Memoirs of West Indian fever : constituting brief notices regarding the treatment, origin, and nature of the disease commonly called yellow fever / by John Wilson.

MEMOIRS

OF

WEST INDIAN FEVER;

CONSTITUTING

BRIEF NOTICES REGARDING

THE

TREATMENT, ORIGIN, AND NATURE OF THE DISEASE

COMMONLY CALLED

YELLOW FEVER.

BY

JOHN WILSON, M.D. R.N.

"In an inquiry, it is almost every thing to be once in a right road.—A man who works beyond the surface of things, though he may be wrong himself, yet he clears the way for others, and may chance to make even his errors subservient to the cause of truth."

BURKE'S PHILOSOPHICAL INQUIRY.

LONDON:

BURGESS AND HILL, MEDICAL BOOKSELLERS,

GREAT WINDMILL STREET.

1827.

LONDON:
PRINTED BY CHARLES WOOD AND SON,
Poppin's Court, Fleet Street.

TO

JOHN WEIR, M.D.

AND

WILLIAM BURNETT, M.D.

Medical Commissioners of His Majesty's Navy,

&c. &c. &c.

GENTLEMEN,

PLACED at the head of that branch of the public service to which I belong, and having seen and studied the subject on which I write, you are at once the most legitimate and fit judges of the merits of these Memoirs. You are anxious to improve Naval Medicine, and thereby to increase the benefits of the healing art generally: it is therefore a matter both of duty and pleasure to dedicate this tract to you. And I take this public opportunity of expressing, with how much respect and esteem, I am yours, faithfully,

JOHN WILSON.

PREFACE.

It is my chief object, in the following pages, to point out the difference between the *inflammatory* and *congestive* modifications of West Indian fever; and to propose the principles of practice which ought to guide us in the management of each. After all that has been published on the subject of the disease generally, this is a task which has seldom been attempted, is yet but very imperfectly performed, and is certainly of the greatest importance. The majority of writers have directed their attention principally to questions regarding the origin and abstract nature of the disease; matters, which, though not without interest and value, ought to be considered secondary and subservient to the paramount business of treatment: while those authors who have made the application of remedies their leading object, have, in my opinion, considered the subject without due discrimination; they

have generally represented the disease as one, not only in its essence and nature, but also in its development and results; and have prescribed a single line of treatment, suited to unity of action. But if my views of *double* action be just; if the cause of this fever, though one, has the power of producing the symptoms of *inflammation* and *congestion* respectively, it follows, that the system of treatment which does not recognise, and is not influenced by such division, must be injudicious: it may have happy results, and certainly often has, but such results can be considered fortunate merely; the process which led to them was not conducted on true principles, and the effect can be considered happy only by accident. I have often been asked, and that within the present week, what I thought the best method of treating West Indian fever; whether by blood-letting, mercury, or purgatives? The inquirers seemed to think it as simple a matter to answer the question, as to decide, in extracting a tooth, whether the instrument ought to be turned inward or outward: and so, according to some writers who have chosen a particular line of treatment applicable to all cases, it might appear. In my view of the subject, such questions cannot be

answered so categorically. All these remedies, and many more, are not only useful, but requisite; they may, or may not, be all required in single instances; it will even happen, that the same remedies are useful both in the *inflammatory* and *congestive* modifications; then, however, they must be employed for different immediate purposes; and when properly adjusted to particular conditions, in their time, manner, and measure of operation, they will contribute, by different or opposite modes of action, to the same general result, the restoration of health. The difference in question, both as regards condition and treatment, appears to me a matter literally of vital importance; I have therefore considered it at some length, and stated what I think sufficient for understanding the distinction.

In thus expressing myself, I trust I shall not be thought guilty of acting petulantly or offensively towards those who have preceded me. For many of them I feel great respect; indeed it were impossible not to respect men, who have toiled, and suffered, and studied, earnestly and conscientiously, amid the miseries of this disease, that they might mitigate human suffering, either immediately by personal labour, or remotely by writing. But in

such an undertaking, no one man can accomplish every thing. While men of great talents are busy investigating one part of an intricate subject, inferior minds may perceive things in another part of it, which they had not an opportunity of seeing; and I may therefore have observed things in the manner of the disease's operation, which had escaped others, without arrogating to myself any extraordinary portion of penetration or industry.

No man has laboured so long, so assiduously, and successfully, as Dr. Robert Jackson, to divest West Indian fever of obscurity, to exhibit its true pathological aspects, and determine the appropriate methods of treatment. From his writings I have derived much benefit, have followed him whenever I clearly could, and when I have dissented, have done so reluctantly, and after much consideration; for as some one (Dr. Hector M'Lean I think) remarks, "it is not safe to differ from Dr. Jackson." Yet it is mortifying to think how little his writings have been read, and how unjustly they have been appreciated. The great work on febrile diseases, which he modestly calls a sketch, never went necessarily to a second edition; for he informs us that it was reprinted for the purpose of enabling him to com-

municate the sum of his knowledge before he died, not that it was exhausted. He has been accused of writing in a quaint, obscure, and unintelligible style. I apprehend that what we condemn, as obscurity of style, arises simply from profundity of thinking. Dr. Jackson's ideas do not float on the surface, and we will not give ourselves the trouble of fathoming his depths, that we may draw knowledge from the spring-heads of his original and penetrating mind. To the untutored and indolent, the language of Newton, Reed, or Stewart would not be very intelligible, not from obscurity of style, but because the train of thinking required more attention than they were able or willing to give it. If Dr. Jackson has fallen into any "of the follies of the wise" (and who has not?) I think it is on the score of over-refinement. His very light, in this way, seems to have misled him. A more ordinary mind would have been contented with a less thorough sifting of the subject, would have viewed it more in the gross, and would therefore probably have been more useful to the mass of readers; for, after the high praise which is due to him is offered, it must be confessed, that his numerous divisions and subdivisions tend, on some occasions at least, to

perplex rather than illuminate; and I think it a misfortune, I will not call it an error, that he attempted to classify on the base of temperaments. That temperaments have much influence in modifying the disease I am convinced, but to render the knowledge of that influence available, it is necessary to study the subject in health, and this we can seldom do; if we cannot, any attempt to connect the appearances presented with temperament must increase difficulty. Respecting the cause of West Indian fever, I differ from Dr. Jackson, and have expressed my opinion freely on that subject in the second Memoir; but on the great points of practice, he certainly surpasses all those who preceded or accompanied him in the West Indies. He is gone, and I cannot be thought guilty of wishing to flatter him, or to disparage others. In some things, especially in regard to bleeding in congestive fever, I have not been able to see things in exactly the same light which he did; but whether that has arisen from our not calling the same things exactly by the same names I cannot say. Had it not been for the circumstances just stated, these Memoirs, as far as they regard practice, would never have been put before the profession; for taking his book as a

whole, I do not know whether to admire most the unweariedness of research, the intensity of zeal, or the deepness and soundness of reflection, which it displays.

The second and third memoirs are dedicated chiefly to the consideration of the cause of West Indian fever, a subject which is still surrounded by darkness and perplexities. The facts stated and inferences drawn, regarding existing opinions, appear to me conclusive; they are sufficient, I think, to show, that these opinions, though apparently well supported in some instances, and therefore plausible, are erroneous. I do not venture to think so confidently of the opinions which I have offered in their stead; that they will remove every obstruction, and overcome all difficulties, I do not flatter myself; but that they may be the means of leading to a juster train of thinking, and of opening a more certain channel of investigation, I presume to hope.

In the fourth memoir, I have considered, as briefly and comprehensively as possible, the question of West Indian fever being a peculiar disease, and have stated the reasons which have led me to think it is.

The fifth memoir contains some remarks and suggestions on the manner in which the cause of fever impresses the body: in such speculations there must necessarily be much conjecture and little proof; and this section might perhaps have as well been spared, had it not embraced practical hints and illustrations which appeared of some value, and which I was therefore unwilling to omit.

Wherever the terms *West Indian fever* occur in the following pages, they are meant to express the rapid, concentrated, continued fever, commonly called *yellow fever;* and I have adopted this illiterate and insignificant title, simply because it does not convey an erroneous idea, while it has reference to the most common endemic source of the disease. The common name of *yellow fever* is allowed to be, and clearly is, inaccurate, inasmuch as it implies the existence of a phenomenon which is not essential to the disease, and which therefore is not universal: and for the same reason, as well as others, *typhus icterodes,* and *typhus cum flavedine cutis,* are improper. Similar objections present themselves to the employment of the appellation *causus,* or *ardent fever*; for in many instances, and

those the most characteristic, and the worst, there is no ardour, at least there is no febrile ardour. In referring to the accounts of others, especially of those who consider *West Indian fever* a modification of *remittent fever*, I may have been led into mistake, and applied the name to a disease which it is not meant to express; but wherever it is applied to the disease under my own observation, it is used strictly in the sense pointed out at the beginning of the paragraph.

A professional friend, for whose opinion I have great regard, has told me, that the *Memoirs of West Indian Fever* are very deficient in arrangement, a defect which will mar whatever usefulness they may possess. Of this I am sensible: I am sensible there is little arrangement; but I do not think that the labour necessary to give a more regular and perfect form would be compensated by any benefit which would follow. Besides, it was not my intention to write a full and historical account of the disease, but merely such notices as had occurred to me. The facts and reflections were noted as they presented themselves, often amid the hurry and distraction of the disease to which they refer, and under the pressure of sickness and despondency; they

were noted at the mess table, a place where noise and confusion render study irksome and unprofitable. These considerations, though they will not screen gross errors, may palliate the minor offences of style and arrangement; they will be understood and estimated by such of my professional brethren as have served in the same place, under similar circumstances. I have sometimes repeated facts, and referred to the same places, nearly in the same terms; part of this might perhaps have been avoided; it could not altogether, without neglecting evidence which appeared important to my purpose. It may be found that I have taken for established facts or certain deductions, things, which recent discovery has proved false or erroneous. If such mistakes are found, I must plead my situation as an apology. After a long period of service in the West Indies, I have been only a month in England; and have not had time, by reading, to correct errors which may have arisen from the limited sources of information to which I had access on board ship, in a distant part of the world.

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MEMOIRS

OF

WEST INDIAN FEVER.

MEMOIR I.

Report transmitted to the Commissioners for Victualling his Majesty's Navy, &c. &c. &c., on the 1st *of November,* 1824.

IN my remarks on the last Nosological Return, for the quarter ending with July, I stated, that although there had been a considerable number of febrile attacks, they were all capable of being controlled by medical treatment, all terminated favourably, and arose from causes different from the influence which occasions the endemic fever of this country. The preceding columns* give evidence of a very different state of things during the last three months, eighteen having died on board, including three officers, of whom my excellent assistant, Mr. Davidson, was one.

The destructive disease in question made its first appearance in Port Royal harbour, on the 8th of August, two being attacked on that day, one of whom died at the hospital. On the 9th there was one case of attack; on

* Alluding to a nosological table, which accompanied the Report.

the 10th, one; on the 11th, one; on the 12th, one; on the 14th, one; on the 16th, two.

On the 17th the ship put to sea, and was out till the 22d, during which period there was no additional case of fever. After this we continued at Port Royal till the 27th, and had only one additional case of fever, *viz.* on the 26th. The day we sailed (the 27th), after getting to sea, one case was added to the list. On the 28th there were two additions; on the 29th, one; on the 30th, one; on the 2d of September, two; on the 6th, one; on the 10th, four; and on the 11th, six. From this date till the 6th of October the disease continued its progress nearly in the same proportion of attack, three, four, five, six, or seven being added to the list daily; but not with the same virulence of character, or fatality of termination, during the entire period. On the 6th of October there was one additional case; on the 8th, two; on the 10th, one; on the 17th, one; on the 18th, one; on the 25th, one; and on the 29th, one. It may, therefore, be presumed, that the material causing the disease is, for the present, nearly exhausted or suspended; or that the susceptibility necessary to its operation is much diminished.

When this fever appeared in the ship, we had lain in Port Royal harbour, with the exception of five days, about six weeks; during which period the weather was perfectly dry, the sky generally without a cloud, and the temperature high, Fahrenheit's thermometer varying from 82° to 90° in the shade. The sea breezes were generally strong; the land winds light, variable, and uncertain. In the night there was much lightning with frequent meteors. The barometer, as usual in this country, stood with scarce any variation, at 30°. During the whole period the ship's company was employed in clearing the holds, setting up the rigging, and dock-yard duty connected therewith.

On the 27th of August, as before stated, the ship left

Port Royal on a cruise to leeward, and on the 30th of the same month came to anchor at St. Andrew's, a small and very beautiful island, distant about seventy miles from the Mosquito shore. It is almost wholly in cultivation, and is celebrated for healthiness. We lay here forty hours, and then stood away for the Spanish main, which we made on the 2d of September, but did not approach nearer than six miles. Heretofore the weather had been various, sometimes fine, but more frequently squally, with much rain, thunder, and lightning; the heat moderate, seldom rising above 84°. Now rain fell almost without intermission, coming down literally in torrents, accompanied by the most terrific lightning and thunder. On the 4th of September we anchored in Chagres roads, where we continued only six hours, standing afterwards to the west, along Costa Ricca, towards the Mosquito shore. The cruise was continued, with the exception of anchoring four hours off the isle of Escudo, till the 18th, when we returned to Chagres roads. Here we remained till the 20th, and then proceeded towards Carthagena, off which place we arrived on the 23d. During the whole time, *viz.* from the 2d to the 23d of September, the weather continued, with very brief intervals of exception, such as it first encountered us on making the coast; a coast notorious for its hostility to human life since the period of its discovery. On the 24th of September, being off Carthagena, we proceeded to Jamaica, where we arrived on the 27th, and in the mean time enjoyed the only three days of fine weather we had had from the time we started.

On our arrival at Port Royal there were thirty-two persons on the fever list, many of them, however, in a state of convalescence, and most of them promising well. Twenty-five of the last attacked and least advanced in convalescence were sent to hospital, the day on which we arrived. From the 23d, the period of clearing the coast and

getting into fine weather, though many new cases occurred, they were generally of a less deadly nature than those which had arisen on the coast of the Spanish main; and a majority of the cases, which originated there, underwent a manifest change for the better about the same time. In some there were favourable critical movements when little expected; and in many there was an acquisition of energy and a general expression of improvement, though not amounting to crisis, with which I was much struck at the time, though I find it difficult now to say in what they consisted. In the subsequent attacks the febrile character was better defined, and the progress of the disease more regular and more perfectly developed. There was, generally speaking, a cold stage of invasion, and a hot stage of progression, followed by critical evacuations; and, therefore, a disease more under the control of remedial measures: at least, a disease in which the sensorial and vital functions were not so stunned and overwhelmed as to cut off, as it were, at once, the powers of reaction and restoration, as happened in many of the former cases.

It may be seen from the preceding statement of dates and numbers, that the fever prevailed during the whole cruise, few days passing without an accession of cases, but about the middle of September it reached its highest degree of extent and virulence. Of six taken ill on the 12th, two died; of five taken ill on the 13th, three died; of five taken ill on the 14th, three died; of seven taken ill on the 16th, two died; of five taken ill on the 17th, two died; making twelve out of twenty-eight, a fearful proportion of mortality indeed.

From what has been stated of the times, places, and circumstances of the ship's service since her arrival in the West Indies, coupled with the season of her entering the tropics, it will be seen, that the causes were not wanting in abundance, which are supposed to predispose, at least,

to attacks of West Indian fever, and which (though I defer speaking of the immediate cause for the present) were sufficient to evolve, in my opinion, the exciting cause itself, as far as it is connected with terrestrial agency, separate from atmospheric, or meteoric influence. All this being granted, however, and though it be likewise conceded, that our apprehensions of fever and death were not more than verified by the event; such circumstances, though they may diminish, cannot remove the regret, with which I reflect on the unsuccessful issue of my efforts to resist this "fearful adversary."

I had read and partly studied the works of the most eminent authors who have written on the fever of the West Indies: I had compared their reasonings, doctrines, and rules of cure, and had hoped thereby to devise for myself a rational, if not highly successful, method of treatment. I may confess that I had grafted on the doctrines of others some opinions of my own, which I trusted would lead to results less disastrous than those which had followed the practice of some of my predecessors. The facts which I have just stated were sufficient to take away my pride, and teach me humility. I saw my patients perish, at one period, in the proportion of nearly one out of two, and that, in some instances, in utter defiance of being influenced even by the powers of medicine. I reflected, amid the despair around me—the black vomiting, comatose muttering, or furious delirium of the dying—on what I had read and heard of the success of others, by the most opposite methods of cure, and asked how these things could be? One man had cured almost all, or all his patients, by blood-letting carried to a great extent; another by brandy; another by calomel; and another by emetics and purgatives. I observed enough, I think, to enable me, to a certain extent, to reconcile reports apparently so contradictory; within the narrow limits of a ship, and in a few weeks, I saw a febrile

disease, the true West Indian fever, so different in development, as to demand a very different course of practice in different cases, if I was to be guided by the character and succession of morbid effects, as they presented themselves, rather than by the name of the disease. I saw sufficient to convince me of the danger of assuming a certain number of principles, and thereon raising a standard to determine the course of the medical practitioner, in all places, and at all times, in treating West Indian fever. I admit the usefulness of nosology, but it must be used only as an index of reference to the great book of nature. It can contain merely the cardinal points of diseased action, the space between which must be filled up by careful observation, varied according to a multitude of varying circumstances—*hic labor, hoc opus est*; but the difficulties must be encountered, for nature, more especially in disease, cannot be outraged with impunity; and if one of these intermediate points be improperly placed, mischief will follow.

If it be true that such difference in the form of fever existed in the Rattlesnake, and I have evidence of my position, shall we wonder that one practitioner found a particular line of treatment at Martinique, another a different method at Barbadoes, a third another at Trinidad, and a fourth a different again at Jamaica, at different times, or at the same time, most successful? It has been so found; and it may repress the vanity of temporary success to inform the authors of exclusive systems, that others have tried *their new methods, or new methods improved*, and that too with all the enthusiasm of a founder or proselyte, and have found them fail. I say this, not to express my opinion against sober inquiry or renewed exertions, which should be encouraged; but because I think that old things often appear new, and that we frequently promulgate, as splendid discoveries, what

our fathers had abandoned, from the conviction that the disease, from modifying circumstances, required a modified, if not an opposite method of treatment.

The fever in the Rattlesnake appeared with differences of form so various and striking, that it might appear difficult to conceive how a disease of the same general character, but so modified in feature, and obscured in expression, as to assume the appearance of distinct orders, could arise from the same cause. That it was, however, one disease arising from the same cause I am satisfied; and I am moreover satisfied, that the original temperament and particular condition of the subject, at the time the cause came into operation, in connection probably with the duration and manner of its agency, before its explosion in fever, will be found sufficient to account for all the shapes in which it was manifested. Those shapes were however so unexpected and perplexing, and apparently so endless and contradictory, that I despaired at first of tracing them to any principles of general action. I saw no basis on which to rear the materials of classification, that would not be arbitrary and capricious, rather than inductive and reasonable; and I felt that my practice, therefore, without any determinate object, must be the mere empiricism of treating individual symptoms as they arose, and combating conditions, of which I knew neither the origin, nature, nor connection. But from this state of obscurity and confusion I soon perceived hopes of emergence. I observed, with all its variation and mutability, that the fever had two pretty distinct modes of general action; that one was characterised by increased vascular action, or what may be denominated inflammation; the other by impaired vascular action, or congestion. I further observed, that each of these modes of diseased action might be divided into three classes or species, in relation to the degree of severity in the symptoms, and

therefore without doing violence to nature, or seeking after unnecessary distinctions.

With this view, the view of simplifying the subject, I divide the inflammatory genus into the three species of *mild*, *violent*, and *intense*; and the congestive genus into the three species of *slight*, *aggravated*, and *apoplectic*; not convinced of strict propriety in the terms, but believing that they will be sufficient, in a hurried and imperfect sketch, to express my meaning.

The most constant and prominent symptoms of the inflammatory were, with or without rigor, frequency and strength of pulse, wiry, compressed, or full; a hot non-secreting condition of the skin, particularly at the præcordia and across the forehead; headach, confined generally to the sinciput, with sense of fulness in the eyes and tightness between the temples; jactitation, and constant rolling or otherwise moving of the head; flushing of face, with prominence, wildness, and sometimes inflammation of the eyes; pain in the back and loins, shooting across the anterior parietes of the abdomen, involving the whole contents in tumult; aching in the lower extremities, especially the knee joints, calves of the legs, and tibiæ; sometimes abdominal tension and tenderness in the early stages, sense of emptiness and exhaustion there as the disease proceeded. In the course of the second evening the symptoms were generally aggravated, and the stomach in many cases became irritable; but this symptom did not occur in the *intense*, and was very tractable in the *mild* species; in the violent it was exceedingly unmanageable, and often could not be arrested till fatal symptoms came in its train. Delirium was an early symptom in the *violent* and *intense* species, and was always attended with great danger. Costiveness was generally obstinate at the commencement, but was sometimes succeeded by troublesome purging and tenesmus. The tongue varied much in ap-

pearance; generally it was white or yellow, loaded and dry, becoming brown or black; urine scanty, high coloured, and attended with dysury in passing; thirst continued and insatiable. The disease generally ran its course within the fourth day. The treatment was briefly as follows.

In the *mild* species one moderate bleeding, with cathartics and diaphoretics, in most cases, left the patient convalescent in a few days.

In the *violent* species it was necessary to bleed largely and repeatedly, *viz.* to the amount of sixty, seventy, or eighty ounces in the first twelve hours; and it was gratifying to observe the benefit resulting. For instance, a man complained of excruciating pain in the head, which he moved incessantly; his eyes were wild, inflamed, and impatient of light; the head, according to his own expression, splitting to pieces; the circulation so hurried and tumultuous, that the body was agitated by every pulsation, and the throbbing of the heart visible through the clothes; the skin scorchingly hot and dry; and the entire aspect indicative of great distress. Yet that man, by copious abduction of blood, free catharsis, and a full dose of calomel and opium, was free from complaint on the second day of the disease, and in a few days more capable of returning to his duty. Such was the general plan of treatment in this form of the disease, and such, in a great majority of instances, the result.

It was far otherwise in the highest grade of inflammatory fever, or that which I have denominated *intense*. There the vascular action was so overwhelming, and the progress to disorganization so rapid, and often so irresistible, that all endeavours to arrest the disease were unavailing. Symptoms could be mitigated, but the character of the disease was seldom changed, or its force broken: within the first twelve hours the patient was delirious, often furiously; at other times it was the delirium of engorgement

and oppression. In both cases the action of the carotids was tremendous; the face red, and frenzied in expression; the eye sometimes clear, quick, and piercing, sometimes dull and darkly inflamed, always indicative of great cerebral derangement. The skin had an intensity of heat scarcely conceivable, particularly on the breast, neck, and head. The tongue was parched, hot, and apparently diminished in size; of various colours, generally brownish. The stomach was retentive, and the bowels costive, but not very obstinately so. This state of inordinate action sometimes continued till near the close of the disease, the patient, in such cases, expiring in a fit of violent convulsion. In other cases the high excitement ceased suddenly, and, as it were, by a momentary act, and was succeeded by coldness of surface, a state of circulation and respiration scarcely perceptible, calmness of countenance, and perfect quiescence of the body, which soon issued in death without a struggle. The disease was of short duration; and no remission could be observed during its course.

In such cases evacuation was pushed to the farthest extremity that prudence could sanction. Blood was removed at once till some obvious impression was made on the vascular system or morbid condition, but the latter was seldom effected, and this was repeated as often as the symptoms seemed to require the operation, till some favourable change took place, or such collapse or disorganization came on, as absolutely prohibited its further employment. At the same time such assistance was solicited as purging, blistering, scarifying, sponging, &c. could afford. Yet in many cases all was of no avail. The patient has been reduced by blood-letting, till faintness, vomiting, and cutaneous relaxation came on, and in an hour every symptom had risen to its former intensity. The blood-letting was repeated till the same state was induced, and in another hour every thing was to be done again; and thus many times in

succession, till destruction of some organ, generally the brain, ensued. There was a fire, so to speak, kindled in the frame, which nothing could quench. I have heard of men, who boasted that they could cure every case of this disease; that was indeed vain boasting, and calls for no remark; but I have read in books of something very similar in pretension. I have no right to measure any man's success by my own; but I may be allowed to doubt, whether in such cases, and in circumstances like mine, such happy results are likely to happen to others.

I come now to the congestive genus of West Indian fever, the most fatal form of the disease, and that which embraced the greater number of our cases.

A sense of stupor, weight, and oppression, rather than pain in the head; a feeling of helpless debility, affecting the spine, most distressing about the sacrum; a paralytic failure of the lower extremities, with pains in the knees and calves of the legs; a pulse having all degrees of celerity and expansion, but always weak, sinking under the finger without resistance; a state of the skin various and difficult to define, but always deficient in tone, sometimes dry and dense, sometimes greasy, and sometimes drenched in sweat; generally without increase of heat, except at the præcordia, where it was confined and smouldering; a most distressing expression of countenance, deadly pale or livid in colour; a drunken idiotic eye, with dilated pupil and sleepy motion; deafness; desire to be left alone; sighing deep and interrupted; early tendency to coma; tension of the hypochondria; and early irritability of stomach; were the principal symptoms by which this division of the disease was characterised.

In the lowest grade of this genus, *viz.* the *slight*, these symptoms were not all perceptible, and those that were had a mild and manageable character. Moderate purging, followed by the use of calomel in five grain doses, two, three,

or four times daily, with or without opium and antimony, according to the state of the stomach and skin, soon roused the slumbering energies of the brain, adjusted the balance between the nervous and vascular systems, and restored health.

In the second or *aggravated* species, more forcible measures and greater perseverance were necessary. I generally bled, as soon as possible, to the extent of ten or twelve ounces at first, for even that quantity induced faintness and vomiting; then administered a purgative medicine; and repeated the blood-letting, or not, according to circumstances. Beyond this I seldom thought it safe to abstract blood; but trusted the remainder of the cure chiefly to calomel. And here I think mercury a remedy of great, of paramount utility. When the patient says that he has no pain; is ill, but cannot tell what is the matter with him; when he turns away with a countenance of helpless despondency from the person addressing him, letting the head drop as in hydrocephalus; when the pupil begins to dilate and hearing to fail; when he shrinks on applying the hand to the epigastrium, and vomits every thing which he swallows, and even in larger quantities; then, in my opinion, nothing can supply the place of calomel. But it must be given largely, and perseveringly. From ten to twenty grains, twice or thrice daily, sometimes with opium, sometimes with antimony, or both, and sometimes alone, I have given in such cases, and, aided by blisters, &c. I have reason to be satisfied with the result. Ptyalism will, in most cases, soon be established; but after all that has been written on the subject, I think I have witnessed the perfect benefit of its operation, without that manifestation. I tried turpentine, but it did not answer my expectations.

In the highest grade of congestive fever, or that which I designate *apoplectic*, I had to deal with a much more formidable and fatal disease. In the preceding species,

there were generally some slight premonitory symptoms; but in this the attack was like the effect of electricity. In an instant its subject was seized with giddiness, dull pain of head, and confusion of ideas; a sense of coldness, weakness, and indescribable uneasiness along the spine; spasmodic pains in the legs, and paralytic incapacity of the lower extremities. He lay as if stunned, and labouring under concussion of the brain, with dilatation of the pupils, and a gloomy, despairing countenance. The pulse was rapid or slow, full or small, but always weak. The skin was cold, generally greasy, or covered with cold liquid sweat, sometimes dry and lifeless. The stomach was sometimes irritable from the beginning, and there was a strange compound sensation of desire for drink and loathing when tasted. From this state the patient, in some instances, never rallied, but sunk down rapidly to dissolution: he became perfectly comatose: the blood seemed to stagnate in the brain and large internal trunks, and, with filmed glassy eye, faltering pulse, and involuntary discharges, he soon expired. In other instances there was reaction, but faint, partial, and irregular: then existence was protracted, and hopes entertained which were seldom realized, profuse hæmorrhage coming on and exhausting life.

In such cases, medicine, I fear, will never effect much; and, placed as we were, I consider the usual methods of treatment altogether useless. In some instances, where vascular action succeeded the first fearful impression, I tried blood-letting, and am persuaded that it did mischief; whether or not it might be useful if hot baths and powerful frictions were premised, I cannot say; but I have the strongest conviction, that, without some such preparatory process, it will only precipitate the fatal event. I endeavoured, by friction occasionally, blisters, and calomel, to rouse the sensorial and vital powers from their appalling

lethargy; but I must admit, that, with thirty-five cases of fever on my hands at a time, and without assistance, these means were not followed up to my satisfaction. That no other method of treatment would have had happier results, I cannot pretend to say; but I should not expect much from any method. Still I would not leave the patient to his fate, in hopeless inactive despondency. With proper "appliances and means," I would endeavour to arrest the disease by the following means: baths of high temperature, with strong frictions, electricity, and galvanism, stimuli diffusible and permanent, enematæ of turpentine, eau de Cologne, &c.

In concluding these extended remarks, for which the importance of the subject will be my apology, I ought to observe, that the different forms of the fever were not always so well defined in character as I have endeavoured to render them in description; that not only the species ran into and mingled with each other, as light and shade are mingled in a picture, but that the features of the genera were often so blended, as to render it difficult to determine which predominated. So it will ever be in the physiognomy of febrile disease. To ascertain, separate, and arrange differences as they arise and suffer modification, to fix the place and appreciate the importance of each, and thereby to regulate the measures of treatment, is the great difficulty and paramount duty of the practitioner.

There is a modification of congestive fever so insidious as to give little alarm, and lead the inexperienced to think the patient is in no danger. The person labouring under this form of the disease will confess, on being sharply questioned, that there is slight pain and heaviness in the head, and the epigastrium is tender on pressure. Otherwise little appears to be the matter, the pulse being natural, or so nearly natural as to escape observation; the tongue clean; the skin cool, or obscurely hot, over

the stomach and liver; the eye clear; and the entire aspect, to superficial observation, promising. Yet in less than forty hours the surgeon will be alarmed and confounded by black vomiting, soon followed by death. Although the patient will say, every time that he is visited, that he is better, and that, could he only eat, he would be well; on looking closely it will be perceived, that his answers do not always bear on the questions put; that amid his accounts of improvement, he never attempts to lift his head from the pillow till desired, and hurriedly lets it down again; that he doses rather than sleeps, sighs frequently, and has difficulty in falling the lungs; and that the eye, though clear, is vacant, or fixed without an object.

In this deeply-masked and very dangerous modification of the disease, it especially behoves the surgeon to examine and prescribe with vigilance and assiduity, never to slumber on his post, or indulge in careless and confident prognostics; for if he misapprehend the nature of the disease, or lose an hour in opposing it, he may lose the opportunity of saving the patient's life: he will be surprised by the near approach of death, when he did not dream of danger. I have known a man ordered to do his duty, in a case like this, because the surgeon could not perceive *much the matter with him;* he continued to do his duty, after a fashion, for nearly two days, when the eruption of *black vomit* gave irresistible and mortifying evidence that the man was no impostor. The method of treatment in this variety of the disease should be similar to that employed in the aggravated species.

THE foregoing Report was drawn up immediately after having suffered considerable loss from West Indian fever, and while it was still in progress, though with reduced force and diminished mortality. It may have received an

undue tincture of gloominess from a recent and still impending calamity; and the picture which it exhibits may therefore appear darker and more disheartening than was requisite to a faithful representation. But since, during the lapse of two years and three months, though I have seen the disease in a variety of forms, and have been enabled to consider its nature and results more coolly and deliberately than I then could; on collating and carefully examining the whole, I perceive nothing which would justify me in altering materially the opinions therein delivered. Truth will not permit me to change the substance of the statement, though regard for appearance might induce me to arrange the materials differently, and to give it a more finished form. I am well aware that it is extremely rude and imperfect; yet I think it better to submit it as it is than to modify it any way, being convinced, that an immediate transcript of impressions, as they are formed, is more interesting; and more vividly impresses others, than any subsequent delineation, however highly finished or imposing it may be; because such delineation is necessarily less faithful than the original sketch from which it was taken. The report, with the facts and inferences which it contains fresh from their source, is therefore allowed to stand as originally drawn up; on which I shall make such remarks, and to which I shall add such particulars regarding the nature and treatment of the disease, as have been suggested by subsequent observation and reflection.

On reconsidering the division and subdivision of West Indian fever which I have proposed, I am persuaded that the arrangement, in a nosological point of view, is faulty; and that the more minute distinctions are, in a certain sense, arbitrary. Instead of dividing the disease into two genera, and then regarding three modifications of each as distinct species, it would have been more scientific perhaps, and more consistent with ordinary methods, to have consi-

dered the disease as forming a genus, the inflammatory and congestive manifestations of which constituted a species respectively, and the modifications of these again as forming varieties. But for the purposes of utility no advantage would be gained by such an alteration of terms. Difference of condition is as readily apprehended according to the former phraseology as the latter; and when difference of diseased action is so apprehended as to enable the practitioner readily and reasonably to adjust his curative means to different conditions, the object of such divisions is obtained.

Whether the inflammatory and congestive modifications of fever be denominated genera or species is of little consequence, so long as the difference of condition, which they imply, is understood, and fully appreciated; and for those purposes we must watch and study the signs of each as they present themselves. In this most important and sometimes perplexing inquiry, we can derive no aid from consulting any nosological scale, and discovering in what division of that scale these modifications are placed. Much less can we obtain from such artifice, just views of what is wanted to remedy these morbid modifications respectively, and thereby ascertain what we ought to do in each. Again, it is of little importance whether we call the subordinate modifications of each of these, species or varieties; but it is of great importance that the different conditions, which they imply, should be accurately ascertained, and the requisite remedial measures accurately adjusted to them. If, for instance, the *mild* and *intense* modifications of inflammatory fever were confounded, and the same treatment applied to each, we should in one case do too much, or in the other too little; in both cases, though not equally perhaps, doing the patient injustice; for whereas in the former, only moderate depletion is necessary or justifiable, in the latter, every measure must be adopted and urged consecutively, or in

combination, which the resources of our art furnish, and the strength of the subject will admit*.

To discriminate the inflammatory and congestive forms of West Indian fever, by carefully observing and collating the symptoms peculiar to each, in order that an appropriate method of cure may be early and vigorously instituted, is of infinite importance to the patient, and therefore becomes a duty of high consideration to the West Indian practitioner: it is sometimes difficult to perform; and it therefore demands imperatively the best resources of the mind, and the most scrutinizing attention. The principal distinctive marks of the two forms of fever are observed

* It is obvious that nosological arrangements must, to a certain extent, be arbitrary, when the limits and nature of our knowledge and the difficulty of the subject are considered. We know little of the intimate nature of diseased action, of the manner in which it arises, or produces its peculiar effects on health: we observe an organ, or tissue, in a state of disease, and thence, as from a root, we observe many branches springing up, and perverting generally or universally the natural motions of the body; but we know not how or where they spring from that root; neither do we know, in most instances, how or where they terminate. To such movements, so intricate, obscure, and various, no scale can be correctly adjusted; because none can be constructed which shall clearly and perfectly embrace the whole. Even the powers of Cullen's mind failed when directed to this object, as human intellect must ever fail when so directed. When we compare the classes pyrexiæ and cachexiæ, we find natural and pretty clear limits to each, and are so far satisfied; but when we consider the orders and genera of those classes, we believe that many of them might change places without suffering violence, and that some of them ought to be excluded from their respective classes: and when we turn our attention to the class neuroses of that author, with its orders and genera, we are forcibly struck with the utter inadequacy of all such means to the end proposed. To simplify the study of disease, by so grouping symptoms according to their constitution and tendency, that the adaptation of remedial agents shall be more intelligible and certain, is all that can be effected by such arrangements.

in the condition of sensation, of the pulse, and of the skin.

In the congestive form, sensation is depraved and imperfect, generally obtuse. The patient does not complain of acute pain throughout a disease, which often in a few days destroys him. When questioned as to his feelings, he will acknowledge that there is heaviness and confusion in the head, sense of fulness and distress at the præcordia, weakness and dead pain in the back and extremities, an indescribable feeling in the loins, which he imagines to resemble that which he would experience if his back were broken, and uneasiness, with twitching pains, in the calves of the legs. In the inflammatory form, sensation, on the other hand, is increased. Sometimes from the beginning, almost always in a short time, the patient complains of distressing pain in the head, generally in the forepart, across the eyes, and in the temples. He is disturbed by noise and light, he is impatient and fretful, he is restless and tosses in his bed continually.

The pulse in congestive fever varies much in fulness and frequency, though it is generally below the ordinary febrile range, as it regards celerity. But whether it be frequent or slow, full or small, it is always weak. It offers no resistance to the touch: when the finger is applied to an artery, it acts as it would on an inanimate elastic tube half full of fluid; it depresses easily the point on which it bears into contact with the opposite point, the contents receding in either direction. This state of the pulse is so striking and characteristic, that I think it can scarcely be overlooked or misapprehended. In inflammatory fever, the character of the pulse is extremely different. Generally speaking it is not very rapid, as a febrile pulse, though more so than in the congestive; and it likewise varies considerably as to volume in different instances, and in different stages of the same instance of disease; but it is always remarkable for strength, excepting when under the direct

influence of treatment: whether small or full, whether comparatively slow or rapid, it is always energetic. The finger is forcibly repelled by every pulse of the artery to which it is applied; and ordinary pressure will not for an instant diminish its calibre, or alter its form.

The condition of the skin and parts immediately subjacent, especially as to temperature, offers instructive signs for distinguishing the congestive from the inflammatory form of fever. In the former, the heat of the surface is frequently less than in health, particularly on the extremities. When it equals or surpasses the healthy standard, as it sometimes does, being highest about the epigastrium, it is of a peculiar kind. The hand is scarcely impressed by it, when applied lightly and hastly to the pit of the stomach, but when kept there with steady pressure, a sensation of deep-seated and accumulated heat is communicated. Its source appears to be at a distance, and its effects are modified by the interposition of matter, which neither supplies nor readily conducts it. With this peculiar condition of the surface as to temperature, there is connected a peculiar want of tone in the skin, which it is difficult to render intelligible by description. It is sometimes dry, harsh, and dense; sometimes moist, with a thin serum-like fluid, or a greasy exudation; sometimes it is smooth, slippery, inelastic and doughy, without moisture: but in whatever manner its functions may be perverted or abolished, its characteristic condition is want of vitality: when grasped in the hand and raised from the parts beneath, it feels like the skin of one who had ceased to breathe. In the inflammatory form of the disease, again, the heat of the surface is high, free, and diffused: often when we first see the patient, always soon after, it impresses the hand instantly and powerfully, being nearly as intense at the extremities as the centre, sometimes even more so; the skin being dry, but not dense, inelastic, or deficient in tone, as in the congestive.

Such are the most certain, and therefore the most valuable diagnostic symptoms of the two primary and essential divisions of West Indian fever. Those which I am now to add are less constant, or less obvious, late in appearance, or referible to internal conditions, and are therefore less useful and satisfactory; but in settling so important a difference as that to which they relate, they should not, even as occasional auxiliaries, be omitted.

The digestive organs suffer more directly and dangerously in the congestive than in the inflammatory form of fever. Nausea is common to both, as it is to febrile disease generally at the invasion; but in the inflammatory, when fully formed, it becomes little irksome, and vomiting is not frequent. In the congestive, vomiting occurs repeatedly throughout the disease, being seldom arrested, in fatal cases, till within a few hours of death. In the *intense* variety of inflammatory fever, the stomach is generally retentive through the whole course of the disease; and when vomiting takes place, it is seldom till near the close of fatal cases. A peculiar sensation in the stomach, œsophagus, and fauces, which appears similar to that experienced in pyrosis; tension and tenderness in the epigastric and hypochondriac regions; and the ejection of more fluid by the stomach than was drank, are conspicuous and distressing in the congestive; they are not much so in the inflammatory form; indeed in the latter they are seldom prominent, and in some instances are not perceptible; in the former they scarcely ever fail. The tongue is generally white, slightly coated and furred, dry, and sometimes brown, in the inflammatory: it is seldom loaded, often morbidly clean, and generally moist in the congestive species. In the former thirst is urgent, and almost incessant; in the latter, the patient sometimes will scarcely ask for drink, and when he does will generally take little, and put the cup away, as in disgust.

The yellowness of the skin in this disease has given rise to considerable controversy, especially since it has been attempted, by difference in its shades, to distinguish the contagious from the noncontagious, the imported, or Bulam, from the indigenous fever in the West Indies; which has been done by some, who have asserted such different modes of origin, and have given diagnostic rules for apprehending their respective operations. But such means of diagnosis have been found useless, as all other means have proved, and must prove, if, as I believe, the difference which they are proposed to mark are imaginary and factitious. *Discoloration* of the skin deserves consideration, however, as a characteristic sign of West Indian fever generally, though, as it occurs late, it is not of much practical value. Yet the *kind* of *discoloration*, according to my observation, is of importance in determining the different conditions of the body, occasioned by inflammatory and congestive fever. In the former, the skin is yellow, of different shades in different instances. Sometimes it is light, sometimes dark; sometimes it is of the colour of an unripe lime, sometimes of a mellow orange; varying constantly, and being scarcely the same in every respect, in any two cases: still it is yellow, and would be instantly pronounced so by all observers; though in endeavouring to describe its exact tinge, different words would be employed by different persons, sometimes inappropriate ones, and erroneous opinions would thence be formed. This yellowness is observed first in the eye; then about the neck, whence it spreads to other parts of the body. In some cases it extends over the whole surface, as in icterus; but it more generally is confined to the face, neck, and trunk, appearing often in stripes and patches. In congestive fever the skin is *discoloured*, but does not become yellow. Its colour is not like that of an icteric patient; it may rather be called petechial, as it resembles, though it is not exactly like, the skin of persons

in the last stage of what is called putrid fever. From the first the skin loses its proper lustre. It becomes blanched, or lurid; and as the disease advances, livid and black patches of various sizes are observed on the breast, back, hips, scrotum, and extremities. They pervade the surface, but are most numerous generally on the trunk. About their margins there is a yellowish, or greenish hue, either of them indistinct, and what is called dirty. It is difficult to describe the aspect of the skin accurately and intelligibly; but, by comparing it with a familiar object, which it very closely resembles, a pretty correct notion of its peculiarities will be obtained. When yellow leather gloves are worn on horseback, in rainy weather, and the glove of the bridle hand is thoroughly wet, the palm part of it exhibits a very just likeness of what I have attempted to delineate—turbid white, and livid or black, intermixed with imperfect margins of yellow and green, or a hue in which yellow and green are blended, and it is difficult to say which predominates. This appearance of the skin in congestive fever, and the yellowness which I have appropriated to the inflammatory, I am persuaded, are characteristic of each respectively; but I am not from my experience justified in saying they are so absolutely, and without any exception: and, as they are generally late in appearance, and sometimes, but rarely, wanting, though interesting in pathology, they are comparatively unimportant as practical diagnostics. The fate of the patient is generally sealed, as far as art is concerned, before either of them, especially that which relates to congestion, becomes conspicuous—I refer of course to the petechial appearance when fully developed, not to the turbid, inanimate hue which may be observed from the beginning.

The appearance of the blood might furnish earlier and more certain information for distinguishing the inflam-

matory from the congestive form of fever. In the former its colour is bright red, approaching that of arterial blood; in the latter it is so dark, as to resemble blood which has been extravasated and secluded from the air. But the information derived from this quarter must be experimental, which would lessen its value at all times, and, in some instances, according to my views of appropriate practice, must render it useless; for, in some instances, the experiment cannot be safely made.

The diagnostic signs which I have detailed will be generally found in pretty close connection, in two separate series, when carefully looked for; and when found, they will render it little difficult to distinguish the inflammatory from the congestive modifications of West Indian fever. The line of practice to be followed in each will then become more obvious, and there will be better prospects of fortunate results. That the disease will continue destructive in spite of all our efforts, till we acquire more powerful therapeutical agents, or a more perfect method of employing those which we now possess, is I fear too true; still I cannot help indulging the hope, that if the difference of condition in febrile action, which I have attempted to delineate, were fully considered in West Indian fever, more scientific and consistent methods of treatment would be adopted. Much of the confusion and contradiction, which we find in the records of its constitution and treatment, have arisen, I am convinced, from not sufficiently marking the difference in question. But the diagnostic signs are not equally distinct and perfect in every instance of the disease. It happens, as might be expected, that instead of appearing in separate series, they sometimes are mingled in one, to a certain extent. Their line of demarcation, so to speak, is occasionally broken, and they either coalesce equally, or appear accumulated respectively, one prevailing in one part

of the body, and another in another part, forming a compound disease, in which diagnosis is difficult, and the requisite treatment perplexing and obscure.

Thus it will be found, that the two forms of febrile derangement sometimes coexist in the same person; that while susceptibility and action are diminished in one part of the body, they are increased in another. When, however, this combination of congestion and inflammation coexist in the same subject, the latter is less intense than when it is alone and unincumbered, as in pure inflammatory fever. The same measure of depletion will not be called for; and means of excitement must be employed at the same time. It is necessary, in such cases, to employ hot baths, friction, calomel, opium, camphor, &c. and bleeding, blistering, &c. at the same time. But bleeding must be employed cautiously; for while the train of symptoms against which it is directed are not strikingly imminent, appearing to denote the subacute inflammation of some authors, the antagonist symptoms may thereby be dangerously exasperated, if injudiciously applied, or unnecessarily urged. Such a combination of contending conditions must be formidable, and often unmanageable: to counteract and correct it, prompt and persevering measures must be employed, and however conscientious and skilful the application, they will often fail.

It happens in many instances of inflammatory fever, that, after the employment of depleting means for the reduction of its urgent symptoms, a species of congestion ensues, which, if neglected, is dangerous or fatal. We are too apt to think, when we have bled our patient, and reduced him by other means, till the skin becomes cool and the pulse weak, that we have done all which art can do, or which nature requires. The congestion which succeeds inflammatory action in such cases, is different in itself from that which is originally impressed on the system, and con-

stitutes the disease, in primary congestive fever, and it is much more remediable: still it involves its subject in peril, and calls for the interference of art. Many means will present themselves for counteracting this state: none has appeared to me better to answer the purpose individually than pretty full doses of calomel and opium, with or without antimonial powder, according to circumstances.

But it is impossible, and would be presumptuous here, to prescribe exact rules of practice in every such emergency. The practitioner, having made up his mind as to general principles, must be guided by appearances, as they present themselves. He will endeavour to subvert diseased actions in the order of their urgency, when coexistent, and in the order of their succession, when consecutive; and he will be obliged sometimes, however unphilosophical it may appear, to combine remedies which have different or opposite powers on the living body; because he will find different or opposite conditions existing together in the disease, which he is called on to treat. Without attempting to specify what should be done in every variety of such cases, I shall communicate what appears to me important in the management of the disease, by stating the measures to be employed generally, in inflammatory and congestive fevers, as they appear in their more obvious and perfect forms.

In regard to the inflammatory, I would urge the application of the opinions delivered in the preceding Report. Depletion, general and local, is here imperiously called for, though its requisite amount is very different in different cases.

In the *mild* species, one moderate bleeding, followed by active purging, saline diaphoretics, calomel and opium, or tepid sponging, and occasional blisters, will be sufficient.

In the *violent* it is necessary to bleed fully, often repeatedly, from a vein or artery, and to abstract blood at

the same time from the neck and epigastrium by scarifying and cupping; to purge, but not to depend so much upon that evacuation as in the former species; to shave the head, and keep the scalp wet with cold water (ice would be desirable if it could be procured); and when the force of the disease is in some measure broken, blisters over or in the neighbourhood of organs principally affected. The surface of the body should be sponged with tepid water or vinegar, at the time the scalp is kept humid with cold water; abundance of cold drink should be allowed, and the patient secluded from light, noise, and other external causes of irritation. In short, prompt and powerful means of reducing the force of the circulation must be employed during the period of violent action; and of such means, the general abstraction of blood being the most powerful, on it we ought chiefly to rely. I do not pretend to specify the quantity that ought to be removed in individual cases; as in two, which shall appear equally violent, that which will make a powerful impression on one will scarcely influence the other. From twenty-five to thirty-five ounces will generally bring on faintness and fluttering of the pulse, if drawn soon after the accession of the disease; to produce similar effects more will be required at a second operation, and, generally speaking, less at a third; beyond which it will seldom be necessary to proceed.

In this way, supposing the operation to be repeated on the return of excitement, which it should be, the quantity of blood removed in the first thirty-six hours will be from sixty to one hundred ounces; and to this evacuation, with occasional scarifying, purging, and sponging, the attention during that period should be chiefly directed. It will then be found, in a majority of instances, that the disease has undergone a sensible change; that the heat of skin is reduced, the strength of pulse diminished, and severity of suffering relieved. It will often happen that nothing more

is requisite in the way of treatment; as natural sweat will follow, thirst cease, appetite return, with sound sleep, and rapid convalescence. But it will also sometimes happen in this state of matters, if we either carry depletion further by abstraction of blood, or stop short in our treatment, trusting to the efforts of nature, that we shall neglect our duty, and endanger the patient. Blood-letting is a remedy of great power and value, but in proportion to its power, its application ought to be nicely regulated: it can scarcely be doubted, that, in febrile diseases, it has of late been urged often in an injudicious manner; and what is injudicious in such a case as this must be dangerous. In the case here supposed—a case of violent inflammatory fever, in which from sixty to one hundred ounces of blood have been abstracted in the first thirty-six hours, and in which at that period the heat has fallen and the pulse sunk—to repeat the blood-letting would be perilous, if not fatal: it would often be perilous likewise, in such circumstances, to leave the patient to himself, as has been remarked. The disease has taken a new direction, and must be opposed on new principles, or by different means. Vascular action, instead of being dangerously increased, is dangerously diminished, at least in some parts of its circle; what was apprehended before from inflammation is now to be apprehended from congestion, of which a secondary and artificial kind has been induced in the course of the disease, and which, though by no means so intractable as that which is primary and essential, must not be neglected, nor carelessly treated. At this stage of the disease, therefore, we ought to keep a vigilant eye on the tendency and progress of its symptoms. When, with partial subsidence of heat and vascular action, there is a general and moderate secretion of liquid, warm sweat; diminution of thirst; increase of urine; tranquil and refreshing, not deep, sleep; then little will be required of us. But when, though the animal

heat be reduced at the extremities, it continues at the centre, appearing to be accumulated there; when the skin continues dry, or is moist and clammy, at times, and in parts only; when the stomach becomes irritable, having been retentive before, and there is fulness about the præcordia; when the patient passes suddenly from a state of pain, restlessness, and tossing, to one of profound repose, and from anxiety and alarm to resignation and indifference, or when there is increased irritation and peevishness at this period; when the pulse loses its strength, but retains its celerity;—then there are diseased actions going on, different from the original, and calling for different treatment. The warm bath is then a valuable remedy; and blisters should be applied over the stomach, the neck, and between the shoulders. Cordial diaphoretics, ammonia, and spiritus ætheris nitrosi ought to be administered; and in some cases small portions of wine and porter may be allowed. But calomel is, of all remedies, the most to be depended on in this state of matters, combined with opium, and sometimes antimonial powder. The quantities proper for individual cases must, of course, be reckoned by the particular circumstances of each; but speaking generally, six grains of calomel, six of antimonial powder, and one of opium, twice or thrice daily, will be sufficient: and, in very many instances, the salutary effects of this combination will soon appear. The heat will become equalized, and the skin soft; secretion, especially that of urine, will be increased; torpor and congestive obstructions will be removed, and the functions gradually restored. In some instances a few doses will be sufficient for this purpose; in others many will be required, nor will danger be completely averted till salivation be induced.

At this stage and in this condition of inflammatory fever, oil of turpentine may, I think, be used with advantage; and I hope that it will prove a powerful auxiliary to the

other remedies recommended; but as it appears to be best suited to the congestive form of fever, I reserve the statement of what has occurred to me, regarding its action and uses, till the treatment of that form come under further consideration.

Having so far traced the morbid movements which take place in the *violent* species of inflammatory fever, and having sketched the principles of practice to be pursued, and mentioned generally the remedies to be employed, the *intense* next in order demands attention; and here the practical directions may be comprised in few words. Our object is to produce a speedy and powerful impression, so as early to arrest the disease towards a fatal termination; for such I believe its progress always to be, when left to itself, or feebly opposed. Its termination is most frequently in death, with whatever skill and energy it may be treated; and in a disease so rapid and destructive, trivial or temporising measures must be useless. If there be no abatement of symptoms within the first thirty-six hours, such violence will too often be done to the vital organs, most frequently the brain, as will render recovery impossible. If, at that period, the intensity of heat, force of circulation, headach, delirium, intolerance of light and sound, jactitation, &c. be undiminished, there will be little room to hope, and there will be doubt and difficulty as to what should be done. Till that period the indication is obvious, and the line of practice clear in most cases. To subdue or moderate the vehemence of vascular action, we must concentrate, so to speak, various powerful means of reducing vascular action; and as, of those, general abstraction of blood is the most powerful, on it we must place our principal dependence. The quantity that should be removed at a time, or how often the operation should be repeated, cannot be specified; in matters so nice and important, every one must judge for himself, under the guid-

ance of present phenomena. I have not met with cases in which it could be carried so far as it is said to have been carried by others; and that not for doctrinal but physical reasons, the recurrence of faintness rendering it impossible. But as syncope forms a necessary, it ought, or faintness at least, to be the only limit to the evacuation; and as often as the intense symptoms return, so often ought it to be repeated. There is no doctrine in medical science more certain than this, that, when acute inflammation involves an important organ, abstraction of blood is the chief mean of cure; and there can be no question about its applicability here. When equal quantities of blood are drawn from an artery and a vein, there is reason to suppose that the former has frequently the greatest effect; and it ought therefore to be employed in this as well as in the *violent* species of West Indian fever.

At the same time that we are urging this most important evacuation, the bowels should be purged, the head should be shaved, and kept cool with cold water; the surface of the body sponged with tepid water, or water and vinegar; saline mixtures administered, and abundance of cold drink allowed; the apartment should be darkened, and every unnecessary cause of irritation shut out.

Should the intensity of the disease not be lessened, however, by the persevering use of these remedies, at the expiration of thirty-six hours, it becomes a serious and most difficult question, what should then be attempted; whether we ought to abstract more blood, or trust to other means, especially to calomel. I should still be inclined to bleed further, in the belief, that till disorganization takes place, or the disease be moderated, in such cases it is not only safe, but essential: I would therefore not forego its use till critical movements of some kind could be discerned. But I would now call in the aid of blisters, applied between the shoulders, to the neck, and epigastrium,

and, if there be tendency to coma, over the scalp. If symptoms of congestion supervene, which will rarely happen however, the measures already recommended for that state, in the *violent* species, ought to be adopted.

The line of practice to be followed in the congestive division of West Indian fever is by no means so clear as it is in the inflammatory, and the results are less certain and satisfactory. It is, upon the whole, the most intractable form of the disease, and in many instances resists every method of treatment hitherto adopted: such at least has been the case in my own experience, and as far as my observation extends. I nevertheless am convinced, that the opinions as to its nature, and the principles of practice proposed in the foregoing Report, are correct in the main. The best consideration I have been able to give the subject has confirmed me in that belief, and forced me to form the melancholy conclusion, that, in many cases, the resources of our art have little influence on the disease, and that, in its worst forms, it is utterly beyond their control. A gleam of hope has been thrown on the subject, from the use of turpentine; but whether it shall be strengthened and confirmed, or shall in its turn prove illusory, further experience must decide. From what I have seen and heard, it appears reasonable to expect, that it will prove a useful auxiliary, and that, when judiciously combined with other means, it will therefore have salutary effects.

Regarding the *slight* species of congestive fever, I have nothing to add: stimulating cathartics, calomel and opium, six or eight grains of the former and one of the latter, with or without antimonial powder, two or three times a day, and blisters occasionally to the epigastrium, will, with few exceptions, conduct the patient safely through the disease.

But in the *aggravated* species, where the nervous torpor and vascular atony are great, and where reaction is tardy, irregular, and imperfect; where the patient, without

complaint of pain, lies prostrate, letting the head fall from the pillow, or pushing the pillow away; the countenance being ghastly pale or livid in colour, and fatuous in expression, and the iris scarcely influenced by light; in such cases, a more complicated and difficult process of treatment must be instituted. Warm baths of high temperature should be used in the first instance, whenever they can be obtained, strong and assiduous friction being conjoined. The bowels should be cleared by the warmer purgatives, combined with aromatics; and the patient should be enjoined, and encouraged in, the use of hot drinks, tinctured with ginger, cinnamon, or some such cordial. Blisters should be applied over the epigastrium, between the shoulders, and to the head; and as they act very imperfectly in many cases, it will be useful to rub the skin strongly with spirits before they are applied. These means are to be employed, as nearly as possible, conjointly. Calomel ought to be administered in most cases from the beginning: it should not be delayed beyond the operation of the purgative medicine. The quantity of this most valuable remedy, and its manner of combination with others, must of course be varied according to circumstances; but the dose must, on the whole, be large and often repeated. If the character of the disease be not changed at an early period, its end will generally be in death; and we must therefore endeavour, earnestly and incessantly, so to modify it, that its career may be arrested. The animal energies, whatever they are, or however they ought to be denominated, must be roused, in the first instance, to give the patient a chance of recovery; for in many cases the inherent powers of the body are incapable of any seasonable and suitable reaction. For this purpose, with the other remedies recommended, I have given, and would give, ten, fifteen, or twenty grains of calomel twice or thrice daily, with a grain or a grain and a half of opium to each dose, according to

the state of the digestive organs; and this course should be continued till salivation be induced, or a crisis observed. Favourable crisis will seldom happen till the gums become swelled, and other signs of general mercurial action become manifest, though I have known it otherwise.

The first object of treatment is to rouse from torpor, and excite to action; the second, to abstract part of the circulating mass; by which processes we endeavour to obviate obstructions in particular organs, and restore the equal and regular movements of the blood; these being changes which appear essential to healthy actions. Some of the remedies employed are fitted to fulfil one indication, and some the other; calomel conduces to both ends, and we therefore use it from first to last. I shall not speculate about the manner in which it produces those effects; the last, however, that of abstracting part of the circulating fluids, and thereby relieving the vascular system, is obviously connected with the power which the remedy exerts on the emunctories, its power of increasing the various secretions. When it has fairly and fully exerted that power, when the saliva begins to flow copiously, when the secernants of the skin, liver, kidnies, mucous, and perhaps serous surfaces begin to act freely, and they are pretty generally contemporaneous, then the patient may be considered safe: the disease will, with very few exceptions, be arrested, and convalesence, under proper management, will proceed, though sometimes slowly and precariously, to health.

But after reaction comes on, and before the calomel acts as an evacuant, there is a period of excitement, which is the more dangerous, because it is of a peculiar kind, and unequal in different parts of the body. This is the only stage of the disease in which blood-letting can be employed with safety; and though it is now called for, and sometimes apparently to large amount, it must be resorted to with caution and vigilance. When the heat rises on the surface, when

the pulse acquires strength, and when there are signs, as there will always be, of internal obstructions, we abstract blood, looking closely however to the effects produced. If, during its flow, the pulse sink rapidly, and the heat suddenly forsake the surface, we may be assured that it is injurious, and ought therefore to be instantly stopped; to be repeated, however, if afterwards the recurrence of excitement be considerable and early. On the other hand, when, during the flow of blood, the pulse loses little, or acquires strength, bounds and becomes firmly elastic under the finger; and when, though the temperature is reduced, it falls slowly, and is attended with moisture on the surface, we may then carry the evacuation further, being careful however not to induce syncope, or even much faintness. I cannot attempt to specify the quantity of blood which should be drawn in this case, any more than in others; such an attempt would be even more difficult and dangerous here than elsewhere generally. The effects occasioned ought to be the measure, as they alone can be the proper measure, of quantity. I have not found it either expedient or practicable to bleed to the extent which some others have done; but whether this has arisen from something peculiar in the character of the disease with which I have had to deal, or from other causes, I do not know.

It is remarked in the foregoing report, that the oil of turpentine did not answer my expectations. It is unnecessary to state what those expectations were, or the reason on which they were founded. From observing the operation of that remedy in some other diseases, I had been led to expect that it would be useful in this; I therefore tried it, but was disappointed. I am now however satisfied, that it was used without due discrimination; that it was delayed till matters had become desperate, and that its uses were therefore not properly estimated. I have already expressed a hope rather than sanguine expectation,

that it will be found a useful addition to the other remedies employed in the disease; and in a disease so little under control, an additional remedy may be an important acquisition. I would however suggest the necessity there is of ascertaining accurately the particular conditions to which it ought to be applied; believing, that, if it be used indiscriminately in every case and stage of West Indian fever, its use will be abridged, and that it will sometimes become an instrument of mischief. It would require considerable observation to determine those conditions precisely; but, as a general rule, it would appear to be safe and suitable wherever the administration of mercury is called for. It may therefore be prescribed generally in the congestive division of West Indian fever; and occasionally in the decline of the inflammatory; in those cases, namely, in which, after the subsidence of the primary and proper symptoms, secondary symptoms of congestion arise; or when high vascular action and temperature being reduced, salutary crisis dose not follow. The dose may be from one drachm to four, repeated every second hour, or according to the urgency of the symptoms and state of the stomach; and it may be given pure, or made into an emulsion with honey and mucilage, to which a portion of some aromatic tincture may be added. Though the amount of dose and frequency of repetition ought to be in some measure regulated by the urgency of symptoms generally, and state of the stomach in particular, we must not be deterred from its use by nausea and vomiting; because few cases will occur in which they are not present from first to last: when, as happens in this form of the disease, the quantity of fluid ejected by vomiting is greater than the quantity swallowed, the cause thereof being of the very essence of the disease, it will appear how difficult the suppression of this symptom must be. It is the most appropriate, and one of the most intractable which characterise the disease; and as it must depend upon

a peculiar morbid secretion from the mucous surface of the alimentary organs, there is reason to expect, that the oil of turpentine, by its direct action on the stomach and intestines, may so impress them as to modify or arrest that secretion; if it do so, it will doubtless have other salutary effects upon the system; and it may have such general effects, even if it fail in directly impressing the alimentary organs. The general effects of this remedy on the body are analogous to those of mercury. Like mercury, turpentine excites the vascular system primarily, and increases secretions ecndarily, though not so steadily or powerfully as the former; it acts however more suddenly, and it appears to dispose the body to be more readily impressed by mercury. It has the power of exciting directly in itself; and of preparing the way for other remedies, inasmuch as it stimulates the absorbents; a most desirable object, and one very difficult to obtain in this disease; and it therefore appears well suited to assist us in the treatment of congestive fever. But it must be employed, as has been already suggested, only in its place, and as one of many agents. If it be attempted to set it up as a specific, the end will be frequently failure and disappointment, probably abandonment; as has happened to many other valuable medicines, when more was required of them than they were capable of performing.

Such, I submit, are the principles which ought to guide us, and such the chief remedies to be employed. I do not pretend to particularize every occasional and less essential symptom. They are not to be altogether neglected, but they ought not to occupy much attention; for if we do not strike at the root of the disease, we do nothing to the main purpose. One of these symptoms however deserves notice, as being pretty constant, and often very distressing; I refer to the sense of burning in the stomach, œsophagus, and pharynx, which appears similar to that experienced in car-

dialgia. Calcined magnesia in mint water will often prove a useful palliative for this symptom, though it will seldom be subdued till the disease itself be cured, or essentially modified: at the same time the patient often expresses a strong desire for wine or spirits, which, under suitable restrictions, should, I think, be allowed him. From the injudicious use of these stimulants in febrile disease, injury has certainly arisen, and a general prejudice has thence been excited against them, especially in the precipitous fevers of the West Indies. In certain conditions of the congestive disease, in that, namely, which exists in the last stage, and sometimes in the first, they are not only safe, but salutary.

With regard to the management of convalescence, I am not aware of any thing that requires to be peculiar. In the inflammatory fever it will generally be speedy and complete; in the congestive it will often be slow and precarious. In the first stage of both, especially of the latter, while it is yet in a state of vacillation, and the patient's fate is in suspense; when, though the force of the disease is broken, the functions of the body are imperfectly restored, or partially obstructed, great care and circumspection are necessary; and in this state of matters the sulphate of quinine promises to be very useful. When this state has passed, and there is return of appetite and of sleep, attention to the nonnaturals, as they have been called, should be our principal care. The food must be limited, light, and nutritious; and this is the most important affair of any, fatal relapses often coming on soon after indulging in a large and indigestible meal. The secretions of the bowels and skin are to be carefully attended to, and daily motions by stool procured. There ought to be perfect cleanliness, thorough ventilation, and, as far as possible, provision for general comfort. Exercise is to be alternated with rest, and suited to the strength of the patient; it must therefore be short

and frequently repeated: and, finally, we should contribute, by every means in our power, to tranquillity and cheerfulness of mind.

In the *apoplectic* species of congestive fever, I can offer no method of treatment which is recommended by any considerable measure of success. When the subject is struck down in an instant, and continues to lie prostrate, with a cold skin, fluttering pulse, and dilated pupil; and when, as frequently happens, there is no natural, consequent excitement, as the first necessary step towards recovery, it will appear how formidable the disease is, with which we have to deal. Of all the modifications of West Indian fever it is the most intractable; and in a great majority of instances has ended in death, without being arrested or even modified by art. Whether the whole amount of mortality has been the necessary result of the nature of the malady, or whether part of it should be ascribed to the inadequacy of means, and the injudicious manner in which they have been employed, cannot be ascertained. It must at any rate be admitted, that the practice adopted has not always been the most reasonable; and every one must have felt, when treating the disease as an epidemic, how far his services fell short of his own desires, and the necessities of his patients; he must have felt, that when six demanded all his thought and exertion, it was impossible to discharge his duty adequately to fifty or a hundred. But however abundant the means, or skilful their application may be, the issue will, I apprehend, be more frequently fatal than fortunate.

The outline of practice to be proposed is therefore, in a great measure, problematical, suggested by the most striking appearances of the disease, and the pathological analogy between it and the *aggravated* species of West Indian fever. On these grounds the principles of practice will be similar, the means being modified and varied ac-

cording to peculiar exigences: hence, in this species, their application must be more prompt, and their power, if possible, greater. In the first place, the patient is to be put into a hot bath of high temperature, from 106 to 110 degrees; the surface, especially of the trunk, to be rubbed briskly at the same time, and the frictions to be continued after the use of the bath, with hot turpentine; and these means are to be continued and repeated, according to their effects. It will be useless, I believe, merely to put the patient into a bath at 98°, and take him out again in three or four minutes; the temperature must be raised above that which a person in health could bear, and continued for a considerable length of time. It is curious to observe, and important to know, as marking the different modes of the morbid impression, the different methods in which the warm bath acts on inflammatory and congestive fever. In the former, especially after copious abstraction of blood, immersion for a few minutes brings on faintness, feebleness of pulse, diminution of temperature, and other signs of reduced energy; in the latter the same continuance of immersion has little effect, longer continuance is followed by increase of heat, rising of pulse, and general increase of animal force; at least such is its tendency, and such commonly its results—results which are peculiarly desirable, though seldom adequately obtained in this, the *apoplectic* species. Calomel next presents itself, according to the indications of analogy; but it will generally be found incapable of exerting much power in this form of disease; at least in the first stage, and without preparative measures, it appears to be almost, if not altogether, inert. Previous to its administration therefore, and simultaneously with the external stimuli, the baths and friction, I would give oil of turpentine, and even brandy, and I would besides use the former hot, in the form of enema. I would then, in the course of three or four hours, have recourse to calomel in

large doses, without opium, except in cases of great gastric irritability. When, after the diligent use of these remedies, there shall be some degree of action excited at the surface, a large blister should be applied to the abdomen, another between the shoulders reaching to the scalp, and small ones to the legs, and other parts of the body. If we succeed, by these and other means, in exciting the vascular system generally, and to considerable extent, blood ought to be abstracted cautiously, and under restrictions similar to those prescribed in the *aggravated* species: but when this shall fortunately be effected, when the pulse shall acquire strength and the skin heat, the progress of the disease will be similar to that of the *aggravated* species; the treatment ought also to be similar, and its particulars need therefore not be repeated here.

Might not galvanism and electricity prove serviceable, if employed in the first stage of this disease? Seeing that the heart loses the power of adequately propelling the blood, and that the blood consequently accumulates in the large interior trunks, the functions of the capillaries being suspended; seeing that this state depends upon the instantaneous abstraction of something essential to the healthy action; and believing that the seat and source of this principle, whatever its nature or name, is in the nervous system; knowing that the restoration of the heart's power is absolutely necessary, whatever else may be necessary of which we are ignorant, and that therefore the restoration of the principle on which it depends is also, and in the first place, absolutely necessary; knowing, moreover, the direct power of excitation which galvanism and electricity possess, when properly applied to the body; — it is reasonable to expect that they would exert beneficial influence here: they appear to be stimuli well adapted to our purpose, inasmuch as they instantly rouse the heart and other parts of the vascular system, through the medium of the nerves;

and they are therefore deserving of a full and careful trial.

The outline of treatment, which I have presumed to sketch for the *apoplectic* species of congestive West Indian fever, is necessarily extremely imperfect, and subject to the graver imputation of being, in a great measure, tentative. It may be thought visionary, and I am aware that it would be easy to throw ridicule on a plan of improvement which had no better foundation than probability and expectation. I am sorry that I have nothing better to offer; that I cannot dismiss conjecture, and substitute proof, in a list of cases cured. But this is not the only disease, in which, after all our experience, pretensions to successful practice must be founded on experiments yet to be made. In how many are we satisfied of nothing but their mortality, our ignorance, and the impotence of our art! It is deemed laudable, in other cases, even on slender grounds sometimes, to try new, when all the old methods have failed; and, if founded on reasonable principles, such an endeavour should not be blamed in this. I have therefore thrown these hints and proposals together "tentare remedia similia illis quæ vicino malo succurerint."

CASES.

The following cases, taken from a great number of similar import, are given in illustration of the division proposed, and the opinions delivered, in the preceding Report. They are defective in detail, as such cases, when compressed into a narrow space, must be, and as the hurry and embarrassment of the time when they were noted rendered necessary: they do not therefore comprise all the appearances and changes, some of them important, which occurred, and which are, even at this moment, present in

my recollection; but they are just, and therefore accurate, so far as they go. Much may have been, a good deal I know has been, omitted; nothing has been added; scrupulous adherence to written records of things, as they presented themselves, being essential to a faithful, though memory might enable me to add circumstances which would be valuable to a more perfect, representation. By such cases, the division adopted, and the opinions exhibited, in so far as they are peculiar, were first suggested, and by such cases those opinions have been strengthened and confirmed; it is therefore proper that some of them, as the foundation of that division and those opinions, should be communicated.

MILD INFLAMMATORY.

John Stewart (seaman, æt. 28, at sea, 10th September), after short and slight rigor, which came on instantly, and while in perfect health, has a smart attack of fever. Complains of severe pain in the forehead, ringing in the ears, pain in the back, knees, and legs. The skin is hot and dry, equally and highly so; the pulse 110, full and firm, much throbbing of the carotids, flushing of the face, and restlessness. The tongue is white and dry, much thirst and nausea, without vomiting. V. s. ad ℥xiv, which brought on faintness, bilious vomiting, and perspiration. In two hours the symptoms were as severe as before, with more complaint: venesection repeated to the extent of thirty ounces, with recurrence of similar effects. Evening. There is again considerable excitement, but he complains less of his head, and appears better. Pulse 106, fuller and softer. A cathartic bolus of calomel and jalap. 11th. Has passed a restless night, but does not appear worse, and the heat and vascular action are moderate. He has not been perfectly purged. A solution of salts and bitters. Evening. Frequent copious motions, and he says that he is much

better, and appears so. Pulse 100, soft; skin reduced in temperature, only partially moist; tongue white and soft. A bolus, containing of calomel and antimonial powder, each six grains. Evening. Report as in the morning; medicine repeated. 12th. Symptoms favourable; he has slept a good deal in the night. Repeat the purgative draught. 13th. Without complaint. 14th. Desire for food. 17th. Returned to duty.

William Darling (seaman, æt. 30, 16th September, at sea), awoke in the morning with severe headach, having gone to bed in perfect health. When first seen, there was considerable febrile force, a full bounding pulse at 106, great heat of skin, anxiety of countenance, with redness of the eyes, a dry tongue with thirst, general distress and complaint of back; he cannot rest in one place; says that he has had the disease before, and knows that he shall be very ill. A full dose of calomel and jalap: two hours afterwards, complains more of the head, and is very anxious to be bled. Forty ounces removed, ending in faintness, fluttering of pulse, and vomiting. Soon after, the purgative medicine operated copiously, and he expressed himself much better. Evening. Some, but no considerable, return of excitement. Aqua ammon. acetat. prescribed. 17th. Has passed a tolerable night, and appears better. Pulse 100, full; skin moderately hot, and dry; headach not severe. A solution of salts in bitter infusion. Evening. Continues tolerably easy; bowels have been frequently and fully purged. Six grains of calomel and the same quantity of antimony in bolus. 18th. Says his head is giddy, and that he is sick at stomach: appearance favourable as to pulse, skin, and general aspect. No medicine. Evening. Appearance promising: bolus of calomel and antimony, with a grain of opium, repeated. 19th. He slept well, and has little complaint. 20th. Convalescent. 28th. Discharged to duty.

VIOLENT INFLAMMATORY.

Joseph Barker (boatswain's mate, æt. 35, 17th September, at sea), after a short cold stage, had a severe accession of fever, characterized by severe darting pain across the forehead, violent arterial action, particularly conspicuous in the temples; flushed face; hot, dry skin; and great distress exhibited in the expression of the face. Fifty ounces of blood drawn from a large orifice, followed by faintness, profuse sweats, and sickness at stomach. An hour afterwards, when he had recovered from the immediate effects of the blood-letting, he had a bolus, containing six grains of calomel and four grains of antimonial powder. In the evening, the symptoms were moderate. 18th. Symptoms of fever continue moderate, with softness of skin; no alvine discharge. A saline purge. Evening. Bowels fully opened, but the skin is dry and rather hot; headach, pulse, and thirst moderate. Calomel and antimony repeated. 19th. Report as yesterday; two motions by stool. Medicine repeated. 20th. Has little headach; the skin is soft; pulse 86, of good strength; tongue white and moist; no thirst. From this date, till the 27th, he continued to get better, when he was sent to hospital in an advanced state of convalescence.

Henry Holmes (carpenter's mate, æt. 36, 20th September, at sea). Accession of fever in this case like the last; but the affection of the head is more severe, and portends greater danger. He complains much, and moves the head constantly; the face is flushed and swollen, the eye wild and rolling, and the temporal artery throbs violently. Venesection. He became faint and sick at stomach when twenty ounces had been removed, but the headach was not relieved, and the skin only partially relaxed. Four hours afterwards the symptoms were more severe than before, and extremely urgent. Venesection repeated. Fifty

ounces were removed before he became faint, and when the arm was bound up the skin was perfectly relaxed; the pulse reduced from 120 to 106; head greatly relieved. A bolus, containing six grains of calomel, four of antimony, and one of opium, given. Evening. Continues relieved; skin moist, and pleasantly warm; pulse 110, full, but not oppressed; tongue white, and rather dry; thirst moderate. No medicine. 21st. Has slept a good deal in the night, and says he is easy; pulse moderate; skin moist; thirst, and bad taste of mouth; bowels costive. A saline purge. Evening. Salts have cleared the bowels; and though the skin be hotter, and not so moist, he says he is easy, and the symptoms have a favourable appearance. Bolus of calomel and antimony, without the opium, repeated. 22d. Crisis apparently perfect; pulse, skin, and general appearance nearly natural. No medicine. 23d. Continues free from complaint. Bitters and laxatives occasionally. 25th. Steadily convalescent. 27th. Report as before.

James Gristone (seaman, æt. 22, 10th September, at sea), complains of severe pain across the forehead, increased by motion, pain in the loins and legs, thirst, and inability to move; symptoms which came on suddenly an hour ago. Pulse 110, full and firm; he seems much alarmed. V. s. ad ℥ xxx, followed by faintness and sweat: a full dose of calomel and rhubarb. Evening. Symptoms as violent as before the blood-letting; arm tied up, and ℥ xxv more withdrawn, which induced syncope, and relaxation of skin: bowels moved soon after. 11th. Skin dry, but not hot, except at the scrobiculus cordis, where it is not well developed, but confined and smouldering. Pulse 100, wavering: mind affected with partial delirium; tongue white at the tip, brown at the root; bowels open; motions partially feculent; stomach irritable. Six grains of calomel thrice daily. 12th. Calomel has been retained; symptoms nearly as yesterday; vascular action rather diminished. A scruple of calomel to be repeated

in the evening, if retained by the stomach. 13th. Calomel has been retained, and appears to have acted favourably; he is more quiet, and though there is no perspiration, the skin has a more natural feel; the mind is more composed; calomel to be continued. 14th. Symptoms nearly as yesterday; stomach retentive; frequent pitchlike discharges by stool: a scruple of calomel in the evening. 15th. Complains of acute pain in the hypogastric region; otherwise nearly as yesterday: a blister to the part affected; calomel repeated in the evening. 16th. Blister has acted well, discharge yellow, and he says the pain is removed: no appearance of ptyalism: a scruple dose of calomel to be repeated in the evening. 17th. Little change, till, on being taken out of bed, he was seized with convulsions, which went off on being replaced in bed; but the pulse failed, the skin was covered with cold sweat, and I expected speedy dissolution. Cordial draughts ordered, and heated bodies to the surface. He gradually recovered heat, the pulse returned, and, in the evening, I indulged the hope, that, unpromising as the circumstances had been, a favourable crisis had taken place. He had now taken 178 grains of calomel, without salivation. 17th. Continues to do well, the pulse having some strength, the skin moderate in temperature, and the mind, though imbecile, being composed. Cordials and tonics. 18th. A very copious discharge of dark flocculent urine, and he continues improving, 20th. Decidedly convalescent. 27th. Sent to hospital to recruit strength.

This case is inserted for the purpose principally of showing the rapid transition from the inflammatory to the congestive mode of action, at an early stage of the disease; the striking difference in the general morbid condition, on the first and second day of its progress, the 10th and 11th of the month. On the former, the symptoms were those of high and dangerous excitement; on the latter, they were those of a more dangerous state, *viz.* sudden and great diminution

of excitement; on the former, blood-letting was indicated, and urgently called for; on the latter, further blood-letting would have been injurious, I believe, if not fatal: it would, at the same time, have been dangerous or fatal, I believe, to have left the patient to himself, or, what is equivalent in such cases, trusted to trivial measures and temporising shifts. It may be noticed, in this case, that the calomel never affected the mouth; it was nevertheless the means, I am persuaded, of saving the patient's life. Similar, though not such striking, changes may be observed in some of the other cases.

INTENSE INFLAMMATORY.

Mr. Drummond (midshipman, aged 17 years, 2d September, at sea), a very healthy, stout young man, of sanguine temperament, complains of great depression, languor, and desire to lie down; pain in the abdomen, and headach, with sense of intense heat over the eyebrows. Calomel and rhubarb, four grains of the former and a scruple of the latter. 6 o'clock P. M. Skin hot and dry; pulse 110, strong and compressed; headach severe, with pain of loins and legs; tongue white and dry. Sixteen ounces of blood taken from the arm induced faintness and reduction of temperature, but had little effect on the disease. 10 o'clock P. M. Blood-letting repeated to the extent of twenty ounces, followed by tendency to syncope, slight moisture on the skin, and lowering of pulse: no effect in arresting the progress of the disease. 3d. Skin, especially on the trunk, intensely hot, scorching as it were the hand applied to it; pulse 116, firm and rather full; intellect clouded, and he is roused with difficulty to answer questions; rolls the head, tosses incessantly, but asks for nothing, and says he is well. Forty ounces of blood removed, which lowered the strength and diminished the celerity of the pulse, relaxed the skin, and induced tendency to syncope, but had

no effect on the intellect; did not appear to relieve the brain, or promise much benefit. Cathartic repeated; a large blister to the neck and shoulders, and the surface to be constantly sponged with vinegar and water. Evening. Has had frequent small, dark-coloured stools, and the skin is cooler, but he is not better; head to be shaved, and a large blister applied to the scalp. Calomel and antimony, four grains each, every second hour. 4th. Pulse 96, softer and smaller; skin cooler, but still dry; head not relieved; tongue brown and dry towards the root, white at the tip; stools still dark and gelatinous, without feculence. Calomel and antimony continued. On applying the hand to the epigastrium, a kind of suppressed smouldering heat is perceptible, combined with a peculiar sensation of harshness; but pressure gives no uneasiness, and the stomach is retentive. Evening: no improvement; vital powers beginning to flag. Calomel and antimony as before. 5th. Dark-coloured, involuntary, alvine discharges; extremities getting cold; pulse failing; progress to dissolution apparently rapid. Cordial draughts of æther and aromatic confection to be given at intervals, and heated bodies to be applied to the extremities. From this time till 10 P. M. he continued sinking, when, after being slightly convulsed, he died.

William Crawford (seaman, æt. 20, September 23, at sea) was sent to duty yesterday, cured of a slight attack of fever. At present the symptoms, which came on in the night, are extremely severe, *viz.* loud complaint of headach, especially in the temples and orbits, which are splitting, according to his own expression; the face is flushed, swollen, and alarmed; the eye watery, rolling, red, and impatient of light; the skin is intensely hot and dry; the pulse 110, firm and astonishingly forcible; the carotids throb vehemently; he rolls the head from side to side; snuffles and snorts in breathing; tosses his limbs incessantly,

and is in great distress; says that he awoke in the state in which he now is, and that he perceived no shivering. The tongue is dry, chapped, and he cannot propel it from the mouth; thirst insatiable. Venesection: twenty-four ounces brought on pallor of countenance, fluttering of pulse, and vomiting; no relaxation of skin. A full dose of calomel and jalap; head to be shaved and sponged, as well as the body generally. In three hours the symptoms were more severe than before; the blood-letting was therefore repeated, and forty ounces removed, which was followed by faintness, and relaxation of surface on the extremities, and the pulse did not rise for an hour afterwards. Has copious alvine discharges. Evening: symptoms risen again, but not so high as before the last blood-letting. Aqua ammon. acetat. during the night, and a blister to the nape of the neck: sponging continued. 24th. Has passed a sleepless, tossing night, and the symptoms of excitement are more intense than ever; pulse bounding, that of the carotids shaking the head at every stroke; eye wild and frenzied, red and impatient of light; tongue dry, brown, and hard, like a piece of board; tries to push it out, but cannot; skin burning hot; incessant crying for drink; delirium; continual and violent jactitation. Thirty-five ounces of blood suddenly removed; syncope; he lay long in a state of apparent asphyxia, and I began to be apprehensive of the issue, when he gradually revived. Purging salts in bitter infusion; sponging, &c. Evening: numerous motions by stool, first feculent, latterly gelatinous, with tenesmus and strangury; pulse soft and full, not strong, sinking readily under the finger; skin cooler, still dry; eye less inflamed, not more composed, or satisfactory; thirst not abated; does not complain, but tosses incessantly; gasping and desire to be naked; catching at imaginary objects; entire aspect unpromising. Two grains of solid opium administered. Towards morning he fell asleep, and on the 25th, when he

awoke, the pulse had fallen considerably, the skin was cool, and he was on the whole apparently better; but his answers were not distinct, and the event very doubtful. A dose of purging salts. Evening: has had a copious discharge of urine, with frequent watery stools; still slightly incoherent; tongue more moist, and softer; pulse 106, soft; skin cool, dry, and harsh. Calomel ten grains, antimonial powder ten grains, opium one grain and a half, in bolus. 26th. Has passed a good night, and is collected; pulse fuller; skin warmer, and pleasantly moist; tongue white and soft. 27th. Little complaint left, but is extremly weak. Sent to hospital, where he recovered rapidly.

SLIGHT CONGESTIVE.

Richard Curran (seaman, aged 42 years, at sea, 11th September) complains of dull, oppressive headach, pains across the loins, in the thighs, and calves of the legs; eyes heavy, languid, and inexpressive, exhibiting the appearance of drunkeness; pulse 108, moderately strong, without compression; skin hot, not intensely so, or dry. Calomel and antimonial powder, six grains each. 12th. Says he is easier; symptoms as yesterday; bowels constipated. A saline purge. Evening: salts have operated fully; little change. Calomel and antimony repeated. 13th. No sleep in the night; pulse, skin, and general appearance as yesterday; tongue white, but moist; calomel and antimony as before, to be repeated in the evening. Evening: frequent dark-coloured stools, with gastric irritability; a grain of opium to be added to the bolus, and a blister applied to the epigastrium. 14th. Stomach retentive; stools more copious and feculent, less frequent; pulse 100, fuller; skin dry; tongue continues white and moist; gums swelled, without increased salivary discharge; head easier; eyes yellow, but exhibit the appearance of perfect consciousness; urine copious, yellow, with deposition of saline matters.

Bolus of calomel and antimony repeated. 15th. Ptyalism induced, and every symptom mitigated; pulse full at 86; skin moist; tongue clearing; stomach retentive. Omit the calomel and antimony; a saline purge, and an astringent gargle. 16th. Continues easy and comfortable, with profuse salivation. 17th. Convalescent.

Thomas Wilkins (seaman, aged 22 years, 13th September, at sea), affected similarly to Richard Curran, and the progress and issue of the symptoms were similar. On the third day of the disease, ptyalism was established, a copious eruption of pustules breaking out around the mouth at the same time. From that time he recovered steadily, and on the 27th was sent to duty.

AGGRAVATED CONGESTIVE.

James Clarke (seaman, aged 25 years, 23d September, at sea), sudden and severe attack, immediately after eating a hearty dinner; prominent symptoms, a full soft pulse at 106; skin nearly of natural temperature, excepting at the præcordia, where it is accumulated, but not intense; dull pain of head, with sense of fulness, heaviness, and irresistible desire to lie down, keeping the head low; countenance swollen and bloated; eyes have the appearance of drunken fatuity; limbs fall as paralysed. He is a stout muscular man, of no decided temperament, but verging towards the phlegmatic. Venesection to the extent of ix℥, which induced faintness and vomiting. Six hours afterwards, the pulse having acquired more energy, and the temperature risen considerably, the blood-letting was repeated to xviii℥, which ended in approaching syncope, followed by convulsive movements*, and frequent vomiting. A brisk purge, to be repeated till the bowels are freely

* A dangerous, according to my observation, generally a fatal symptom.

opened. 24th. A bad night, though he gives no distinct account of his sensations; frequent stools, at first feculent, afterwards gelatinous; pulse full, but without strength; skin clammy; tongue white and dry; face dark and haggard; eye heavy, and inexpressive. Hair to be removed from the scalp; and ten grains of calomel, with one of opium, to be given three times daily; friction to the surface, and cordial drinks. 25th. Says he is better, though there is no obvious amendment, and the stomach is irritable; matters ejected have a sour taste, and there is sense of burning at the cardia; treatment continued, and a drachm of magnesia with mint water added. 26th. Says he is better, and appears so; pulse and skin more energetic; countenance more natural in colour, and placid in expression; bowels costive; a saline purge, and the other medicines continued. 27th. Obvious improvement; mind clear; pulse 100, full, of good strength; skin pleasantly warm, and moist; stools feculent; stomach retentive; gums slightly swollen; from this date recovered rapidly.

William Prendergast (seaman, aged 27 years, at sea), after getting drunk three days before at St. Jago de Cuba, lying on the wharf under a powerful sun, and afterwards falling out of a boat, was on the 13th of December seized with the first symptoms of alarming fever; the countenance pale, haggard, and alarmed; the eye confused, watery, and drunken-like; the pulse rapid, small, and weak; the skin cold in an extreme degree; universal tremor, confusion of intellect, and heaviness, rather than pain, in the head. The bowels to be freely purged as soon as possible; this took place in the evening, without improvement or change. To take a bolus, containing six grains of calomel, four of antimonial powder, and one of opium; to be well covered in the night, and to have plenty of warm drink. 14th. In the morning there was little change, no reaction having

come on; at 3 P. M. however the pulse had acquired considerable fulness, and the skin some heat, especially about the breast, and he complained of headach; fourteen ounces of blood drawn from the arm brought on tendency to syncope, and deterioration of symptoms; bolus of calomel, antimony, and opium to be repeated. 15th. Febrile character continues ill developed, the pulse being unsteady, and the heat irregular; stomach irritable, and frequent vomiting. At 4 P. M. from heat of skin and fulness of pulse, I was induced again to open a vein, but instantly perceived that I was working mischief, the pulse falling, and the skin losing tone, without moisture; a cordial draught, and heat applied externally. At night he lay in a state of stupor, answering questions indistinctly, the head falling from the pillow, the stomach being irritable, with tension in the hypochondria, an unsteady pulse, skin hot in one place, cold in another. A large blister to the epigastrium; and a scruple of calomel with a grain of opium. 16th. Blister has acted well; stomach more retentive; heat more equable; cerebral symptoms not changed. Calomel to be repeated, without the opium. Noon: another scruple of calomel. Evening, stomach retentive; general aspect gives hope; bowels open. A scruple of calomel and grain of opium. 17th. There is no perceptible change; stomach retains the medicine, and he says he is well, but this is a false promise; calomel to be repeated as yesterday, should nothing arise to prevent it. Evening: tongue remarkably clean and red, like a piece of pure muscle; medicine retained; eyes and breast darkly yellow; intellect still confused; pupils not affected by light; bowels open; there is great danger, but from the state of the stomach, and probable operation of the medicine, there is still hope; calomel and opium continued as yesterday. 18th. Nothing appears to change the opinion of last night; medicine to be continued as before; bowels open. 19th.

Temperature of skin higher and more equally diffused; pulse full and soft, 106; ideas clearer; gums swollen; breath fœtid; hope of crisis: calomel only twice a day, with opium at night. 20th. Copious salivation; general condition, that of comparative health. From this time, though the convalescence was slow and precarious, he continued, with much nursing, latterly at the hospital, to get better, and finally recovered.

APOPLECTIC CONGESTIVE.

Edward Austin (seaman, aged 25 years, 16th September, at sea), while working, was suddenly taken with symptoms which had much the character of apoplexy; could not stand; said his legs would not support him; that he was giddy and very unwell, but did not refer to any particular symptom; and said that he could not tell what was the matter with him; pulse 110, soft, and without resistance; body covered with profuse, greasy sweat; extremities dry; large drops of sweat on the upper lip; eye heavy and without expression; pupils rather dilated, and little affected by difference of light. Active cathartics, to be repeated every hour till the bowels be moved. In the evening the purgative medicines operated freely, and he expressed himself relieved. It was now attempted by frictions, epispastics, and by calomel every third hour in four grain doses, to determine to the surface, and relieve the cerebral congestion. 15th. No improvement; he lay nearly as he was placed, with the eyes half shut; low moaning; pulse faltering; with difficulty got to answer questions; treatment continued. 16th. Disease progressive; symptoms worse. Evening. Hæmorrhage from the nose and mouth; pulse small, irregular in strength and time; extremities getting cold. 17th. Profuse hæmorrhage from the nose and mouth; state of stomach, skin, and cerebral symptoms unchanged; towards evening the pulse rallied, and the skin became

generally moist, but he was completely comatose, and could not be got to take any thing; no hæmorrhage. 18th. Lies nearly in the same state as last night. About noon *black vomiting*, but apparently no sensation. In the evening the breathing became hurried and stertorous; and he sunk on till midnight, when he died.

In this case the treatment was too inert; at least according to my present views; it was by no means commensurate with the urgency of the symptoms; but whether the remedial measures, in the form which I have proposed in the preceding pages, would have essentially influenced the event, it is impossible to know.

David Harris (carpenter's crew, aged 23 years, 16th September, at sea). Accession in this case like that of the preceding. Venesection to twenty ounces, followed by faintness, and sickness at stomach, and, he said, relief; but it was easy to see, an hour after, that it had done no good. A smart cathartic of calomel and rhubarb, followed by feculent stools in the evening. After this there were frequent dark, gelatinous-looking discharges by stool, and complete exhaustion of vital action, and sensorial power. 17th. Pupil dilated; pulse full and soft, sinking under the finger without resistance; skin soft, moist, and doughy; does not answer questions, and lies apparently without sensation. In the evening the stomach became irritable, and he shrunk when pressure was made on the epigastrium; a blister applied there. 18th. Feces and urine passed involuntarily; pulse flagging; skin covered with sweat; cold; eye fixed, insensible, and glassy. 19th. As yesterday. Evening; died.

Mr. Davidson, assistant surgeon, complained at noon, on the 14th of September, at sea: was busily employed all the forenoon among the sick, as he had been almost con-

stantly day and night for some time before, and was fatigued, but not unwell, till now. A stout and very healthy young man. Says that he feels inexpressibly weak and depressed; cannot move his legs without effort and difficulty; is giddy, and has a dull oppressive sensation in the forehead, with nausea, and occasional indistinctness of vision. The pulse is 112, full, soft, and weak; the skin doughy, greasy, and here and there hot, at other places cold; in some places, particularly on the face, large drops of sweat standing. To be freely purged. Evening: scarcely any change; no pain anywhere; general sensations as before; bowels not moved. Purgative repeated. 15th. Has been frequently purged; tenderness at the epigastrium on pressure; stomach irritable; frequent vomiting of a watery fluid, with sense of burning in the region of cardia and œsophagus; pulse soft and weak at 104; skin moist and clammy, cool, excepting at the præcordia, where the heat is deep and depressed; eye heavy, turgid, and inexpressive; has no pain, and dozes much, but is collected when moved. Six grains of calomel, to be repeated at noon. Evening. Report as in the morning. Calomel repeated, with a grain of opium. 16th. Vomiting continues; frequent dark gelatinous motions by stool; skin dry, cool, and harsh; pulse weak; tongue white and moist; eyes yellow; face dark and desponding, with a peculiar smile at times*;

* It would be difficult to give a just notion of this peculiar smile to a person who has not seen it. Unlike the smile of health and happiness, it is confined to the mouth; the face generally, and the eye in particular, having no share in its expression, but rather counteracting or contradicting it, by their fixedness and despondency: neither has it any of the hideousness and distortion of the *risus Sardonicus*. It is a quiet, transient, smiling movement of the lips alone, melancholy in itself, and by contrast with the general aspect; and rendered more melancholy, by being associated with apprehensions of a fatal issue; for in my observation it has always been the forerunner of death.

seeks for change of place; keeps the head low; says he has no pain; epigastric tension, and tenderness on pressure. With difficulty procured a hot bath, and had the surface briskly rubbed while in it, and afterwards; no effect on the skin; some rising of pulse. A large blister to the epigastrium, and the calomel to be repeated every third hour, with draughts containing tincture of cinnamon, peppermint, and magnesia. Evening: little change; no improvement, though he says he is better; gastric irritability, vomiting, frequent calls for stool, with tenesmus; discharges dark and unfeculent; skin cool, but says he is hot, and wishes for cold sponging; blister has not acted. Treatment continued. 18th. Early in the morning there was frequent *black vomiting*; describes the sensations minutely connected therewith, but says he has no pain; perfectly rational; pulse feeble; tongue clean, and red in spots; breast and neck mottled with livid and yellow patches; skin cool, coldish at the extremities; desires wine and porter, which are allowed him, with cordial draughts. Evening: has ejected large quantities of *black vomit* during the day; the vomiting occurring two or three times every hour, and the quantity each time being great, the amount is scarcely conceivable: pulse feeble; ideas beginning to wander, his imagination being busily engaged about the patients whom he had been attending, and in whom he was much and very intelligently interested: desires me to give a positive opinion about the issue of his own disease, to which he recurs at intervals; wishes to get up and visit a particular patient; picks at the bed-clothes; sinking fast. 18th. Lies as he is placed; cannot articulate; vomiting ceased; dark yellow spots extended on the body; petechiæ about the hips, thighs, and arms; eyes half shut and turned up; breathing hurried; expired at 10 o'clock A. M.

The following case exhibits that complex form of the

disease, which I before noticed, which is constituted by the rapid alternation or coexistence of the congestive and inflammatory conditions, and which is extremely perplexing in practice, inasmuch as it presents conditions, which, in close succession, or at the same time, indicate different or opposite lines of treatment.

William George (marine, aged 20 years, 27th August, at sea), a man of remarkably torpid temperament, and whose skin has been habitually dry since his arrival within the tropics, has complained occasionally of headach, since a fall he had about three weeks ago, but said it was worse on the 27th. He was ordered a smart purge, and desired to keep his hammock; but refused to stay in bed, wandered about the different births, and his manner was observed to be unusual; the cathartic however operated freely, and in the evening he seemed better. 28th. Febrile symptoms strongly developed; pulse 116, full and strong; headach; pain in the loins and calves of the legs; skin hot, intensely so at the pit of the stomach, but has no pain there; tongue white, and dry in the middle; great restlessness, with intellectual incoherence. Venesection to thirty-four ounces, followed by faintness, cutaneous relaxation, &c. About an hour after, the skin having become again hot and dry, but not in the same degree as before the bleeding, he was ordered a bolus, containing twelve grains of calomel, four of antimonial powder, and one of opium; to be repeated in the evening, without the opium. Evening: skin cooler, with slight moisture; pulse reduced; mind more clear. 29th. Symptoms aggravated, particularly the mental affection. Nape of the neck to be cupped and scarified. Evening: six ounces of blood were removed by the scarifications, which was followed by reduction of temperature, moisture of surface, and clearness of thought; expresses himself relieved. 30th. The mind continues correct, but the stomach has become irritable, rejecting every thing he

swallows; and the bowels have not been moved. Cupping and scarifying over the scrobiculis cordis, to be followed by a blister; little blood obtained; to take the following draught: ol. terebinth. ʒ iv, ol. menth. pip. gtt. iii, muc. gum. acaciæ, ℥ i. In three hours there was a copious alvine dejection of dark mixed mucous sordes, and less irritability of stomach. Made earnest request for a little wine and water, which was allowed. Evening: another evacuation by stool, and appears better, although the state of the pulse and skin gives no indication of crisis. Mind clear and collected, but apprehensive of the event. Draught of turpentine, &c. to be repeated. 31st. Frequent discharges by stool; stomach retentive; pulse 110, small; skin cool, but not uniformly so. Cordial draughts of aromatic confection and peppermint. Evening: delirium; requires to be kept in bed; extremities cold; eye wild; countenance haggard; tongue dry, and brown in the middle. A full dose of laudanum, with camphorated mixture, to be repeated. At 10 o'clock P. M. he fell into a state of stupor, and continued sinking till 4 A. M. of the 1st of September, when he died, slightly yellow about the mouth and neck.

I have alluded to a modification of congestive fever, in which the symptoms are so insidious, that the inexperienced and unwary will scarcely apprehend danger, and the most careful and practised may sometimes be taken by surprise, the tendency to fatal issue being great, and the progress of the disease rapid, notwithstanding its apparently mild and trivial impression; the following case is given in illustration of that modification.

William Witham (boy, aged 16 years, 27th August, at sea), has the appearance of languor and depression, but does not complain. When questioned as to his feelings, says that he is chilly, with inclination to sleep, but is not in bad health. No medicine. 28th. Complains of head-

ach, with incapacity of keeping the head up, but is otherwise not unwell; pulse 86, and of good strength; temperature increased about the breast, not generally; tongue moist, and little thirst; says that his bowels have not been confined. Submuriat. hydrarg. gr. iv, pulv. rhei gr. xv in bolus. Evening: the bowels have been freely opened by the medicine, and his chief complaint is a bitter taste in the mouth, and a strange compound sensation, of desire for drink, and loathing of fluids when tasted. 29th. Symptoms nearly as yesterday, but there is more heat about the præcordia, and he complains of pain in the right hypochondrium on pressure; skin dry. Calomel and antimony in repeated doses, so as to act on the chylopoetic viscera and skin. Evening. There is slight moisture on the skin, and he expresses himself much relieved; complains merely of want of appetite, both for meat and drink; bowels open. No medicine. I was called early next morning, and found the extremities cold, and the pulse at the wrist imperceptible, and he was vomiting frequently a dark fluid matter, known by the name of *black vomit*. He would take nothing, firmly shutting his mouth when any thing was offered him. At 9 o'clock he was occasionally convulsed, complaining, as if from acute pain, but with reference to no particular organ. At 11 A. M. on the same day, 30th, he died.

MEMOIR II.

The prevailing Opinions regarding the Cause of West Indian Fever considered.

THERE has been more controversy respecting the cause of what has commonly been called yellow fever than that of any other disease; and, though many arguments have been employed in support of the different views of the subject, there has been no lasting conviction from any of them.

While one party has asserted, and laboured to prove, that it is always the effect of a specific contagion; another maintains, that it never does, or can arise from human miasmata, but is as universally the product of marsh, and marsh-like miasmata, as ague is the product of fens in Lincolnshire. Other writers, seeing reason to doubt the exclusive application of either, have supposed, that it may arise from each separately, or from both compounded: and others again contend, that the disease derives its origin from vegeto-animal decomposition, but that, in some instances from adventitious circumstances, it is so far changed in its nature, as to acquire the power of self-propagation; that it is primarily the effect of local causes, but, in the course of its progress, obtains a new property, which is capable of communicating the disease to healthy bodies coming within the reach of its influence. Some, setting aside contagion altogether, and not being able to satisfy themselves as to the nature of the soil from which it emanates, seeing no common qualities by which it is characterized, have contented themselves with the general conclusion, that it has a terrestrial origin; while

others, despairing of being able to trace it to any thing in the earth or its inhabitants, have ascribed it to certain undefined qualities and conditions of the air. The principle of vegetation in excess has been assumed, but not insisted on, by high authority; and many have considered it as casual in its origin, believing, that, though a general cause might be suspected, from the epidemic manner of its attack, it is the effect of accidents merely, such as debauchery and fatigue, cooperating with tropical temperature. The opinion has likewise been maintained, that atmospheric heat alone, acting directly on the body, is sufficient to produce West Indian fever.

To say, that it is the effect of something in the earth or in the air, is but to repeat the physical truism, *ex nihilo nihil fit*; and at the same time to confess entire ignorance of its origin. Inferences so vague and indiscriminate cannot satisfy the inquirer; nor can they enable the physician to give reasonable rules for prevention, either immediately or remotely, either as they regard precautionary measures to be employed by individuals in their own persons, or as directed against the cause itself, so that it may be diminished, or destroyed.

It is my object to bring the question within narrower limits. I shall endeavour to render the investigation more precise, if its results should not prove more satisfactory than preceding ones. Where so many with better opportunities and means of using them have failed, I can scarcely look for success, though I have brought to the task a considerable portion of observation, and the best reflection of which I am capable. Yet, if I should not have the satisfaction of attaining the exact point of truth, or of convincing others that I have done so, I trust that my employment will not be altogether useless, being convinced, that I am travelling, however tardily or unsteadily, in the road which leads to a true account, and that in this path alone it can be found.

But before I enter on the consideration of this subject, it is necessary to glance at the principal views which have been taken and are entertained, by those who have assigned a specific cause; and to show why they do not appear tenable.

ATMOSPHERIC HEAT*.

It is a familiar idea, that caloric excites the heart and vascular system to increased action; and hence it may be supposed, that atmospheric heat, if sufficiently high and continued, may occasion fever. Such notions would probably suggest themselves to unprofessional observers, and such conclusions be adopted by them, as sufficient to account simply, if not philosophically, for West Indian fever. It is not easy to conceive, that such reasoning could satisfy the medical inquirer; yet by some it has been entertained. It is believed, that the heat of the West Indies, acting especially on the bodies of men recently from Europe, is sufficient to occasion West Indian fever, with all its complication of symptoms and extent of mortality.

The disease, for which this cause has been assigned, is called by some inflammatory fever; but as it exhibits, in their description, the symptoms characteristic of the disease generally called yellow fever; and as no line is drawn by which it can be separated from the disease as it arises from other causes; it appears, that what is, in this instance, called inflammatory fever, is the fever to which, in general terms, I have given the name of West Indian fever, including the inflammatory and congestive species, and their varieties.

The supporters of this hypothesis imagine, when the human subject is transported from the north of Europe, where the medium temperature may be stated at 45°

* Mr. Dickinson and others.

of Fahrenheit, to the West Indies, where it generally ranges between 80° and 90°, and the migration is accomplished in a few weeks, that fever is produced by sudden transition from low to high temperature. They assert, that such heat, acting on the susceptible European, by its mere stimulation, and without reference to its action on the soil, impels the body, by some process, *analogous to friction it is presumed*, into West Indian fever: for, if I understand the doctrine, fever would arise as readily in the West Indies, on the summit of a granite column raised ten thousand feet above the surrounding surface, as in the plain below, provided the same degree of heat were acting on it.

At first sight, this view of the subject is unsatisfactory. There is apparent want of relation between cause and effect, and we therefore become sceptical regarding the agency of the former. We entertain doubts as to the sun's rays producing West Indian fever, by their direct influence on the body; though, when of sufficient intensity and continuance, we are convinced of their indirect power in its production. It does not appear, that they act immediately, but mediately through something in the earth, whence they generate or evolve the cause of this fever; and we therefore believe, that it is not the sun's rays, but their agency on the earth, which we have to dread. This however is a notion rather than conviction; it is more a matter of feeling that things ought to be so, than a reasonable account that they are so. But if we look a little farther, and examine the subject more carefully, we shall find reasons enough for rejecting atmospheric heat, as the cause of West Indian fever.

Few, in the strength of manhood, pass eighteen months after arriving in the West Indies, without an attack of their endemic fever. Many are seized in a few weeks, some in a few days; and there have been instances, by

account, in which the disease ended fatally before the subject landed, or the ship in which he passed came to anchor. In certain states of constitutional predisposition, and concentration of the cause of fever, a few hours will suffice for its production. Thus, a ship arrives from England at Port Royal, in the afternoon; and two hours afterwards an officer goes on shore, where he spends two or three hours, returning to his ship at 10 o'clock at night. Two days afterwards, without additional exposure, he shall be taken ill with fever, and die in three more, with all the unquestioned signs of the West Indian endemic. Such things I have known, and am therefore warranted in saying, that, in certain circumstances, the cause of West Indian fever acts instantly, and without preparation of any kind, excepting that of individual susceptibility.

It is also well known, that persons residing in the mountainous parts of Mexico, where the oak and pine grow, and where the disease was never seen; if, when on their way to other parts of the world, they have occasion to pass a single night in Vera Cruz, are there frequently affected with the fever in question, which, as in other cases, often ends in death. Let these things be remembered, and compared with the climate of the North of America, and of Europe, as regards temperature.

In Canada the thermometer falls below zero during the winter, and in summer rises to 90° or 96° of Fahrenheit, at which it sometimes continues pretty steadily for weeks; but it often suffers great depression in the night, sinking sometimes to the freezing point. Here then we have in a few weeks, transition from severe cold to intense heat; the temperature of the arctic and equatorial circles in close contact, besides great diurnal vicissitudes. In the North of Russia similar changes take place.

It will be observed, that the inhabitant of Canada, as regards change from cold to heat, is in a very similar con-

dition, when his summer succeeds his winter, to the native of the Orkneys, who migrates to Jamaica; while, in the diurnal vicissitudes to which he is exposed, he is placed on a footing similar to the inhabitant of Upper Mexico, who descends in a day, from the moderate temperature of his healthy habitation, where the thermometer indicates a medium heat of 60°, to the burning soil of Vera Cruz, where, in summer, it seldom falls below 90°.

So far things appear the same rather than similar; the suddenness and greatness of change from cold to heat are alike; these are matters to be identified rather than compared. But when we look at the effect on health, how striking is the contrast! In Canada, the fever in question is unknown; no one need be informed what ravages it commits annually at Jamaica and Vera Cruz. Why, in the former place, the high summer temperature does not so operate on the earth as to evolve the cause of the disease; whether the immunity of its inhabitants depends on the heat not being steadily and for sufficient time at the requisite point of elevation, or upon the soil not containing the materials necessary to its production, I shall not inquire. The facts stated are notorious, and the inference unavoidable—that intertropical heat, acting on the unassimilated body, cannot directly and independently produce West Indian fever; for if it could, the same fever would be found every summer at Archangel and at Kingston in Canada, as well as at Vera Cruz and Kingston in Jamaica.

CONTAGION.

I shall use this term, without reference to its derivative sense, as implying the necessity of personal contact for its operation, or endeavouring to distinguish it from infection. I shall use it simply to signify that matter, which, in certain febrile disorders, is supposed to be generated or propagated

by the human body, having the power of producing the same disease, as that from which it emanates, in other human bodies, placed within the circle of its operation, whatever the extent of that circle may be.

The subject is surrounded by manifold difficulties, inherent and adventitious; and has long been and continues to be the source of much controversy in West Indian fever. The inherent difficulties, which oppose us in examining the subject, arise chiefly from our ignorance, not only of the nature of this agent, but also of the laws which regulate its operations; for, after all, there is no certainty, in what time it operates, at what distance it operates, or in the circumstances most favourable to its operation; and partly from the scanty knowledge we possess of the terrestrial cause, the miasmata of the soil, to which it has been principally opposed. Hence many general arguments become nearly alike available to the supporters of either hypothesis.

Epidemic and contagious fevers were at one time considered identical, and are still so considered by some; it having become a sort of medical axiom, that a disease which occurs occasionally, without juxta-position of its subjects, is the effect of causes which terminate in the body on which they act; but that a disease, which prevails extensively, affecting great part of a community at the same time, is the effect of a specific poison, eliminated in one body, communicated to another, and producing the same effects in all.

In conformity with this rule it is asserted, that West Indian fever arises sometimes in the one way, and sometimes in the other; that it is contagious when it prevails epidemically, not contagious when it occurs sporadically. The spring cases of the disease, according to this hypothesis, possess no contagious properties; the autumnal ones are endowed with them all; and we have, therefore,

in Jamaica, the same fever, annually, occasioned by two distinct species of causation. For, while it is admitted that the disease has the same character, in both instances, it is maintained to be different; and the difference, it appears, is to be estimated chiefly from our *knowledge*, that in one it is contagious, and the other not contagious*. This is surely a most comprehensive use of the *petitio principii*, and has the appearance of being neither very candid nor very logical. There is an unfair demand made upon our credence, and a species of induction, which, if it be conformable to the methods of the schools, is not consistent with those of Bacon and of Locke. But it is extremely difficult to bring the subject of contagion, in West Indian fever, into an open and unembarrassed field of investigation, from the factitious causes of perplexity thrown around it, and the divisions into which it has been split by its various supporters; as well as from the inherent sources of doubt and obscurity to which I have already alluded. It is not my intention to enter on a discussion of their various claims, even if I had the means of prosecuting it to my own satisfaction.

I shall notice the opinions of two parties only; those, namely, who contend that the difference between sporadic and epidemic cases is absolute, the former never having the power of passing into the latter, that the fever which springs from common causes, and that which is occasioned by contagion, are essentially and constantly different: and those who believe the difference between the two modes of fever to be only incidental; who argue, that though, in the first instance, it arises from common causes, it may afterwards be propagated by a specific cause, the contagious property being generated by contingencies affecting the subjects of the disease.

* Sir Gilbert Blane's Elements of Medical Logic, p. 231, 2d edit.

According to the former, the causes of both forms of fever (should we speak of the forms of the same thing, or what manner of phraseology would express such ideas intelligibly?) are in perpetual existence, though not in constant operation. Contagion, the sole cause of the epidemic, has periods of repose, but never loses its power. It is always hovering over these islands, pregnant with the seeds of destruction. Whence this contagion came, or how it arose, its advocates have not agreed upon amongst themselves, and others will not inquire. Were its existence established, the inquiry into its origin might be abandoned, as a matter, though curious, not possessing practical interest. But they who maintain its operation, and wish to make proselytes, ought to inform those who doubt or disbelieve, why it acts at particular times and in particular seasons only. It is incumbent on them to show us the circumstances, which render it at one time active, and at another dormant. No connection has been traced between those different conditions, and hurricanes, rains, or other meteoric agents. The difference can scarcely be supposed to depend upon difference of temperature, because, in many places, there is little difference in that respect. At Port Royal it does not amount to more than twelve degrees during the year; and this we can scarcely conceive to affect matter so subtle as the essence of contagion. Besides, the disease seldom appears as an epidemic, till the temperature has been at its highest points for some weeks, often not till it is on the decline. In what way can such a change affect the matter of contagion? How so vivify, as to give it operative power in its dormant state? If augmented heat possessed the power, it would exert it at once, not after the lapse of weeks or months, or after it had ceased.

If the soil afford the pabulum of West Indian fever, which high temperature disengages and calls into action,

we can understand why a continuance of high temperature should precede an epidemic visitation of the disease. The chemical changes, necessary for its production, proceed gradually, and it is necessary that their products should accumulate to a certain extent, before it break forth as an epidemic.

But at the time contagious fever is desolating a community, the noncontagious is destroying individuals. They may be acting in close vicinage, or together, so that we cannot tell the victims of each. If two causes, as different perhaps as the causes of dysentery and small pox, produce diseases so exactly alike, that we cannot by any means distinguish them, how can we know, that in the same family some were not affected by one cause, and some by the other? We have no means of ascertaining the limits of each, and therefore, after vainly endeavouring to trace them to separate sources, reject the most uncertain and incomprehensible of the two, being satisfied that one is sufficient for the whole effect. We get weary of distinctions where we cannot find difference, especially when we are told to estimate the difference by our *knowledge*, that the one is a contagious and the other not a contagious disease.

The doctrine of contingent, is more common, I believe, than that of essential contagion, and prevails pretty generally, among those especially who have considered the subject out of the West Indies. It is, when admitted, so easy of application, so fitted to remove difficulties, and so suited to all perplexing cases, that it is not strange many should believe it. When the disease occurs occasionally, in its usual situations, it is said to be the effect of ordinary causes; but when it prevails epidemically, and there is no obvious cause in the soil, then it is said to have acquired contagious power, by which it propagates itself.

This, though specious, is probably not free from error;

for what are the circumstances supposed to confer the contagious property? They are generally stated to be, crowding, filthiness, and insufficient ventilation. But where shall we find those circumstances? Not in the ships of war, barracks, and encampments of the West Indies. In all of them, I am warranted in asserting, there is greater cleanliness than in the private dwellings of the citizens; indeed, the system of purification is perfect, and the result perfect cleanliness, nothing being left which can be imagined to influence unfavourably, either health or disease. And I am satisfied, that there is nothing in the want of room or ventilation, on board ships of war, to deteriorate health, much less to change the character of fever; though to others they may appear capable of such effects. But in barracks, constructed for war complements, and occupied by the detachments of peace establishment, crowding cannot have place; and there is certainly sufficient ventilation; in the opinion of some there is too much, the windows being so numerous and so placed, as to allow the wind to pass, without interruption, through the apartments; and being generally without sashes, there is not proper shelter. Yet fever is as often epidemic now as it was during the war; as many die in proportion to the number employed; and therefore the disease may be supposed as often contagious, whether its contagion be original or acquired, necessary or contingent.

I have stated two special obstacles to the belief, that in the West Indies there are in constant existence, though not in constant operation, two distinct species of agency for the production of fever, one contagious, and the other not contagious. Those obstacles were, first, that no reason appears why contagion acts at one time and is dormant at another; and, secondly, that, as no limits are assigned for the operation of each, and the effect being the same, we endeavour in vain to appropriate their separate results; and

are therefore involved in such darkness, as ought to palliate our scepticism, if not to justify our unbelief of a twofold cause.

Similar difficulties arise in the consideration of contingent contagion. According to this doctrine, West Indian fever generally springs from ordinary causes, terrestrial exhalations of some kind; and it is only in some special circumstances, that the property of contagion arises from, or is engrafted on, the disease, having the power however, when engendered, like other contagions, of propagating disease, independent of every thing except its subject, the human body. But the special circumstances must be made manifest before they command credence. It has already been stated, that those usually assigned do not exist in the ships of war, barracks, and camps of the West Indies, situations in which there is more of the fever in question than in every other part of the world. No other cause, as far as I know, has been proposed; and that there must be a powerful cause for such an effect will be admitted; though I grant that powerful causes may exist, which it is difficult to detect or demonstrate. Still, as far as our perceptions are concerned, they do not exist till they are shown to exist; and if, as I believe, the common causes are sufficient to account for all the appearances of this disease, there will be little wisdom in searching for them.

Another difficulty, in this view of the subject, arises from uncertainty as to the time when, and manner how, the fever is so far changed in its nature as to pass from a noncontagious to a contagious disease. It does not appear that the change can be determined by rapidity or extent of progress, severity of symptoms, or amount of mortality; or by any thing within the cognizance of the senses, on which the mind can fix as a limit, to separate the disease as originally constituted, from the disease as altered in its nature and power by contingents. If there be then no-

thing, in the character or effects of the disease, to determine when it has acquired the contagious property, how can the existence of that property be ascertained? Its operation appears to be inferred, from difficulty, in some instances, of accounting for the progress of the disease on any other principle. When it arises from ordinary causes, but continues after they are removed, or extends to places where they have not existed, it is believed that contagion has been generated. And if there be no other way of accounting for the disease, if it be proved that there is no cause in the soil on which its subjects live, we shall be forced to admit its existence, however little we understand the manner in which it came into existence, as we are constrained to believe, that a toad may live in the centre of a tree or stone, and many other things "which are not understood in our philosophy."

I am not aware, however, of any case, in which such negative evidence has been offered. Let a single authenticated case of the disease appear, in the neighbourhood of another person under its influence, and where a local cause for its appearance cannot be found, and conviction of contagion must follow. But while the earth on which we tread, and the habitations in which we dwell, contain the ample and palpable means of producing the disease, we are unwilling to admit the operation of an agent so far beyond our comprehension as contagion. In ships there are abundant materials for the production of West Indian fever, as I shall show in the sequel; and all over the West Indies and adjacent shores, it is every now and then springing up and disappearing, in ways which preclude the suspicion of contagion. It frequently appears in the United States, and sometimes in the south of Europe, in circumstances which satisfy every one that it does not arise from contagion, excepting those who maintain that it arises from contagion alone, and with them I have no argument.

Now this being the case, it will be seen, that, if fever break out in any of those places, soon after the arrival of a ship in which it prevails, nothing more can be concluded, than that the indigenous cause was ripe, and ready to act, about the time the ship went into harbour; even if there be free intercourse between the ship and the shore. The same sun ripens the sugar cane in Jamaica, and the vine in Andalusia; and the same sun eliminates the cause of fever in both, it may be at the same time, or at different times.

If a ship, in which there are many cases of dysentery, put into Havanna, and the disease appear about the same time among the resident inhabitants, we should not infer, that it was communicated by the former to the latter, because we know that there is sufficient cause for the disease within the walls of the city, without the intervention of a ship, or any other external agency. We should believe, that the contemporaneous appearance of the disease was as much an accident as the arrival at the same time of two ships, one of which had sailed from Cadiz and the other from Vera Cruz. Such would be the belief of medical men almost universally; and if similar incidents happen in West Indian fever, why should they be judged by different laws? Why, in this disease more than the former, reject an obvious and sufficient cause, to search for one that is unknown and unnecessary?

Should a ship on leaving Havanna, or soon after, become sickly, as it is called in the West Indies, that is, have many of her crew affected with fever, and proceed to New Providence; there, ten chances to one, the same fever which was destroying her men would be found cutting off numbers of the garrison of Nassau; especially if the troops were recently from Europe, and the sickly ship arrived in advanced summer or autumn. But if she put in early in summer, the fever at Nassau would more probably appear a week or a month after her arrival; or, if the garrison

consisted of seasoned troops, there might only be a case here and there on shore, which would not attract notice sufficient to excite alarm. Now, if the sickly ship anchored in Nassau harbour in the circumstances first supposed, she could not be accused of having imported the fever; because there it was before her arrival, and, of course, independent of her influence. Nor ought the disease to be connected with her presence, as supposed in the other case, that is, if it broke out soon after her arrival; because, without any such incident it prevails there frequently at that season; and because it is notorious that the soil can furnish abundant matter for its production, without foreign agency of any kind. If therefore, once in seven years, a fever ship should put into Nassau, and fever should not exist there at the time, or appear soon after, it ought to be matter of inquiry why it did not appear; because the island's exemption, during that season, would be an exception to a general rule.

Let it be supposed (and such things happen frequently), that two ships leave Port Royal on a cruise to windward, each having a few cases of fever on board; that the numbers increase during the cruise; and that the ships, after being two or three weeks at sea, have lost so many, and have so many ill, as to become unmanageable. In these circumstances, one of them makes Antigua and the other Barbadoes; and as soon as possible all the sick are landed from each. At this time fever may be epidemic in both islands, or both may be healthy, or in one there may not be a case of the disease, while in the other there may be many hundreds; but let it be supposed, that in neither is there a case, when the ships land their sick respectively, who are received into hospital without fear or restriction. In one case the fever shall be confined to the crew of the strange ship: her men continue to be attacked and sent to hospital, where some die, some recover; but the disease does

not attack an individual person beyond the walls of that hospital; nay, it does not attack one of the surgical patients within its walls. Things proceed in this way for a month or two, till, the cause of fever in the ship being exhausted or arrested, and the men in hospital fit to be embarked, she leaves the harbour, and is forgotten; as nothing is connected with her visit, except her own loss, to render it memorable.

But it happens, in a short time after the other ship sends her sick men to the hospital, that fever appears among the resident inhabitants; alarm ensues, that it has been imported by the strangers; and it may be, that this alarm shall have the effect of preventing, by official interference, the further landing of patients from the ship, where the disease may take its course, unmitigated by the ordinary offices of humanity*. Such alarm would be unfounded,

* We are told by the advocates of contagion, that measures restricting or preventing intercourse never can do any harm. In the following case—a case which I could scarcely have believed to have happened at Barbadoes in 1819—it is pretty clear they did do harm.

In October 1819, his Majesty's ship Euryalus anchored at Barbadoes from Bermuda. She had a number of fever cases on board, and two men died after her arrival in Carlisle bay. So strongly did the fear of contagion operate on the minds of men in power, that the hospitals were shut against the sick of the ship: it was intended to have taken measures for putting her in quarantine, but the safer method was wisely thought to be that of getting quit of her altogether. She accordingly went to sea, the sickness increased, and she put into the Danish island St. Thomas, where the governor, actuated by the ordinary feelings of humanity, unfettered by prejudice, gave orders for the immediate reception of the sick into hospital. Seven men were landed at first, and numbers afterwards, as they became affected: they were treated by Danish physicians, most of them recovered, and when the work of kindness was completed they returned to their ship. It was never believed or alleged, that they communicated disease to a single person on shore. "Look on this picture, and on this."

and its effects unnecessary and cruel: for had the ship been placed in the most rigid quarantine, had she, without ever having communicated with the shore, been put under the ban of authority, insulated at her anchorage as "an accursed thing," and cut off from every mean of comfort; who will assert, that the fever would not have manifested itself on shore at the time it did? Not the man, who, believing ordinary causes sufficient for ordinary effects, believes that the soil of the island contains the materials of fever, as certainly as the marshes of Kent contain the seeds of the agues which prevail there; for to him it cannot be matter of surprise, that the fever should appear on shore at the time the ship arrived, more than at any other. It is not limited by seasons in its visitations, though, as an epidemic, it most frequently appears in the autumnal or hurricane months; and hence, if the sickly ship arrived in that season, the concurrence of fever on shore might be expected; if at any other period, it ought to excite no wonder.

If there be persons who would trace the fever from the land to that on board ship, in defiance of former experience showing its rise without such concurrence, and in disregard of its abundant sources around them; they must be persons fond of seeking for problematical agents when positive ones are before them. They resemble a man, who would shut up crevices in a powder magazine, to prevent ignition of its contents, while, in his precautionary measures, he was lighted by a naked torch.

So long as those islands contain, within their own bosom, the cause of fever, from that cause alone fever may be expected to arise, at uncertain periods; and when it appears, if it do so appear, soon after the arrival of a sickly ship, no proof can be given, or any legitimate evidence advanced, that the ship introduced the fever. It would be as fair to argue, when a ship in which dysentery prevails anchors at Havanna, and dysentery soon after appears on

shore, that the first produced the last; or that a Cretin proceeding from Switzerland to Naples, introduced bronchocele in the latter place, because cases of that disease arose soon after his arrival.

If it be granted, that the contemporaneous appearance of fever, in a strange ship on one of those islands, affords no evidence that the one occasions the other; it must likewise be granted, that no proof can arise from a similar coincidence in any other tropical situation, where it cannot, at the same time, be proved, that there is no cause of fever inherent in the soil. And who will attempt to prove such exemption, and admit the indigenous origin of the disease at Brimstone Hill in St. Kitts? More especially it will be granted, that the fever on shore was not occasioned by that in the ship, if, in the situation supposed, it be shown, that a single case of the disease has formerly existed as the offspring of the soil; because the matter which one year could furnish cause for one case, might be so acted on the next year, as to furnish cause for a hundred*.

* Here the cases of the Bann and Ascension naturally present themselves, but I do not feel myself either bound or warranted to enter on their consideration at any length; because I do not know that the disease in those cases was the same as that about which I am writing; and because I have not the means of acquainting myself thoroughly with the circumstances. They have been represented as sufficient to remove all doubt respecting the existence and operation of contagion in that instance, and they may be so to those better acquainted with the whole circumstances than I am; but I take the opportunity thus presented me of stating why the evidence, which has been published, does not appear absolutely irresistible and conclusive.

The disease did not appear at Great Mountain, though the intercourse between that post and the sick of the Bann was as open, as between the posts where it did appear and the sick of the Bann, and though a man belonging to the Great Mountain post had

Thus, I think, it appears, that in all places, where tropical heat, or heat of equal intensity, is acting on a soil

been some time on board the ship. This difficulty is noticed by Dr. Burnett.

The first person affected by the epidemic fever was a boy, the son of one of the sergeants, who was not believed to have had any nearer communication with the sick of the Bann than passing daily, at no great distance, to feed his father's poultry. Even in diseases generally accounted essentially contagious, such intercourse as that would not, I believe, be considered sufficient for the contagion's operation.

The disease made its appearance in the Driver, three were affected, and one died on board, but it did not extend further. Is it agreeable to the common operations of nature, that a disease, strongly and virulently contagious, should stop short after affecting three persons, in a dense population, where it is presumed all were susceptible in nearly equal degrees? Is it not, at least, a thing of some difficulty to conceive, why a fever, which had the power of propagating itself so rapidly ashore, had not the same power in a ship; especially when it is remembered, that the ship, according to all we have been taught, was more likely to foster and perfect the contagious property, than the comparatively open and well-ventilated garrison quarters?

It appears, as noticed by Dr. Burnett, that the same disease had occured in Ascension before, and that without the apprehension of being imported. Is it not *possible*, that what, in kind, could on one occasion produce one case, might on another produce a hundred? Is it not therefore *possible*, that the fever which prevailed at Ascension in 1823 was the offspring of the soil, as well as that which occurred in 1818? Would not this appear more than *possible*, if it should afterwards be found that a similar fever could prevail without the suspicion or possibility of importation? And is it not therefore *possible*, that the concurrence of fever in the Bann and at Ascension was a mere coincidence.

After the preceding note was written, I had an opportunity of reading the last edition of Sir Gilbert Blane's Elements of Medical Logic; had I seen them before, they might have had the effect (a

containing the materials of fever, spontaneous fever may be expected; and as I am satisfied, that, in all places

good one certainly), of making me consider the subject more deeply before I ventured to express even a *half-formed doubt*; they would not, I trust, have frighted me from my purpose or my belief. Should Sir Gilbert Blane ever take the trouble of reading these pages, he will, I fear, give me little credit either for clearness of thought or integrity of purpose. I am sorry to think thus of any man, especially of such a man as Sir Gilbert Blane; but the manner in which he has treated other humble, but I believe honest, and certainly intelligent persons, who have differed from him on this subject, gives me little reason to think otherwise. Perhaps he may extend to me the contemptuous pity, with which he treats a *certain American journalist*, who *dares not to be convinced*, after having considered the arguments adduced by Sir Gilbert Blane, in proof of the contagious nature of West Indian fever. This does not appear the most likely method of obtaining proselytes to a faith, which Sir Gilbert thinks of such importance to humanity. Should not a reasonable being be reasoned with clearly, forcibly, but patiently? Is the mere word of authority, the oracular declaration of any man, however talented, and however exalted, to *convince* another man, who has the power, and uses the privilege, of thinking for himself? But Sir Gilbert Blane tells us that the controversy is to be decided by facts, not by reasoning. Are the facts then so clear, that he who runs may read? Has no reasonable doubt ever arisen in the minds of rational inquirers respecting the import of facts, which, according to Sir Gilbert Blane, ought to remove all doubts and answer all questions for ever? Were all the men who disbelieved, are all the men who yet doubt or disbelieve, either idiots or knaves? for to this conclusion would the *declamation* of Sir Gilbert Blane lead. To answer such questions would be at once idle and insulting. Men of equal penetration, with better opportunity of judging, and not less anxious to learn the truth, have been led to different conclusions from those drawn by Sir Gilbert Blane; nay, some of them have been *driven* by irresistible evidence, and in opposition to early and long cherished modes of thinking, to draw different conclusions. Was Rush, was Jackson, was Hunter, destitute of common sense or common honesty? Are all the men of the present time, who differ from Sir Gilbert Blane, incapable of *observing facts*, or unwilling to

where it has appeared, such circumstances existed, I am also satisfied, that the difficulty of accounting for its rise and spread, in any of those places, was apparent only, and ought not therefore to lead to the belief, that in any of them the disease had acquired contagious properties.

But if it shall ever happen that the fever in question is carried beyond a certain atmospheric circle, and there arises in persons who have not been within that circle, it must be admitted to possess contagious properties, whether essential or contingent; because, in such a case, we should be unable to account for it otherwise. If, for instance, it shall happen, that a ship shall carry West Indian fever

interpret their meaning faithfully. Facts are not always obvious at first sight; they are often encumbered by factitious circumstances, or disguised by false appearances; and ordinary observers do not always find it an extremely easy matter to remove such incumbrances and to strip such disguises: we do not know how Sir Gilbert Blane accomplishes these things so perfectly; we are not told how he has learned, so easily and so unerringly, to *interpret the handwriting on the wall.* It is strange that the men who have had the best opportunities of *observing these facts*, who have lived long, laboured arduously, and I will add conscientiously, amid the perils and perplexities of West Indian fever, have not seen them in the same or even similar light with Sir Gilbert Blane; for I believe, that, since the time of Dr. Chisholm, no respectable practitioner in the West Indies, who has seen much of the disease, has believed it to be inherently and essentially contagious. And it is passing strange, that Sir Gilbert Blane, who has taken so much pains to teach us the right mode of reasoning on medical matters, should here turn round, and tell us, that all reasoning, in such cases, is not only idle but pernicious! I desire to treat Sir Gilbert Blane with respect, and for his character generally, I do feel respect; but such feelings cannot be forced; and I cannot respect dictatorial intolerance in any man, or any place, more particularly in the exercise of a liberal, but not exact, art; and I cannot admire the temper, which would lead a man, in possession of executive power, *to dismiss from the service of their country such men as could not in conscience subscribe to his articles of professional faith.*

from Havanna to Halifax, and that, in the last place, the same fever shall be proved to arise among the inhabitants; or if, when a person labouring under the disease is carried from Vera Cruz to Xalapa, it shall be proved that the same disease appears in a single person, who has not been lately in the plains; in either case, but especially in the last, there will be such evidence of contagion, however derived or generated, as must command belief*.

It may be said, however, that this is asking too much,

* I shall here notice another locality, from which evidence equally convincing might be derived, and which is more constantly under the observation of Englishmen. The plain of Liguanea contains abundantly the cause of West Indian fever, and there the disease every now and then makes its appearance; but it never extends six miles further in a northern direction. I run no hazard of contradiction when I state positively, that the disease was never seen six miles up the mountain side, except when carried there from the plain; and that it was never known to extend itself to a single person in the neighbourhood, or to affect any one who had not been recently in the plain. The cause constantly ceases with the individual disease which it excited. The daily intercourse between the plain of Liguanea and the mountain is considerable; in the plain the disease is often epidemic, persons affected with it there sometimes remove to the mountain, where they recover or die, as the case may be; yet during a period of two centuries it was never known that the disease was propagated, in one instance, only six miles from Liguanea, on that mountain. At Stoney hill, having 1300 feet elevation, it has sometimes, though not often, prevailed in its worst epidemic form; 2000 feet higher it was never seen, except when carried there; the difference of temperature is not great between the two positions, for during the autumnal months it often rises to 80°, at the mountain height in question. Can we conceive, without positive proof, of a disease possessing contagious power, being deprived of that power, by such change of place and temperature? Are not these things strong evidence of the disease being the effect of a particular soil, acted on by a certain degree of heat?

and that proof of contagion in any other disease, if tried by the same test, would fail; a certain range of temperature being essential to each. But of this there is no good evidence. The diseases generally accounted contagious, to which it has most affinity, plague and typhus, are not placed under such severe restrictions. The last, indeed, is favoured by low degrees of temperature, and the first by that ill-defined range which is called the high temperate, but neither of them is confined by these limits; for typhus has been known within the tropics, and the plague has ravaged London. The parallel therefore between those diseases and West Indian fever fails, as far as it regards a precise range of temperature being necessary for their existence; and, in this respect, it must be different from every other disease allowed to be contagious. There is in this alone enough to perplex, or dissuade from, the belief of contagion; and it must remain difficult, I should say impossible, by any accumulation of argument or display of eloquence, to convince a man resident in Vera Cruz, who there sees it yearly cutting off the inhabitants, but never can trace it a few miles up the mountain side, that it possesses such power. He observes a line, as distinct as that which separates the soils which produce the *arundo sacchirifera* and *pinus sylvestris*, and on one side of that line he finds West Indian fever springing up as the indigenous offspring of the soil, but he never sees it pass to the other side; he cannot transplant it if he would, any more than he can make the sugar cane and fir tree change places; and he cannot conceive how such a line can be an impassable bar to contagion.

Such are a few of the general reasons, which must render the agency of contagion in any form questionable, if not incredible, in this disease. Many more might be adduced, without repeating those advanced by former writers, were such extended argument consistent with my

object. But I have already overstepped the limits which I proposed, and shall therefore finish this part of the subject, by referring to the fever as it appeared in the Rattlesnake, and noticing a few legitimate inferences to be adduced from its phenomena, in that instance.

The first case appeared on the 8th of August, 1824, in the person of George Taylor, who died on the fifth day of the disease. For some months previous to that date, there had been very few cases of fever at Port Royal, either in the squadron, or on shore. Two came to my knowledge, during the time the Rattlesnake was in harbour, but with neither of them had George Taylor the power of communicating. From the 8th of August till the 10th of September, we had occasional cases, most of them severe, many proving fatal; in all nineteen cases. But after the last date, it assumed an epidemic appearance, passing rapidly through the ship's company, and affected ninety persons within the month. From the 10th of October it gradually declined, and finally disappeared about the end of January 1825.

On the 27th of August, 1824, the ship went out of harbour on a cruise to leeward, and returned to Port Royal on the 27th of September, with forty men ill of fever. Twenty-seven cases were sent to hospital the same day, in different stages of progress; and for some time afterwards, three, four, or five were sent thither daily.

So great an influx of patients soon filled the wards, and it became necessary to place many in the galleries, which are thrown out from the second story of the main building, and which they bound on either side. In the main building there are numerous doors and windows, which are seldom shut, and the galleries being projected on a level with the second story, the whole of the upper range may be considered as one large apartment, extending in length from gable to gable, and in breadth, from the verandah of one

gallery to that of the other. The main building and its two galleries form literally one apartment, as far as atmospheric communication is concerned, the breeze which enters by one verandah passing freely through the main building, and out by the opposite verandah.

Now, as the patients in hospital on the 27th of September, labouring under ulcers, accidents, hepatic and other chronic complaints, occupied part of the main building; and as the fever patients from the Rattlesnake first filled up the main building, and were then placed along the whole length of both galleries, their cradles being in contact with the walls of the wards; the communication between the two classes of patients must have been complete.

The hospital is so placed, that it is freely perflated alternately by the sea and land breezes, as its sides are extended in a transverse direction to the ordinary courses of those winds. Hence the non-febrile patients occupying the central parts of the hospital, must have been not only at the source, but in the very centre of a febrile atmosphere, as every breeze which blew must have passed over the fever patients ere it could reach them; yet not one of them was affected by the fever. *Black vomiting, furious raving, comatose muttering, the yellow, livid and spotted skin, and all the attributes of West Indian fever, were around them, or in contact with them; yet was not one of them affected by that fever.* Here we may safely affirm, that the disease was not contagious; for, while it attacked the people on board in rapid succession, it was not propagated, in a single instance, beyond the sides of the ship.

When fever is epidemic on board ship, and when strangers who visit her take a similar disease, it is asserted, that this coincidence must be admitted as evidence of its contagious power; but it will be granted, that if fever can become general on shipboard from any other

cause than contagion, the same non-contagious cause, which produces fever in the ship's company, may produce it in the strangers; and, therefore, such coincidence cannot be admitted as any evidence at all. When we were sending so many men to hospital daily from the Rattlesnake, almost every fresh man whom we received was attacked by the fever; and it might thence have been inferred, that the strangers received the disease from the men among whom they lived; but we had, at the same time, proof, that those men could not communicate it to another when removed from the ship. We had, therefore, "the bane and antidote both before us," and conviction arose from the whole, that the disease at that period could not possess contagious properties; for it were futile to say that it possessed those properties in the ship, but lost them the instant its subjects were removed a hundred yards from its source.

From what has already been stated it will appear, that this fever did not originate in contagion. It began by occasional cases, the subjects of which had not been near people labouring under the disease out of the ship, and proceeded in that manner for about a month; when, on the 10th of September, it assumed the character of a severe epidemic. If it did not originate in contagion, and did not possess contagious properties on the 27th of September, when twenty-seven cases were sent to hospital; will it be asserted, that in any stage of its progress it possessed these properties? From the 10th till the 24th of September was its worst period. Between those dates its progress was rapid, and its effects appalling. It attacked indiscriminately people of all ages and temperaments, and exhibited every variety of character, from the mild inflammatory to the apoplectic congestive. There were cases to suit the descriptions of all the writers on the subject, by whatever names they have designated the disease—malig-

nant, pestilential, comatose, concentrated, or slight yellow fever; and if contagion be a property of any of them, here it must have been: yet here it was not; or, in such cases, it must be an agent so capricious and inscrutable, so uncontrolled by physical laws, and so incapable of being investigated, as to render it hopeless that it should either be identified or understood. For, that the fever in the Rattlesnake should acquire contagious properties about the 10th and lose them on the 27th of September is incredible.

It is true, that, during that period, it cannot be demonstrated that the fever was not contagious; but if it be made evident that it was produced independently of contagion, both before and after, proof as to that period cannot in fairness be required; especially if it be allowed, that the cause which was sufficient from the 8th of August till the 10th of September, and from the 27th of September till the end of January, was not likely to have failed during the intermediate period. And let it be remembered, when scurvy is epidemic at sea, that if such evidence were required to show that it is not contagious, it could not be given; and on that ground sea scurvy might be declared a contagious disease.

During the period in question, that is, from the 10th till the 27th of September, the ship was cruising on the coast of Darien, in the neighbourhood of Porto Bello, excepting the three last days, which were spent in returning to Jamaica. Between the 10th and 24th, the air was highly charged with electricity, there was a continuance of tremendous thunder, and great quantities of rain fell. To give the people shelter it was necessary to shut the ports and cover the hatches, which prevented thorough ventilation for the time; and which, together with the accumulated gaseous excretions of so many sick people, deteriorated in some way, I believe, and to a certain extent, the air in the

ship; and I think it probable that the general condition of the atmosphere as to electricity was prejudicial to health; for it was sufficiently remarkable, when we got clear of that coast and into dry weather, that the fever, though not diminished in quantity, was improved in character: and such I believe to be the general influence of such circumstances; they contaminate, in a way not understood, the air on which they operate, and thereby aggravate disease; but they do not confer a new and peculiar property on disease. Such at least were their effects in the case before us. They rendered the body more susceptible of the cause of fever, and its effects more severe; but they did not generate contagion; or it must have been contagion so ill organized and so loosely attached to the disease, as to be dissipated or destroyed by sunshine and fair weather: a kind of agency which did not arrest the original cause of the fever; for as many cases occurred after as before the change of weather.

MARSH MIASMATA.

It is my object in this section to show, that West Indian fever does not arise from marshes, simply as such, their exhalations not necessarily containing the cause of the disease; and consequently, when it is occasioned by exhalations from such soils, it is from something adventitiously added, not from any thing essentially belonging to them. The term marsh I understand in its most extended sense, as meant to designate bog, morass, swamp, or quagmire; and by marsh miasmata I mean the gaseous products of any such soil. Stagnant water, varying in extent from such small quantity as exists in land not deserving the name of dry absolutely to that which forms a bog, is essential to this soil; and it is the only thing which is essential; for, though clay is its principal earthy ingredient, other ingredients in various quantities are found in it.

Vegetation on its surface varies, and is abundant, scanty, or wanting, according to circumstances; and animals, chiefly insects and reptiles, live, die, and are decomposed in it, partly from their peculiar instincts, and partly from accidents affecting them, as happens on or near the surface of other kinds of soil. By the miasmata of marshes in all this latitude of meaning, I shall endeavour to show, that West Indian fever is not occasioned; and for that purpose I shall adduce two forms of evidence, one to prove that the disease in question frequently prevails where no such cause can have operation; and another to prove that marshes abound where it rarely if ever occurs. But in prosecuting the subject I shall not be scrupulous about separating the two forms of evidence, being content with stating a few incontrovertible facts, as they present themselves. These facts, however, though few and ill arranged, will be sufficient, I think, to accomplish the object I have in view, namely, to demonstrate, that marsh miasmata, so often assumed as the cause of this disease, has been so assumed in error.

And, in the first place, the history of fever, as it prevails on board ships, may be adduced in strong evidence of the rise and spread of the disease, in places where marsh miasmata cannot operate. Such history I cannot give fully or accurately, and if I had the means of doing so, such detail would be inconsistent with the plan of a memoir like this; but were such history fully and faithfully written, so many striking and pertinent facts would be accumulated, as would be sufficient of themselves to prove my argument. Even the few cases and limited views to be communicated here will go far towards establishing the point.

Few ships continue eighteen months on the Jamaica station without an epidemic visitation of West Indian fever; in some slight, in others severe, but in most cases so severe as to deserve the name of epidemic. If, in such

instances, the disease arise from any thing in the ships (and that it generally does so will be shown in the sequel), the cause cannot be marsh miasma, because in no ship is there a marsh; in few is there any thing equivalent to or approaching the nature of a marsh. There is neither earthy nor vegetable matter, nor any thing to give the most imperfect notion of marsh land. Formerly, when shingle or gravel ballast was used, a portion of earth might be carried into the holds, and then something like a marsh might be formed. But now, when the ballast is iron, clean itself, occupying little room, and so placed as not to harbour or conceal filth, such a state of things can scarcely happen.

Much pains are generally taken, when ships are first equipped, to purify the holds thoroughly; the iron ballast, in pigs, is then arranged along their floors, and over it are placed the iron tanks, and other necessary furniture of the ship. In the British naval service there is great, in some cases excessive, attention paid to cleanliness; the decks, besides being washed or otherwise cleaned daily, being regularly swept after meals, and every kind of refuse thrown overboard. When ships are so cleaned in the first instance, so furnished in their holds, and so regulated throughout, any thing allied to a marsh cannot be supposed to exist within them; and that such is their general condition, every one acquainted with their structure, equipment, and economy, will testify.

Exceptions there certainly have been, probably many in former times. Since the peace, and the employment of iron ballast, two cases are recorded, those, namely, of the Childers and Pyramus, in both of which, from whatever cause, there was a considerable accumulation of extraneous matters. In the Pyramus, however, they were confined to the space under the limber boards, a space so limited, that the accumulation could not have been great; and as the matters in question are said to have consisted chiefly of coal

tar and chips of wood, their affinity to a marsh cannot be supposed very close. But granting, that, in this case, the matters found under the limber boards possessed all the qualities of a marsh; and that, in the Childers, from the holds of which they were removed in large quantity, they likewise possessed those qualities; to what conclusion will they fairly lead us in investigating the cause of the disease? Not that those matters necessarily caused it in the Pyramus and Childers, much less that similar matters are essential, or contribute in any way, to the production of the disease generally in ships; because, in opposition to the occurrence of those matters, in those ships, an immense preponderance of counter facts instantly presents itself.

Within the last twelve years, the Childers and Pyramus alone have been noted, as containing such, or indeed any ostensible cause of the disease; and it is needless to repeat what is known to every one acquainted with the subject, that of the many ships employed on the station during that period, very few have escaped without either a severe or slight epidemic visitation of fever. As having suffered severely, the following ships occur at this moment to memory: Iphigenia, Wasp, Tribune, Sapphire, Scout, Tamar, Bustard, Thracian, Rattlesnake, Lively, Isis, Scylla, Pylades, Ferret: they all suffered from the disease which prevailed in the Childers and Pyramus; nor was it, in many of them, less malignant than in those two vessels. In none of them is any connection traced between the fever and the accumulation of extraneous matters, like those found in the Pyramus and Childers; and I therefore conclude, that in none of them did any such matters exist: for, besides the general regulations for enforcing cleanliness in ships of war, there is too much professional zeal among the officers to permit an epidemic disease, so destructive as this was, to proceed without scrutiny. Inquiry would be made, though perhaps not in the right way, after

the cause of the disease, that, if it existed in the ship, means might be adopted for its removal; and as the holds are reasonably suspected, these being places where it would most likely accumulate, to these the search would first be directed.

In the Iphigenia, a medical inquiry was instituted, with the view of detecting the cause of the fever, which was unmanning her, and in the hope that, when detected, it might be possible to arrest or remove it. The result of the inquiry was extremely unsatisfactory; no cause for the disease, after the most rigid examination, being found; but it afforded positive evidence, not only that nothing at all resembling a marsh is necessary to the production of the disease, but also that it may rage in its most destructive shape, in a ship conspicuous for cleanliness, among ships where all are clean. The physicians, in their report to the commander in chief, state in effect, that having carefully examined every part of the ship, they found her in every part, and in all her furniture, absolutely clean, dry, and thoroughly ventilated. Regarding the internal state of the Rattlesnake, I can speak from personal observation, which is satisfactory as to internal cleanliness, as perfectly so as is the evidence concerning the state of the Iphigenia; but as I shall have occasion to detail the circumstances of both ships for another purpose hereafter, I shall not enter on particulars at present.

Enough has been stated to show, that marsh or marsh-like exhalations are not necessary to the production of epidemic West Indian fever on board ship. Of this, the Iphigenia and Rattlesnake afford clear and positive proof, and all the other ships named above would, if the circumstances were as well known, afford evidence as clear and positive. Hence it is fair to infer, that of all the ships which suffer epidemic attacks of the disease, an extremely small proportion, one perhaps in a hundred, would be

found to contain any thing at all resembling a marsh; and hence also it is obvious, that the disease must arise from something else than marsh or marsh-like miasmata in ships.

The fever as it affects ships is generally severe, frequently malignant and mortal in a high degree, producing, in some instances, almost complete depopulation. The proportion of seafaring men, who sicken and die in the harbours and on the shores of the West Indies, is greater than of the inhabitants of the soil, exposed as many of the latter must be to the constant influence of marsh miasmata; nay, taking the whole of the King's sea and land forces, as great a proportion of sickness and death will be found among the first as the last, from the disease in question; though there is a considerable difference in the amount of mortality between the two branches of the service, as the troops suffer from intermittent and remittent fevers, and other diseases, by which sailors are scarcely affected. Now when these things are considered in connection with what has been shown above, namely, that marsh miasmata are not essential to the disease in ships; and since it must be acknowledged, that what is not essential in one case cannot be essential in another for the same effect; it will be allowed, that a certain quantity of evidence is given in proof of the position, that West Indian fever is not caused by marsh miasmata.

Barbadoes, the most southern of the Caribbean chain of islands, lies in the thirteenth degree of north latitude, and is exposed to the full and unbroken influence of the trade winds. It is the longest settled, and most thoroughly cultivated of our West Indian possessions, almost the whole of its surface, where there is any soil, having been frequently trenched and cleaned for the growth of the sugar cane. It has little elevation, compared with most of the other islands, and has a flat and level appearance over its

whole extent. The formation is generally calcareous, of that soft, uncompacted kind called tuffa, which rises in many places to the surface, the superincumbent soil being a productive clay. Little rain falls here comparatively; there are no lagoons, few running streams, and no standing water, excepting immediately after rain. The positive heat differs little from that of the other islands; but from the steadiness of the breezes which blow over it, and the form of the shores, which occasion little obstruction or reflection of the sun's rays, it is less oppressive to sensation, and probably less injurious to health, than in some of those to leeward.

But the great and striking peculiarity of this island is the dryness of its soil. There is scarcely any marsh land in Barbadoes; none, I believe, unless a small muddy spot at the bottom of the carenage, in Bridgetown, be called such. When rain falls, a portion of it will be retained by the soil, and afterwards exhaled by the sun's heat; but those things happen in every part of the world, and ought not to have powers ascribed to them here which they do not possess elsewhere. Could they, of themselves, produce West Indian fever, it must prevail in every place where rain falls and a tropical sun shines. They do not occasion this fever in other tropical soils; they cannot be said to constitute a marsh; nothing else, excepting the small space at the carenage already specified, in the shape of marsh, exists on the island; and therefore marsh miasmata cannot be the cause of the disease at Barbadoes.

Yet the population of this island has suffered severely from that disease. As a wide spreading and irresistible epidemic, it has been more terrible here than in any other part of the West Indies, not excepting even its memorable visitation of Grenada in 1793; though it has returned of late, in this fearful form, less frequently than at Jamaica and other islands to leeward. The epidemics of 1647 and

1694, appear to have surpassed any thing which has happened in modern times. The writings which have reached us from the authors of those periods are generally vague and unsatisfactory; their descriptions are loose, and their conclusions often unwarranted by the facts stated; but they are explicit and impressive, as to the loss sustained. They leave no doubt that the fevers of that age were more widely extended and fatal than those of the present age. And there is evidence on record, that, so early as the last of those periods, 1694, the island was dry, free from marshes, and cultivated throughout. Though all the epidemics which have affected Barbadoes are not historically detailed, or perhaps even stated, there can be little doubt that the fever has continued to return and ravage it at frequent periods, from the time it was first colonized till the present day. During the whole of the French revolutionary war, it occasioned annually a considerable loss of life, and does not appear to have been materially diminished till its termination. Since the last general peace, as will always happen in peace, from the very limited garrisons and squadrons employed, there has been little comparative loss. There has been less even than the ordinary proportion; though the epidemic of 1817 furnishes a melancholy exception, the mortality of that year being great, when compared with that of former years, even during war. How shall we reconcile these facts with the hipothesis, which would connect marsh miasmata and West Indian fever, as cause and effect? The first do not exist in Barbadoes; the last has been rife there, even of late.

But besides the positive proofs deducible from the structure and superficial condition of this island, there is, in its nosological history, strong negative evidence to substantiate my position.

According to the preceding topographical sketch, there is nothing which can be supposed to occasion intermittent

fever; and it must be strongly impressed on the mind of a medical stranger, on examining its structure and surface, that no cause for that disease is anywhere apparent. The results correspond with the appearance. Of the various modifications of diseased action comprised under the head of intermittent fever, not one is to be found in this island as an endemic production. Agues derived from other soils may continue here for a while; and when they have taken such hold of the constitution, and become so far habitual, as to be renewed by a debauch, exposure to cold, or other debilitating cause; when they are connected with incurable organic disease, and functions essential to health are permanently impaired; they cannot be cured here or anywhere. But if the impression be less severe and lasting, speedy recovery under ordinary treatment may confidently be expected at Barbadoes. Instances to warrant such expectations frequently occur. Men who have contracted the disease in England, Africa, and Guiana, and who have arrived here, feeble and emaciated from its operation, have recovered rapidly. In a few weeks, they have regained the strength and spirits of perfect health, the salutary changes being more rapid and complete than could have been expected, more so indeed than they are generally found to be.

Abundant negative evidence of similar import is likewise furnished by the health history of ships. Intermittent fever is scarcely found in the catalogue of their diseases in the West Indies; and when, now and then, a solitary case presents itself, it may always be traced to some obvious source on shore. It will generally be found to be the recurrence of a disease contracted long before, in some other part of the world on former service, and which debauchery, privation, or exposure to wet, has called into renewed action. In such cases the disease will generally

be interrupted only; it will not be radically cured by any mode of treatment or local position. But if it be of recent origin, and has not materially affected visceral structure, it may, in most cases, be easily and permanently cured on board ship. Here, as at Barbadoes, it is not endemic, but is, when it exists, imported, and then is often easily removed; though the cure is not perhaps so rapid as in that island.

Let the facts contained in the foregoing statement be considered and compared; let it be remembered, that there are no marshes on board ship, or in Barbadoes, and that, in accordance with what we know and should expect, intermittent fever is not an endemic production of either; but that West Indian fever arises copiously from some cause, whatever it may be, in both; and it will appear, that the two diseases cannot, in those situations at least, spring from the same source. It has been attempted to be proved, and much industry and talent have been brought to the undertaking, which has had some success, that intermittent, remittent, and West Indian fever, are all diseases of the same genus; that they are all equally the effect of one cause, marsh or marsh-like miasmata, and must all therefore be considered as members of one family; and that difference of violence, arising from different degrees of power in the cause, and difference in the constitution of the subjects, is the sole cause of difference in each. But it would appear, from the facts stated, and reasons assigned above, that the doctrine is erroneous, and that all the labour and ingenuity employed in its support, have been employed in vain.

In addition to the proofs derived from these sources of the disease, so extensive as to embrace a large portion of its entire circle, a great mass of evidence may be deduced from the more limited sources of its operation, to show,

that marsh miasmata are not necessary to its production. I shall content myself, at present, with noticing two of the many places of this kind to be found in the West Indies.

Brimstone Hill, in the island of St. Kitt's, is a remarkable conical mount, rising to the height of 700 feet above the level of the circumjacent plain. It is a volcanic rock, dry, nearly destitute of vegetation, and desolate in its entire aspect. According to the marsh miasmal doctrine, there is nothing on or near it to occasion fever. There is no marsh on its surface, or in the surrounding plain. It is the principal military station of the island; and when the barracks were constructed, it was probably thought, as the supposed cause of the fever did not exist there, that the soldiers would be exempted from that disease. Generally speaking, the troops have been healthy, and so far expectation has been answered; but the experience of 1811 and 1812 shows, that there is no absolute immunity in such soil. Then fever became epidemic, and cut off great numbers of the garrison. The disease was sudden in attack, rapid in progress, fatal in effect, and exhibited all the peculiar features, including the black vomit, which characterise West Indian fever. Then, at least, the cause of that disease was developed on Brimstone Hill, and that cause was not marsh miasmata. So irresistible is this conclusion, that the supporters of the doctrine, that contagion is the sole cause of the disease, have triumphantly held up this epidemic as irrefragable proof of its truth; though they have not shown whence, in this instance, the contagion was derived, or how it found its way to Brimstone Hill barracks.

Stoney Hill, in Jamaica, though different in general appearance and relative situation, bears strong testimony to the same effect as Brimstone Hill. It stands about eight miles north-west from Kingston, its summit being about

two miles north of the plain of Liguanea, a fertile sugar-cane district, which varies in breadth, from a mere strip to five miles. This plain has the town of Kingston on the south, situated on the beach of an arm of the sea, which forms its limit on that side, and is bounded on the north by a mass of mountains running east and west along its land side. Towards its western extremity, this mass of mountain appears as if it had been cleft to its foundation, and in the gap, Stoney Hill seems to have been placed. Its base juts further into the plain of Liguanea than the extremities of the mountain ridge, between which it is interposed; it does not extend so far to the north, nor does it rise so high as the line of mountain, which it separates into two portions. It has 1300 feet elevation above the sea: it is an entire mass of calcareous rock, produces scarcely any grass or herbaceous plants; but it is covered with lofty trees, excepting on the summit, which has been cleared, and is occupied by a considerable extent of barrack building. There is little soil on its surface; so little, that it is difficult to conceive whence such trees derive sustenance, or how they fix their roots. Along the white and naked rock these roots extend themselves, like the branches of a vine along a wall, dipping their extreme fibres into every crevice which presents itself; and chiefly in these crevices, the only soil which exists on the rock is found, formed apparently by the decomposition of the rock itself, and the trees which cling to it.

Here, from the form of the rock, so unfit to retain moisture, the composition of the surface, and the absence of earthy substances, it is obvious, that, however much we might be disposed to discover marsh miasmata, no such miasmata could be detected or imagined.

There are no marshes, in any sense of the word, within a considerable distance. I do not know the exact distance,

but I may state confidently, that it amounts to miles, and is so great as to preclude the idea of miasmata reaching thence to the summit of the hill.

The temperature here is moderate, seldom rising above 80°, averaging about 76° of Fahrenheit's scale, and therefore considerably under that of the plains. The air is generally clear and pleasant; and the situation altogether such as would, according to the opinions generally entertained of salubrious and insalubrious soils, be pronounced healthy; highly so for a situation in Jamaica. Yet the troops stationed here have, on several occasions, suffered severely from West Indian fever. I have not the means of knowing the history of the disease, in Stoney Hill barracks, since their occupation; but its epidemic violence on one recent occasion was too great to escape the notice of persons in the vicinity, and too impressive to pass unheeded by any one interested in the disease, and therefore desirous of learning its details.

It is customary to send troops on their first arrival from Europe to this station, that they may here pass in comparative security the first and most dangerous months of their service, as far as the necessary distribution of the military force will admit of the regulation. The 77th regiment was stationed here on arriving from England in 1825, and was soon after affected by West Indian fever, which in a short time became epidemic, assumed its most destructive form, attacked indiscriminately the whole regiment, and carried off a large proportion. Stoney Hill was to the 77th regiment literally and emphatically a grave. Such it has likewise proved to other regiments on former occasions, but I am not acquainted with the details.

Having so far considered, in connection, the soils and circumstances of the West Indies, and the fever occasioned by, and existing in them, for the purpose of showing, that marsh miasmata are not necessary to the production of

that fever, since it prevails where such miasmata do not exist; I shall now endeavour to afford further support to the position, by pointing out places, in which marsh miasmata are copiously evolved, but do not occassion the fever in question. Marshes, however, do so generally, though not necessarily, contain what I believe to be the cause of West Indian fever, that the evidence for this purpose will not be so clear, as that for the former. Still it will be effective to a certain extent, and will be sufficient, I think, when added to the former, to accomplish my object.

Here I might frame an extended and a plausible train of argument, by comparing the diseases of the East, with those of the West Indies. I might state, that though there is abundance of marsh-land in Asia, and temperature so various in different places and at different times, that some of them must be equivalent to the temperature of the West Indies; and though there is no apparent reason why the same degree of heat acting on similar soil should not produce the same disease there as here; that, notwithstanding, the fever in question, so frequent and fatal in the West Indies, does not exist on the shores, or in the harbours of the East Indies, or, if it does exist, is so rare as to be an exception to the general condition.

With the same view, the great plain of Egypt might be brought forward, and made the ground of commentary. There an immense surface is annually covered with water, which is gradually and slowly removed by the exhaling influence of a powerful sun. This annual inundation is essential to the fertility of the Delta; the soil is a rich clay, into which the water penetrates during its stagnation on the surface; and, during the latter part of the evaporating process, when the contained vegetable and animal matters are undergoing rapid decomposition, what part of the earth's surface can be supposed more abundant in the cause of West Indian fever, if the hypothesis, which would trace

it to marsh or marsh-like exhalations, be a just one? Arguments of similar kind, from similar sources, might be multiplied almost without end, for they might be obtained from great part of the intertropical circle, and, to a considerable distance on each side, from the temperate zones.

Such a line of argument, though plausible, and difficult to refute, would not however be candid or strictly available; for the same line of argument would prove equally subversive of every cause hitherto proposed. High temperature, the principle of vegetation, &c., would all fail if tried by this rule, for none of them is peculiar to the West Indies.

So great, indeed, is the difficulty of ascertaining the cause of this disease, that, in the present state of our knowledge, we must begin the investigation by taking something for granted; we must start with a postulate. When the localities and natural productions of these islands are better known; when the structural constituents, chemical mutations, and organic products, are more thoroughly examined, and more carefully compared with similar things in other parts of the world, such confession of ignorance will, I think, be spared. But, looking in the usual cursory way at the West Indies, we see nothing in them, or their productions, to distinguish them from many other intertropical parts of the earth; we perceive nothing peculiar to them, on which we can fix as the cause of West Indian fever. We therefore feel the necessity of acknowledging, that there is something singular in the atmosphere of these islands, derived, in a way unknown to us, from the soil, the co-operation of which is requisite for giving effect to the more apprehensible and palpable cause of the disease. Hence, it would not be fair, in an attempt to prove marsh miasmata unnecessary for its production, to deduce arguments from distant though similar parts of the globe; and I therefore choose to limit their sources to places within the operation

of the assumed atmospheric influence, whatever its nature may be.

The kingdom of Brazil deserves notice in this place, not as being within the influence of the power in question, but in close proximity with it. So close indeed is the vicinity, that Brazil appears to be separated from that influence, only by the breadth of the river Amazon. This river divides Brazil from Guiana, and seems likewise to be the boundary of the West Indian fever towards the south; for, while it prevails in Cayenne, it has seldom, if ever, been found in the most northern parts of Brazil, namely, on the south bank of the Amazon*.

* It is possible there may be error in this statement; for in so extensive a region as Brazil the disease may have occurred without being described or recorded; it is not probable, however, that it has occurred, or at any rate prevailed, without finding a historian of some kind; and, as far as my knowledge of the disease's history extends, it has not been noticed. Sir Gilbert Blane, it is true, says that it has been clearly described by Joam Ferreyra De Rosa, in his Constituciam Pestilencial da Pernambuco; and that it was introduced there by the Oriflamme, on her way from Siam to Martinique, to which place, according to Pere Labat, she afterwards carried it. I understand the language in which De Rosa has written very imperfectly; and as I could not procure a translation, I may have misunderstood his meaning; but as far as I may be allowed to judge, from a hasty and unsatisfactory perusal of the work, it does not appear that the fever which prevailed at Olinda, in 1687, was the same as that generally known in the West Indies by the name of yellow fever. The fatal symptoms, according to De Rosa, were an icteric appearance of the skin, and suppression of urine, symptoms which cannot be said to characterise West Indian fever; for a yellow skin is common in ordinary remittent fever, and suppression of urine is as often found in other precipitous fevers, as in the peculiar fever of the West Indies. It is true the author notices the ejection of what he calls *humoses atrabiliarios;* but knowing the fanciful hypotheses and vague notions connected with these terms by the ancients, we cannot affix any definite meaning to them here, especially when we find the writer endeavouring to make them conformable with the

It is curious to contemplate this mysterious limit of the disease. It lies immediately under the equator; it has, of course, the same or nearly the same degree of heat on either bank, the soil and natural productions varying little, in general appearance, north and south. As there has been no permanent European settlement of any consequence on the south bank, or close to it, while the Portuguese, French, and Dutch planted themselves on the north, they cannot be well compared in their effects on health: but St. Salvador and Rio Janeiro are sufficiently near for interesting and satisfactory contrast. The latter town, which is within the tropical line, is placed near an extensive swamp, and has nothing in its aspect or position, from which we should infer its exemption from West Indian fever: yet such fever was never known, I believe, within or near it. Inflam-

aphorism, "si atra bilis aut sursum aut deorsum prodierit." They cannot be supposed to mean *black vomit*, a symptom so peculiar and unequivocal, that it was employed by the Spaniards to designate the disease in question. Besides, it does not appear that De Rosa had any suspicion of the disease which he describes being introduced by the Oriflamme, or from a foreign source of any kind. From his astrological discussion regarding planetary influence, he appears to have thought the disease connected with what has been called an epidemic constitution of the air (*Constituciam Pestelencial da Pernambuco*), or the effect of an immediate and inscrutable infliction of Heaven for the punishment of their sins. The discovery of the Oriflamme's power of propagating a disease, said to have been received at a place 10,000 miles distant, and where there is no proof of its ever having existed, was reserved for Labat, a French monk, who did not leave Europe till seven years after the epidemic fever at Martinique in 1686, a year before it is noticed at Pernambuco by the bye, where it ought to have appeared first, had it been derived in both places from the Oriflamme: Father Labat's information was derived from hearsay, prejudice, and vulgar sources; he considered the subject with a gross and unphilosophical mind; and therefore found it much easier and congenial to fix on a palpable coincidence, than to scrutinize the evidence, and determine whether the coincidence was accidental or necessary.

matory visceral fever occurs there as in other places, and the inhabitants sometimes suffer from intermittent fever. The former are seldom severe, and, like the latter, yield readily to the usual means of cure. The general influence of what is called climate is highly favourable to health, few people dying from acute disease.

Compare this with the health accounts of Vera Cruz. The two cities are placed under nearly equal parallels of latitude; they are both built on a low sea-shore, their plan and style of architecture being much alike; they are both skirted by high mountains, though not at equal distances; the religion of the people is the same, and their habits of life and modes of subsistence similar, I might say the same; and yet, while Rio Janeiro is surpassed by few cities in the old or new world in salubrity, Vera Cruz is a very hotbed of West Indian fever. In endeavouring to account for differences so great and striking, it has been said, that though Rio Janeiro is within the tropic of Capricorn, it nevertheless enjoys cool breezes and a temperate climate. But this is the case only during a few months of the year. For the most part its temperature is high, even when compared with that of the West Indies, Fahrenheit's thermometer not seldom rising to 96° in the shade. Like the West Indies, it has dry and rainy periodical seasons; and its general meteoric phenomena are like theirs. It is plain that lower degrees of heat cannot be the cause of its immunity from West Indian fever. This is confirmed by comparing its temperature with that of New York. There the heat seldom obtains the same elevation as at Rio Janeiro, while in winter the weather is excessively cold, the rivers being frozen, and the surface of the country ice-bound for months. Yet, in the seasons of heat, the cause of West Indian fever has been evolved at New York; and so great have been its accumulation and virulence, that the disease has spread like the breath of

pestilence, destroying great part of those whom it attacked, and filling with panic the minds of observers. But it may be alleged, in opposition to common opinion, that the winter colds of New York co-operate in some way with the summer heat, to the production of the disease, and that the comparison is therefore not to the point. If such objection should be started, let Rio Janeiro and Vera Cruz be compared as to temperature. In that, as well as most other apparent circumstances, they will be found to agree. In the latter, like the former place, the heat is high in summer, seldom below 90°; in the winter the air is cool, sometimes cold, falling, during the prevalence of the north winds, as low as 60° of Fahrenheit. Such facts (and many of similar import might be adduced) must convince us, that in seeking for the causes which render Rio Janeiro so healthy, and give it exemption from West Indian fever, we must look for other agents than cool breezes and a moderate climate.

It is probable, that those agents are negative, if I may so express myself; aud we should therefore find, if inquiry were properly conducted, that something is wanting in the geological structure of Brazil, on which this exemption depends, which exists in the regions of West Indian fever, and which there gives rise to that specific atmospheric influence, to which I have alluded. The nature of that influence is perhaps placed far beyond the reach of research; but its causes may, I think, be discovered, as already hinted, by accurate scientific scrutiny of the component parts of the earth's surface, and its indigenous productions, and the comparison of one region with another. The line which, towards the south, separates the region of West Indian fever, from a region where it does not exist, is so definite, and the proximity of the soils so close, that the probability of such investigation being satisfactory is much increased. Let it be carried into effect in the

West Indies, and on the coast of Brazil, and there is reason to conclude, that that which has been so long a mystery will be understood.

Leaving Brazil, which is interesting in this inquiry, chiefly by comparing it with the West Indies as to physical condition, and contrasting their individual influence on health; without seeking support to my position from facts or deductions gathered from remote parts of the world; but confining my proofs to facts derived from places within the clear and acknowledged limits of the disease's sphere of action; I shall here notice a few circumstances, regarding the locality and health condition of Guiana. Whatever be the nature of the atmospheric influence, which I believe necessary to co-operate, in some way, with the cause of West Indian fever for the production of that disease, it is presumed that Guiana is exposed to it; seeing it operates to the east, south, and north of that division of South America; Tobago, Trinidad, Cayenne, and Cumana, all suffering from the disease. I have no personal knowledge of that coast, and cannot speak of it with the confidence and precision I should wish; but as far as I can learn from written and published relations, it is little else than a great swamp, only partially reclaimed, and rendered habitable, by drains and canals. It appears, from the same accounts, that the interior settlements of Surinam are extremely unhealthy, while its sea coast enjoys a salubrious climate. The causes of such difference I do not pretend to know; but in my remarks on this district, I shall confine myself to the British settlement of Demerara.

The part occupied and cultivated is a strip of land, only a few miles broad, running along the sea coast; it is flat and level, encroached on by the sea, and literally deluged with fresh water in the seasons of rain. From the flatness of the surface and depth of the soil, the rain sinks far and continues long in it, notwithstanding the canals and ditches

for draining it. Hence it is, in truth, an extensive swamp after the rains, and can scarce be called any thing else at any time. In many respects, it closely resembles the great plain of Lower Egypt. It is like it in some of the features noticed. It is in like manner intersected by canals, but for a different purpose. Like the soil of the Delta, the soil of Demerara is prodigiously fertile; and both are exposed to the influence of high, though not equal, temperature. From quantity of water, form of soil, luxuriance of vegetation, and intensity of heat, there must be an immense quantity of gaseous matters exhaled from the surface; and gaseous matter, it is to be presumed, according to general opinion, abundantly imbued with fever-exciting qualities. In Demerara, besides, the canals, cut for removing water, often furnish additional matter for decomposition and evaporation. From want of declivity in their course, the water moves slowly at all times, and a slight accident occasions obstruction sufficient to make it stagnate. On its surface, and along the sides and banks of the ditches, weeds spring up rapidly, and in astonishing quantity; they perish, and a new crop springs up, nourished and invigorated by the remains of the former; and thus, successive crops grow and wither, till, in a short time, if the ditch be not cleared, a mass of decomposed and decomposing matter is formed, which chokes the channel; the water accumulates, and finally oozes over the banks, green, thick, and loathsome in appearance, from the vegetable matter it contains, into the adjacent fields. Here then there is marsh added to swamp. The soil is generally swamp, and the ditches become marshes; and the effect of the whole we should expect to be highly deleterious to health. As to extent, uniformity, and completeness, in the qualities which are supposed to constitute an unwholesome soil, there is nothing to be compared with it in any of the West Indian islands. Looking at the surface

merely, without the sure correction of experience, we should, according to common opinion, pronounce it uninhabitable. But experience informs us that it is otherwise. We find that the cultivators of the soil, merchants, and military occupants, are not swept away periodically by the fury of epidemic fever. On the contrary they enjoy health, and obtain longevity, on an average of years, little inferior to the inhabitants of Great Britain. To intermittent and remittent fevers they are subject, but do not suffer from them severely; for I find by an official statement, that during six successive years, from 1805 to 1812, the proportion of deaths to the number of sick was very far below that of any of the other British settlements in the West; it was as low indeed as in England, among troops at the same period. The concentrated, continued fever, which is here called West Indian fever, is seldom if ever found in this settlement. On this subject I do not speak positively, as I do not proceed on my own observation, and have not met with accounts, sufficiently comprehensive and pointed, to enable me to come to an unhesitating conclusion. But as far as I can gather from published and oral statements, such is the fact; and I am fully warranted, by their tenor, in saying, that this disease is exceedingly rare in Demerara. Let this fact be considered in connection with the nature and condition of the soil, temperature, &c., and it will alone be sufficient to infuse great doubt of the marsh miasmal origin of West Indian fever. I shall here specify some particulars regarding the position, soil, and climate of another British settlement in the West, which will, I think, convert the doubt into unbelief.

Honduras has been held many years of Spain, for the purpose of cutting and carrying to England mahogany and dye woods; and has became a tenure of value, from the number of hands and ships employed in the service. The portion of land so held extends about two hundred miles

inland, and a considerable distance along the coast. The whole is flat and low, at least the rise is not perceptible for many miles from the shore. It lies between the 15th and 18th degrees of north latitude, and is therefore exposed to the effects of high temperature; the soil is rich, and scarcely cultivated in any part; great quantities of rain fall, and the surface is covered with wild and rank vegetation, in growth or decay. Belize is the chief town, in which reside the superintendant, merchants, and most of the labourers, except during the season of cutting and bringing down the timber; and near it are placed the barracks. The situation is close to the sea, and occupies part of a large bog or quagmire, on which foundations for the buildings have been formed by degrees, principally from the ballast of ships which have gone there to load; for so soft is the earth, that houses could not be reared till an artificial groundwork was laid. The rainy season begins in May and continues till December, during which long period, great part of the land in rear of the town and barracks is flooded, pools standing close to the buildings. I was there in May, and in October and November 1825; at both of which periods it rained heavily almost every day, the earth being generally covered with water, and the houses standing in the centre of a lake. The officers confined to their quarters, endeavoured to lessen the tedium of so irksome a life by shooting teal, snipe, &c. from the windows. Myriads of mosquitoes, sand-flies, and other teasing insects, infested the place; and at night, thousands of bull-frogs, under the windows, bellowed with a force, continuance, and strangeness of sound, which effectually prevented sleep. When the dry weather sets in, the heat is intense, the ground becomes parched, and the inhabitants, who were deluged with rain for six months before, have difficulty in procuring water before its termination.

What combination of things can be more favourable to the copious and concentrated production of marsh miasmata, the gaseous results of decomposing vegetable and animal matter, in all forms and during all stages, than these? Yet Honduras is not unhealthy. With Porto Bello to the east, and Vera Cruz to the west, and presenting an aspect which would lead us to conclude, according to common belief, that it must be much more deleterious than either, it is infinitely less so.

I took a good deal of pains to inform myself on the subject, and the result was, as derived from medical authority, that though the inhabitants have a good deal of intermittent, and sometimes, but rarely, remittent fever, the continued concentrated disease, here called West Indian fever, is scarcely known among them. Such was the opinion of two medical officers long on the staff of the West Indies, and intimately acquainted with the disease; but they had not been long enough at Belize to enable them to judge for any length of time. Popular opinion, however, confirmed their judgment, at least in regard to the healthiness of the place. There was universal conviction, that there has been little sickness, and very little death, from precipitous disease; and these are matters about which the people are not likely to be mistaken. I had some conversation with the commander of a ship, who had been twelve years in the Honduras trade, and generally made two voyages in the year, staying two or three months at Belize each trip. He was an intelligent man, who had been often in the West Indies, where he learned, by suffering, and repeated loss of the ship's crew, the effects of fever there. At one time he used to touch at Jamaica on his way to Honduras, but had given that practice up; and assigned as a reason, that his ship got unmanned at the former place. At the latter, he had never lost one from death; he consequently felt

secure, and sent away parties in boats up a river, for two days and a night, on watering duty, without apprehension or detriment. The Rattlesnake passed the most unhealthy period of 1825 at Belize in safety, and such has been the experience of other ships of war which have gone there*.

I shall not cite more instances of this kind, though many might be added; because they could scarcely tend further to elucidate the subject, and I am unwilling to multiply examples, without increasing interest or diminishing difficulty. If it do not appear, from what has been stated, that extensive marshes may exist, within the general limits of West Indian fever's action, without producing a single case of that fever; it at least appears, that the quantity of such fever bears no proportion to the extent of marshes, especially as compared with other places in the West Indies. And I trust, that in the preceding pages there is enough of fact, fair inference, and illustration, to prove, as far as demonstration is attainable in medical investigation, the proposition with which I set out, namely, that West Indian fever does not arise from marshes, simply as such, their exhalations not necessarily containing the cause of the disease.

* It is in proof of West Indian fever being a peculiar disease, the effect of a peculiar cause, and not of the common cause of intermittent and remittent fever, that the places in which it abounds are not otherwise generally unhealthy. The inhabitants of the West Indian islands suffer little from disease, except from the disease in question; and those who survive it are found to enjoy health, and obtain longevity, in a proportion not much under the inhabitants of the British islands. They do not, like the people of Java, Porto Bello, Chagres, and other places where periodic fevers prevail, and marshes abound, languish under the effects of visceral obstructions, and die dropsical ere they reach half the ordinary period of human existence.

THE PRINCIPLE OF VEGETATION*

Has been assumed as the cause of West Indian fever; or, as it is expressed, "the cause of this fever is analogous to the cause which moves vegetation." This method of expressing an opinion about an agent, or a form of agency, is so ambiguous as to be little intelligible without definition or illustration; and as no explanation has been offered, and little argument urged in its support, there is perhaps little propriety in noticing it; for to attempt the refutation of a doctrine, about the meaning of which we are obliged to form conjectures, must be an unsatisfactory and unprofitable employment. The expression here used must refer, I suppose, either to the principle of vegetation, or its products, most probably to the former. If we speak of the principle of vegetation in the abstract, we speak, I apprehend, of what we do not understand. As in many similar cases, we borrow terms from the arts, to which, when transferred, we attach a vague or erroneous meaning, we connect them by some weak conceit, and then imagine that we thereby explain the primary and inherent motions of living bodies; by inherent, I mean something altogether above our comprehension, impressed on or communicated to them, by the Omnipotent First Cause of all things. If the principle of vegetation imply something analogous to vitality in animal bodies, it must mean, as far as I understand the terms, a mode of being, not a distinct essence, and cannot therefore be a cause of fever†.

* Dr. Jackson; Mr. Doughty.

† When I speak of a mode of being, I am aware that I have recourse to such vague phraseology as we often use to flatter vanity and cloak ignorance. I would rather have used the term quality, as being more explicit; but I avoided it on purpose, lest it should appear even for a moment, that I entertained the belief, that the manifestations of animated bodies, which we call life, are the results

It is presumed then, that "the cause which moves vegetation," as applied to the production of West Indian fever, must mean something given out by vegetable matter, either in growth or decay.

In either sense it does not appear well founded, the quantity and power of the disease not being proportioned to the extent and luxuriance of vegetation in any place; and there being in many places no relation between them at all. In support of these assertions, I may again refer to Demerara and the rest of Dutch Guiana, and to Honduras. The whole, as has been stated before, is rich in soil, and fertile in production, beyond most parts of the world, greatly more so than in the adjacent islands of the West Indies. Vegetation is rapid and rank in an extraordinary degree, whether left in its natural course, or influenced by the operations of man. When there is rapid growth there must be extensive decay; so that, whatever be the influence of vegetable matter, it must be experienced in full force here. I need not repeat what has been stated regarding the salubrity of Demerara and Honduras, or point out its application to the present case; for it must at

of the mere accidental juxta-position of the clods of the valley, and the tenants of the dunghill. If our humility bore any proportion to our ignorance, we would at once ascribe these things to the inconceivable agency of the "God of all," feeling that they are mysteries which we cannot penetrate; that to understand them is incompatible, not only with our attainments, but with our condition. That the mere aggregation of inert matters, or the changes which they are capable of effecting upon each other, can rear the structure of an animal body, "so fearfully and wonderfully made," and that this again can of itself furnish forth a "living soul," no man will believe, till his mind become perverted by the inane subtleties, bewildering sophisms, and idle jargon of a system of fables, falsely and ridiculously called philosophy. When man desires to be deceived, and succeeds in placing himself under the dominion of such delusion, "there is more hope of a fool than of him."

once appear how disproportionate is the process of vegetation to the power of West Indian fever. I have said, that there is, in many places, no relation between them; for the proof, it is only necessary to recollect the prevalence of the disease on board ship, where there is no vegetable growth, and little vegetable decomposition.

This is perhaps as suitable a place as any other to express my opinion of the manner in which sporadic cases of West Indian fever are occasioned. While the majority of those, who have considered the origin of the disease, have ascribed it, in its epidemic form, to some generally predominating, or specific cause, in the earth, the air, or human body, they have assumed, that the cases which occur occasionally only are the effects of mere accidents affecting single bodies. When individual members of a community suffer from the disease, it is often observed, that they have been exposed to fatigue, have committed acts of debauchery, or suffered privation in food, sleep, or rest. In Jamaica, sailors desert at Port Royal or Kingston, and travel across the island to Anotto bay, or Port Antonio; or deserting at the latter places, they travel to the former. To escape apprehension and punishment, they walk a great way by day, sleep on the ground at night, and, from scanty means and bad habits, feed ill and irregularly. After being exposed to such a combination of debilitating causes, they seldom, if fresh men, escape a severe attack of fever, at a time probably when fever does not prevail; and it is alleged, that those acts of indiscretion or necessity occasion the disease directly and independently. Exposure of the body to a breeze in a state of profuse perspiration, to the night dews, and many similar things, have been supposed to produce the same effects. Such things, having palpable influence, attract notice, and have power allowed them

which they do not possess; for, notwithstanding the currency of the opinion, that they, of themselves, are capable of inducing West Indian fever, I have no hesitation in questioning, and more than questioning, its justness.

The abuse or improper application of these and other agents and conditions, called non-naturals, lay the foundation, it is true, and rear the great mass of diseased actions to which man is liable. They occasion the primary independent inflammations, from which the various tissues suffer so much, and by consequence the febrile commotions which attend them; and the many states different from health, in which, though said to be cured, they too often issue. They occasion an immense host of morbid conditions and aspects, said to arise from derangement of functions, but in which there is nevertheless some undefined and unappreciated derangement of structure, and are therefore the causes of most of the maladies which affect mankind. But there is a class of diseases, which, though they influence, they cannot induce; the class which we trace to the agency of specific poisons, whether more palpable, and requiring contact for their operation, as syphilis; or less so, and capable of acting at some distance, as small pox. Equally are they incapable, in my opinion, of occasioning idiopathic fever, or essential fever — fever not symptomatic—by whatever epithet of ridicule or unbelief it may be called.

I do not wait to argue with those, who deny the existence of such fevers. With as much reason it might be contended, that measles and small pox are not idiopathic and independent fevers, but the mere manifestation and effect of some previously inflamed organ; seeing that, when those diseases end in death, the cause of death, as far as it is understood by us, is inflammation and its effects, in one or more of the viscera, or congestions so great as to be incompatible with life. In some of the worst cases, where

the system never recovers from the first impressions of the febrile cause, where the pulse continues to flutter, and the skin without heat and animation, the patient sinks rapidly, and no inflammation can be found; then, however, there is great congestion. It happens thus, at times, in West Indian fever. In the worst form of the congestive species, no vascular excitement comes on during life, and on death no inflammation can be perceived. The whole system of capillaries is empty, while the large veins and sinuses are gorged to an extent scarcely conceivable. Whether these things form the cause of death, or whether they are symptoms and parts only of a general state of the system incompatible with life, and not consummated till the last act of expiration, it is not necessary to inquire here; one thing they show clearly, *viz.* that the attempt in such cases, to trace the appearances of disease, and the fatal issue, to simple inflammation, were equally futile as in similar cases of small pox and measles. Similar phenomena present themselves in other idiopathic fevers; in the endemic fever of the British islands, whether called typhus, nervous, low, continued, simple, or otherwise designated; all being, I believe, essentially the same, the endemic products of British soil; and in Indian cholera, which appears strictly entitled to the name of idiopathic fever, and is the endemic production of Asia. From description, it is a congestive fever of the most aggravated character. The predominance of congestion and of inflammatory action respectively, in the fever of Britain, will modify its aspect, and call for modified treatment, and has probably laid the foundation and furnished materials for the different accounts given and different names applied at various periods, malignity and putrescence being ascribed to the worst congestive species. These conclusions may be too general, considering the uncertainty of the premises and scarcity of facts; and as they are not necessarily connected with my subject, I do not

insist upon them, but return to the cause of sporadic cases of West Indian fever, and, in connection with them, to the cause of endemic fevers generally.

Endemic are idiopathic fevers; and according to my views are not the effect of incidents only, but of a peculiar cause, given out, in whatsoever manner, by their proper soil. The operations of nature, though infinitely diversified, and though often intricate and obscure, are nevertheless subject to steady laws and permanent influences. The lichen islandicus will not grow in Jamaica, nor the sugar-cane in Greenland. Bronchocele, which is endemic in Derbyshire, is not found in a neighbouring county; and the *Barbadoes leg* is not found native in Antigua. For these things there must be effective though unknown causes, peculiar to the place where the peculiarity of product exists; and to what else than the soil can it be reasonably ascribed? Hence, it is necessary to conclude, that no combination of incidents, no abuse or improper application of the *non-naturals*, can occasion the endemic disease of any country, whether fever or not, because these, being the same, would produce common effects everywhere, and the endemic character must be lost. It may be said, that difference of climate is sufficient to account for the whole; but endemic character exists in spite of climate, showing itself in the same climates, as far as temperature is concerned; and, in other respects, what is called climate must be referred to the earth, or water on the earth, on which heat operates. That which gives character to disease must be something peculiar in itself; accidents may alter features, or modify expression, they cannot change its nature or destroy its entity*.

* It may sometimes happen that medical men think loosely, and draw erroneous conclusions from the unfitness of the terms "predisposing" and "exciting" cause, as applied to fever. Predisposing means priority of action, and we are therefore apt to think,

If these opinions be correct, they must have equal application to fever, whether prevailing epidemically, or occurring sporadically; for what is necessary to constitute character, must be equally necessary to one case as to a

that predisposing must precede exciting causes, and so, according to the proper use of words, they ought. Hence, when we find those agents which generally precede, and are therefore called predisposing, coming after what is reckoned exciting; and those, on the other hand, which are generally called exciting, as being last observed, apparently acting first, it is reasonable to think the terms convertible in such cases; and therefore, I suppose, it has been thought, that the causes themselves might change places, not only in the order, but also in their manner of acting; or that the predisposing, if in sufficient force, might produce the whole effect. This may be true in some cases, but it is not so in many: it is not true as applied to endemic fever generally. Suppose such a case as the following—a case within my own knowledge. A man was three months on board a ship in the river Thames, when many of his shipmates suffered from ague; he however was in strong health, and escaped an attack at the time. But ten weeks afterwards, when he was within a few weeks sail of Barbadoes, during which time he had not been within the ostensible reach of the cause of ague, having previously been debilitated by an inflammatory disease, regular tertian ague made its appearance. We cannot doubt, in this case, that the cause of ague was imbibed in the Thames, and that it remained latent in the body, in a way we cannot apprehend, not having the power of producing its specific effects till the body was reduced by another disease. Now, in this case, we might say, according to the literal meaning of the terms, that the miasmata on the banks of the Thames predisposed to, and the inflammatory disease excited ague; but if we suppose, that the miasmata and inflammatory fever changed places, not only in the time, but also in their manner of acting; or if we suppose that such agents as the inflammatory fever, that is to say, debilitating agents, might of themselves produce the ague, we suppose what, according to our present knowledge, is physically impossible. Marsh miasmata are necessary to the production of the disease, simply debilitating agents are not; no accumulation or repetition of such agents could occasion a case of genuine ague. The same reasoning applies to West Indian fever, and

thousand. When it happens, as it often does, that one man in a ship or camp exhibits the peculiar signs of diseased action which denote West Indian fever, and his colleagues continue healthy around him, it will often be found that he has been exposed to the influence of the debilitating causes in question; that he has suffered great

endemic disease generally. Something peculiar, the product of the soil, is essential to each; and common causes, in whatever force, cannot alone produce a single case of any of them.

To avoid the obscurity and error arising from the use of these terms, it would be well to abandon them altogether. The term "dispose" would express our meaning more perfectly than predispose; it has been proposed by Dr. Dickson, and would clearly be a great improvement. But I take the liberty of saying, that it would be better to change the whole vocabulary regarding the causes of fever. They may all be clearly and appropriately ranged under two heads, *viz.* first those which are essential, and secondly those which are accidental. The first will express the cause or causes, without which a disease cannot exist; the second, those, which, though not necessary to the disease's being, are often powerfully operative in its production, inasmuch as they bring the body to the state best fitted for the impression of the necessary cause; and which, in some fevers perhaps, but certainly not in all, may of themselves induce the disease. We have thus two obvious terms, sufficient, I think, to denote the whole causal agency of fever, namely, *essential* and *accidental*; and get rid of the divisions and subdivisions in common use, predisposing, exciting, occasional, and so forth. The term efficient, as opposed to final cause, may, with propriety, be expunged; because the latter is seldom employed, and is never necessary, at least in medicine. The term "proximate" cause appears to be going out of use by common consent, from despair of fixing its meaning, or determining what it is or ought to be. Cullen meant, not even a primary morbid action, but an advanced phenomenon in the diseased catenation. It has been said by Parry, that proximate cause is that phenomenon which is next to the disease. If this mean a condition of the body, which it appears to do, it is a misconception, for that which is next to disease anteriorly must be health. Did it mean something exterior to the body, I presume it must mean that which induces disease, and must therefore be either an *essential* or *accidental* cause.

fatigue, or has gratified appetites to the extent of debauchery. But such sufferings or indulgences do not *per se* occasion the disease. By debilitating the body they render it more easily affected by the peculiar and essential cause, which is then weak, and requires some such co-operation to make it effective; but, on other occasions, becomes so concentrated as to affect communities, independent of every thing but its own inherent power.

Such is the difference in the origin of sporadic and epidemic cases of West Indian fever, the essential cause and the effect being the same. In the former, the essential cause is not sufficiently powerful to excite the disease without the assistance of predisposing agents; in the latter it is so energetic as, unaided, to disseminate it far and wide. Were it otherwise, had debauchery, privation, exposure, or fatigue, singly or together, the power of producing it, it would not, in its occasional attacks any more than in its general visitations, be confined to the places in which it is found; it would occur, every now and then, at Madras and Rio Janeiro, as well as at Jamaica or Havanna, since such things happen among British soldiers and sailors, nearly in the same proportion everywhere.

MEMOIR III.

New Opinions regarding the Cause of West Indian Fever proposed.

In the last Memoir, I examined the principal opinions which have been suggested or maintained, regarding the cause of West Indian fever, and stated some of the reasons why, to me, they appear erroneous; the important and more difficult part of my undertaking remains to be accomplished, namely, to supply with correct opinions the place of those which have been rejected, and thereby exhibit, in its real aspect, the cause of this formidable disease.

It is proverbially easier to demolish an old structure than to rear a better one in its stead, especially when the old, from bad foundation, fragile materials, or faulty workmanship, is hastening to decay, and ready to fall of itself. It may be found, that, instead of removing obstructions and eliciting truth, I have only substituted one error for another, and have increased the darkness and confusion in which the subject is involved. Adverting for an instant to the industry and talent which have been employed unsuccessfully in the inquiry, I feel how unjustifiable it were in me to count upon fully obtaining my object; yet in a matter at once so interesting and obscure, it is laudable to remove one difficulty, or to add a single illustrative or corrective fact; and I am therefore not only warranted, but in some measure bound, to publish the reasons and results of honest, though it may be erroneous convictions.

The West Indian islands, and parts of the adjacent

coasts of South and North America, constitute the proper soil, and are the perpetual sources of West Indian fever. From the mouth of the Amazon to Charleston in one direction, and from Barbadoes to Tampico in another, the causes of this disease are in constant, though unequal force, in regard to different seasons and localities. It sometimes extends as far north as New York, and has appeared, by accounts, in the south of Europe. But without including these, its occasional and extraordinary out-breakings, we find it embracing a considerable portion of the earth's surface. It reaches from the equator to the thirty-second degree of north latitude, and from the sixtieth to the ninety-eighth degree of west longitude, including the Caribbean and other islands called the West Indies and Bahamas, the contiguous coast of Columbia and Guatimala, and the extensive shores of the Mexican gulf, sweeping from Cape Catouch on the west to Cape Sable on the east, and running thence along the coast of America to Charleston.

The space contained within these limits exhibits great variety of aspect, comprehends much difference of soil, and is exposed to very different degrees of atmospheric heat. But on observing the whole, and comparing its various parts with one another, and with other parts of the world, we perceive nothing to account for its uniform fertility in the production of West Indian fever. At least we perceive nothing, looking in the usual way, nothing in heat, moisture, nature of soil, or condition of subject. Hence, as has been remarked before, it is necessary in this inquiry to take something for granted. We are compelled to view the more palpable cause of this disease, in connection with a certain atmospheric influence, peculiar to the space in question, and essential in its co-operation, either as disposing the body to be acted on, or uniting with and perfecting the qualities of the more palpable cause itself.

This atmospheric influence (we must speak vaguely of what we do not understand), it is reasonable to suppose, is derived from the earth's surface; for we have no notion, can have no notion, of any thing contaminating the air, excepting the earth or its productions. It is therefore to be presumed, as I have already hinted, that the cause, if not the nature of this influence, must be learned, if ever it be known, from a careful and scientific survey of the more superficial parts of the crust of the globe. If, in the West Indies, the component parts of the soil, productions, and immediate substrata, were examined, compared with each other, and with similar matters in other parts of the world, where, from situation and general aspect, we might expect to find the disease, and where it does not exist, the result would doubtless be highly satisfactory, inasmuch as it would solve this difficult and interesting physical problem.

The medical officers of the army are well qualified to accomplish the object. They are stationed at so many points of our intertropical possessions, both within and without the pale of West Indian fever; they possess knowledge to qualify them for the investigation, and have generally sufficient leisure to enable them to pursue it. Nothing of the kind can be expected from the naval members of the profession. They visit, it is true, every coast, but they stay so short a time at the places where they touch, that they can see them only in glimpses. And the medical practitioners in civil life will never effect much in such an enterprise; for whatever zeal and talent they possess (and many of them possess much of both), as a body they are too much detached in pursuit, and too much divided by individual interest, to obtain an end, which, from its nature, must be prosecuted by the united and persevering exertions of many. To the medical men of the army, then, we ought to look for the execution of this work; and, from the com-

bination of qualities which they possess, we might confidently look to see it executed well. The field is extensive, and the labour would be great; but when shared among many cultivators, the one would become comparatively narrow, and the other light. Besides, the promise of reward would at once excite to exertion, and render it pleasing. The prospect is before them of removing obstructions, which have long hindered the progress of medical science, and the development of truth; which have occasioned intemperate and undignified discussions, unworthy of a liberal profession, and injurious to the great concern about which it should be busy, the diminution of human misery. The prospect is before them of determining what there is in the soil of the West Indies, which gives out the peculiar influence, rendering them subject to West Indian fever; of tracing, at the same time, the cholera of India to its source; and of unfolding the heretofore hidden things in nature, which constitute and characterise endemic disease in general. The consummation of such a work will assuredly be honourable to the name, and gratifying to the feelings of the men who shall achieve it; and when achieved, would it be visionary to hope, that the causes might be so controlled, as to render the scourges of our race, to which they give rise, less extensive or less severe?

Impressed with the belief, that more intimate knowledge of soil and subsoil than we possess is necessary for comprehending the causation of West Indian fever, I have been led to look for something peculiar in the geological structure and superficies of the places in which it prevails. But from the nature of my professional duties, my observations, in that line, were necessarily superficial and scanty. They were directed to the mere surface, and have no pretension to accuracy even there; they have little relation to the method which I have suggested above; and I beg it to be understood, that they, and the few remarks attached to them, are

submitted as hints and surmises only, possessing little value in themselves, but which may call forth valuable information from others. Should they lead to temperate discussion, and induce others possessing knowledge of the subject to bring that knowledge forward, either for the purpose of refutation or correction, they will so far at least be useful.

From those cursory observations it appears, that lime of secondary formation is a predominating ingredient in the surface of the West Indies; and that in many places there is much recent volcanic matter; and further, where such surface is most conspicuous, from being scantily or not at all covered with earth and herbage, that there West Indian fever is peculiarly frequent and severe, if the more palpable cause of the disease exist in the vicinity. These things appear sufficiently remarkable in Jamaica. A calcareous crust is spread over great part of the island, and in many places is exposed without superincumbent soil. Fever pervades every part of it which is not mountainous, but is most frequent and severe where these phenomena are most conspicuous. I will here specify what has occurred to me on the subject, in this and some other islands.

Rock Fort is placed three miles east of Kingston, at the eastern extremity of the plain of Liguanea, where it is narrowed to the breadth of a few yards, being bounded southward by the sea, and northward by a precipitous mountain. The contracted and scanty soil, east and west, is alluvial, but dry, with the exception of a small spot to leeward; and the appearance of the whole would not give an impression of insalubrity, the sea washing one side, the structure beneath being coral, and the mountain, which rises behind a naked calcareous mass, destitute of soil and vegetation. Yet this has been one of the most fatal spots to our troops of any in the West Indies, the detach-

ments employed in its garrison having been swept off in rapid sucession by West Indian fever. The officers' quarters stood at a short distance from the general barracks, on a dry indurated foundation, close to the mountain, and occupied presumptively a more healthy site than even the general building. But experience has shown, that it was more unhealthy. So unhealthy was it, and so concentrated is the cause of fever upon it, that scarcely an individual quartered there, if lately from Europe, escaped with life. It is for the present abandoned.

Even to the men composing the naval force of the station, this spot has been terribly destructive. Near it there is a spring of excellent water, from which the ships of war are supplied. The crews of boats employed in bringing water thence to the ships were detained a considerable time at the watering place, and scarcely ever escaped suffering from that detention. Entire crews were affected with fever, and great loss ensued; greater, I believe, than from all the other harbour service put together. Disease from this source is now avoided among the men of war. Floating tanks, wrought by black men, are used to carry the water from its source alongside the ships; and watering, which formerly occupied many days, is now completed in a few hours, as far as ships' companies are concerned, without exposure or danger. There can be no doubt that this measure has spared much suffering and saved many lives, and that it is really an important improvement, deserving more commendation than many other changes, which have made greater pretension and obtained more praise.

Stoney Hill, though not a constant, is at times a fertile source of West Indian fever, as the returns of the 77th regiment will testify, and to which I have before made reference. It has been stated, that the barracks occupy the summit of the hill, which is 1300 feet above the

sea, that they enjoy a comparatively cool climate, that there is neither marsh nor marsh-like land in the vicinity, and that their situation is inviting, and gives promise of extraordinary healthiness. In this place I shall only add a few words, to give a clearer notion of the formation of the hill on which they stand. It is, as its name implies, stony in external structure. From its visible base in Liguanea to its top, a distance of about a mile by road ascent, it is one great mass of calcareous rock, intersected by deep fissures, and split, in many places, into large fragments, pushed often from their original position, heaped on each other, and crumbling into powder. Over its whole surface there is scarcely any soil, and little grass, weed, or herbage of any kind. But it has been and is still generally covered with forest trees, the roots of which are seen expanded over, and clinging to, the naked rock, the extreme fibres dipping into the crevices, and hiding themselves amid the detritus of rock, decaying leaves, and other ligneous matters which are lodged there.

Fort Haldane, at Port Maria, occupies the extreme point of a promontory, which projects considerably from the main land, and divides the bay into two basin-like recesses. The promontory, which is 150 feet above the sea, is about 200 feet across, is so nearly perpendicular, and so much alike in its faces, that it has the appearance of an artificial structure reared for the defence of the harbour. It is formed of a pure carbonate of lime, so white and regular as to look like a whitewashed wall; it is level, smooth, and dry on the surface; and when it was chosen as a point of defence for the construction of a fort and barracks, it was probably thought to be one of the most healthy situations in the West Indies. Merely looking at it as an elevated, regular, dry mass of limestone, washed on three sides by the sea, we should think so still; but the experience of the troops, and the sick returns, prove it to

be, in every way, the reverse. So productive of fever and so deadly has it proved, that for some years past it has not been garrisoned. In November, 1824, on urgent representations by the local authorities, that troops were necessary, from apprehended insurrections of the slaves, a detachment of the 50th regiment was sent to occupy the barracks. Fever appeared among them so early, and was so destructive, that, at the expiration of six weeks, the detachment was removed, having lost, I believe, one-third of its number from fever, and the remainder mostly suffering from its effects; and this happened at a season generally healthy, to troops who had been some years in Jamaica, and who were landed in health. Two streams fall into the bay, one on each side of the headland, at about a quarter of a mile distance. They move slowly, and their banks are covered with mangroves, which, it is to be presumed, furnish the more palpable cause of the fever; but it is remarkable, that the inhabitants of the village of Port Maria, which is situated on both sides of one of the streams, do not appear to suffer from their situation*.

* The old barracks at Montego Bay stand upon the side of a dry limestone rock, about 500 paces from the sea, and at an elevation of nearly 200 feet: there is very little soil incumbent on the rock, and nothing, using the word in its ordinary acceptation, to retain moisture. The hill, except where artificially cleared, is pretty thickly covered by trees; but the appearance of the place would not give a stranger the impression of unhealthiness; nay, looking at low wet soils and swampy situations as the sources of fever, he would be disposed to consider the site of the barracks a very healthy position. Yet these barracks have proved so abundant in the cause of West Indian fever, and so destructive to the troops, that after years of suffering, and a great loss of life, they were some time ago abandoned, and barracks built at some distance in the plain below, where vegetable matter and moisture abound.

In strong contrast with Montego Bay, both as to locality and effects on health, Falmouth, another position on the same coast, and

The bay of Port-Antonio is likewise divided into a double harbour by a somewhat similar promontory. Where it joins the main land, however, it is so low as to be covered with water, in ditch form, almost across, and is therefore peninsular; it is less elevated and regular in form than that at Port Maria; but it is equally dry, and of the same calcareous structure; it is the situation of a fort and barracks, the men quartered in which have suffered severely from West Indian fever. It is now nearly abandoned: a few men have been stationed there merely to look after the works.

Port Royal may be noticed in this place, as occupying a bare and unproductive calcareous surface. The naval arsenal and hospital, the fort and barracks, and the town of Port Royal, are placed at the extremity of a neck of land which forms the harbour to the south. Where occupied by those buildings it is not more than 200 yards across, scarcely rises above sea level, and is destitute of soil. It is a coralline structure, covered with sand, which is white and dry superficially, but which is percolated by sea water below; it has the appearance of a recently formed and fragile appendage to the island, and has already been partly destroyed by earthquakes. In a northern direction, and distant about half a mile, there is an extensive mangrove marsh, occupying a broader and irregular part of the promontory, which furnishes, there can be little doubt, the more palpable cause of fever, with which this place is affected. It is to be observed, however, that the sea and land breezes, which blow very steadily here, do not traverse the mangrove marsh ere they

distant to the eastward about fifteen miles, may be noticed. It is low, swampy, and abounds in rank vegetation; yet it is one of the healthiest posts, not mountainous, in Jamaica; the old barracks at Montego Bay were high, dry, and nearly destitute of herbaceous matter, and they were at the same time one of the most unhealthy.

reach Port Royal. During the day the sea breeze ranges from about north-east to south-east, for the most part inclining to the south; and the land wind by night descends pretty steadily from the mountains in a north-west direction; they therefore pass alternately almost directly across the promontory, and not along it, which, it might be supposed, would carry the deleterious influence of the mangrove marsh, whatever it may be, clear of Port Royal. If they did so, what should we find to account for the prevalence of fever there? A narrow coralline promontory running miles into the sea, on the extreme point of which there is nothing incumbent, excepting sand and gravel, and the public institutions, and private dwellings of the inhabitants. Such things, according to common opinion, would afford no ground for apprehending West Indian fever. It is unnecessary that I should write a word concerning its prevalence there, or expatiate on the number of its victims. Thousands of families in the mother country can furnish, from loss of members, memorials of the fatal influence of this hand-breadth spot of our colonial possessions.

Port of Spain, which stands in a plain of the same name, is the capital town, and principal place of strength in the island of Trinidad. The plain has the gulf of Paria to the south, and is bounded on the east, north, and west, by a winding ridge of mountain. The town is situated on the sea margin, within and near the south-east extremity of the ridge, which bounds the plain landwards, and which forms a large segment of a circle, the diameter of which is about three miles. The plain is low, alluvial, abounds in moisture, and being situated in a mountain recess, is imperfectly perflated by the breeze, and therefore hot; yet it is not unhealthy, generally speaking. The inhabitants have attacks of intermittent and remittent fever occasionally, which are not untractable. The south-east extremity of the mountain ridge in its rear, however, is singularly un-

healthy. For the purpose of raising works of defence, it has been cleared, and is now a naked, dry, calcareous surface, containing, it might be presumed, no cause of fever in itself. Nevertheless, this spot, having 1500 feet perpendicular elevation, so arid, so destitute of moisture, so clear of organic matter—this bed of limestone has been a very hotbed of West Indian fever. The number of men who have perished in fortifying and garrisoning it is immense as a proportion; and to station men here is considered as consigning them to the grave. South-east there is an extensive swamp, to which we must trace the more palpable cause of the disease. But it cannot escape observation, that many places in the West Indies, and on the opposite coast, not remarkable for unhealthiness, are as abundant in the apparent sources of fever as this marsh; and it is singular, and strongly arrests attention, that the entire deleterious influence of the marsh appears to be concentrated on the mountain position, distinguished by its white, clear, calcareous surface.

Fort Louis, in the island of Martinique, stands upon a peninsular projection, similar in most respects to that which I have noticed at Port Antonio. It juts into the spacious bay of Fort Royal towards the west, and forms the eastern boundary of the small bay, at the bottom of which the town is built. Close on the east side of this projection, a small river falls into the sea, the mouth of which is sufficiently spacious to form a tolerable carenage and harbour. The stream has little descent, winds along the back part of the town, and its banks are covered with mangroves. The composition of the rock on which Fort Louis stands, I had not an opportunity of examining satisfactorily, but, judging from appearance, it is calcareous, of that kind called tuffa; its form is regular, arising in right lines from the sea to its summit, a height of about 100 feet seaward, whence it slopes to the neck of junction with the main land. The

whole surface is smooth, clean, and dry superficially, with little apparent capability of absorbing or retaining moisture. This position, like many similar positions, and the contiguous carenage, have been noted for their insalubrity, the garrisons of one, and the crews of ships stationed in the other, suffering severely from West Indian fever. To the Lamentine, an extensive swampy tract lying some distance eastward, the cause of fever in Fort Louis, and the carenage under it, has generally been ascribed, though no reason has been assigned for its settling almost exclusively there. Whatever the cause of that selection may be, the more palpable cause of the disease exists abundantly, I believe, much nearer the places of its operation, namely, in the mangroves which abound on the banks of the stream, which has been noticed as falling into the carenage close to Fort Louis.

Guadaloupe, judging from a cursory view of its exterior, is generally a calcareous formation. In many places there is exposure of naked lime surface; and while the island is generally unhealthy, those places are peculiarly so. Point a Pitre is especially worthy of attention. The district is generally marshy and unhealthy, but the dry and more elevated positions are productive of West Indian fever, and deadly in a very high degree. About a mile east of the town is placed Fort Louis, and a mile further east Fort Fleur de Pays; they occupy points of a calcareous ridge, which extends some distance close along the sea coast, and which, from its comparative elevation, absolute dryness, and contiguity to the sea, might have been deemed a situation favourable to health. These forts have been distinguished by the mortality of the fevers which have assailed the troops, a mortality scarcely equalled in other parts of the West Indies. On the land side of the calcareous ridge, the soil is swampy, and the base of the ridge is covered by trees and underwood, but the people who live in the centre of

the swamp, suffer much less than those who occupy a more elevated and distant position.

It is generally known how fatal St. Domingo proved to the troops at the beginning of the French revolutionary war. Entire regiments were cut off by West Indian fever; despondence paralysed the mind as disease did the body, and the effect of the whole was frightful. The physical causes, which then occasioned desolation, still exist; but, from political changes, they are comparatively inoperative and unnoticed. In a medical point of view, they possess perpetual interest, which will vary according to varying circumstances affecting them, but cannot be lost. I shall allude to the topographical aspect of Port-au-Prince, formerly the principal quarter of the British troops. It stands at the bottom of a deep bay; hills of various height nearly surround it; the soil of the plain is not remarkable, and does not deserve the name of swampy. But the hills adjacent are calcareous, naked, white and shining on their summits, and the sea beach is thickly beset with mangrove bushes; a combination which has struck me as forming peculiarly the soil and source of West Indian fever.

Fort Bizoton, a place of considerable strength, was one of the principal fortified places at the bottom of the bay, during the British occupation, and proved a chief source of suffering and death to our troops. It stands three miles west of the town of Port-au-Prince, on the southern shore, within two hundred paces of the sea, and occupies the summit of a dry, calcareous mound, about eighty feet above sea level, having no vegetation on its lateral surface, except dwarf and stinted shrubs. Around its base there is a stripe of alluvial soil, which is succeeded landward by dry lime surface, covered with trees and shrubs; the land directly behind rises first gradually, and then boldly, till it reaches the height apparently of sixteen or eighteen hundred feet, the whole being, as viewed superficially and hastily, a

secondary calcareous formation. Two streams of pure water, descending from the mountain, rapidly pass the fort, one on either side, to fall into the bay at points where there are many mangroves. Between the fort and sea there is a small level spot, where water moves slowly, perhaps stagnates partly, but it is covered with grass, and does not appear a spot to be dreaded.

The superficies of Barbadoes appears to be calcareous throughout. Tuffa abounds in the neighbourhood of Bridgetown, and is exposed, in many places, without superincumbent soil. The extent and mortality, with which West Indian fever formerly prevailed in the island, have been noticed; of late it has been comparatively rare.

New Providence, the principal of the Bahama islands, is situated to the north of the northern tropic, and enjoys the alternation of seasons, similar to those of more northern regions. Summer and winter are marked by the difference of temperature and the different conditions of the earth thereby induced. In so far it differs from the West Indian islands; it likewise differs from them in many of its natural productions, and in general aspect, as modified by those productions. But it has, in common with them, the power of producing the rapid, concentrated, continued fever, to which the name of West Indian fever is given in these sheets. A severe fever of remittent type also occurs here; but I can have no doubt, that West Indian fever, from the accounts I have met with, is an endemic product of the island, and that it has been the chief cause of loss among new comers. The loss among such has sometimes been great, even when compared with the West Indies.

Behind the town, and south of the harbour of Nassau, a ridge of hill, about three hundred feet above sea level, runs east and west. Half a mile west of the town, Fort Charlotte is erected on the brow of this hill, overlooking the harbour, and on the summit of the ridge there is an extensive range

of barracks. East of Fort Charlotte, and about the same elevation on the hill, there is a neat barrack for the artillery. Lines drawn between those three buildings form a triangular space over the harbour of Nassau, the base extending from Fort Charlotte to the artillery barrack, the general barracks on the top of the hill forming its apex; and the included space is a dry limestone structure, naked, or covered with short grass, and exhibits nothing in itself, which, according to common opinion, can occasion fever. Yet all these positions are infested by fever, especially the large barrack on the summit of the calcareous ridge, which has proved fatal to so many successive detachments, that it is now determined to abandon it, and build elsewhere. At the base of the hill, on the south or land side, there is an extensive swamp, thickly covered in many places by underwood; and near the artillery barrack eastward, a large tamarind tree is undergoing rapid decomposition.

Carthagena, the principal seaport of Colombia, while it resembles New Providence in the production of West Indian fever, is very unlike it in many respects, especially as to situation and climate. It lies in the tenth degree of north latitude, and is exposed to the constant influence of high temperature; and hence its seasons have no affinity to those of extra-tropical countries. They are not divided into summer and winter, but into dry and rainy; and the productions and appearance of the place are all in accordance with that constitution of things. Carthagena possesses one of the finest harbours in the world, was formerly a chief emporium of the mineral riches of South America, and a place of much magnificence and strength. It had a great influx of strangers in quest of wealth, during peace; and in periods of war became a desirable place of occupation to an enemy. From its general insalubrity, and being the constant seat of West Indian fever, great numbers have perished in both pursuits.

Its geological structure, judging from appearance, is similar to that of the places which I have noticed as furnishing most abundantly the cause of West Indian fever. The subsoil is calcareous, and is in many places exposed without incumbent soil; and the shores are covered with mangroves. East of the town, which is insulated artificially, there is a low swampy space of several acres, saturated with sea water, and abounding in mangroves. Beyond this tract, on a bare calcareous mound, is placed the citadel, behind which rises the mountain of the Poppa. The harbour stretches towards the west, and is fenced from the sea by a stripe of land parallel to the main, extending from the south extremity of the town to where it approaches the main land, and forms the Bocca Chica, which is the entrance to the harbour. This stripe is calcareous, covered on its inner shore, like the low land east of the town, with mangroves.

The predominance of lime in these situations, viewed in connection with their great power of producing West Indian fever, deserves consideration, I think, in an attempt to investigate the causation of the disease. It occurred, of course, to my observation, at successive periods, and in the first instances attracted little notice: it presented nothing which could be supposed to have affinity to the causes which have been said to excite the disease. But when, on visiting various places distinguished as the scenes of the disease, I found the same kind of subsoil, with slight difference, presenting itself over and over again, such uniformity arrested attention. Whether the connection of lime and West Indian fever, in those situations, be accidental and inoperative; whether the former be in any way essential to the latter; whether it forms or contributes to form the peculiar atmospheric influence, which is here assumed to be essential to the existence of the disease, I do not pretend to know. The observations are too few and

too imperfect to warrant any general conclusion, or even to justify extended speculation; and I therefore proceed to the consideration of the more palpable cause.

This, I believe, is furnished by wood, being a gaseous product of trees and shrubs, in a state of decomposition; generally given out by them in a cut or dried state, but which may arise from a living forest, trees being capable, in different parts of their frame, of simultaneous growth and decay; and further, that wood, after it has passed from the green to the dry state, is still capable of generating the cause, certain degrees of heat and a certain quantity of moisture being supplied. I therefore believe, that decomposing vegetable matter, in a certain sense, but not in the sense generally received, furnishes the cause of West Indian fever; as it is not to herbaceous but *ligneous* matter that I trace it, and on which I shall endeavour to show its dependence.

In the last Memoir I made some remarks on the prevalence of the disease in ships, for the purpose of showing, that marsh miasmata are not necessary to its production; I recur to the subject to prove its ligneous origin: and from this source, evidence amounting to proof may be obtained, that the matter in question, if not the only cause of the disease, is a frequent, and in many places the exclusive cause.

Before I enter on the statement or consideration of the evidence, however, I think it right to anticipate an objection which may be raised to its validity; for it may perhaps be objected, that West Indian fever, when it prevails on board ship, is not occasioned by any thing in the ships themselves, but is derived from the land to which they are lying contiguous. To obviate this objection, little more will be necessary than to state the fact, that the disease sometimes breaks out at sea, far beyond the influence of marsh miasmata, or any other influence from the land. Two instances of the kind occur to my memory at this

moment, those, namely, of the Thracian and Lively, in which the fever appeared during a cruize. Many cases are known to me, in which fever having manifested itself in harbour, the ships were ordered to sea, in expectation that the united benefit to be derived from the salubrity of ocean atmosphere, and absence from causes of sickness operating in port, would mitigate or arrest the disease; but it proceeded in its course, uninfluenced by change of place and circumstances; nay, it often proceeded with increase of virulence and mortality after, though not in consequence of, the change.

It sometimes happens, that two ships arrive in the West Indies from England at the same time, that they anchor a few yards from each other, and at the same distance from the shore; and then it sometimes happens, that one of them suffers severely from West Indian fever, while the other lies by her untouched. In such cases we cannot conceive how a cause of fever, coming from the shore, should attach itself to one ship and pass the other *.

* This was strikingly exemplified at Port Royal in 1824. The Primrose was commissioned about a month after the Rattlesnake, and arrived at Port Royal about a month later, *viz.* about the end of July; she was employed, I may say exactly as the Rattlesnake was. After a very short cruize, she lay six or seven weeks, including September, within a few yards of where the Rattlesnake lay, and was employed in the same manner; she then followed the Rattlesnake to Chagres, and the coast of the Spanish main, where she encountered similar weather, and remained about six weeks; she then returned to Port Royal healthy, and continued so; and that while the Rattlesnake was converted into a complete hospital, from the epidemic prevalence of West Indian fever.

On the other hand, in 1825, while the Lively, Pylades, and Ferret were half unmanned, the Rattlesnake lay close to them, in Port Royal harbour, for the space of eight weeks, and did not lose a man. Some sporadic cases of fever occurred certainly, but they were generally slight; and the whole effect differed entirely from the epidemic which prevailed so fatally in the other ships.

Sporadic cases, and sometimes many cases together, arise from causes on shore; as when boats' crews, parties at dock-yards, and persons on board, are exposed to it there. But in its more general and fatal form, when it attacks many in rapid succession, without distinction, and almost without limitation, assuming the character of a destructive epidemic; in such cases it is the effect of a morbific process, going on in the ships in which it prevails. What the manner of that process is, and what is the nature of its products, may not be determined. It will not be attempted to determine them here; but an endeavour will be made to establish their existence, and to trace them to their source of operation.

It is often said, when fever prevails epidemically on board ship, that it is occasioned by a foul state of the holds, though it is not said in what that foul state consists. No explanation has been given respecting its source or nature generally, and that which has been offered for a few cases, has been altogether unsatisfactory, as applied to the whole. It has been alleged, that artificial marsh, or a composition having the same power, is formed in the holds of ships, which occasion fever among their crews. I have already controverted that opinion, and return to the internal condition of ships, as connected with this disease, for another purpose; but for this purpose it is essential that the facts of the case should be precise and indubitable. The following reports, addressed to the naval commander in chief, have been alluded to before; but as they are clear and authentic documents, bearing on the point in question, they are here inserted at length.

"Kingston, 6th February, 1820.

"SIR;

"In compliance with the desire signified in the letter, with which you honoured me on the 1st instant,

I repaired on board his Majesty's ship Iphigenia, in Port Royal harbour, the day following, with Dr. Adolphus, deputy inspector of army hospitals, whom I requested to accompany me, where we had the advantage of meeting Captain Parker, and Dr. M'Namara, surgeon of the naval hospital. We then proceeded to examine very minutely every part of the ship's decks, tiers, store-rooms, and holds, making all necessary inquiry into every circumstance, which seemed connected with any of the possible causes of the disease raging on board; and we afterwards visited the naval hospital on shore, and saw every case of fever sent thither out of the Iphigenia. From Captain Parker, and also from his officers, we received, in the fullest manner, every information we called for, and we have likewise been furnished with such documents as were deemed requisite; since which I have received from Captain Parker unreserved explanations on numerous other points.

"Being thus prepared, and having in the mean time maturely considered all the particulars we had become acquainted with, Dr. M'Namara and myself met on the 4th instant (Dr. Adolphus being unfortunately hindered from joining us by military duty); and after discussing the various points in succession, we have entirely concurred in drawing the following conclusions, from the evidence before us, which, as regarding the subjects embraced by those conclusions, was ample and sufficient, *viz.*

"1st. That the disorder prevailing in the Iphigenia, is the genuine yellow fever of the West Indies.

"2d. That all we have been able to discover in its history and progress tends to convince us, that the fever in the Iphigenia has neither originated in, or been propagated by, contagion.

"3d. That the singular state of cleanliness, and dryness, in which every part of the ship has constantly been main-

tained, together with the sound and perfect state of all her provisions (including wood for fuel), as well as the planks and timbers, and her iron water tanks, forbid all rational supposition that the cause of the fever could have been generated or have existed on board.

"4th. That the fever in question can therefore only have been produced by an external cause.

"In reading this last conclusion you will not, Sir, I trust, suppose that we are willing to rest here, and to close our inquiry with a result so unsatisfactory; we are deeply afflicted, on the contrary, at seeing that the excellent regulations, which you have framed for the preservation of health in the vessels under your command, and the fidelity, solicitude, and judgment, with which these regulations appear to have been uniformly carried into execution by captain Parker; that every precaution, in short, which a long experience in hot climates recommended, or which human foresight could suggest, have in this particular instance been defeated by an overpowering cause, whose existence and operation might have long remained unsuspected, but for the manifestation of its fatal effects, in a vessel which an admitted superiority, as to cleanliness, interior economy, and discipline, had promised to secure from any similar calamity; and feeling, therefore, the importance of discovering that external cause to the future safety of the navy on this station, we are desirous, with your approbation, of exerting our humble powers towards removing the uncertainty, in which the seat of its existence is at present involved.

"But we apprehend, that the data which the Iphigenia singly may afford will be too scanty, and that it is by an investigation of leading facts, in the history of those vessels of war which have been either most healthy, or most sickly, in this harbour for some years past, that legitimate and practical inferences, such as the welfare of the

service demands, can be obtained. Under this impression, we abstain, at this time, from offering any opinion as to the probable cause of the disorder in the Iphigenia; though we think it right to state, that we have not hitherto found evidence sufficient to authorize the belief, that her anchorages in the harbours of Curacoa, off Puerto Cabello, or Port-au-Prince, within the last five months, have at all contributed to produce the fever. Awaiting your farther commands,

"I have the honour to be,

"E. N. BANCROFT, M. D.

"Rear Admiral
Sir Home Popham."

"Kingston, 4th February, 1820.

"I have no hesitation in declaring it as my opinion, that the fever which has manifested itself on board H. M. S. Iphigenia has not arisen from any cause whatever within that vessel, as, after a most minute inspection of every part of the ship, it is with sincerity I declare, that the order, regularity, and cleanliness on board never can be excelled.

"From the observations my experience of the diseases of this climate has enabled me to make, it appears that at least nine-tenths of European subjects, whether in civil or military life, are not only liable to, but are actually seized with fever within the first twelve or fifteen months after their arrival in a tropical climate; that when the disease has once shown itself among a considerable body of men (as in the army and navy), it is sure to spread with unerring certainty to a greater extent, and under a more aggravated form than when persons in civil life are the subjects of its attacks; not from any contagious properties, which I distinctly and wholly deny, but occasioned by the dreadful presentiments, which the men invariably encourage

under such circumstances; by that terror of disease which is sure to predispose the constitution to febrile attacks, upon the slightest deviation from the established rules for the preservation of health, amongst persons unassimilated to the climate. When it is considered, therefore, that the crew of the Iphigenia had never undergone what is emphatically called a 'seasoning,' in its terrific form, it is not to be wondered at, that sickness should at length make its appearance among them.

"With regard to the nature of the fever, I consider it, what time has proved it to be, the indigenous offspring of this climate; and its degree of mildness and severity to depend on the circumstances of season, of locality, and constitutional temperament; and lastly, I consider the fever to have been produced by increased temperature, and other atmospheric causes.

"I. ADOLPHUS, M.D.
"Deputy Inspector of Hospitals."

"Naval Hospital, Port Royal, 7th February, 1820.

"SIR;

"On the 14th of last month, when I had the honour of waiting upon you on board of H. M. S. Sybille, the number of patients in this hospital amounted to only seventeen, the greater part of whom were convalescent; however, it is with infinite regret I find it my duty to state for your information, that on the evening of that day, two petty officers were received from H. M. S. Iphigenia, labouring under fever, the symptoms of which were of so marked and unequivocal a nature, as to induce me to represent to Captain Parker the absolute necessity of his proceeding to sea, to avert, if possible, the extension of the disease, which seldom fails of communicating its baneful influence to the whole of any ship's company, where it has once displayed itself. Such was the promptitude with

which my suggestions were carried into effect, that in consequence of orders from you, the ship proceeded to sea the following morning, with a view, if possible, to avert the threatened danger. However, notwithstanding this precautionary measure, fourteen cases of fever occurred on board from the time of her sailing, until her return on the 19th, of which number four died.

"Immediately on her arrival thirteen cases of fever were sent to the hospital, three of them in the last stage of the disease, who only survived a short time after their being sent ashore. On the 21st, 22d, and 23d, only two patients were received, one on each of the two latter days. On the following days four patients came in, one of whom died. On the twelve following days, the number expressed opposite to their respective dates were received, and in the third column the cases which terminated fatally are also inserted.

January	26	6 received	4 deaths.
	27	14	5.
	28	12	0.
	29	4	1.
	30	8	4.
	31	12	3.
February	1	8	3.
	2	5	1.
	3	4	1.
	4	8	1.
	5	6	0.
	6	7	0.

making a total of one hundred and twenty-one received, of which number thirty cases, or the proportion of one in four, have terminated fatally. Notwithstanding every inquiry which I have been able to make, in endeavouring to ascertain the remote cause of this destructive malady, it still continues to elude my research, nor have other

medical gentlemen of great talent and experience been more successful in theirs.

"Indeed, Sir, it is difficult to account for the generation of disease in a ship so well regulated, and in such a state of high discipline, as the Iphigenia; and I am most positively and decidedly of opinion, that the disease, which has already committed such ravages on board that ship, is to be solely attributed to a particularly vitiated state of the atmosphere, the influence of which has been experienced along the whole of the American coast, from the northern bank of the Orinooko to Boston in New England, and in the adjacent islands.

"With respect to the local management on board the Iphigenia since the appearance of this disease, every precaution which human foresight, and experience of the past, could devise, has been had recourse to, to prevent, if possible, the extension of so insidious an enemy. Her hold has been cleared, and fires kept constantly burning therein; and I can with confidence assert, that her timbers are, at this moment, as dry as the head of her mainmast.

"I. MACNAMARA,

"Surgeon of the Naval Hospital."

The Rattlesnake, a new ship, was commissioned in December 1823, and arrived at Jamaica on the 29th of June 1824. On the 2d of July she sailed for St. Jago de Cuba with convoy, and returned to Port Royal on the 8th of the same month. From that date till the 17th of August she continued in harbour, the crew being employed for three weeks in clearing the holds, and in removing and replacing the various stores which they contained. This duty was performed carefully and thoroughly. The water tanks, provisions, &c., every thing, in fact, which the holds contained, were got on deck, and most of them sent to the dockyard, where they were cleaned, aired, and dried. The

holds themselves were then cleaned, the limber boards were taken up, the hatches kept off, stoves let down, which were kept burning for days, and entire ventilation kept up, by large windsails suspended over the holds, and reaching within two feet of the floor. When the process of purification was considered complete, I examined every part of the hold's surface, and found it in every part, from the hatches to the kelson, clear, clean, and dry, scarcely capable of soiling a white glove. Such perfect dryness was of course the effect of previous rubbing with dry cloths, &c.; for more or less of water generally, and of moisture always, will be found in the lower parts of the holds, excepting immediately after some such process of drying.

Matters being thus managed, the tanks and other furniture of the holds were replaced, and we thought ourselves secure from fever as far as the internal state of the ship was concerned. We had hitherto continued healthy, though the weather was hot, and oppressive to new comers, and the men were necessarily much exposed to the sun, and to fatigue. It should be stated, that, when the holds were cleared of their ordinary contents, there were found such extraneous matters as will generally be found in ships similarly circumstanced, *viz.* chips of wood, portions of mineral tar, and a small quantity of water, under the limber boards; a collection similar in kind, though by no means equal in quantity, to that found in the Pyramus. A fortnight after these matters were removed, and when in our case they could not therefore have any effect, whatever their power of inducing disease generally may be supposed to be; when the Rattlesnake was as clean interiorly and as clear of extraneous matters as any ship ever was or ever can be, the first case of fever made its appearance, in the person of George Taylor. The rate at which the disease proceeded afterwards, the numbers it

affected, the time it continued, the manner in which the ship was employed, and other circumstances connected with its progress, have been stated already, and need not be repeated here.

But when some of those cirsumstances are coupled with the account just given of the ship's state at the time when the fever appeared; when the whole are collocated, and for an instant fairly considered, it will be seen, that the usual methods of accounting for the disease cannot be applied here; because none of the methods, which have been assumed as operative in those methods, existed. And therefore, if no other cause can be discovered, we shall be under the necessity of contemplating and endeavouring to counteract a disease, regarding the origin of which we know nothing: we must look upon this sweeping pestilence as an effect without a cause, excepting such a cause as that *which smote the fourteen thousand Israelites in their tents*, the immediate and miraculous agency of offended Heaven.

I have stated facts and rendered reasons to prove, that the fever in the Rattlesnake was not contagious; and I am persuaded that the facts of the case will be sufficient to satisfy the most zealous advocates of the contagious nature of West Indian fever, that in this case it had no operation, either as a primary or secondary agent; that it neither existed at the beginning, as an essential component part of its nature, nor was developed during its progress, as a contingent property.

That the mere increase of atmospheric heat is incapable of occasioning West Indian fever, and could not have such effect here, will, I think, be admitted.

The principle or the products of vegetation can have little influence on the health of sailors generally, and could not produce the fever in question.

Neither marsh miasmata, nor terrestrial exhalations of any kind, could have propagated the disease; because no-

thing allied to the soils whence these exhalations arise existed in the Rattlesnake.

Finally, the disease could not depend on the effects of what has been vaguely called a foul state of the hold; because the holds and other internal parts of the ship were thoroughly clean.

The reports of Drs. Bancroft, Adolphus, and Macnamara, on the state of the Iphigenia, lead necessarily to the same negative conclusion, and thereby overturn all the doctrines which have been promulgated and entertained regarding the cause of West Indian fever on board ship, at least as far as the Iphigenia is concerned; and what is false doctrine in one case may be false doctrine in all similar cases, and probably is so when applied to this disease. In a fever possessing so much identity of operation, it is to be presumed that there is much sameness of cause; for though its less essential modes of expression are various, and often confused and contradictory, there belong to it radical and permanent features, marking it as a peculiar disease. The same cause may be so modified as to produce different, but not opposite effects; and when, in any disease, with much diversity of general aspect, we yet find that there are some fixed and prominent signs, by which it is distinguished from all others, we infer, that the cause, though acted upon by different, and perhaps conflicting circumstances, is still one and the same. Hence, when in a disease so peculiar as West Indian fever, it is asserted, that in one instance it arises from exposure to the sun, in another from a debauch, in another from marsh miasmata, in another from a peculiar animal poison, and so on till the whole volume of accidents is exhausted, and when it is maintained, that each or all of them have such power singly or collectively, things are affirmed, which, *prima facie*, are contradicted by the nature of things, and which all inquiry must tend to subvert.

The writers of the preceding reports, all of them respectable from their professional rank and attainments, one of them distinguished by great critical acuteness and research in tracing the cause of this disease, agree that the fever in the Iphigenia was not contagious; they state facts demonstrative of the non-existence of miasmata derived from marshy, or any other earthy or vegeto-animal matter; they cannot connect it with the influence of any land which she had visited; and nothing is concluded, except that no cause appears. One of them indeed ascribes the disease to a morbific condition of the atmosphere; and another makes some general remarks on the effects of atmospheric heat, seasoning, and the tendency of the malady to extend itself in dense bodies of population — modes of dealing with the difficulty which will scarcely satisfy any one. But Dr. Bancroft, whose labours in this field are so well known and so highly appreciated, who has collected so much information and exerted so much talent to prove, that this disease depends on earthy exhalations for its cause, as certainly as does intermittent fever, finds the whole of his elaborate and highly-finished structure sapped by the fever in the Iphigenia. He finds there a stumbling-block, which he had not expected and cannot remove, and he fairly confesses the insuperable nature of the difficulty.

There are then two instances of West Indian fever, prevailing as a malignant epidemic, those of the Iphigenia and Rattlesnake, in which the causes generally assigned for the disease did not exist, and in which the usual methods of accounting for it utterly fail. Regarding their applicability to other ships, I have not the means of forming a positive opinion; but I believe, that, were the facts of every case in which the disease has proved epidemic on board ship fully stated, they would be found equally to fail in them all. Reasons for this belief having been stated already, I will not reiterate or add to them here. How rigidly the general

orders for ensuring cleanliness and comfort on board ships of war are executed, and how perfect their effects are, every one knows who knows any thing of the economy of our naval service. Yet it is remarkable, and particularly calls for remark in this place, that, with all this systematic cleanliness, ships of war do not suffer in a lower proportion from West Indian fever than trading vessels, in which such measures cannot be so effectually employed. It is true, a correct comparative proportion cannot be easily obtained; but when it is considered that merchant ships are moored close to the shore, frequently in contact with the wharfs, and that the men are thereby exposed to an additional source of danger, it does not appear that they suffer more from the disease, as derived from an internal source, than the crews of men of war. But besides the general means of purification employed in ships of war, it is a general practice to submit them, soon after arriving in the West Indies, to a process of thorough cleaning, embracing their whole interior, similar to that which I have described in the Rattlesnake. It therefore appears fair to conclude, that, when West Indian fever prevails epidemically in King's ships, the usual methods of accounting for the disease cannot be admitted; and that, if extraneous matters be found in their holds, they cannot be allowed the power of generating, though they may be capable of aggravating it.

It being thus necessary to abandon the causes which have hitherto been assigned for the disease in such cases, I have been led to conclude, that it arises from the decomposition of the wooden materials of the ships themselves, and from such loose timber as they may contain, from the following considerations*.

Wood, in some circumstances, undergoes a rapid process

* To be considered, however, in connection with the peculiar atmospheric influence formerly alluded to as the endemic product of the West Indies.

of decomposition, whatever the nature of its products or their power of inducing disease may be.

Decomposition in the wood of ships' holds will be accelerated in the West Indies by high temperature, and the ordinary presence of moisture.

The wood forming the interior of ships' holds does undergo conspicuous change, in which it cannot be doubted, that some of its constituent principles suffer decomposition, and pass off in a gaseous form. This change is manifested chiefly by the wood becoming darker in colour. It appears likewise, when thoroughly dried, to have shrunk in size, and become denser in structure, at the same time losing weight. It has the appearance of being partly charred, and the effects produced on the wood, by this chemical process, are probably nearly allied to those occasioned in the usual method of charring; for besides similarity of appearance, there is also similarity in the conservative results. When the process in question is completed in ships, not only will epidemic fever from that cause cease, but the wood in the holds is so changed as to become the most durable in the ship.

The extent, however, to which the process is carried, and the nature of its results, are modified by the previous condition of the wood, the degrees of heat, and, probably, by interior arrangements in individual ships. When a ship recently built arrives at the hottest season of the year, continues for weeks in harbour, and when, as happened in the Rattlesnake, the holds are cleared, and high temperature kept up in them by means of stoves, the process will be rapid and complete. The fever will appear early, and proceed rapidly; but when it ceases, it will cease finally. Men fresh from Europe entered on board such a ship, after the fever has ceased, if they avoid the cause of disease on shore, will continue as secure from West Indian fever as if they served in any other part of the world. On the other hand, when a ship arrives during the cooler months, is kept much

at sea, and stoves are not let down into the holds, the process will often be slow and imperfect: in some cases it may never take place. In such ships there will not be severe and sweeping visitations of fever; but then there will never be security. Men will continue to fall under the influence of the disease, till the last day of the ship's term of service on the station. These things are frequently exemplified in different ships in the West Indies.

It appears that the process may be arrested before it is completed, by certain circumstances, and afterwards be re-excited by other circumstances. Of such circumstances, diminution and increase of atmospheric heat are probably the principal.

In the autumn of 1824, fever appeared in the Isis as an epidemic. About the end of October, when the disease was progressive, she was ordered to the gulf of Mexico, where, at that period, the north winds prevail, and the thermometer rarely rises above 75° of Fahrenheit, frequently falls to 66°, sometimes lower. The fever ceased, and the ship continued healthy, till the following autumn, when, at Port Royal, the fever returned, and prevailed extensively, with considerable loss. Then it is probable, that the chemical process in the holds, eliminating the cause of fever, was completed, that it could not therefore be re-excited, and that the ship will continue free from fever as regards internal causes. In this case it is pretty obvious, that the temperature of the gulf of Mexico was not sufficiently high for the continuance of the decomposing process which had been going on, and giving out the cause of fever; but the process was arrested without being completed; and when the ship lay in Port Royal harbour, in September 1825, the heat was then, being at 90°, capable of renewing it. Had the ship, when the fever first became epidemic in 1824, continued in harbour, or gone on a cruize in the neighbourhood of Jamaica, where the temperature was little lower than at Port Royal,

in either case the process would most probably have continued till it was complete, when the fever would have ceased. It would have been more widely diffused, and presumptively more fatal, than as circumstances determined it was in 1824: but, if this view of the matter be correct, it would not have returned epidemically, as it did in 1825. Whether the final result would have been more favourable in one case than in the other, and in which it would have preponderated, it is impossible to know, and therefore needless to inquire. When the fever re-appeared in 1825, it is not probable that it could have been arrested, within the limits of the West Indian command, because at that time, early in September, great atmospheric heat pervaded the whole; two months later, its progress might again have been stayed in the gulf of Mexico, because, the north winds having set in, the temperature would have fallen so low as to be incompatible with the existence of the decomposing process on which it depended.

Ample evidence exists of the total insufficiency of merely sending a ship to sea, to stop the epidemic progress of fever. Out of a great number, with which this measure has been adopted without effect, it is sufficient to specify the Iphigenia, Scout, Bustard, and Rattlesnake. In none of them had the disease made much progress when ordered to sea: in all of them it extended rapidly while at sea. In the case of the Scout and of the Rattlesnake, no benefit was likely to arise from such a change of place, because it happened to one in June and to the other in August; the Bustard's fever likewise appeared, I believe, during the summer months, and could not be expected to be annihilated by a cruize in the neighbourhood of Jamaica. But in the Iphigenia's case, the disease breaking out in January, there is every reason to believe, that a cruize in the gulf of Mexico, instead of the Caribbean sea, would have put a stop to it for the time.

After what has been stated, it is scarcely necessary to observe, that the decomposing process, to the product of which I ascribe ship West Indian fever, is altogether different from that which is generally known by the name of dry rot. In the former, chemical action only changes the ligneous structure, without injuring the ligneous fibre, by the dissipation probably of its more volatilable parts; in the latter, the change is not of modification of parts, but destruction of fabric.

Whether mineral tar exert any influence in precipitating and perfecting this process, and if it does, the manner and extent of its influence, deserve consideration. It does not appear to increase the effect.

With the information we at present possess, it were idle, if not presumptuous, to assume any thing regarding the nature of the resulting product furnished by the process in question. In the places where it is evolved, there may be found carbonic acid in greater quantity than exists in the ordinary conditions of the atmosphere. There may likewise be found hydrogen, carburetted hydrogen, and other gaseous matters alien to the air we generally breathe; but when we have found these things, what knowledge have we obtained of the cause of West Indian fever? It is clear that they, of themselves, do not constitute the cause, seeing they have place abundantly in England and elsewhere, in which the disease is not known. Besides, the effects of those gases on the animal frame have little analogy to West Indian fever. They induce faintness, vertigo, torpor, or they suddenly extinguish life; but though some of these effects bear some similitude to what we observe on the accession of congestive fever, they must be different, both in cause and manner of impression, from the total difference of their results. When a person suffering from the effects of those gases is removed into pure air, he generally dies suddenly, or recovers without exhibiting any

of the signs which characterize fever. When the febrile mode of action is completely established, it is extremely doubtful whether the patient recovers faster by being removed from the place in which its cause continues in force; at least we see people recover rapidly in its very centre, as happens every day in ships and camps.

Many words may be employed, and words which apparently have much meaning, to designate the nature of the agent which occasions West Indian fever; but we shall be little the wiser for them. It is too subtle for the cognizance of sense, by means of mechanical experiment, till science shall have become more perfect than it is at present; and therefore all discussion regarding it must be an accumulation of vain terms and useless conjectures. But though ignorant of its essence or composition, we can scarcely, in a ship where it prevails, continue in ignorance of the source whence it springs, and the substance by which it is supplied; for besides the facts already stated, every thing connected with the rise and progress of the disease, conduces to show, that it is a product of the internal parts of the ships themselves, and that it is derived neither from the air, the earth, nor the pollution of extraneous matters within ships.

It is always at the beginning confined to a small space. It often continues for a while in one birth, whence it sometimes crosses to the opposite birth; sometimes it travels along one side, returning pretty regularly by the other; and sometimes it traverses the ship from the rear to the forepart, or in a contrary direction. But in a majority of instances it begins in the vicinity of the pumps and main hatchway, where the shell of the ship is most dependent, where water draining from other parts collects, and where heat is most intense. On either side of the pumps and main hatchway are the births of the midshipmen and marines, in frigates; and in those situations the disease

almost invariably first appears, and commits the greatest ravages. Its after progress appears connected with the manner in which the ship is trimmed, or with the inclination of the keel from a horizontal position. When a ship is so trimmed that the fore part becomes the most dependent, it appears that the disease extends in that direction, and that it takes the opposite direction when the stern part is the lowest. In one case the gun-room officers will suffer severely, in another they will entirely escape, when the general effects of the disease in two ships are nearly equal.

As men fall under the influence of fever they are removed from their births, on the lower deck, to cots or hammocks, on the main deck, which becomes the hospital of the ship for the time; yet the origin of the disease does not thereby become diffused. It continues local and circumscribed, and is extended only as the cause beneath is generated, more slowly or rapidly, in different instances. Thus, a man is removed from his birth to the main deck labouring under fever one day, another is removed the day after; one, two, or three the next day, and so on; the disease beginning then to extend itself, in one or more directions, goes on till it pervades the ship generally. Yet all the while it cannot be traced from those on the main deck to any other part of the ship. This gradual and regular extension of the disease will not be equally conspicuous in all cases, from the operation of causes which I shall not stop to enumerate, but if it happen in one case, it is sufficient to show its local and strictly limited origin. It is moreover generally observed, that, when men sleep and pass most of their time on the main deck, they are seized later and more lightly than those who sleep and live chiefly below.

Now the application and importance of these phenomena to my argument, the manner in which the disease

arises and advances in ships, will at once appear. They can leave little doubt that the cause is internal; that in ships perfectly clean, like the Rattlesnake and Iphigenia, the internal cause cannot be the accumulation of extraneous matters, or filth; and that, as happened in the Rattlesnake and many others, it could not be derived from the land, because the disease was propagated many weeks at sea. They add force to the objections, which have been stated to the contagious power of the disease; and in every view therefore they strengthen, if they do not confirm, the opinion here advanced. But the account being given in general terms, and lest that should deprive those phenomena of the weight which belongs to them, I shall refer to them in a case which was under my constant observation, and was to me full of interest.

In the Rattlesnake the fever was first manifested near the main hatchway; the marines, and the midshipmen of one birth, suffered its earliest and most severe effects. Afterwards it proceeded forward rapidly, but pretty regularly, till it had affected almost the entire ship's company; but it did not go beyond the steerage in the opposite direction, no one being attacked in the gun-room, except the purser, and I have good reason to conclude, that in his case it was derived from the shore. In my own case, which preceded the epidemic appearance of the disease some weeks, I am satisfied that it also arose from exposure to the cause on shore; and it is with the disease as an epidemic only, that I am at present concerned. These cases therefore form no objection to what has been said of the gun-room officers being exempted from the cause of fever, or at least not falling under its influence, when prevailing as an epidemic indigenous disease.

But its local origin, and limited range of action, were most strikingly exemplified in the births of the midshipmen,

and other officers of that class. They were placed exactly opposite to each other, with the pumps at equal distances between them. Only one gentleman was affected in the starboard birth, while every member of the larboard birth was laid up nearly at the same time. The hatch of the pump well is opposite to, and within three feet of the larboard birth. The members of that birth were generally the junior officers of their class, and were therefore, it may be said, most susceptible of the disease; but when it is remembered how extensively it prevailed, and how indiscriminately it attacked persons of all ages and temperaments, among the ship's company, after every allowance is made for the greater susceptibility of those young gentlemen, the exemption of the others, senior only by a few years, is too striking and complete to be accounted for by their former service, or by any accidents which can reasonably be supposed to have affected them. It can fairly be attributed only to their not having been exposed to the cause of the disease, with the same concentration of power, or permanence of operation, as the others; and here the contiguity of the pump well hatch to the larboard birth cannot be overlooked. Besides, two of the gentlemen in the larboard birth were not what are called youngsters in sea phrase, but were, as to age, on a parity with those of the opposite birth; yet they had the fever in common with their junior messmates. One of them had just returned from the East Indies when he joined the Rattlesnake, and his case furnished an instance, among many others in my own observation, that residence in other tropical regions affords little or no security against the fever of the West Indies.

It deserves remark, that the Rattlesnake was much *by the head*, that is, the keel declined less from the horizontal towards the stern than is generally the case.

With a few notices of one or two other ships, in further

support of the opinion advanced, I shall conclude the arguments, as far as they are founded on facts derived from those quarters.

In the Isis the fever made its first epidemic appearance, as to place, as it did in the Rattlesnake. It was confined, with scarcely any exception, to the midshipmen and marines, attacking more especially the former. Towards the end of September, the ship was ordered on a cruize to windward of Jamaica, where she continued about ten days, during which time the disease proceeded, but was still confined to the midshipmen and marines. After lying some time at Port Royal, the fever extending, but not rapidly, she sailed, near the close of October, for the gulf of Mexico, as has been stated already, when, as has likewise been stated, the fever ceased. During the whole period, the subjects of attack were confined to the close vicinity of the pump well. The cause appeard to rise solely and directly from the centre of the ship, neither reaching the gun-room officers in one direction, nor the ship's company generally in the other. But when it reappeared in 1825, though it commenced similarly, it pervaded the ship generally, and scarcely any of the gun-room officers escaped.

The Pylades arrived at Port Royal from England in June 1825, and soon after fell under the epidemic influence of West Indian fever. In August it was in general operation, and by the end of September had affected the majority of the ship's company, and all the officers except the captain. But in this ship (and I refer to her only on that account), it began among the officers, the most of whom it attacked, before it became general among the crew.

In the Ferret and Scylla, it commenced and extended, as far as I have been able to ascertain, in the same way; while in the Lively its rise and progress, as to place in the ship, resembled what happened in the Isis and Rattlesnake.

And from my information it appears generally, that, in frigates and ships of that construction, the cause of fever is first extricated in the central parts, whence it is diffused in different directions; while in brigs, and vessels of brig form, it more commonly begins to act further aft, whence it afterwards proceeds towards the fore part; but my information is too limited to warrant a positive conclusion.

The evidence hitherto adduced I consider irresistible in its negative import. It proves beyond doubt, that in two ships, none of the causes assigned for West Indian fever could have operation, while the ships' crews were suffering severely from that disease. It affords reason to question their application to ships generally; and it points certainly I think, if not clearly, to the cause in such cases existing. But in whatever light it may be viewed, whatever inference may be drawn from the facts from which it is derived, it shows, that the general doctrines regarding the cause of this disease, if not entirely erroneous, are not capable of universal application; that they fail in some cases, and are probably false or defective in all.

Convinced as I am of the ligneous origin of the disease on board ship, and believing also that the same source exists on shore, and has general operation, I shall state briefly the reasons of that belief. Some of the localities to be considered on this account, have been referred to more than once already, with different views; and I am aware, that repeated reference to the same places and things, on the same subject, must have the appearance of clumsiness and tautology. But they are important to my purpose, and I am not inclined to sacrifice, for appearance sake, any thing of value in this inquiry. By drawing inferences from more extended and distant, but less certain sources, I might avoid such unseemliness; I choose however to trust to things which I know, though limited, rather than to things of which I have only heard; and will therefore

submit to the imputation of having drawn conclusions from narrow grounds, and of having made wearisome applications to them.

Port Royal stands on the extreme point of a long irregular projection, extending about nine miles from the main land of Jamaica. Its direction is nearly south-west, but it winds considerably in some places, and it has many jutting points and inlets on its interior or north side; its breadth varying from 200 feet to nearly half a mile. Its structure is coralline, covered to various depths, in different places, with dry sand, having masses of madrepore scattered on its surface. This account of surface applies to the extreme point and the south side of the promontory; not a blade of grass is to be found there, scarcely a weed, or any vegetable production, except a few cocoa-nut trees, and ornamental shrubs. But about a mile north and by east of the extreme point of the promontory, and where it is broadest, there is an extensive mangrove marsh, on the northern shore. The mangroves, at their western limit, and where they look towards the extreme point, run out in a northerly direction about a quarter of a mile, and, with the extreme point on which Port Royal stands, form a small bay, in which the ships of war lie. The mangroves extend irregularly towards Kingston, and cover many acres.

But when I call those mangroves, and the place on which they grow, a mangrove marsh, I do so in compliance with common phraseology, not to express the nature and composition of a marsh, as generally understood; for they scarcely, if at all, resemble such soil. The mangroves grow in sand, either covered to some depth, or completely wet with salt water. Where they terminate seawards, they are immersed some way up the stems, and there they are most vigorous, and acquire the largest size; as they recede towards the dry sand, they diminish in bulk, and they soon die when the water fails. They occupy the

surface on which they grow, scarcely any other plant existing over the whole. In these respects they have no resemblance to marsh-land, neither is there the kind of evaporation which takes place there, *viz.* slow, gradual, and partial, or complete, according to the power of the sun and the quantity of rain, leaving the surface perfectly or imperfectly dry, in dependence upon those circumstances. Here the mangrove bush alone presents itself, with its roots in the sand, and its stem in the salt water. There is nothing like a marsh, in the common sense of the word, nothing by which the exhalations of marsh can be produced. But there is much in support of the ligneous source of West Indian fever.

The mangrove grows and runs to decay rapidly. The branches, after rising a few feet, bend towards the root, on which they engraft themselves; from the bow thus formed other branches spring up, which, in like manner, insert their extremities into different parts of that from which they grow, and so on, till an impenetrable dwarf forest is spread over the surface of the water. While the lower part of this *water forest* is undergoing rapid decomposition, the upper part is in a state of luxuriant growth and beautiful verdure, so that the appearance of the whole is singular and striking: it looks like a piece of basket-work supporting a shrubbery. From its structure and habits, the mangrove appears destined to speedy decay, to which surrounding circumstances are highly favourable. In consequence of the rise and fall of the tides, part of it is alternately wet and dry, and the whole is exposed to the influence of high temperature; hence, whatever deleterious product is furnished by decomposing wood, must be furnished abundantly here.

But I ought to notice the burial ground, in the rear of these mangroves, to which some are disposed to trace the cause of West Indian fever at Port Royal, though this is

more a popular than a medical opinion in the present day. Directly to windward of the mangroves, near the south side of the promontory, where the sand is a little elevated and perfectly dry, almost every one who dies at Port Royal, either ashore or on board ship, is buried, and many bodies are therefore deposited in this place annually. Dr. Bancroft has adduced so much direct evidence, and so many convincing arguments on this subject, that I think it scarcely necessary to enter on its consideration. I shall therefore only state the fact, that, in the West Indies, the disease in question is not more prevalent in the neighbourhood of burial grounds than elsewhere. Besides, if decomposing animal matter gave out the cause of West Indian fever, it is difficult to conceive how or when it should be arrested; as this material is never deficient, and the temperature is at all times so high as to occasion rapid putrefaction of its parts. When an epidemic ceases, it might be presumed that the bodies of its victims were capable of furnishing its cause, and perpetuating its operation; because they continue in a decomposing state around, in all stages of the process, from its commencement to its close; from the moment that life is extinguished, till the most enduring particles of the organized fabric are scattered in the air, or mingled with the dust. To what then shall we attribute West Indian fever at Port Royal*?

* At Vera Cruz, where from the difficulty, nay impossibility of accounting for the prevalence of West Indian fever, according to the generally received hypotheses, it has been assumed by some, that decomposing animal matters, particularly the refuse of the shambles in the city and its environs, are the cause of the disease. But when it is known that animal food is scarce, and very expensive, it will not be thought likely that such refuse will be very abundant; it is not in fact abundant, and is speedily devoured by the vultures, which swarm in the very centre of the city, which are not allowed to be molested, and in consequence meet you at every turn, and will scarcely move out of your way. The inhabitants think the removal

If we reject the products of animal decomposition, and I think they must be rejected, nothing remains to which we can reasonably make reference, except the mangroves. On examining the promontory, nothing else is found but a bed of pure sand, and from such soil, I may safely say, in accordance with received opinions, that fever cannot arise; and it therefore becomes a necessary inference, that the mangroves furnish the more palpable cause of the disease.

It has been stated, that, from the position of the promontory, and the direction of the winds, the latter blowing almost directly across the former, the deleterious influence of the mangroves might be supposed to be carried clear of the town and anchorage, lying as they do to the south-west of those mangroves. But between the subsidence of the sea breeze and the springing up of the land wind, and *vice versa* in the morning, there is an interval of calm, varying in duration at different times. During the inter-

of such matters a thing of importance, and therefore hold the vultures, which devour them instantly, sacred birds, there being a very considerable penalty imposed on any person attempting to destroy them. In these circumstances, it will not be supposed that the decomposition of animal substances goes on in any extraordinary degree at Vera Cruz; it does not proceed to near the extent which it does at Rio Janeiro, and other parts of Brazil; in the latter place the disease in question is not known; it is peculiarly destructive in the former.

The locality of Vera Cruz might have been cited in disproof of the marsh miasmal origin of West Indian fever. About a mile inland, there is a small lake, or rather stream, formed into a lake by artificial obstruction, with clear, grassy margins, and presenting nothing of a marshy appearance. With that exception, the country to a considerable, I believe a great distance, is dry in the strictest sense of the word. The plain, where the city stands, is considerable, exhibiting nothing but dry shining sand, and sickly dwarf trees; the structure beneath being an extensive bed of coralline formation, from blocks of which the city is chiefly built.

val, I presume, that the more palpable cause of fever, ascending from the mangroves as a centre, is precipitated to some distance around, frequently including the town and anchorage of Port Royal in its circumference. The health of those places appears to depend much, at all times, on the strength and regularity of the breezes; for it is notorious, that, when they fail, or become very languid, fever is frequent and fatal, in an extraordinary degree.

Between the dockyard and the mangroves, a number of spars for masts, &c., have been long buried, on the northern margin of the promontory, close to the sea, for the purpose of giving them durability, a purpose which I understand this method of disposing of them has answered. But the disinterment of these spars is attended with great danger: so many cases of fever occurred among the parties employed on that service, that an order was at one time issued to have it performed by slaves.

A few years ago, a party from his Majesty's ship Euryalus was employed in raising one of the spars; they had heard so much of danger, that they kept to windward of the log during their labour, believing that some human bones which they turned up were pregnant with mischief. One of them, however, a midshipman, thinking that such precautions were tokens of unmanly fear, sat down to leeward of the trench, laughed at the apprehensions of the others, and taunted them with the imputation of weakness unworthy of sailors. It was remarkable, that, of the whole party, that gentleman alone was attacked with West Indian fever next day, of which he died soon after, and that at a time when the disease was not in the ship or in the harbour. It is a general opinion, that the danger incurred in this service arises from the labour with which it is attended; but it will not be denied, that labour as severe in other places does not subject the labourer to the same peril; and in the instance to which I have just alluded,

the person attacked was the only one of the party who had not been working; while he was, from length of residence, &c. presumptively not more susceptible of the disease than the others. That the disinterment of those logs should be eminently hazardous, being likely to let loose a concentrated cause of fever, might be expected, in accordance with the testimony I have adduced regarding the source of the disease on board ship; and being thus hazardous, it is in proof of the ligneous origin of the disease generally.

I refer to the topographical account of Stoney Hill, and the history of fever there, in support of my position. A limestone structure covered by forest trees alone meets the eye, and directly leads to the conclusion, that the more palpable cause of the disease is furnished by the latter, whatever influence may be exerted by the former.

In further support of the position, it may be observed, generally, that the topographical sketch of Port Royal will apply, in one material point, to most of the harbours and great part of the coasts in the West Indies. The material point of agreement to which I allude, is the abundance of mangroves over the whole; and I believe, *ceteris paribus*, that where they most abound, West Indian fever is most general and most destructive.

In North America, there has been, and continues to be, much controversy about the causes of West Indian fever; and with difference of opinion regarding its etiology, there has been connected, in too many cases, contrariety of statement, discreditable to the parties at issue, and injurious to the cause they would serve. In this respect, there is remarkable difference between the state of the professional mind there, and in the West Indies. In the former place, the dispute respecting the contagious or non-contagious origin of the disease is as hot and hopeless as ever; while in the latter, there is no medical man of any

standing, who believes that it is either produced or propagated by contagion. Some practitioners, on their first arriving from Europe, bring with them apprehensions about the operation of contagion, and the danger of unrestricted intercourse; but as they mingle with their fellow practitioners, and become familiar with the disease; as they watch it in its rise, progress, and decline, and are fairly impressed by the appearances, as they present themselves, the doctrines of the schools, and the fears which they engendered, lose their influence together, till, in a year or two, they have no more dread of themselves or of others deriving fever from their fever patients, than from the same number of cases of ulcer or pneumonia.

But amid all the discussions and contradictory statements regarding the disease in North America, one thing, from almost every statement, is obvious. It has always arisen about the docks, wharfs, and jetties, of seaport towns; and on this circumstance has been founded one of the most peremptory and constant arguments of its being a contagious disease. The origin of the disease, however, in those situations, admits, in my opinion, of a very different explanation, an explanation which is at once more consistent with the phenomena, and more conformable to truth.

Those wharfs, &c., are wooden structures, and their materials are therefore undergoing decomposition. The rise and fall of the tides, and great heat, will hasten the process; and as those structures fail and are repaired, timber in various conditions of greenness and dryness, health and decay, will be employed in their composition; hence gaseous products, capable of inducing fever, will be disengaged. The materials are always in existence, ready to be acted on by high temperature, and other agents perhaps of which we are ignorant.

The endeavours to trace the disease to ships arriving

from the West Indies have notoriously failed, and must fail when considered as cause and effect. It is true, that the first cases of an epidemic may appear in a ship at Charleston, or New York, and that ship may have arrived lately from the West Indies. But from the facts already given to prove the origin and extension of the disease on board ship, independent of any external cause, the spontaneous evolution of its cause in a ship lying at Charleston may be expected; and therefore, in such a case, no proof of importation can be deduced from its appearance; the internal cause, which occasions the disease at Port Royal or Vera Cruz, being capable, it is reasonable to suppose, of occasioning the same disease at Charleston or New York. As might be expected, it sometimes occurs in residences close to the wharfs, before it is observed on board the vessels in harbour, and it is sometimes difficult to determine whether it appeared first on shore or in ships in harbour. But whether it appear first in one place or in the other, or whether it appear in both simultaneously, it is excited by a cause operating where it does appear, and is as independent in one place of the cause acting in another, as is the ague in Kent of the cause which produces ague in Lincolnshire.

Moreover, it may be said of the ligneous origin of the disease, that its material is uniform and constant, which cannot be afirmed of any other agent, to which the disease has been ascribed; for wherever West Indian fever has appeared, wood in some condition has existed, either in its natural soil, or in the structure of ships, wharfs, houses, or otherwise; and, therefore, if to the ordinary causes of decomposition, there be added the peculiar influence, known only by its effects, which is here assumed to be essential, in some way, to the production of the disease, the disease may be expected, as the source of its more palpable cause will not be defective. I am aware, that to fix upon any

concomitant of a disease, however constant, as a cause, without showing its operation, would be absurd as well as arbitrary; but if, in connection with the proofs already adduced, showing that, in some instances, West Indian fever is occasioned by the invisible products of wood alone, it appear, that wood, and nothing else, constantly coexists with the disease, such constancy of coexistence is at least presumptive evidence of its operation.

According to the view here given, some difficulties connected with the rise and progress of the disease may be explained in a more satisfactory manner, than according to any other etiological account with which I am acquainted.

There can be little doubt, that residence in the West Indies and on the adjacent coasts was much less fatal to the first settlers than to those who some time afterwards succeeded them; for though cases of fever might often happen, without attracting much notice, or being recorded at the time, it is incredible that such an epidemic, as that which desolated Barbadoes in 1647, should have passed among so many keenly interested observers, without a historian of some kind. How shall we account for the comparative immunity enjoyed by the first colonists? All the indigenous agents, to which the disease has been ascribed, existed in their time, as well as in the time of their followers. Marsh and other terrestrial exhalations, the products of decomposing vegetables, atmospheric heat, and so forth, must have had place and influence, whatever their influence be, then as well as now. Contagion, if proved to exist, would at any time account for its general prevalence, if no case had ever appeared before its introduction; but it is especially incumbent on those, who would trace the first epidemic visitations of West Indian fever to a foreign source, to show whence and how it was imported to the West Indies. The story of its being

carried from Siam is too fabulous to make any impression at present, though strictly and dispassionately considered in itself, it is perhaps as consistent with appearances, and as much deserving of credit, as others of more recent invention, and more general influence.

The first labour of the colonists was necessarily directed to clearing the soil which they were afterwards to cultivate, to cutting down and uprooting the trees and brushwood, with which the surface was almost everywhere covered. This work was accomplished with much toil, and consequently by slow degrees, when the number of labourers was small, unaccustomed to tropical heat, and beset and dispirited by difficulties which they had not contemplated. Dead ligneous matter would thus be slowly and gradually accumulated on the surface, whence the gaseous products of decomposition would be extricated in every stage of the process, and of every kind; and to those products, in a principal degree, I would ascribe the earlier and more virulent epidemic fevers of the West Indies.

It is often observed, that the disease extends in an unaccountable and apparently capricious manner. Becoming manifest at one or more points, it leaps, as it were, thence to other points of population, without any trace being perceived by which it proceeded; it sometimes adheres to one corner of a town, or attacks one line of building, leaving one that is opposite untouched; nay it sometimes affects the inmates of a barrack on one side of the building, while those on the opposite side do not suffer from it at all. Now, in those cases, especially the last, the usual methods of accounting for the disease are obviously inapplicable. Volumes of argument could not more perfectly demonstrate its local nature, and limited range of action. It does not pass from one side of a building to the other, separated only a few feet. Whence then can it come, but from the building itself? and from

what part of the building, but from its wooden materials, subject as those materials are to decomposition?

When, as it sometimes happens, West Indian fever attacks the members of one family, living in one house, without appearing on either side; or when it occurs in epidemic form in a barrack, where no cause can be detected around, as has happened at Brimstone Hill, St. Kitt's, I submit, after an impartial consideration of the facts and arguments formerly stated, whether we should not, in such cases, seek for the cause of the disease in the decomposing materials of the building. It would surely be more reasonable, and more likely to end in satisfactory results, than to ascribe the whole to the power of contagion, without showing whence the contagion came, and how it reached the seat of the disease's action.

Much more of this indirect, though not ineffective evidence, might be adduced without exhausting the subject; but I have written as much regarding the cause of West Indian fever as I intended, more perhaps than may meet with patient perusal. There is enough, I hope, to excite attention and encourage inquiry; at least there is enough, if the ground which I have taken be tenable, and the inferences I have drawn be correct. I shall therefore conclude this paper, by repeating summarily the principal proofs which have occurred to me, of the ligneous origin of the disease, in the following terms.

In the accounts of the disease on ship-board, we find two instances of its epidemic prevalence, in which it is physically impossible to connect its existence with any other cause; this is the *argument cumulative:* and we have reason to infer, that the same cause has general operation in ships in which the disease prevails epidemically, nothing being wanted to establish such general operation, but to trace the disease accurately, and to con-

sider its progress impartially, with reference to the phenomena, as they are developed in each case.

The consideration of the soil, productions, and other local circumstances of Port Royal; the consideration of the same things at Stoney Hill; and the general aspect of the harbours and shores of the West Indies, all lead directly and forcibly to the same conclusion.

Amid much dispute and contradiction, regarding the origin and extension of the disease in the United States, it appears, in all cases, to have made its first appearance in, and to have adhered pretty closely to the vicinity of wharfs, arsenals, and other wooden structures of seaport towns: such structures are subject to decomposition, the products of which are capable, in some seasons, of occasioning the disease in question.

Of the various agents, or modes of agency, which have, from time to time, been specified as causes of the disease, none is capable of universal application; cases occur in which any and all of them fail. This objection, which it appears so necessary to obviate, does not apply to the cause under consideration; for wherever the disease is found, there ligneous matter in some form and condition will likewise be found, at least as a constant concomitant.

Finally, this view of the subject appears to furnish a more easy and satisfactory solution of certain difficulties in the history and progress of West Indian fever, than any other which has come to my knowledge.

MEMOIR IV.

The Question, whether West Indian Fever is a peculiar or common Disease, considered.

It is disputed, among many other things regarding the origin and constitution of this disease, whether it is peculiar and permanent in its essence, or whether it is merely a modified and aggravated form of fevers which prevail in every situation where marshes abound, and which have a regular type, marked by periodical returns of intermission or remission.

The writers who have maintained, that the disease in question is contagious and imported, have asserted, that it is a fever *sui generis*, being as peculiar and immutable, in its nature and essence, as small pox or syphilis. This conclusion indeed necessarily arises from the belief of its foreign extraction and contagious power; because, without proof, it is inconceivable that a fever, arising from common causes, should be transported many thousands of miles, either through the medium of fomites or of the human body, and should acquire in a distant part of the world the power of propagating itself, a power which it did not possess in its native soil. This doctrine would be strange and doubtful, if it required us to believe, that the acquired contagion was feeble and limited; that it could not extend far beyond the spot where it was generated, and therefore was soon dissipated and destroyed, in a thin population, and under the influence of ventilation and cleanliness, so necessary and general in the West Indies; but that a fever

arising from an indigenous cause on the coast of Africa, or from any thing in a ship during her passage thence, or elsewhere, should not only acquire but retain contagious properties; that, after being carried into a West Indian port, it should obtain peculiar qualities, and start from that point afresh, endowed with new powers of destruction, and a permanent quality of contagion, by which it might diffuse itself, not only over an island, but over half the world, is incomprehensible and incredible, without such evidence as shall force conviction.

Those writers, on the other hand, who have maintained that West Indian fever is an endemic disease and void of contagion, have generally, I believe universally, argued, that it is in no way different from periodical fevers of regular type, excepting in severity of symptoms and rapidity of course. They regard it merely as an aggravation of remittent fever, as that is considered an aggravation of intermittent; and they affirm, that to produce the disease generally called yellow fever, and which is here denominated West Indian fever, it is only necessary that there be present the soil from which intermittent fever arises, atmospheric heat of certain elevation, and generally a particular kind of susceptibility in the subject. They view intermittent, remittent, and West Indian fever, as essentially the same, both in cause and constitution, being merely varieties of one disease, and different only inasmuch as they differ in violence respectively; and hence the appellation of bilious remittent is often applied to the last.

The consideration of some circumstances in the history of this disease, and of certain facts regarding its origin and nature, has led me to dissent from the integral doctrines of both parties. I have been led to believe that the disease is, in the strictest sense of the word, endemic; that it never has been carried into, or from, the West Indies, with the power of self-propagation; that wherever it appears, it

arises directly and solely from inanimate matter close to the persons whom it affects, and is as incapable of being produced by any other agency as ague or goitre. But while I believe it to be thus local and limited in origin and operation, I cannot subscribe to the doctrine, that it is a mere aggravation of intermittent fever. The character of the disease, and the places and circumstances in which it prevails, will not permit me to draw such conclusion. In the whole range of febrile diseases there are many common symptoms, and it is often difficult to determine the order to which individual cases belong at their commencement, excepting in such as are attended by particular eruptions, and even in them till such eruption appear. This difficulty will sometimes occur in West Indian fever, especially at the beginning of sporadic cases. Still the disease exhibits distinguishing signs, which are sufficiently striking, constant, and peculiar, to mark it as a specific disease. It possesses as much peculiarity of character as plague or typhus. Whether the cause of intermittent and West Indian fever is essentially the same or radically different, I shall not inquire. They may have something in common, as it is probable there are certain points of affinity between the causes of all endemic fever. But if the radicle of the efficient be the same, it must be so modified and changed, as to acquire the power of producing widely different effects. We do not know in what respects the causes of plague and cholera differ or agree, as to their elements; but we know, from the difference of effect, that their constitution must differ, and we classify and consider them accordingly. Similar difference exists between West Indian fever and the disease which has been called bilious remittent, marsh remittent, or jungle fever, which is supposed to arise from the same cause as intermittent fever, and to differ from it only in degree of concentration. I have had occasion, in another place, to state some arguments bearing upon this

point, and proving the existence of such difference; but as those arguments were in a great measure extrinsical, independent of the nature of the disease, and therefore unfit for practical purposes, being deduced chiefly from the places and circumstances in which West Indian fever prevails, I shall notice, at present, such phenomena in the disease itself, as will be sufficient, I think, to establish its peculiar, specific, character, and thereby to distinguish it from remittent fevers.

1st. *It does not remit.* Much has been written to prove that it does, and when I offer evidence to the contrary, founded on limited experience, and put forth without the authority of a name, I know that it is likely to be treated with neglect or incredulity. But I hold myself bound to state fully and frankly the result of careful, if not extensive, examination. From the tenor of my reading and reflection previous to seeing the disease, I believed it to be an aggravated form of periodical fever, and therefore expected to find it assume a remittent type. When it first fell under my observation, I looked anxiously for remissions, but I did not find them. Remarkable changes were seen generally in the course of the disease, which I was at first disposed to consider remissions and exacerbations; but on more careful consideration it was obvious, that they did not possess the character or deserve the name.

The most remarkable of those *changes* occurs towards the close of the disease. More especially in its inflammatory form, and generally on the third or fourth day, there is a great mitigation, or rather cessation, of the more violent symptoms; the pulse falls suddenly, the skin becomes cool, the tongue clear, and the mind serene. These are the signs of critical movements, which lead rapidly to death or restoration, generally to the former; for such a precipitous fall from violence to quiescence is seldom the index or forerunner of salutary change. In the former event

(and appearances are often so promising and fallacious that it is impossible to foretel the event), this cessation of violence has been called, by a manifest perversion of terms, the fatal remission. No increased action follows, and in a few hours the patient either dies, or is free from danger. It is in truth a solution of the morbid catenation, whether fatal or salutary, and no more deserves the name of remission, than the deadly repose which accompanies mortification of the intestines, or the sudden and entire cessation of inflammation, which sometimes follows the hasty and copious subduction of blood.

Another pretty remarkable and constant *change* occurs within the first twenty-four hours of the disease's course. After the first alarm and tumult of invasion have passed, the patient will often express himself as being better, though the external signs by which we estimate the force of the disease be not abated; little difference in that respect appears till from twelve to twenty-four hours after the attack, when the symptoms become exacerbated. I mean simply, that there is increase of violence at the period in question, not that there has been precedent remission, which the term exacerbation might seem to imply. But besides the absence of precedent remission, the exacerbation does not occur with the suddenness of a paroxysm; it comes on slowly and gradually, as happens in ordinary cases of disease becoming aggravated. From this period till the appearance of crisis, either in the manner noticed above, or otherwise, there may be occasional rising and falling of symptoms; but there is no alteration, which can be specified as regular either in time or character.

Now it appears to me, that these *changes* have been set down and quoted as remissions; and that, in this way, a plentiful source of error has been introduced into discussions regarding the character of the disease.

Another source of error appears to have risen from the

simultaneous prevalence of remittent and West Indian fever. This has indeed been employed speciously, but not justly, as an argument of their identity. It would be as logical to argue, that cholera and remittent fever, or ague and scarlatina, are the same disease, because they are found in the same places at the same time. The difference between those diseases is more striking; it is not more certain, I believe, than between remittent and West Indian fever. Yet as the last frequently prevail together, as remittent is, in many cases, as well as West Indian fever, a violent and suddenly mortal disease, and as they have many common symptoms, there can be little doubt that they have often been confounded; and as distinct remissions occurred in one case, while it was assumed that both cases were identical, a feature has been ascribed to West Indian fever which does not belong to it. Thus a fever, which is as strictly continued as any in the long list of febrile diseases, has been declared remittent. This source of confusion and error will constantly present itself in camps, and other masses of population, in marshy situations; it will scarcely be found in ships, and there the nature of the disease may be investigated in the most satisfactory manner, and its peculiarities most easily and certainly determined. There, we are not perplexed with the intercurrence of disease in many respects similar, which it requires some pains to separate: when it prevails epidemically we find it alone, or singularly uncumbered with other forms of disease. One hundred and seventeen cases occurred in the Rattlesnake, within a few weeks, and, though they varied much in violence, and in many other points, they were uniformly continued, nothing like remission could be detected in any of them, unless the *changes* of which I have taken notice shall be called by that name.

But in answer to the argument, that this cannot be the same disease as remittent fever, because it does not remit,

it has been said, by one of the latest and best writers on the subject*; "that for any disease to observe regular laws, it is necessary that the vital organs principally affected should continue in a certain degree of integrity; that their functions should only be disturbed and perverted to a given point; that they should still be discernible as functions, and not be utterly overwhelmed and extinguished, by the violent cerebral action, and speedy gangrene of the stomach, that take place in aggravated yellow fever." Now, to render this answer satisfactory or valid, it ought to be shown, that continued West Indian fever always attains such degrees of violence, and that, before it becomes thus desperate, it displays a remittent type, as the condition which is here supposed to prevent the development of its true character must be the work of time, and is the precursor of death; that remittent fever is necessarily a milder disease in all cases, and makes less fatal impression upon organs essential to life; whereas it is known, that fevers, having distinct remissions, often end fatally in a few days, by the same destruction of essential organs. It ought in short to be shown, that continued fevers, in the case given, always terminate in death; because the condition which is said to render them continued, inasmuch as it prevents the remittent character from appearing by the destruction of organs, is a condition clearly incompatible with life.

2d. *The affection of the alimentary organs is peculiar in West Indian fever.* At the accession of almost all fevers there is nausea, and generally vomiting, first of the matters contained at the time, and, when the vomiting continues, of whatever is taken into the stomach, mixed often with bile. In West Indian fever there is sometimes, but not frequently, a small quantity of bile vomited during the period of invasion. But after the fever is formed, the

* Dr. Ferguson.

ejection of bile ceases, while the vomiting continues, and cannot, in a great majority of instances, be arrested till the disease be subdued. Thirst, especially in the congestive form, is not urgent, and the patient often drinks nothing; when he does drink, the quantity vomited is greater than the quantity swallowed, and it is considerable, often copious, when he does not drink at all. The fluid ejected is clear and colourless, sometimes sour, sometimes insipid. The patient lies prostrate, and often complains of nothing, save tension and tenderness in the epigastric region, and scarcely of those, except on the application of pressure; while he vomits the fluid in question two or three times an hour, during the first two or three days of the disease; and this constitutes its most valuable and certain diagnostic. It is valuable from the earliness of its appearance, and it is strictly entitled to the name of pathognomonic, inasmuch as it distinguishes West Indian fever, not only from all varieties of remittent fever, but from other kinds of febrile disease. After the vomiting has continued two or three days, the ejected fluid often loses its transparency, becoming turbid, brown, and finally black. Such is the general manner in which the *black vomit* comes on, though it sometimes, but rarely, breaks forth suddenly, without the continued precedence of the vomiting of colourless fluid. It is a late, and, with scarcely any exception, a mortal symptom, and therefore unimportant as a practical diagnostic; but in a pathological point of view it is doubtless of great moment, and strongly characteristic of West Indian fever.

By those who have maintained the existence of a peculiar contagious fever, in the places where the disease is found, *black vomit* has been pronounced a positive and perpetual diagnostic; while by those who have denied the being, not only of contagious, but of any peculiar fever in those places, by those who agree, that the disease de-

nominated yellow, pestilential, and Bulam fever, is merely an aggravation of ordinary remittent fever, it has been utterly rejected. It is neither so constant, nor so consequential, as the vomiting which generally precedes it, as just noticed; yet, that it is the result and sign of a peculiar disease, I see no reason to doubt. Were it otherwise, how does it happen that it is limited to particular parts of the earth's surface? How does it happen, that in the many mortal fevers observed on the intertropical shores of Africa and Asia it is scarcely ever found, while it occurs daily in the West Indies? It is notorious that the causes of intermittent and remittent fevers, in every variety, exist and operate in the former as well as in the latter situations. Why does *black vomit* appear in the latter and not in the former? It is idle to reply, "because the disease is more concentrated," without showing whence the concentration arises, and in what it absolutely consists; for if concentration mean accumulation of power merely, remittent fever sometimes possesses as much of power, or fatal force, as the disease in question. It cannot depend upon higher degrees of heat, or indeed upon any particular range of tropical temperature; for granting, that the requisite heat were limited to four or five degrees above 80°, or supposing, that the higher it arose the greater its power, which is more probable; according to either hypothesis the temperature required will be found to operate extensively on the shores of Asia and Africa, as well as many parts of South America, in which the disease is not found. Upon what else can this concentration, of which we have heard so much, and know so little, depend, which has the power of changing remittent into continued fever, in particular parts of the world only, and there, among other peculiarities, giving rise to *black vomit?* This most striking, but not most certain symptom, occurs very rarely indeed in other complaints; as symptoms of hydro-

phobia have arisen in a person, in whom there has been no evidence of poison from a rabid animal. In the latter it would not be denied, that dread of fluids, spasmodic dysphagia, convulsions, &c., were symptoms of a peculiar disease, the effect of a peculiar poison, though the cause in that instance could not be traced; no more should it be denied, that black vomit characterises a peculiar disease, because it has once in ten thousand times appeared in others. Though it does not belong to West Indian fever exclusively, and universally, it is as characteristic of that disease, as bubo is of plague, or red efflorescence of scarlatina.

But besides peculiarity in the matters vomited, there is peculiarity in the act of vomiting itself. In febrile disease generally, and in remittent fever more especially, the contents of the stomach are forced up with much exertion and pain; there is often retching, with severe spasmodic action, when the stomach is empty; and there is straining, with desire to vomit, after the vomiting has ceased. In West Indian fever, the stomach is emptied in an instant, and with scarcely perceptible effort. The evacuation is speedily completed, and when it is completed, the desire to vomit ceases, till fluid accumulates again, when vomiting recurs, and ends as before; and thus it recurs and terminates over and over again, throughout the disease, without distress in the act of vomiting, no more power being employed than is sufficient to relieve the stomach from its morbid contents.

About the time that the fluid vomited becomes black, there is discharge of similar fluid by stool, in most cases. The latter discharge sometimes precedes by a few hours, and sometimes follows the former; it sometimes does not appear conjoined with, and sometimes, though rarely, appears without, the other. It is sometimes unmixed, and then very exactly resembles the matters vomited; at other

times it is mixed with portions of mucus, and then has much the appearance of the dejections which occasionally occur in the last stage of dysentery; but it has not the same offensive smell, being either inodorous, or having a faint animal scent, at least generally speaking. Soon after this discharge takes place, the patient complains of sense of emptiness in the abdomen, and sinking within; whereas before he had been annoyed by feelings of fulness and tension. These appearances, connected with the condition of the intestines, I have thought it right to notice in an attempt to delineate the diagnostic character of West Indian fever, both because they form part of the affection of the alimentary organs, peculiar to that disease, and because they assist in determining the difference between it and other fevers; but I do not estimate their importance highly, taken by themselves; and I would not be guided by them, without the concurrence of other and more certain tokens.

In a somewhat similar point of view, I consider the appearances of the tongue and fauces. They exhibit, I am persuaded, much that is peculiar, and therefore much that might be useful to our purpose; but they are subject to so much variation, and differ so materially in individual cases, that it would require a long continuance and great closeness of observation, to render them generally available. I have not noted and studied them so as to be able to represent them minutely, or with any satisfactory degree of accuracy, as to relative expression; I shall only remark generally, therefore, that the morbid changes are not so striking as in some other fevers. They are not less important, but they would not give a stranger so strong an apprehension of danger. The teeth and fauces are rarely encrusted; and the tongue is seldom foul or loaded; it is often clean in an extraordinary degree, presenting that florid, denuded aspect, which it often exhibits in chronic diarrhœa. I have known a stranger to the disease, when he observed this

state of the tongue, the composure of the patient, and the absence of excitement and distress, generally attendant on fever, pronounce a favourable prognostic, when dissolution was not an hour distant.

3d. *There is a peculiar want of consent between the power with which West Indian fever impresses the body, and the symptoms by which it is manifested.* Hence the epithet "insidious," so often applied to it. While it is rapidly sapping the powers of life, there is often little to inform us of what is going on within. In many cases, the external signs are so obscure, so singular, and apparently so little commensurate with the internal things which they represent, that, till we have learned scrupulously to observe them, and ascertain their import, they do not prepare us for their frequently fatal issue. Scarcely any one has begun practice in the West Indies, without being surprised by the sudden approach of death in this disease, when he imagined things were going on well. In the midst of his security, he has been shocked by the eruption of *black vomit*, or the accession of profound coma, speedily followed by death; and mortified by the reflection, that he has been treating the disease in ignorance, and therefore without the use of appropriate or adequate remedies. It may sometimes happen thus, even to those who have seen much of the disease, studied it carefully, and acted most conscientiously; and I doubt whether the man is to be envied, who, having had much practice in West Indian fever, has not had some feeling of remorse connected therewith; because I doubt whether ever any man so circumstanced, and entertaining a proper sense of duty, has at all times been exempt from occasion for such feelings. Whatever his discrimination and assiduity may have been, he will more than once, perhaps, have to regret, that he was overtaken in carelessness and inaction, when he ought to have been watching and working.

The pulse is not rapid, considered as a febrile pulse; it seldom rises to 120, generally does not reach 110, often is under 100, and sometimes not more than 60 strokes in a minute. The temperature at the surface frequently does not exceed, sometimes falls below the healthy standard. The patient, in many cases, does not complain of pain; his tongue is clean; and he has little or no thirst. He keeps the recumbent posture, and vomits frequently a clear, bland, or subacid fluid. He sometimes complains of tension and tenderness in the epigastric and hypochondriac regions, but seldom excepting on the application of pressure; and when questioned as to his head, he will admit, that it is heavy and dull, but not painful, in the common sense of the word.

Such, in many instances, are the most prominent appearances, if I may so express myself, which present themselves in this disease; and according to the usual methods of measuring the force of an internal morbid condition, by the extent of its external effects, they would not indicate imminent danger. Yet when those appearances continue, without material change, for the space of two, three, or four days, *black vomit*, profuse hæmorrhages, and the peculiar petechial state of the skin which I have endeavoured to describe, will often follow closely, and to them death will soon succeed. Here we are struck forcibly with the want of correspondence between the internal force of the disease, and its external manifestations. There is surely something peculiar in this. Can such a disease have the same origin, the same qualities, and essential character, as remittent fever? Would a man accustomed to treat remittent fevers in other parts of the world, fear a fatal termination in such a case? On first seeing a patient, whose principal appearance of illness was a prostrate position, with frequent vomiting of colourless fluid for two or three days, would he apprehend the death of that patient on the day following,

preceded by *black vomit*, hæmorrhage, and party-coloured skin? He would indulge no such apprehension, till experience taught him to scrutinize appearances and fear the result; but he would feel strong assurance, that he had now to deal with a disease of a peculiar nature, and which he had not before encountered. Afterwards, from a desire to generalize perhaps, from specious arguments adduced by others, or from a wish to disprove the doctrine of its contagious power, which has generally been coupled with that of its peculiar nature, he might alter his opinion. But in the first instance, when he saw his patients die so contrary to his expectations, and in a way so incompatible with his former observations and opinions, without waiting to inquire about the cause, he would confess, that to him it was a new and most peculiar disease.

This want of correspondence between the positive and apparent power of the disease, is not however equally conspicuous in all its cases and forms. It is different in the varieties of *congestive*, and is not so striking in the true and more perfect *inflammatory* form; but it is noticeable in all, and obvious in a great majority of instances. Contèmplating the disease as a whole, it is one of its most remarkable features, and goes far to establish its distinct nature; to prove, that it is peculiar, not only in its attributes, but also in its essence.

4th. *The discoloration of the skin is peculiar in West Indian fever.* This, however, is the most irregular and unsatisfactory of its diagnostic symptoms; inasmuch as it is different in the two forms of the disease, the *congestive* and *inflammatory*, and varies considerably in regard to the individual cases of each. It is generally late in appearance: as far as it consists in yellowness, it sometimes does not become manifest, in fatal cases, till a short time before death; and when the disease terminates favourably, it frequently does not appear at all. In

many fatal cases, and in some fortunate ones, the skin, though discoloured, does not become yellow, but exhibits a peculiar party-coloured aspect, in which the most extensive and conspicuous hue is leaden, or black. Yellowness is common in remittent fever, and occurs in some others. An attempt therefore to prove that this is a peculiar disease, from the presence of any particular shade of yellowness, must fail; because it varies much in different cases when it appears; because it is frequently wanting; and because it is often found, of various tints, in other fevers*. There is some difficulty in founding clear diagnostic opinions, even on the discoloration of the skin, considered generally; and this difficulty arises from the differ-

* Mr. Pym is, I believe, the only writer who has attempted this; he informs us, that a particular tinge of yellowness, namely, that of a *pale lemon*, is, when it does occur, characteristic of the disease generally called yellow fever; which, according to him, is not only a peculiar disease, but one radically and essentially contagious; which is not the endemic production of the West Indies, America, or Europe, but is imported, whenever it appears in any of those places, from Bulam, from which place he has named it, and which, with Siam, appears to be the only place where he believes the disease in question to have originated. Had not Mr. Pym endeavoured to show, that the epidemic fever which prevailed at Gibraltar, in 1804 and 1813, was identical with that which is commonly called yellow fever in the West Indies, I would not have questioned the accuracy of the proposed diagnostic, having never seen the disease in Europe; nor would I, for the same reason, have presumed to dispute the truth of other doctrines which he has promulgated, in his work on *Bulam Fever*. If the fever which prevailed in Gibraltar, and that which prevailed and does exist in the West Indies under the name of yellow fever, are identical, I need not repeat, after what is written in the text, that a *pale lemon* colour cannot be admitted as a diagnostic symptom of the disease. It does occur, but not so often as other shades of yellowness, alone or mixed with other colours, and therefore cannot constitute a characteristic feature of the disease.

Another diagnostic of *Bulam Fever*, according to Mr. Pym, is

ence or contrariety of colour which it exhibits, consisting, as has been said, of various shades of yellow, and of a

excruciating headach, confined to the orbits and forehead, not affecting the temples. This appears an artificial refinement, which nature does not acknowledge; the line which separates the orbits and temples is a very narrow one, and insufficient to bound diseased actions, except perhaps in affections strictly neuralgic. In West Indian fever, I have never been able to perceive such a limit; the pain has been pretty generally confined to the forehead, extending from temple to temple, but was complained of most in the regions of the orbits. The *excruciating* headach, spoken of by Mr. Pym, cannot be admitted as a diagnostic; because in very many instances, and those the worst, there is little headach: there is sense of weight, giddiness, and stupefaction, not of headach, much less of excruciating headach.

Another peculiarity of the disease, according to Mr. Pym, is, that it cannot affect the same person twice. After the evidence to disprove this position adduced by Dr. Bancroft, Dr. Burnett, and Dr. Fergusson, evidence so clear and conclusive that it cannot be invalidated, much less subverted, by arguments however specious, or explanations however subtle, it were waste of words to discuss the question further; and I shall therefore only state, that I have seen positive proof of the disease affecting the same person more than once, in the West Indies.

Mr. Pym, while he endeavours to show that the fever, which has prevailed epidemically in the West Indies, America, and Europe, since 1793, originated in the island of Bulam (a place where I find no proof of its ever having existed, and that was a point which ought surely to have been established, before it could, in any sense, be considered its source), finds in Dr. Johnson's work on "Tropical Climates," an account of a disease prevailing at Edam, "in many respects resembling this fever," and cannot, it would appear, even fancy how it found its way from Bulam there. In this difficulty, and difficulties do not easily stagger the sturdy supporters of inherent contagion in this disease, he asks whether it may not have been carried from Siam to Edam. Of all the extraordinary explanations offered regarding the introduction of contagion, this is, I believe, the most extraordinary. Edam, at the time it was unfortunately and injudiciously chosen as an hospital quarter by the En-

compound colour, in which black is the most predominant hue. It would therefore be insufficient, by itself, to determine the character of the disease in sporadic cases. It is of little avail in any case, as far as the regulation of practice is concerned, since little can be effected by art after it is fully developed. Yet, that it is the specific result of a particular morbid condition, having the same cause probably, but different modifications, and that it is therefore symptomatic of a peculiar disease, will be admitted, especially by those who have seen much of West Indian fever, unmixed with other kinds of febrile disease. It will have appeared to them so constantly, considered as a whole, towards the conclusion of fatal cases, as to preclude the notion that it was the effect of fortuitous circumstances, and to induce the belief, that it arose from something essential to the disease, in which they saw it so constantly. They will acknowledge, that there must be some pathological peculiarity, and, by induction, some peculiarity as to origin, in a disease which so regularly, at some point of its progress, when fatal, produces the appearance of the skin in question. Is there any other disease in which it appears thus regularly? The question refers, not to yellowness alone, but to that in connection with the party-coloured appearance of the skin to which I have so often alluded. The

glish, was a small desolate island, uncultivated, and, I believe, uninhabited for some time before we occupied it. Now, that the contagion of a disease should move, no one knows through what channel, from Siam, a place where it has not been shown to exist, to Edam, and there, where there had not been subjects for its propagation, lie in wait till the arrival of the British, is totally incredible, and forms one of the most singular acts of imagination on record.

There are, in the history of the fever at Edam, the most convincing proofs of its local origin; and I venture to affirm, that when the soil, subsoil, and other localities, in connection with atmospheric heat, are examined, they will be found similar to those things in the West Indies where the disease mostly prevails.

last may appear, but it certainly, from all accounts, appears very rarely in the other precipitous fevers of tropical regions; it appears constantly in this, with some shade of yellowness; that is, in almost all fatal cases, and in many favourable ones, some patients exhibit the one appearance, and some the other. Now, if these things be so, I ask, whether, according to all the laws of evidence, the discoloration in question is not peculiar, and therefore a proof of West Indian fever being a specific disease? And that they are so, I appeal to every one who has treated the disease as an epidemic. I call to witness more especially those who have had to deal with it alone, and uncumbered with other kinds of fever having many points of similitude. In this view, the opinions of such medical men of the navy, as have seen much of the disease, are entitled to particular regard, and to them above others I beg to refer. It has appeared to them in its most pure, often in its most appalling forms. They have seen it in a few weeks affect almost every person in a ship, many of whom it destroyed; and they have observed the discoloration of the skin, with scarcely an exception in the last. To them also I refer, in a particular manner, for the confirmation of the different allegations regarding diagnosis; because, from their position, they have better opportunities of determining their justness than others, inasmuch as they may observe appearances, and draw conclusions, unembarrassed by various sources of perplexity which operate on shore. And to the profession generally I submit the question, *whether a fever, which does not remit; in which there is a peculiar morbid affection of the alimentary organs; in which there is peculiar want of consent between the power of the disease and its external manifestation; and which exhibits a peculiar discoloration of the skin, is not a disease sui generis?*

MEMOIR V.

Cursory Remarks concerning the manner in which the Cause of Fever, especially that of West Indian Fever, impresses the Body.

In the *apoplectic* species of congestive fever, there are no premonitory symptoms. After eating a hearty meal, and in an instant, the patient is affected with faintness, vertigo, and confusion of thought; sense of coldness, especially along the spine and in the extremities; deficiency of force in the circulation, the pulse varying much as to fulness and rapidity, but being generally small and much accelerated at first; incapacity of the lower extremities, and desire to get the head low; the patient being unable to walk alone, and when assisted, dragging his legs like a paralytic person; paleness and shrinking of the face, dilatation of the pupil, and a wild, alarmed, or idiotic expression of eye; diminution and depravation of sense; oppression at the præcordia, with laborious, interrupted respiration, and sighing; a cold and inanimate skin, sometimes dry and shrivelled, sometimes moist and relaxed; sweat, when it occurs, being generally clammy, adhesive, and cold, seldom liquid, scarcely ever warm.

Is there not here debility in the strictest sense of the word; and is it not positive and direct, rather than secondary and apparent? It has been the fashion, for a while, to ridicule the notion of debility in the first stage of fever; and to ascribe the apparent prostration of strength,

the manifest and manifold signs of weakness presented to us, to vascular oppression. But whence comes this oppression? Is it not the effect of precedent exhaustion? We cannot apprehend in what manner the causes of fever, whatever they may be, can produce, in a case like this, such instant effects on the vascular system directly, as those which are understood by oppression. We cannot conceive the immediate induction of that state, excepting by some palpable or mechanical agency, such as depression of the skull, beating in of the ribs, or internal extravasation. But we can conceive how the causes of fever, through the medium of the nervous system, may induce debility, and how the condition which has been called oppression may follow; these things we can conceive, though we do not understand the manner in which the first is occasioned.

It is customary to contemplate debility as an effect only, as an indirect condition therefore, the consequence of some foregoing derangement; generally, increased action in the circulating system. Hence the question so often asked—is it possible that a person, previously in health, can be so much reduced by the operation of fever in a few hours, as not to be able to bear the loss of a few ounces of blood? and the answer, in the form of a commentary—that the appearances observed, though they simulate the effects of exhaustion, do not arise from it, but from a contrary condition; that the vital powers are not weakened, but oppressed by a load of blood, and that therefore the appropriate remedy is the abduction of blood.

There is probably fallacy in this reasoning, and error, in many instances, in the practice to which it leads. The fallacy appears to be connected with the importance assigned to the vascular system in the pathology of fever, to the inadequate consideration, or exclusion, of the sensorium and nerves; and the error of practice arises partly from the same cause, and partly from the benefit which follows

blood-letting, when strong vascular action has followed, as it often does, the previous nervous exhaustion.

The symptoms detailed above, as characterizing the accession of a particular form of fever, give strong evidence in themselves of direct debility. The attack is instantaneous, and there is no indication of increased vascular action, but of the reverse there is the strongest assurance; sometimes it succeeds, sometimes not. When it does not, to this state of exhaustion and torpor are added congestive oppression, &c.; and without one sign of rallying, life is destroyed. Would it be wise to remove blood in this state? It will not be affirmed that such a measure is likely to restore the nervous exhaustion, on which as first morbid condition the phenomena depend. Besides, while it is clear to me, that the state of body in question does arise from diminution of nervous power, without the precedent agency of the vascular system, it does not appear that the converse is true, namely, that blood may be abstracted without reducing nervous energy. This is stated in general terms, and without reference to organic pressure from an external or local cause. According to this hypothesis, the elder physicians were not always wrong in connecting the primary symptoms and constitution of fever with debility; and blood-letting cannot be admitted as an universal remedy in its treatment.

It is often imputed as matter of reproach to medical men, that they generalize too much in theory, and dogmatize unreasonably in practical directions; that they construct systems more under the domination of caprice than the guidance of nature; that they misrepresent, if they do not misapprehend morbid appearances, so that they may bring them within the forced limits of their systems, and then lay down a set of fixed rules, by which the whole, changeable and ever changing as they are, may in all times and places be managed. There may be some exaggeration in the

statement, and more blame perhaps is imputed than is deserved, yet it cannot be denied that there is a just foundation for the charge, as will appear, more especially, from the doctrines of fever, within the last thirty years. During that period, idiopathic fever has, at one time, been held as the offspring of debility, and, at another, of genuine inflammation of the brain, or other internal organ. In the first case blood-letting was utterly proscribed; it is declared to be universally essential in the last, and is too often regarded as a kind of specific, possessed of sovereign power in itself to subdue the various and mysterious deranged movements which constitute fever, nothing being necessary to that effect, but to urge the evacuation, with unshrinking hand and unhesitating resolution.

The contemplation of fever in the West Indies, in the distinct forms of *inflammation* and *congestion*, and their varieties, would go far, if they were not sufficient, to destroy the specious structure of such exclusive theories, and to show the absurdity of the exclusive rules of practice founded on them. The description of fever at the head of these remarks was copied from what I observed over and over again, in that part of the world, as characterising the *apoplectic* species of the congestive form of the disease; and it were impossible, by any sophistry of argument, or stretch of imagination, to make that description represent inflammation. It was the very type and image of debility, followed speedily, or accompanied by a condition of the blood-vessels widely different from that which obtains in inflammation, *viz.* gorging of the venous trunks, and inanition or emptiness of the capillary vessels. This condition would not, on dissection, be taken for, or described as, inflammation. If it were, and if it be conceded, that inflammation may have existed, and occasioned death, without leaving a trace behind it—for even this has been maintained by the supporters of the inflammatory hypo-

thesis—then all arguments deduced from *post mortem* appearances, as well as from the aspect of the living body, were offered in vain. It would be more idle to attempt shaking the faith of that party, by any evidence which can be drawn from analogy; but to those who do not see their way so clearly, who find fever by no means so simple in nature or uniform in character; to those who have doubts as to the constitution, and difficulty in the treatment of the disease, even the feeble light derived from this source may not be unacceptable.

When a person falls from a height, it frequently happens that the condition known by the name of concussion of the brain is induced. There is neither fracture, nor organic lesion of any kind, yet the subject of the accident lies insensible, with a scarcely perceptible pulse, cold extremities, and dilated pupil. In some instances death follows almost instantaneously, in others violent inflammation follows. In the cases which terminate suddenly in death, it is impossible to account for the event. No disorganization can be detected, nor any vestige of disorder in the vascular or solid parts of the system; yet was the nervous energy so exhausted—I know no more appropriate terms—as to occasion death. Much misunderstanding and useless discussion have arisen from the vagueness of medical phraseology, and the misapplication of words, and it may be said that the state in question is not one of debility. To me it appears a state of the body, to which the term debility may most rigorously be applied, which indeed can be expressed by no other term, unless by a synonyme. But waiving the question of the propriety of the terms, it is enough for the present purpose, that bloodletting could not be reasonably employed in this affection. Medical men are not agreed about the proper line of practice, in the second stage of concussion of the brain, but I believe no one would be hardy enough to order such an

operation, in its first stage, which may be so short as to escape observation, but is nevertheless most peculiar in kind and character. When inflammation or congestion, or a combination of excitement and oppression consisting of both, have followed in succession, a different species of derangement and of phenomena are manifested, calling for different measures; but in the state in question, when the body labours under the first impression of what has been called shaking of the brain, from our ignorance of its true nature; when the surface is cold, the pulse fluttering or imperceptible, the pupil fixed, respiration almost suspended, and sense lost; in this state no man, who has learned any thing of the animal economy, would abstract blood, or rather would attempt it.

Hence, whatever be the nature of the febrile cause, it *may* be capable of inducing suddenly a condition of the body directly, through the nervous system, without any *wear* and *tear* of the vascular, which may properly be denominated debility; and hence it may be apprehended, I think, how a man in perfect health may be so operated on by fever in a few hours, as not to bear, without detriment and danger, the loss of a few ounces of blood. How mere precipitation occasions the state of the body called concussion, and in what that condition consists, we do not know; but we know certain effects thence arising, and we know that those effects are induced in an instant. If we confess that we know little more of the cause of fever, and the manner in which it acts, we may also confess our ignorance of the extent to which it may operate directly on a particular tissue, and thereby on the body generally, and may therefore conceive, though we do not fully understand, how it may occasion a change in the system, so far similar to concussion, as utterly to interdict blood-letting.

Something similar happens occasionally in gun-shot

wounds, in great operations, and from the action of particular gases, as well as from strong and sudden emotions of the mind. When a limb is struck off by a cannon ball, there are often tremors, coldness, faintness, and the most striking aspect of debility; a condition sometimes issuing suddenly in death, sometimes followed by strong reaction. When death accompanies, or speedily follows a great operation, with the loss of only a few ounces of blood, and without previous exhaustion, how is it occasioned? When life is extinguished in an instant, by the operation of particular gases, how do they operate to that end? Not by the mere suspension or destruction of the functions of respiration; because in other cases those functions may be much longer interrupted without such fatal effect. Between those affections and fever, there appears a very close analogy, both in the tissues affected, and the succession in which they are affected. The tissues affected are, first the nervous, then the vascular, and through them, directly or indirectly, in different degrees, every part of the body. Sensation, excitability, and action, and the organs of their manifestation, are so closely associated, that it is difficult to say which are primarily affected, and which by implication; but in the cases to which I have alluded, the nervous tissue appears to suffer the first impression, from considering the suddenness of the effect. It is like that of electricity, and it is consonant with our notions of the nervous constitution, that it may be suddenly and strongly impressed by particular agents; the kind of effect also, privation or depravation of sense, tremors and coldness, depression of the heart's action, and suspension of that of the capillaries, leads to the same conclusion. This condition of the heart and blood-vessels may succeed the nervous exhaustion so suddenly, as to appear simultaneous, but it is in relation to it, as effect to cause, and therefore secondary. In a short time, if life be not destroyed by the

first stroke, congestion, or inflammation, and their consequences, follow, the vascular derangements rising as the nervous recede.

The manner in which the nervous tissue is affected, positively in itself, and relatively to others, is a question of much, perhaps insuperable difficulty, till more knowledge shall be acquired of the animal economy, more especially in regard to the brain and nerves. But, though we know nothing of the nervous power, as to its essence, or manner of operation, we are warranted in saying that its agency is either *impaired* or *impeded* by the first influence of the febrile cause; as vertigo, faintness, nausea, rigor, and failure of the senses testify.

It is probable, that in some cases the nervous power is *abstracted*, in others only *obstructed*.

The first condition is induced, I believe, in pure congestive fever (for a certain degree of congestion happens in all), and the extent of congestion is commensurate with the quantity of *abstraction*. In the worst form of congestive fever, it would appear that the nervous power is so completely withdrawn, and receives so little supply, as to be unequal to the carrying on of the functions of life; that it is so subverted, as to be unsusceptible of repair, and that death is the effect of direct exhaustion, and consequent stagnation. In the slighter forms, it appears, though the nervous power is *abstracted* to a certain extent, that sufficient remains to supply sensation, and *something* essential to vital action, till, in favourable circumstances, the loss is repaired, and derangement appears chiefly in the vascular system.

On the other hand, when the nervous power is only *obstructed* by the febrile cause, the inflammatory form of fever will follow, the force of inflammatory action being proportioned to the extent and duration of the *obstruction*. In this form of the disease, as in the other, the first step of

derangement in the vascular system will be congestive, but greatly different in its nature and results. Here, as soon as the cause of *obstruction* is exhausted, or rather perhaps arrested, by some inherent power of the living body, reaction takes place, various in degree in various instances; in some rising to the most intense inflammation, destroying organs essential to life, by a process somewhat like gangrene, in others proceeding, with less violence, to a regular issue in suppuration, and in others affecting chiefly the investments of viscera, and with tendency to terminate in effusions of serum and coagulating lymph.

I have seen a remarkably stout young man, of sanguine temperament, labouring under the first impression of fever, which literally struck him down in an instant. With a rapid, weak, irregular pulse, sunk eye, haggard countenance, and cold skin, he lay prostrate in great distress, but incapable of describing his sensations. There were languor and feebleness which he tried in vain to resist; confusion of thoughts; tremors, not like the rigor of intermittent fever, but an affection like that which arises from alarm; a feeling of extreme, but unspeakable suffering, extending from the spine to the umbilicus, involving the abdominal contents in tumult; and a state of the skin in which reduction of temperature was not the most striking, though most easily defined, deviation from that of health. When the hand was applied to the surface, there was communicated a sensation, similar to that which is experienced by touching the scalp, when separated from the subjacent muscles, a sensation of want of vital connection with the parts beneath. And yet in two hours all those signs of diminished vitality had disappeared, and the most furious inflammatory action followed. The case was a fatal one. Bloodletting and other means of reduction were urged to the uttermost, without making more than momentary impression on the disease. The force of the circulation was

for an instant abated, and then rose again to the height of its former violence; and thus over and over again, till the structure of the brain was subverted.

Now, in contemplating such a case, we are first of all struck with the two great stages into which it is divided, and then with the necessary connection between them. In the first we see the nervous power *obstructed*, or suspended in its operation, not arrested in its source or abstracted from the system; and in the second, we observe, when the suspending or *obstructing* cause is removed, the force accumulated by inaction and continued supply, poured forth with irresistible impetus, and giving rise to such intensity of inflammatory action, as puts to defiance all our means of staying its course. At least, in comparing the two phases of the disease, the subsidence of its first, and the rising of its second stage, the conviction is forced upon us, that they are not fortuitous and inconsequential, but are necessarily connected the one with the other, as cause is with effect; and moreover, that in the origin and progress of this disease there is something essentially different from what happens in congestive fever.

There we observe, as in the first stage of this, failure of nervous, and feebleness of vascular, power, but of a much deeper and more enduring character. In the inflammatory form of fever, the stage of exhaustion, connected with nervous *obstruction*, is always short, sometimes imperceptible, or rather, in the common routine of complaint and inquiry, it is not perceived. But in the congestive form, arising from the *abstraction* of nervous power, the exhaustion lasts long, in the worst forms, till the last. There is, analogous to what happens in inflammatory fever, failure of nervous power and vital action; but, unlike what happens in that form of the disease, the reparation is slow and scanty, or altogether wanting. In some there is no supply consequent on *abstraction*, in others there is supply, but it is

always slow in accession, often inadequate in quantity. It is, when compared with the overflowing energy of inflammatory fever, as a summer brook to the force and celerity of the Ganges.

According to these views, the foundation of fever is laid in the nervous system, on which are placed two separate bases, the basis of inflammation and of congestion, proceeding in opposite courses, and terminating by different acts, whether the termination be in the restoration of organic and functional integrity, or in death: and, according to the same view, it will be seen, that, though the cause of fever may be one and uniform in its essence, it is not so in its products; but that, in consequence of circumstances affecting itself, or the state of the subject, or of the original temperament of the subject, it gives rise to forms of disease radically and thoroughly different, in regard to development of character. Hence all discussion, respecting the nature of the cause of fever, whether it be debilitating, stimulating, irritating, or have other uniform power of subverting health, may be abandoned; for, although the primary effect may properly be called debilitating, as manifesting the *abstraction* or *obstruction* of nervous power, and reduction of vital energy, the general aspect and tendency of the disease is so often of an opposite, or at least of an inflammatory character, that any argument founded on the phenomena of fever, to demonstrate the attributes of its cause, must be unsatisfactory, if not illusive. Hence likewise an important practical inference is deduced.

We infer, that since there is such difference in the basis and structure of fevers, there ought to be essential difference in the means employed for their subversion; which we find confirmed by experience of every kind, whether derived from signs in the living body, or the appearances presented by the various organs of the dead.

The remedies used will be the same, or similar, but the principle and object of their application must be different. Thus blood-letting, purging, and blistering, are generally employed in both cases, but the modifications in the use of each, as to time, manner, and quantity, will render the results as various as the principles on which we direct those modifications are different.

In the inflammatory division of fever, blood letting is the most important remedy, and therefore we resort to it early, conduct it rapidly, and carry it to a great extent; but in the congestive, we must wait and watch for the opportunity of resorting to the same remedy, and must employ it with much caution; lest by precipitancy, or carrying the evacuation too far, we should weaken the heart's action, instead of removing the load by which its action is impeded, and thus increase the evil which it is our object to remove. Besides waiting and watching, it is necessary to prepare the body by artificial means for this evacuation. The aid of warm bathing, frictions, diffusible stimuli, and calomel, must be called in, to induce a condition analogous to inflammation, before we are justified in the abstraction of blood. We are compelled to rear with one hand what we would pull down with the other; to nourish a pest, if I may so express it, for the purpose of destroying it more effectually. How formidable then must this form of fever be, when compared even with the most intense kind of the inflammatory; and how inadequate, in many instances, our means of overcoming it; seeing, that after the loss of time, and the anxious employment of preparatory measures, even when they succeed in their object, the patient is in greater danger than at the beginning of inflammatory fever!

Purgative medicines constitute a valuable class of remedies in both forms of fever; but in the inflammatory, they are only of secondary importance, while in the congestive,

we are frequently under the necessity of trusting to them as our principal evacuant, beginning with, and depending on them throughout; for though mercury generally acts as an evacuant, when strikingly useful, it is presumed that salivation, and increase of other secretions, are more signs of beneficial changes than benefits in themselves. Purges can, in most cases, be got to operate from the beginning, with advantage. By stimulating to increased action and secretion the intestine villous vessels, they, in proportion, relieve the congestive condition of those vessels, and of those from which they arise; and though these remedies do not determine to the surface directly, yet, by contiguous sympathy, they so far increase the action of the heart and great vessels connected with it, they give so much propulsive power to the central organs of circulation, as to render the application of sudorifics more easy and effectual; while, by management, they may be made to act on the urinary system, producing considerable diuresis, an excretory evacuation of great importance in the healthy body, which is probably too much neglected in fever, from its power of removing deleterious matters, independently of the relief afforded to loaded vessels, and which, in the present case, is the most important. This class of medicines, therefore, furnishes a powerful engine for relieving congestion. Through the medium of the alimentary canal, they excite the vessels of contiguous parts, arteries, veins, absorbents, and secernants, exciting on one hand, and evacuating on the other, urging the torpid vessels to propel their contents, and opening the natural outlets for their expulsion; effects of great value in themselves, and having their value increased in congestive fever, from the consideration, that they do not directly impair, but rather increase the energy of the heart. I have been led into these remarks on the utility of purgative remedies, in showing how the modified use of the same means may conduce to

different issues, not to estimate them at the highest rate in this form of fever; for, in many respects, I think mercury a remedy of much geater importance; on its uses, however, it is not my object in this place to enter.

In the application of blisters, the same principle will lead us to such regulations as shall contribute to corresponding variety of results. When symptoms of congestion are imminent, and death is apprehended from paralysed vessels and stagnating blood, blisters may be employed from first to last. In the worst cases they will often not act; but by previous frictions with spirits, and the use of the strongest epispastics, we should endeavour to secure their operation, applying them extensively, and without fear of their being misplaced. Such practice would be injudicious and detrimental, if employed indiscriminately in inflammatory fever. Till the ardour of vascular action be reduced, blisters are inadmissible to the surface directly over the inflamed organ. If used at all, they ought to be ordered on the principle of *revulsion*, though even with that view they had better be omitted. If the brain, for instance, be the organ in danger, blisters should not be applied, if applied at all in the high state of excitement, to the scalp, but to the neck, and between the shoulders. But when the pulse begins to flag, the skin to cool, and there is feeling of weight and confusion rather than acute pain; when after blood-letting, purging, &c., congestive symptoms come on, blisters, applied as near as possible to diseased organs, will render essential service, and should therefore not be neglected.

The same principle will apply to, and similar results be obtained from, the regulated use of other remedies; but it is unnecessary to prosecute the subject. By thus timing and adjusting the curative agents common to both forms of fever, and by appropriating those that may be considered more peculiar to each, such as mercury in congestive, and

cold affusion in inflammatory, we render assistance to nature, in her weak and ineffectual efforts to throw off, by different processes, the manifold evils of fever. There is scarcely any disease in which the sanative powers of nature are so prostrated as in the *apoplectic congestive* fever, and it seldom happens that so much is required of art, where it can do so little; so in most cases the disease ends in death, in spite of our best contrived and best executed endeavours. Still, in struggling with a disease so deadly, we must ply our means, in a line like that which I have drawn, a line of operation distinct from that in which we oppose *inflammatory fever*.

In the *congestive* we must not bleed till the disease has made progress, and exhibited some change of character; and then we must conduct the operation often sparingly, always cautiously, and with most scrupulous regard to its effects; in some cases it is inadmissible throughout, in others it may be carried a considerable length. In the inflammatory, on the other hand, it must be employed early, extensively, and perseveringly, there being little hope from other means, if the disease have much intensity. In the congestive form of the disease we draw blood, in expectation, as the heart is weakened by the febrile cause, and cannot propel properly the healthful quantity of blood, that, by diminishing the matter to be moved, we may facilitate the action of the moving power, which may be considered, for temporary purposes, equivalent to increasing absolutely its momentum. We imitate the policy of an engineer, who, when the motive power is so much impaired as to be inadequate to moving properly the whole range of machinery, turns off a number of the wheels, that he may have the remainder wrought effectively, rather than the whole should stop, or be continued imperfectly. But this is a mere mechanical view of the subject; and between the movements of the living body and those of dead matter,

comparison can be carried but a short way with propriety. There is little analogy between hydraulics and vascular action; and of the many theories of animal life and economy, which have become at once wrecks and beacons, none now appears more frail and unfit for its purpose than that of Pitcairn and Borelli. When the engineer's propelling power is diminished, by arresting part of the machine, he knows with certainty, that the remainder will be moved efficiently, and that a determinate object will be gained. But the physician possesses no such knowledge. The apparatus which he has to direct is an animated body, complicated not only in parts but in systems, of the nature and connection of which he knows so little, that his deliberations are embarrassed, his determinations often erroneous, and his operations consequently unsuccessful. In congestive fever, he observes the heart oppressed by a load of blood, and would, if he acted on mere mechanical principles, remove blood in confidence of affording relief. But the question presents itself — is not the matter which oppresses, the power which moves, the heart, or at least the chief agent in that office? And further to embarrass him, other questions arise regarding the condition of the nervous system, and its connection with the vascular. Such considerations, while they multiply the difficulties of decision, satisfy him that abstraction of blood must often fail in the cure of congestive fever, and that, if resorted to indiscriminately, it must not only often fail, but in many cases greatly aggravate the evil to be resisted. When it ought to be resorted to, or refrained from, constitutes one of the most difficult and important questions in medical inquiry; a question for the determination of which little direction can be given; but in which careful experiment must be our chief guide, and for conducting which, much caution, industry, and discrimination are requisite.

Whether or not these notions be just, must be deter-

mined by future observation and inquiry. It may be said, that they are true only in as far as they are borrowed, and erroneous as furnished by me. Of this matter, however, I can form no proper judgment, and will not attempt to anticipate that which may be awarded by others. They appear to have a reasonable foundation, and I therefore submit them to the profession, crude and imperfect as they are. If I be right in my belief, if these opinions be correct, there will follow from them conclusions regarding the nature and agency of fever, different from those generally entertained. It is not my intention to propose a theory of fever: remembering the splendid names which have been obscured in this department of science, I am warned from such an attempt, and feel that it would be presumptuous, and, in a tract like this, impertinent. It is my intention merely, by considering some of the foregoing facts and arguments, in connection with each other and the ulterior phenomena of fever, to offer a hypothesis which shall account for the more constant and remarkable symptoms of the disease.

The cause of fever acting primarily on the nervous system, induces therein two conditions of derangement. In one condition there is *obstruction*, in the other *abstraction* of nervous power. The first condition is followed by inflammatory, the last by congestive fever, as effect follows cause; the inflammatory and congestive states pervading the whole system of vessels; and herein fever differs widely from original inflammatory diseases, such as gastritis or phrenitis. In fever, the inflammatory and congestive states, though more striking in particular viscera, or venous trunks, are perhaps not less important in the capillary vessels, whether of the skin, internal secreting organs, or other parts of the body. The active state of the vascular system in inflammatory, and the passive state in congestive fever, extends from the heart to the most distant termina-

tions. The first condition may be denominated *erethism*, the last *quiescence*. By *erethism* I understand, not only increased action, but also tenseness and irritation. *Quiescence* means, not rest properly, but inaction from want of power.

Now the vascular *erethism*, extended to the capillaries, occasions one of the most remarkable appearances of fever, the arrest, general or partial, of secretion. Hence the dry skin, parched mouth, thirst, constipation, and scarcity of urine; hence, increase of heat, and therewith increased irritation, from the retention of matters which ought to be thrown off. Hence the aggravation of the inflammatory process in one or more of the viscera; for the mass of blood being scarcely reduced by secretion, a state of plethora arises: and irritating matters being retained, as happens either when there is no secretion at all, or when, taking place imperfectly, absorption instead of excretion follows secretion, the heart is stimulated to more impetuous action, and greater violence is done the inflamed parts, seeing the force of the disease will continue to fall on the parts which were first, from whatever cause, inflamed. This general condition of *erethism* and its effects therefore, must render inflammation in fever much more dangerous and intractable than when it is simple and original.

The quiescence of the blood-vessels, consequent on *abstraction* of nervous power, in like manner, pervades the whole system, extending from the heart to the capillary terminations, and giving rise to the phenomena which most palpably characterize congestive fever. The heart is incapable of adequately circulating the blood, and it therefore accumulates in the great trunks near the source of the circulation, and chiefly in those vessels, the action of which is naturally languid, *viz.* the mesenteric, splenitic, and hepatic. Hence arise tension of the hypochondria, and oppression at the scrobiculus cordis. And though general

congestion of the lungs, to any great extent, is seldom observed, such condition being incompatible with life, the pulmonic circulation is low and imperfect, as sighing, gasping, yawning, and other instinctive efforts to facilitate the flow of blood, evince. Hence duskiness of aspect, and darkness of the blood, as drawn from veins. There is seldom acute pain in this form of the disease. Blood accumulating in the large internal organs occasions swelling and tension, and especially in those of the abdomen, from their structure, economy, and position; so that the patient says he has no pain, but shows signs of uneasiness when pressure is applied. At the same time the secernant vessels of those organs either cease to act, or act inordinately, their *chemico-vital* action being perverted, and furnishing wrong products. Consequently there is either obstinate costiveness, or frequent discharges by stool of watery or mucilaginous consistence, and of various colours, pale, olive, brown, or black. The most loathsome and fatal symptom of West Indian fever, the *black vomit*, appears to arise from this perverted chemico-vital action of the secernants in the stomach, intestines, and liver, especially in the first. On dissection, the matter of which it consists is found, not only in the *alimentary canal*, but likewise in the *ductus communis choledachus*, *gall bladder*, and *hepatic ducts*; and it appears that nature, in her distress, to relieve the turgid vessels of those viscera, pours forth the redundant blood through secretory vessels, which, though from perverted and impaired power they have not the faculty of furnishing the ordinary healthy products, are yet capable of changing the blood to the state in question; a change similar probably in extent, though not in kind, to that which takes place in the menstrual discharge. It is unnecessary to repeat arguments to prove, that the matter of black vomit is neither vitiated bile nor extravasated blood, since it possesses not the properties of either, but is as much a

peculiar product as the matter of any other secretion. It is peculiarly a symptom of the congestive form of West Indian fever, is not observed in the intense inflammatory, but sometimes appears towards the close of the more moderate species, when the *erethismatic* has passed into the *quiescent* condition, as occasionally happens when the ordinary terminations of inflammation do not take place.

Mr. D. an intelligent medical man, was my patient under a fatal attack of congestive fever. From first to last he had no pain, and, excepting a sense of peculiar debility, and of weight and tightness across the forehead, made no complaint. He suffered no intellectual derangement, till near death, and conversed with me calmly and clearly respecting his sensations and condition. There was fulness in the epigastric and hypochondriac regions, and some impatience of pressure there. The skin was alternately clammy and dry, with chilly feeling, and desire to cover himself with blankets; and the pulse had the peculiar want of energy which accompanies congestive fever. From the beginning the stomach was irritable, with the bringing up of large quantities of clear, sour-tasted fluid, which was succeeded in the course of the second day, by *black vomit*, and dark dejections by stool. I do not attempt to estimate the quantity of the former; but some opinion may be formed of its amount, when it is stated, that the vomiting continued thirty-six hours, that it recurred two or three times every hour, and that each time the stomach appeared to be distended with its contents. All this time there was no pain in the epigastrium, or elsewhere, but, according to his own account, the stomach gradually became uneasy, as if by flatulence, and in the same manner as when the ejected fluid was clear and colourless, till sensations of distension were irksome; then he felt the œsophagus assume inverted action at the pharynx, which, rapidly descending to the cardia, was followed by

full and forcible vomiting, but without pain or straining. After that he was easy for some time, till the same train of sensations followed, and terminated in the same way. In reflecting on this case, and comparing it with others of similar character; when we consider the nature of the fluid ejected, its immense quantity, and the absence of pain, a process of new and morbid secretion must suggest itself, as the means, I was about to say the only means, of its production; and it will also appear, that this secretion was connected with the turgid and paralytic condition of the blood-vessels, and that together they are constituent parts and features of congestive, not of inflammatory fever.

The state of the skin in this form of fever is strikingly different from its state in the inflammatory. Here its temperature is generally under that of health, and it is either covered with clammy adhesive sweat, or is harsh and inanimate, like a piece of leather. The natural secretion is either arrested or perverted, as in the chylopœtic viscera. The blood scarcely circulates in the cutaneous capillaries; and it is perhaps from some change impressed on this fluid, as well as from perverted action in the secernants, that secretion, when it takes place, is so different from healthy sweat; it would appear that the serous part of the blood oozes from the torpid vessels, as from lifeless tubes, having undergone little change in its passage.

Intimately connected with this perverted action of the secernants and deterioration of the blood, I believe that appearance of the skin to be, which is so generally observed in the last stage of congestive fever, and which I have endeavoured to represent; the dusky, party-coloured maculae, beginning on the breast, and extending over the body, so different from the bright, diffused, yellow colour of the skin in the inflammatory form of West Indian fever.

I have stated my belief, that the morbid secretions in congestive fever depend, in part, upon a deteriorated state of the blood, and partly upon perverted action in the secreting vessels. Deterioration is a vague term, but it would be difficult to find one more definite, which would not convey an erroneous idea. One thing is very remarkable in blood drawn from the veins of a person labouring under congestive fever, namely, its dark colour, and this becomes more striking when compared with the venous blood of patients in inflammatory fever. In congestive fever the blood is much darker than in health, it is almost black; in inflammatory fever, on the other hand, it has a bright scarlet colour, approaching so nearly the appearance of arterial blood, that I have more than once been startled, on its first flow, from an apprehension of having wounded an artery. In what other respects the blood of patients under the two forms of fever differ from each other, and from healthy blood, and on what the changes depend, I do not pretend to say; but that they do differ materially in other respects must be admitted. At first sight it might appear, that the changes from the healthy state, and the respective difference of each, were dependent on the circulation alone; that, in congestive fever, from the weak and imperfect action of the heart, and the blood not passing with sufficient vigour through the pulmonary circle, it was either not defecated of some injurious quality, or did not receive from the air some essential principle, whence arose the accumulation of carbon, or whatever is the cause of colour, in venous blood; and that the reverse having place in inflammatory fever, was the cause of the increased brightness of colour in the blood, and the other changes connected therewith.

There may be something in these matters; but I apprehend that the original cause lies deeper, that the condition of the blood in question is coeval with the existence of

fever, and is *part* and *parcel* of its constitution. I believe that the cause of fever, acting through the medium of the nervous system, impresses the blood directly, in congestive fever diminishing its vitality, and in inflammatory fever increasing it; and that the difference of colour, and whatever else of difference there may be, are connected with difference of condition in that respect. This belief is founded chiefly on the fact, that the colour of the blood is the most constant symptom of the two forms of the disease, respectively, continuing, more or less, from the first to the last periods, in which we have an opportunity of judging from inspection. In treating the two forms of fever, we endeavour to make each approximate the other; seeing them occupy the opposite extremes of the febrile range, it is our object, by elevating one and depressing the other, to bring them to a mean point, the nearest to healthy action, whence they may proceed in parallel directions to restoration. But when this is accomplished; when by reducing the force of circulation in inflammatory, and increasing it in congestive fever, we have destroyed, in great part, the character of each; when the functions are retrieved, and convalescence commences, still the appearance of the blood peculiar to each, in some measure, continues, and does not perhaps entirely cease till cure is complete.

It has been remarked, that there is much difference in the state of the skin in the two forms of the disease, and that in the inflammatory, the temperature is above, while in the congestive it is under the healthy standard. But besides this difference in degree of heat, there is also difference in manner. The heat, in inflammatory fever, is superficial, free, and diffused, and impresses the hand instantly and strongly; in the congestive, it must be sought for. The surface generally is cold, but at the pit of the stomach there will be found a confined, deep-seated,

smouldering heat, on applying the hand with some pressure. The heat is evolved by the abdominal viscera principally, and the integuments act like dead matter, through which it is communicated, but by which it is not furnished; for the blood is accumulated in the parts beneath, and scarcely reaches the parts which the hand touches. The heat and the manner of its evolution in this case are, when compared with similar things in inflammatory fever, as the languid and imperfect combustion of a charcoal pile, to the conflagration of flax.

As the weight of the disease falls on the abdominal viscera in congestive fever, so, in the inflammatory, the encephalon is the chief seat of distress and danger. This organ, it is obvious, cannot exhibit appearances of such extensive congestion as the organs placed in the abdomen; but though incapable of such congestion, as to quantity, it may not the less suffer from its effects; and such appears to be the case sometimes, when, as happens in the worst cases of apoplectic congestive fever, the patient dies in a few hours, like one labouring under concussion of the brain. Still, in general terms, it is true, that the chief seat of congestive fever is in the abdomen, and that of inflammatory, in the cranium. In the first instance, there is no complaint of the head, though the patient, on being interrogated, will admit that there is sense of heaviness and torpor; while the whole aspect and demeanor indicate deficient energy, physical and moral. He generally lies quiet, letting the head sink from the pillow, and appears to desire nothing but to be left to his fate, till within a few hours of death, when he would leave his bed, and crawl or roll about unconsciously. In the inflammatory form of fever, on the other hand, there is manifestly strong action in the brain; the temporal arteries act vehemently; the face is flushed; the eyes inflamed and impatient of light; and,

when the membranes are implicated perhaps, complaint of violent pain, the patient crying out that his head will burst; in a short time delirium, often furious, comes on, accompanied with horrid imprecations, attempts on himself and attendants, and convulsions, speedily followed, in most cases, by death. But here the abdominal contents suffer little comparatively; there is little epigastric tension; and with insatiable thirst, in the most intense variety of the disease, I have seen the stomach tranquil and retentive from first to last.

It may therefore be said, with a few exceptions, from which no doctrine in pathology can be free, that the encephalon is chiefly affected in inflammatory, the abdominal viscera in congestive, fever.

THE END.

LONDON:
PRINTED BY CHARLES WOOD AND SON,
Poppin's Court, Fleet Street.