time at which the collapse supervened, in each individual case, has been noted in the Tables.

By referring to these Tables, it appears that, with the recoveries, the average duration of the attack, exclusive of the diarrhœa, *before the commencement of treatment*, was five hours, forty minutes, and forty-nine seconds. Of those cases, however, that were in the second stage of the disease, when first seen, and that afterwards fell into a state of collapse, the average duration was six hours, fifty-seven minutes, fifty-one seconds, being a difference of one hour, seventeen minutes, two seconds. This circumstance, therefore, will account for the supervention of collapse in the latter instances; it having been found that the loss of one hour and thirteen minutes, in India, was sufficient to prevent the recovery of the patient.* In the fatal cases,

* In Mr. Balfour's interesting pamphlet, it is stated that of 377 cases, which occurred in the Madras N.I., the mean duration of

however, the duration of the attack was less than with the recoveries, contrary to what might have been expected. The average time, in these cases, was three hours, sixteen minutes, and twenty-one seconds—only half that of the recoveries. This apparent anomaly, however, admits of a ready explanation. The majority of the patients that I lost in Jamaica were attacked suddenly with the disease in its severest form, falling almost immediately into a state of collapse. Although seen, therefore, at an earlier period of the attack, the state in which the patient was, at the time, prevented my accomplishing as much as in the less rapid cases. With these brief remarks, we may now inquire what the ratio of deaths and of recoveries was with this class of patients.

The total cases of collapse are 204; 120 recoveries and 84 deaths. Taking those cases that were in a state of collapse when first seen, the ratio will amount to 54·05 per cent.; while, of those that fell into this state after the treatment had been commenced, only 33·84 per cent. died. Including both classes, the ratio is 41·17 per cent. What the rate of mortality of the same class of patients was with other practice the disease, before admission into hospital, was 5 hours, 2 minutes, and 4 seconds. It appears, further, that the average duration of the fatal cases was 5 hours, 41 minutes, and 20 seconds; while it was only 4 hours, 28 minutes, and 12 seconds with those that recovered. The difference, therefore, of 1 hour, 13 minutes, and 8 seconds decided the question of life or death.—*Statistics of Cholera in the Madras Army.*

titioners in Jamaica, I am unable to say, as no facts of this kind, that I am aware of, were made public before my departure from the island. Judging, however, from what I saw and heard, we may conclude that very few recovered after the supervention of the state of collapse. It is probable, therefore, that this rate, high as it may appear in comparison with my general one, would still be as favourable as the latter, if compared with the same class of patients treated by other methods.

We will now inquire what the ratio of deaths has been with this class of patients in other quarters of the globe. There are no general returns, that I am aware of, either in India or in Europe, of the proportion of recoveries, after the supervention of the state of collapse; while, in the isolated examples to be met with, the detail is often incomplete. It appears, by the army medical returns, that of 60 patients at Ceylon, in 1832, who were in a state of collapse, 38 died—being a ratio of 63·33 per cent. We are not informed what the symptoms were, when first seen; or, how many were in a state of collapse *before the treatment was commenced*; nor what was the duration of the stage of collapse. Judging from the result, as well as other circumstances, we may conclude, that this state supervened, in the majority of the cases, after the commencement of medical treatment. That this inference is a correct one, may be presumed from the fact that, of 12 patients in the 24th N.I., treated

by Dr. Lorimer, who were in a state of collapse when first seen, *only one recovered*—being a mortality of 91·67 per cent. Comparing these results with that obtained by me, there would be a difference, in the one case, of 22·16, and, in the other, of 37·62 per cent. As the latter is the only instance I have met with, among the returns in India, in which the distinction has been drawn, it is impossible to carry the comparison farther, as regards that country.

In Europe, it might be possible, by a certain amount of labour and research, to arrive at some general idea of the proportion of recoveries, in cases of collapse, but only isolated instances are to be met with. As, however, the disease in Europe has hitherto presented a comparatively mild form, the comparison is hardly a fair one. Notwithstanding, the difference is very marked. According to Dr. Griffin there were 625 cases of collapse in the Limerick Hospitals, with 481 deaths—a ratio of 76·96 per cent. Compared with the result obtained by me in Jamaica, there would be a difference of 22·91 per cent. in favour of my mode of treatment—a great difference when the type of the disease, in these respective instances, is taken into account.*

* As Dr. Griffin has remarked : “The reports transmitted to the London and Dublin Boards of Health from all parts of the kingdom are entirely useless as materials for medical reasoning, the distinction of cases as they happened to be admitted, with or without a perceptible pulse, having never been observed. If it

Although the result obtained by me in Jamaica in 1851 is much more satisfactory than could have been anticipated, there is yet one circumstance that requires a few words of explanation. It has been stated, both by myself and other writers, that the administration of the antidote, before the supervention of collapse, has always been sufficient to arrest the progress of the disease. Such, in fact, was the case in Europe; but in Jamaica, as we have seen, the same favourable result was not always obtained. There, of 672 patients, who were in the second stage of the disease, 130, or 19·34 per cent., fell into a state of collapse; and 44, or $6\frac{1}{2}$ —6·54—per cent., died. This difference in the result may be referred to two circumstances: the severity of the disease, and the maladministration of the remedy. When the rapidity with which the disease ran its course is taken into consideration, there will be little difficulty in accounting for the supervention of collapse in these cases, without reference to any other circumstance. But the result, in many instances, may be ascribed to the want of care and attention in administering the draughts, and to the carelessness and ignorance of the attendants and friends, in not following the directions given to them. All the patients, with the exception of those in the

was the general design to mystify the subject, and prevent the possibility of fair comparison under different modes of treatment, it could not have been more effectually done than in those loose and useless records."—*Medical Gazette*, vol. 21, 1838, p. 691.

town of Lucea, being in country districts, it was impossible that I or my assistants could superintend the first administration of the draughts in the majority of cases, our time being more particularly occupied with the severe cases. And yet, almost everything depends on the proper employment of the remedy; for if the gas is allowed to escape before the patient takes the draught it will be all but useless. That this cause had much to do with the result under discussion, will be rendered probable from the following extract, taken from the *Trelawney* (Jamaica) newspaper, of February 2nd, 1852:—

“The following method of treating Cholera has been transmitted to us by Mr. James H. Casely, a gentleman by whom it was employed with much success; and whose exertions in the cause of suffering humanity will ever be remembered in Hanover with feelings of the deepest gratitude. It is known as Dr. Parkin’s treatment:—

“With regard to the use of the effervescing draughts, I affirm, and am prepared to prove that, in cases of Cholera, they, as Dr. Parkin says, “PREVENT COLLAPSE.” In many cases the patients have died from having discontinued the use of them, because, after having taken a few, say ten or twelve, doses, the purgings have not been checked. I have in the course of a day—in about nine hours—given from 30 to 40 doses, with success; and I now have to state, in support of the foregoing (Dr. Parkin’s) treatment,

that I don't remember a case among the many of which I have had the treatment for the last twelve months, when the patient had the foregoing treatment for *two* hours, without getting into collapse, that he ever did, after that time—the treatment being continued—get into that state.' ”

The truth is, this remedy, like all antidotes, is intended to remove the cause, *not* to remedy effects, as has been previously pointed out. When the question has been put to me, Is Carbonic acid a remedy for the collapsed stage of Cholera? my answer has been—No; it is not a remedy *for* collapse, but a remedy *to prevent* collapse. This is a distinction which ought always to be borne in mind; it being certain that this particular state would seldom be witnessed, if we possessed the means of arresting the progress of the disease in its first stages; as it rarely happens, except in situations where the population is without sufficient aid, or in inter-tropical regions, that a patient arrives at the state of complete collapse, before the commencement of medical treatment. If so, and if we possess a certain remedy for the Epidemic Cholera in its first stages, as it has been my purport to show in this work is the fact, few or no cases of collapse would be witnessed—provided that this particular remedy were resorted to in time.

As Dr. Lewis has rightly observed: “The question at issue is, what mode of treatment will save the greatest number of lives *in the early stages*; for when

the serum of the blood is all drained, and the process of death has set in, of course no method would restore life in such a case: it is then too late. Therefore, if the medical man be called in sufficiently early, he is, in some measure, morally responsible for the safety of his patient."—On the Treatment of Cholera on Specific Principles.*

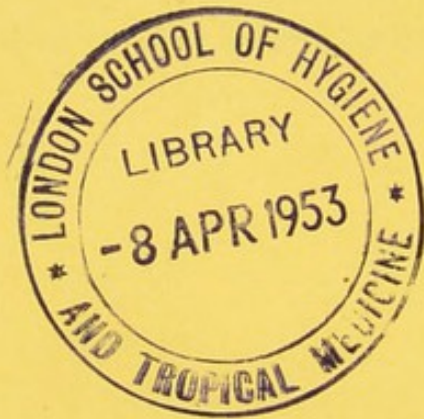
As an example, I may refer to the official lists at Valencia, Spain, by which it appears that, out of 5,115 attacked, 3,582 were slight cases when first seen. Now I am bold enough to affirm, that nearly all these cases, or *ninety-nine out of every hundred*, would have recovered by the adoption of the plan of treatment before proposed. If, also, we allow that one-half of the severe cases might have recovered by the employment of the same remedy—and which a reference to the statistical facts before adduced will show is not too high an estimate—instead of 3,854, the actual amount, the number of deaths would not have been more than 800.

If, however, medical men, either from confidence in their own measures or from any other cause, refuse to resort to this remedy until the state of complete and confirmed collapse; when the blood has ceased to circulate in the body, perhaps, for hours; when absorption is utterly impossible; and when liquids, poured into the stomach, fall as into an inert vase,

they have an undoubted right to do so; but they will have no right to say, afterwards, that they have given the remedy a fair and proper trial. Such a trial would be not only useless in itself, but an injustice to me, and the means of preventing, as far as they are individually concerned, the solution of a problem that has hitherto baffled the skill and the talent of the medical world; and in which the greater part, if not the whole, of the human race is more or less deeply interested.

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